EARLY SCIENCE IN OXFORD

VIII

EARLY SCIENCE IN OXFORD

BY

R. T. GUNTHER

VOL. VIII

THE CUTLER LECTURES OF ROBERT HOOKE

OXFORD
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PREFACE

THE reissue of these six tracts by Robert Hooke is to be justified by their rarity. Although not so scarce as his first little booklet on Capillary Attraction the complete series of *Lectiones Cutlerianae* has become one of the rarities of scientific literature, and few physicists now have the opportunity of reading in the original these papers written 250 years ago by their predecessor. With the great monograph *Micrographia*, they present a good picture of the many-sided character of his scientific studies and of the ingenuity of his mechanical genius.

The text has been reproduced by Messrs. Percy Lund's Replica process with the addition of a continuous pagination. It is unfortunate that Hooke's own tables of contents to Tracts II and IV, on pages 31–7 and 213–16, have thus been rendered less helpful than they would otherwise have been, but it is hoped that the provision of a short comprehensive index may be some compensation.

I have been informed that Hooke's *Diary* in the Guildhall Library, to which reference was made in the Preface to Vol. VI of this series, may be published at no distant date with another important Diary for 1688 to February 1690 recently recognized as his by Mr. W. H. Robinson. This work, MS. Sloane 4024, was disguised under the title of 'Petiver's Diary'. Although there are only 96 small leaves, measuring

$3\frac{1}{2} \times 2\frac{1}{2}$ inches, Hooke managed to pack a vast amount of information on all manner of topics into them. They give one a vivid picture of the busy life he continued to lead even in his sixty-fourth year.

In conclusion it may be reaffirmed that much of the success of the Royal Society in its infancy was due to Robert Hooke. No one more richly merited some share of the eulogium which the poet Cowley addressed to that precocious body:

> With Courage and Success you the bold work begin;
> Your Cradle has not idle bin:
> None e're but *Hercules* and you could be
> At five years Age worthy a History.

R. T. GUNTHER.

THE OLD ASHMOLEAN,
OXFORD,
January 1931.

ADDENDA TO VOLS. VI AND VII

To p. 246.

Variation of Magnetic Needle.

June 8, 1665. Prof. Robert Hooke with Mr. William Mar, Mr. Richard Shortgrave, a gentleman since deceased and Henry Bond, sen. in the King's Private Garden at Whitehall observed the Variation and found it to be 1° 22′ 3″ W,—showing a marked decrease since Mr. W. Burrowes' Observation in 1580.

(Henry Bond, *Longitude Found.* 1676.)

To p. 717.

Patent for Glass-making for ten years.

May 19, 1691. There was granted to Robert Hooke Esq. and Christopher Dodsworth, merchants, their exors, administrators and assigns, 'the license and privilege for the sole use of mixing metal, so as to make glass for windows of more lustre and beauty than that heretofore made in England, red crystal glass of all sorts, and also the art of casting glass, particularly looking-glass plates, much larger than ever blown in England or foreign parts'.

Oct. 7, 1691. A warrant was signed to prepare a bill for incorporating a company for making glass, to be called the Company of Glass Makers. Power was given to the Company to hold property 'not exceeding in value £1000 per annum'. Robert Hooke was nominated Warden.

Calendar of State Papers Domestic. 1690–1.

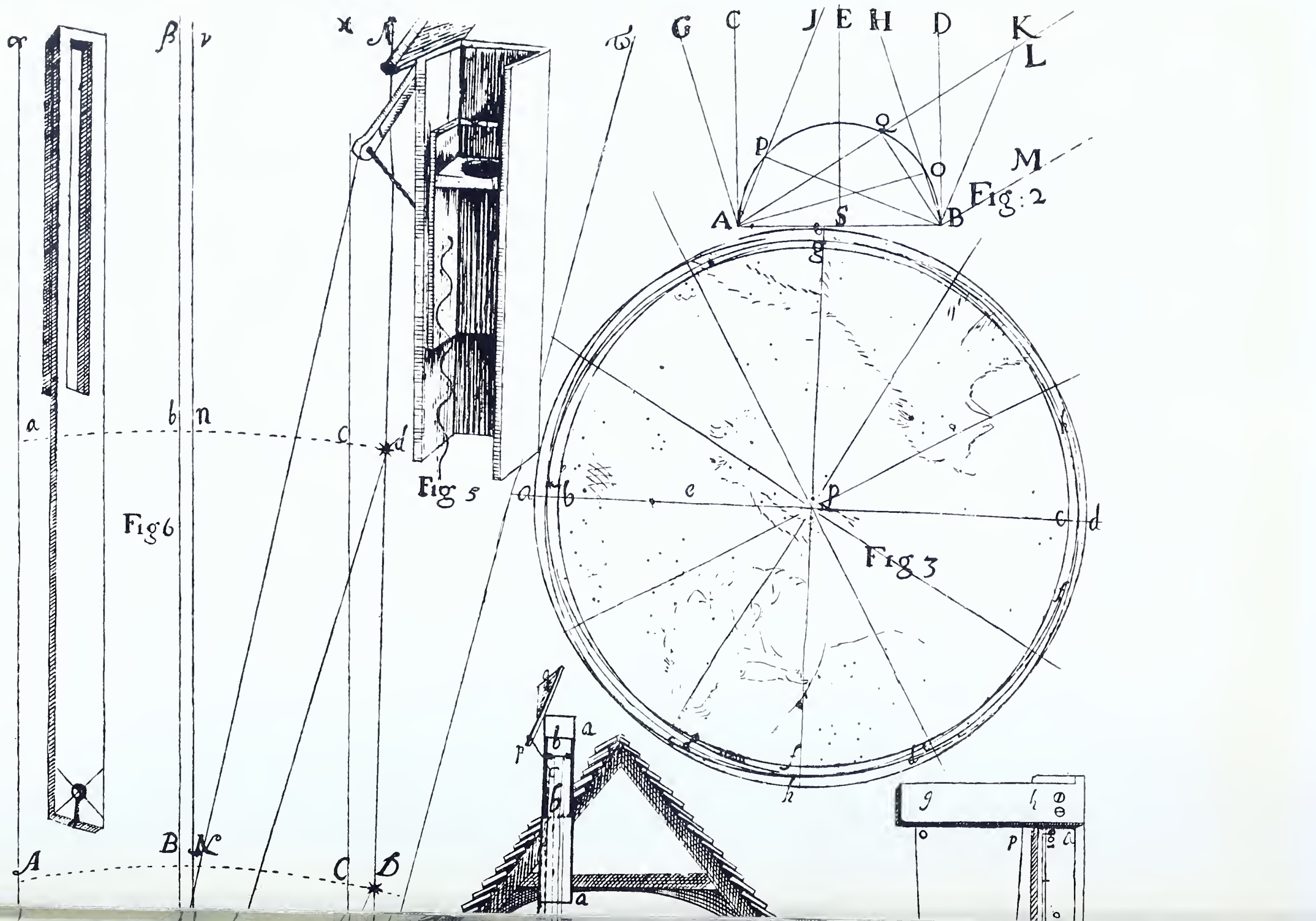
Fig 6
Fig 5
Fig: 2
Fig 3

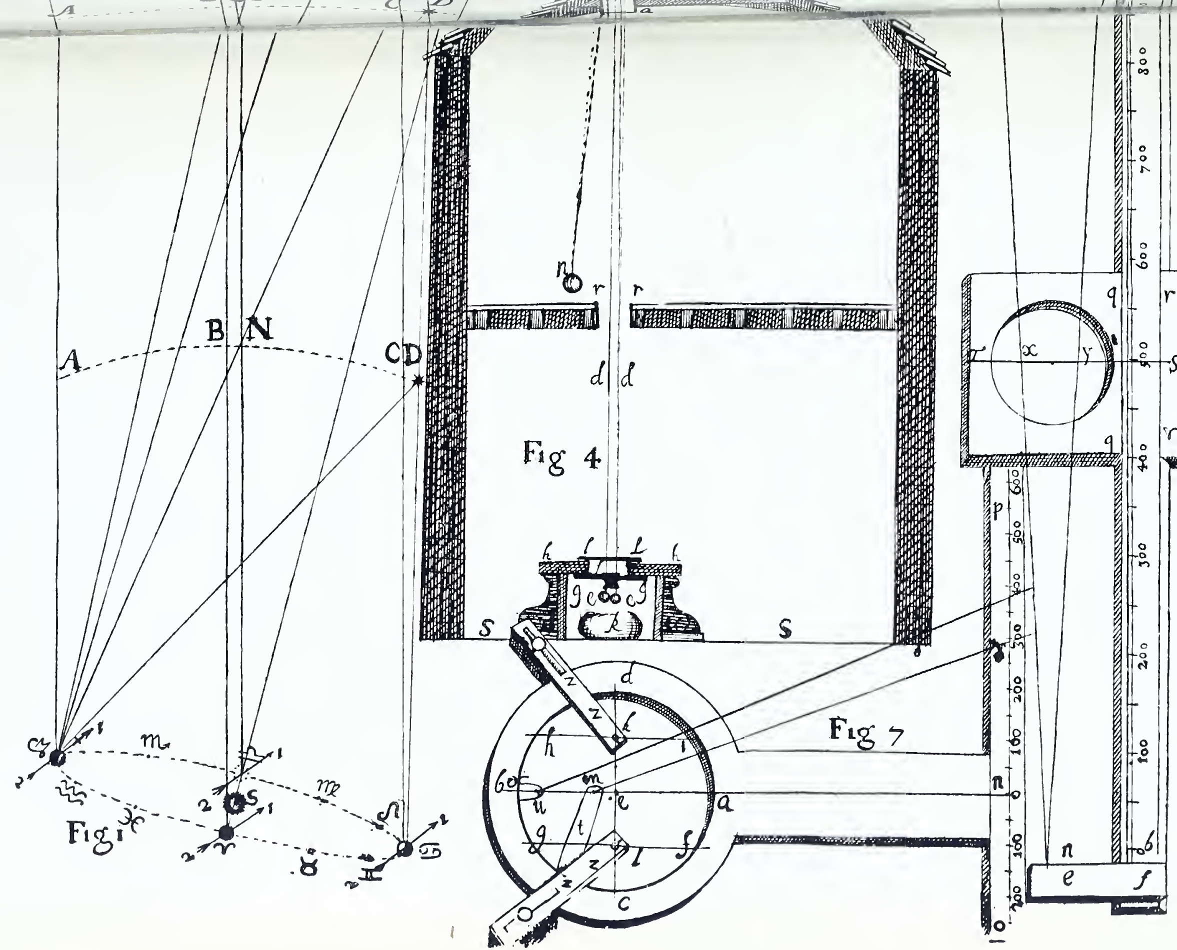

Fig 1

Fig 4

Fig 7

Lectiones Cutlerianæ,

OR A

COLLECTION

OF

LECTURES:

PHYSICAL,
MECHANICAL,
GEOGRAPHICAL,
&
ASTRONOMICAL.

Made before the *Royal Society* on ſeveral Occaſions at GRESHAM Colledge.

To which are added divers

MISCELLANEOUS DISCOURSES.

By *ROBERT HOOKE*, S.R.S.

LONDON:

Printed for *John Martyn* Printer to the *Royal Society*, at the Bell in S. *Pauls* Church-yard. 1679.

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3 Amen Corner, London, E.C.4
and at Bradford

The Titles of the ſeveral TRACTS.

I. AN ATTEMPT to prove the Annual MOTION of the EARTH, by Obſervations made with accurate Inſtruments: wherein is ſhewn the Impoſſibility of doing it, by the moſt exact Inſtruments and ways uſed by preceding Aſtronomers. The Inſtruments and method uſed in theſe Obſervations: The way of ſeeing the fixed Stars in the Day time; and a new Hypotheſis for ſolving the motions of the Heavenly Bodies is hinted.

II. ANIMADVERSIONS on the Machina Cœleſtis of Mr. Hevelius, wherein is detected the imperfection of Aſtronomical Inſtruments hitherto uſed, and divers ways of reforming and perfecting thoſe and ſeveral other Inſtruments are explained and deſcribed. And ſeveral other new Inventions are added and explained, as particularly Water-Levels: The Circular Pendulum, the Perfection of Wheel-work for Clocks and Watches, &c. together with their uſes, and the great advantage of theſe above other Inventions of the like nature.

III. A DESCRIPTION of Helioſcopes with other Inſtruments. Wherein are Diſcovered and Deſcribed, ſeveral new ways of making Glaſſes to look upon the Body of the Sun without offence to the Obſervers Eye. 2. A ſhortning Reflective and Refractive Teleſcope. 3. A way of uſing a Glaſs of any length without moving the Tube. 4. An Inſtrument for taking the Diameter of the Sun, Moon and Planets, or other ſmall Diſtances in the Heaven, to the certainty of a Second. 5. An Inſtrument for deſcribing all manner of Dials by the Tangent Projection. 6. The uſes of the ſaid Inſtrument, Firſt, for adjuſting the Hand of a Clock, ſo as to make it move in the ſhadow of a Dial, whoſe Stile is parallel to the Axis: Or, Secondly, in the Azimuth of any Celeſtial Body, that is, in the ſhadow of an upright, or any other way inclining ſtyle, upon any plain. Thirdly, for making a hand move according to the true Æquation of Time. Fourthly, for making all manner of Elliptical Dials, in Mr. Foſter's way, &c. Fifthly, for communicating a circular motion in a Curve Line, without any ſhaking: And for divers other excellent purpoſes. To which is added an Obſervation of the Eclipſe of the Moon, Jan. 1. 167$\frac{4}{5}$. And a Poſtſcript concerning the Invention of regulating Watches, by Springs applyed to their Ballances: together with a Decade of other uſeful Inventions, part diſcovered, part deſcribed in Anagrams.

IV. *LAMPAS, or Deſcriptions of ſome Mechanical Improvements of Lamps and Water-poiſes, with other Phyſical and Mechanical Diſcoveries. Wherein are diſcovered beſides the ways of obviating the inconveniencies of other contrivances of Lamps, Eight ſeveral ways of making Lamps ſo, as to regulate the flame of them for various uſes: ſeveral of which are therein mentioned and explained: Beſides which, various ways and uſes are deſcribed of poyſing liquors, by the by, ſeveral Theories and Explications are inſerted, particularly about Flame and Burning, about Light, Colour, Gravity, Local Motion, Preſſure of Fluids, &c. in Anſwer to ſome Objections of Dr.* More, *againſt ſome former Diſcourſes publiſhed by the Author. To theſe are added the Deſcription of a new ſort of* Clepſydra *or Water-Clock.* 2. *A new Principle for regulating Pocket Watches.* 3. *Several Microſcopical Obſervations about the Seeds, of Moſſe, Muſhrooms, all kinds of Ferns, Wall-Rue, Harts-Tongue, Oſmund Royal, &c.* 4. *An Obſervation of ſpots in the Sun.*

V. COMETA, *containing Obſervations on the Comet in* April, 1677. *Alſo for the years* 1664. 1665. *Sir* Chriſtopher Wren's *Hypotheſis and Geometrical Problem about thoſe Comets. A Diſcourſe concerning the Comet,* 1677.

Mr. Boyl's *Obſervation made on two new* Phoſphori *of Mr.* Baldwin, *and Mr.* Craft.

Mr. Gallet's *Letter to Mr.* Caſſini, *together with his Obſervation of* ☿ ſub ☉.

Mr. Caſſini's *Reflections upon thoſe of* Gaſſendus *and* Hevelius, *and upon this.*

Mr. Hally's *Letter and Obſervation of the ſame made at St.* Helena.

Mr. Caſſini's *Obſervation of the Diurnal motion of* ♃, *and other Changes happening in it.*

MICROSCOPIUM, *containing Mr.* Leeuwenhoecks *two Letters concerning ſome late Microſcopical Diſcoveries.*

The Author's Diſcourſe and Deſcription of Microſcopes, improved for diſcerning the nature and texture of Bodies.

P. Cherubines *Accuſations Anſwered.*

Mr. Yonge's *Letter containing ſeveral Anatomical Obſervations.*

VI. LECTURES de Potentia Reſtitutivâ *or of* Spring, *Explaining the Power of Springing Bodies. To which are added ſome Collections,* viz.

A Deſcription of Dr. Pappins *Wind-Fountain and Force-Pump.*

Mr. Yong's *Obſervation concerning natural Fountains.*

Some other Conſiderations concerning that Subject.

Capt. Sturmy's *Remarks of a Subterraneous Cave and Ciſtern.*

Mr. G. T. *his Obſervations made on the* Pike *of* Teneriff, *Anno* 1674.

Some Reflections and Conjectures occaſioned thereupon.

A Relation of a late Eruption in the Iſle of Palma.

AN ATTEMPT
To prove the
MOTION
OF THE
EARTH
FROM
Obſervations

MADE BY

ROBERT HOOKE Fellow of the *Royal Society.*

Senec. Nat. Qu. lib. 1. cap. 30.
Nè miremur tam tardè erui quæ tam altè jacent.

LONDON,
Printed by *T. R.* for *John Martyn* Printer to the *Royal Society*, at the *Bell* in St. *Pauls* Church-yard. 1674.

TO THE

TRULY HONORABLE

Sir John Cutler

KNIGHT and BARONET,

My Worthy

PATRON.

SIR,

AMong ſeveral Eminent Marks of your Greatneſs of Mind for promoting the Publick Good, that of your Bounty for the Advancement of *Experimental* and *Real Knowledge*; by the Founding a *Phyſico-Mechanical Lecture*, deſerves to be Recorded as One, and more eſpecially by me whom you have honoured by eſtabliſhing your firſt *Lecturer*. As an Earneſt of others more conſiderable ſhortly to follow, I here preſent you with one of my *Diſcourſes* in that Employment, which though ſhort and plain, conteins ſomewhat of *Information* which the *Learned* have hitherto deſired, though almoſt with deſpair. As I hope their kind Acceptance will produce their thanks to you to whom they are juſtly due, ſo your Acceptance will incourage me in the further proſecution of theſe *Inquiries* to approve my ſelf,

Noble Sir,

From *Greſham Colledge*,
March 25. 1674.

Your moſt obliged, and

moſt humble Servant

ROBERT HOOKE.

READER,

I *Have formerly in the Preface of my* Micrographia *given the World an account of the founding a* Physico-Mechanical Lecture *in the Year* 1665, *by Sir* John Cutler,*for the promoting the History of Nature and of Art. In prosecution thereof, I have collected many Observations both of the one and the other kind, and from time to time (as obliged) I have acquainted the* Royal Society *at their Publick Meetings, both at* Gresham Colledge *and* Arundel House *therewith, by* Discourses *and* Lectures *thereupon.*

Now in order to the further promoting the End and Design of this Lecture, *I have complyed, with the desire of several of my* Friends *(though otherwise not thereunto obliged) to commit divers of those* Discourses *to the Publick, though of themselves for the most part incompleat, and* Essayes *or* Attempts *only upon several Subjects which have no dependencie or coherencie one with another. In the doing hereof, I design to avoid any kind of Method or Order, that may require Apologies, Prefaces, or needless Repetitions of what is already known, or might have been said upon that Occasion, or may necessitate me to follow this or that Subject, that doth not some way or other offer it self as it were, and*

prompt

prompt me to the consideration thereof. But because they may possibly admit of some better order hereafter, I design to print them all of the same Volume, that so they may be, when ranged, either stitched or bound together, and may, as occasion requires, be referred to under the Title of their Number and Page. This way I chuse as the best for promoting the Design of this Lecture; *for as there is scarce one Subject of millions that may be pitched upon, but to write an exact and compleat History thereof, would require the whole time and attention of a mans life, and some thousands of Inventions and Observations to accomplish it: So on the other side no man is able to say that he will compleat this or that* Inquiry, *whatever it be, (The greatest part of Invention being but a luckey hitt of chance, for the most part not in our own power, and like the wind, the Spirit of Invention bloweth where and when it listeth, and we scarce know whence it came, or whether 'tis gone.) 'Twill be much better therefore to imbrace the influences of Providence, and to be diligent in the inquiry of every thing we meet with. For we shall quickly find that the number of considerable Observations and Inventions this way collected, will a hundred fold out-strip those that are found by Design. No man but hath some luckey hitts and useful thoughts on this or that Subject he is conversant about, the regarding and communicating of which, might be a means to other Persons highly to improve them. Whence 'twere much to be wished, that others would take this Method in their Publications, and not torment their* Readers *with such nauseous Repetitions, and frivolous*

Apologies,

Apologies, as Method and Volumes do neceſſitate them to; But would rather inrich the Store-houſe of Art and Nature with choice and excellent Seed, freed from the Chaff and Droſs that do otherwiſe bury and corrupt it.

The communicating ſuch happy Thoughts and Occurrences need not much take up a mans time to fit it for the Preſs; the Relation being ſo much the better the plainer it is. And matter of Fact being the Kernel Readers *generally deſire (at leaſt in theſe Subjects) it will be ſo much the readier for uſe if it be freed from the thick and hard ſhell of Impertinences. This way alſo is more grateful both to the* Writer *and the* Reader, *who proceed with a freſh ſtomach upon variety, but would be weary and dull'd if neceſſitated to dwell too long upon one Subject. There are other conveniencies alſo in this Method of Communication not leſs conſiderable then the former, amongſt the reſt the ſecuring of Inventions to their firſt Authors, which 'tis hardly poſſible to do by any other means; for there are a ſort of Perſons that make it their buſineſs to pump and ſpy out others Inventions, that they may vend them to Traders of that kind, who think they do ingenuouſly to print them for their own, ſince they have bought and paid for them. Of this there have lately been ſome Inſtances, and more may be expected, if this way prevent not.*

When things cannot be well explained by words only (which is frequent in Mathematical *and* Mechanical Diſcourſes) *I adde Schemes and delineatious Deſcriptions of that kind being eaſier to be made and underſtood. As near as I can I omit the repeating things already printed,*

and

and indeavour to deliver ſuch as are new and my own, being my ſelf beſt pleaſed with ſuch uſage from other Authors.

I have begun with a Diſcourſe *compoſed and read in* Greſham Colledge *in the Year* 1670. *when I deſigned to have printed it, but was diverted by the advice of ſome Friends to ſtay the repeating the Obſervation, rather then publiſh it upon the Experience of one Year only. But finding that Sickneſs hath hitherto hindered me from repeating the Tryals, and that ſome Years Obſervations have already been loſt by the firſt delay: I do rather haſt it out now, though imperfect, then detain it for a better compleating, hoping it may be at leaſt a Hint to others to proſecute and compleat the Obſervation, which I much long for.*

This firſt Diſcourſe *is upon an Obſervation of Nature, and may therefore be properly referred to that Head, though it contein alſo ſomewhat of the Improvement of Art: The ſecond ſpeedily to follow, will more properly be referrable to* Artificial Improvements, *though it will contein alſo many Obſervations of Nature; and I deſign alwayes to make them follow each other by turns, and as 'twere to interweave them, being apart but like the Warp or Woof before contexture, unfit either to Cloth, or adorn the* Body of Philoſophy.

AN ATTEMPT To prove the Motion of the EARTH BY OBSERVATIONS.

WHether the Earth move or ſtand ſtill hath been a Problem, that ſince *Copernicus* revived it, hath much exerciſed the Wits of our beſt modern Aſtronomers and Philoſophers, amongſt which notwithſtanding there hath not been any one who hath found out a certain manifeſtation either of the one or the other Doctrine. The more knowing and judicious have for many plauſible reaſons adhered to the *Copernican* Hypotheſis: But the generality of others, either out of ignorance or prejudice, have rejected it as a moſt extravagant opinion. To thoſe indeed who underſtand not the grounds and principles of Aſtronomy, the prejudice of common converſe

B doth

doth make it ſeem ſo abſurd, that a man ſhall as ſoon perſwade them that the Sun doth not ſhine, as that it doth not move; and as eaſily move the Earth as make them believe that it do's ſo already. For ſuch Perſons I cannot ſuppoſe that they ſhould underſtand the cogency of the Reaſons here preſented, drawn from the following obſervations of *Parallax*, much leſs therefore can I expect their belief and aſſent thereunto; to them I have only this to ſay, 'Tis not here my buſineſs to inſtruct them in the firſt principles of Aſtronomy, there being already Introductions enough for that purpoſe: But rather to furniſh the Learned with an *experimentum crucis* to determine between the *Tychonick* and *Copernican* Hypotheſes. That which hath hitherto continued the diſpute hath been the plauſibleneſs of ſome Arguments alledged by the one and the other party, with ſuch who have been by nature or education prejudiced to this or that way. For to one that hath been converſant only with illiterate perſons, or ſuch as underſtand not the principles of Aſtronomy and Geometry, and have had no true notion of the vaſtneſs of the Univerſe, and the exceeding minuteneſs of the Globe of the Earth in compariſon therewith, who have confined their imaginations & fancies only within the compaſs and pale of their own walk and proſpect, who can ſcarce imagine that the Earth is globous, but rather like ſome of old, imagine it to be a round plain covered with the Sky as with a Hemiſphere, and the Sun, Moon, and Stars to be holes through it by which the Light of Heaven comes down; that ſuppoſe themſelves in the center of this plain, and that the Sky doth touch that plain round the edges, ſupported in part by the Mountains; that ſuppoſe the Sun as big as a Sieve, and the Moon as a *Chedder* Cheeſe, and hardly a mile off. That wonder why the Sun, Moon, and Stars do not fall down like Hail-ſtones; and that will be martyr'd rather then grant that there may be Antipodes, believing it abſolutely impoſſible, ſince they muſt neceſſarily fall down into the Abyſs below them: For how can they go with their feet towards ours, and their heads downwards, without making their brains addle. To one I ſay, thus prejudiced with theſe and a thouſand other fancies and opinions more ridiculous and abſurd to knowing men, who can ever imagine that the uniformity and harmony of the Celeſtial bodies and motions, ſhould be an Argument prevalent to perſwade that the Earth moves about the Sun: Whereas that Hypotheſis which ſhews how to ſalve

ſalve the appearances by the reſt of the Earth and the motion of the Heavens, ſeems generally ſo plauſible that none of theſe can reſiſt it.

Now though it may be ſaid, 'Tis not only thoſe but great Geometricians, Aſtronomers and Philoſophers have alſo adhered to that ſide, yet generally the reaſon is the very ſame. For moſt of thoſe, when young, have been imbued with principles as groſs and rude as thoſe of the Vulgar, eſpecially as to the frame and fabrick of the World, which leave ſo deep an impreſſion upon the fancy, that they are not without great pain and trouble obliterated: Others, as a further confirmation in their childiſh opinion, have been inſtructed in the *Ptolomaick* or *Tichonick* Syſtem, and by the Authority of their Tutors, over-awed into a belief, if not a veneration thereof: Whence for the moſt part ſuch perſons will not indure to hear Arguments againſt it, and if they do, 'tis only to find Anſwers to confute them.

On the other ſide, ſome out of a contradicting nature to their Tutors; others, by as great a prejudice of inſtitution; and ſome few others upon better reaſoned grounds, from the proportion and harmony of the World, cannot but imbrace the *Copernican* Arguments, as demonſtrations that the Earth moves, and that the Sun and Stars ſtand ſtill.

I confeſs there is ſomewhat of reaſon on both ſides, but there is alſo ſomething of prejudice even on that ſide that ſeems the moſt rational. For by way of objection, what way of demonſtration have we that the frame and conſtitution of the World is ſo harmonious according to our notion of its harmony, as we ſuppoſe? Is there not a poſſibility that the things may be otherwiſe? nay, is there not ſomething of probability? may not the Sun move as *Ticho* ſuppoſes, and the Planets make their Revolutions about it whilſt the Earth ſtands ſtill, and by its magnetiſm attracts the Sun, and ſo keeps him moving about it, whilſt at the ſame time ☿ and ♀ move about the Sun, after the ſame manner as ♄ and ♃ move about the Sun whilſt the Satellites move about them? eſpecially ſince it is not demonſtrated without much art and difficulty, and taking many things for granted which are hard to be proved, that there is any body in the Univerſe more conſiderable then the Earth we tread on. Is there not much reaſon for the Hypotheſis of *Ticho* at leaſt, when he with all the accurateneſs that he arrived to with his vaſt Inſtru-

B 2 ments

ments, or *Riccioli*, who pretends much to out-ſtrip him, were not able to find any ſenſible Parallax of the Earths Orb among the fixt Stars, eſpecially if the obſervations upon which they ground their aſſertions, were made to the accurateneſs of ſome few Seconds? What then, though we have a Chimera or Idea of perfection and harmony in that Hypotheſis we pitch upon, may there not be a much greater harmony and proportion in the conſtitution it ſelf which we know not, though it be quite differing from what we fancy? Probable Arguments might thus have been urged both on the one and the other ſide to the Worlds end; but there never was nor could have been any determination of the Controverſie, without ſome poſitive obſervation for determining whether there were a Parallax or no of the Orb of the Earth; This *Ticho* and *Riccioli* affirm in the Negative, that there is none at all: But I do affirm there is no one that can either prove that there is, or that there is not any Parallax of that Orb amongſt the fixt Stars from the Suppellex of obſervations yet made either by *Ticho*, *Riccioli*, or any other Writer that I have yet met with from the beginning of writing to this day. For all Obſervators having hitherto made uſe of the naked eye for determining the exact place of the object, and the eye being unable to diſtinguiſh any angle leſs then a minute, and an obſervation requiſite to determine this requiring a much greater exactneſs then to a minute, it doth neceſſarily follow that this *experimentum crucis* was not in their power, whatever either *Ticho* or *Riccioli* have ſaid to the contrary, and would thence overthrow the *Copernican* Syſtem, and eſtabliſh their own. We are not therefore wholly to acquieſs in their determination, ſince if we examine more nicely into the obſervations made by them, together with their Inſtruments and wayes of uſing them, we ſhall find that their performances thereby were far otherwiſe then what they would ſeem to make us believe. The Controverſie therefore notwithſtanding all that hath been ſaid either by the one or by the other Party, remains yet undetermined, Whether the Earth move above the Sun, or the Sun about the Earth; and all the Arguments alledged either on this or that ſide, are but probabilities at beſt, and admit not of a neceſſary and poſitive concluſion. Nor is there indeed any other means left for humane induſtry to determine it, ſave this one which I have endeavoured to make; and the unqueſtionable

certainty

certainty thereof is a moſt undenyable Argument of the truth of the *Copernican* Syſteme; and the want thereof hath been the principal Argument that hath hitherto ſomewhat detained me from declaring abſolutely for that Hypotheſis, for though it doth in every particular almoſt ſeem to ſolve the appearances more naturally and eaſily, and to afford an exceeding harmonious conſtitution of the great bodies of the World compared one with another, as to their magnitudes, motions, and diſtances, yet this objection was alwayes very plauſible to moſt men, that it is affirmed by ſuch as have written more particularly of this ſubject, that there never was any ſenſible Parallax diſcovered by the beſt obſervations of this ſuppoſed annual motion of the Earth about the Sun as its center, though moved in an Orb whoſe Diameter is by the greateſt number of Aſtronomers reckoned between 11 and 12 hundred Diameters of the Earth: Though ſome others make it between 3 and 4 thouſand; others between 7 and 8; and others between 14 and 15 thouſands; and I am apt to believe it may be yet much more, each Diameter of the Earth being ſuppoſed to be between 7 and 8 thouſand Engliſh miles, and conſequently the whole being reduced into miles, if we reckon with the moſt, amounting to 120 millions of Engliſh miles. It cannot, I confeſs, but ſeem very uncouth and ſtrange to ſuch as have been uſed to confine the World with leſs dimenſions, that this annual Orb of the Earth of ſo vaſt a magnitude, ſhould have no ſenſible Parallax amongſt the fixt Stars, and therefore 'twas in vain to indeavour to anſwer that objection. For it is unreaſonable to expect that the fancies of moſt men ſhould be ſo far ſtreined beyond their narrow dimenſions, as to make them believe the extent of the Univerſe ſo immenſly great as they muſt have granted it to be, ſuppoſing no Parallax could have been found.

The Inquiſitive Jeſuit *Riccioli* has taken great pains by 77 Arguments to overthrow the *Copernican* Hypotheſis, and is therein ſo earneſt and zealous, that though otherwiſe a very learned man and good Aſtronomer, he ſeems to believe his own Arguments; but all his other 76 Arguments might have been ſpared as to moſt men, if upon making obſervations as I have done, he could have proved there had been no ſenſible Parallax this way diſcoverable, as I believe this one Diſcovery will anſwer them, and 77 more, if ſo many can be thought of and produced

produced againſt it. Though yet I confeſs had I fail'd in diſcovering a Parallax this way, as to my own thoughts and perſwaſion, the almoſt infinite extenſion of the Univerſe had not to me ſeem'd altogether ſo great an abſurdity to be believed as the Generality do eſteem it ; for ſince 'tis confeſſedly granted on all hands the diſtance of the fixt Stars is meerly hypothetical, and not founded on any other ground or reaſon but fancy and ſuppoſition, and that there never was hitherto any Parallax obſerved, nor any other conſiderable Argument to prove the diſtances ſuppoſed by ſuch as have been moſt curious and inquiſitive in that particular, I ſee no Argument drawn from the nature of the thing that can have any neceſſary force in it to determine that the ſaid diſtance cannot be more then this or that , whatever it be that is aſſigned. For the ſame God that did make this World that we would thus limit and bound, could as eaſily make it millions of millions of times bigger, as of that quantity we imagine ; and all the other appearances except this of Parallax would be the very ſame that now they are. To me indeed the Univerſe ſeems to be vaſtly bigger then 'tis hitherto aſſerted by any Writer , when I conſider the many differing magnitudes of the fixt Stars, and the continual increaſe of their number according as they are looked after with better and longer Teleſcopes. And could we certainly determine and meaſure their Diameters , and diſtinguiſh what part of their appearing magnitude were to be attributed to their bulk, and what to their brightneſs , I am apt to believe we ſhould make another diſtribution of their magnitudes , then what is already made by *Ptolomy* , *Ticho* , *Kepler*, *Bayer*, *Clavius*, *Grienbergerus*, *Piff*, *Hevelius* and others.

For ſuppoſing all the fixt Stars as ſo many Suns , and each of them to have a Sphere of activity or expanſion proportionate to their ſolidity and activity , and a bigger and brighter bodied Star to have a proportionate bigger ſpace or expanſion belonging to it , we ſhould from the knowledge of their Diameters and brightneſſes be better able to judge of their diſtances , and conſequently aſſign divers of them other magnitudes then thoſe already ſtated : Eſpecially ſince we now find by obſervations , that of thoſe which are accounted ſingle Stars , divers prove a congeries of many Stars, though from their near appearing to each other , the naked

ked eye cannot diſtinguiſh them; Such as thoſe Stars which are called *Nebulous*, and thoſe in *Orion* Sword, and that in the head of *Aries*, and a multitude of others the Teleſcope doth now detect. And poſſibly we may find that thoſe twenty magnitudes of Stars now diſcovered by a fifteen foot Glaſs, may be found to increaſe the magnitude of the Semidiameter of the viſible World, fourty times bigger then the *Copernicans* now ſuppoſe it between the Sun and the fixt Stars, and conſequently ſixty four thouſand times in bulk. And if a Teleſcope of double or treble the goodneſs of one of fifteen ſhould diſcover double or treble the ſaid number of magnitudes, would it not be an Argument of doubling or trebling the former Diameter, and of increaſing the bulk eight or twenty ſeven times. Eſpecially if their apparent Diameters ſhall be found reciprocal to their Diſtances (for the determination of which I did make ſome obſervations, and deſign to compleat with what ſpeed I am able.) But to digreſs no further, This grand objection of the *Anticopernicans*, which to moſt men ſeem'd ſo plauſible, that it was in vain to oppoſe it, though, I ſay, it kept me from declaring abſolutely for the *Copernican* Hypotheſis, yet I never found any abſurdity or impoſſibility that followed thereupon: And I alwayes ſuſpected that though ſome great Aſtronomers had aſſerted that there was no Parallax to be found by their obſervations, though made with great accurateneſs, there might yet be a poſſibility that they might be miſtaken; which made me alwayes look upon it as an inquiry well worth examining: firſt, Whether the wayes they had already attempted were not ſubject and lyable to great errors and uncertainties: and ſecondly, Whether there might not be ſome other wayes found out which ſhould be free from all the exceptions the former were incumbred with, and be ſo far advanced beyond the former in certainty and accurateneſs, as that from the diligent and curious uſe thereof, not only all the objections againſt the former might be removed, but all other whatſoever that were material to prove the ineffectualneſs thereof for this purpoſe.

I began therefore firſt to examine into the matter as it had already been performed by thoſe who had aſſerted no ſenſible Parallax of the annual Orb of the Earth, and quickly found that (whatever they aſſerted) they could never determine whether

there

there were any or no Parallax of this annual Orb; especially if it were less then a minute, which *Kepler* and *Riccioli* hypothetically affirm it to be: The former making it about twenty four Seconds, and the latter about ten. For though *Ticho*, a man of unquestionable truth in his assertions, affirm it possible to observe with large Instruments, conveniently mounted and furnished with sights contrived by himself (and now the common ones for Astronomical Instruments) to the accurateness of ten Seconds; and though *Riccioli* and his ingenious and accurate Companion *Grimaldi* affirm it possible to make observations by their way, with the naked edge to the accurateness of five Seconds; Yet *Kepler* did affirm, and that justly, that 'twas impossible to be sure to a less Angle then 12 Seconds: And I from my own experience do find it exceeding difficult by any of the common sights yet used to be sure to a minute. I quickly concluded therefore that all their endeavours must have hitherto been ineffectual to this purpose, and that they had not been less imposed on themselves, then they had deceived others by their mistaken observations. And this mistake I found proceeded from divers inconveniencies their wayes of observations were lyable to. As first from the shrinking and stretching of the materials wherewith their Instruments were made, I conceive a much greater angle then that of a minute may be mistaken in taking an altitude of fifty Degrees. For if the Instruments be made of Wood, 'tis manifest that moyst weather will make the frame stretch, and dry weather will make it shrink a much greater quantity then to vary a minute: and if it be Metal, unless it be provided for in the fabrick of the Instrument accordingly, the heat of Summer, when the Summer observations are to be made, will make the Quadrant swell, and the cold of Winter will make it shrink much more then to vary a minute: Both which inconveniencies ought to be removed. Next the bending and warping of an Instrument by its own weight, will make a very considerable alteration. And thirdly, the common way of Division is also lyable to many inconveniencies: And 'tis hardly possible to ascertain all the subdivisions of Degrees into minutes for the whole Quadrant, though that be not altogether impossible. But I will suppose that they did foresee, and in some manner prevent all these inconveniencies, especially *Ticho* and *Riccioli*, who seem to have been aware thereof. But there was one

one inconvenience which was worſe then all the reſt, which they ſeem not to have been ſufficiently ſenſible of, from whence proceeded all their own miſtakes, and their impoſing upon others, and that was from their opinion that the ſight of the naked eye was able to diſtinguiſh the parts of the object as minutely as the limb of the Quadrant (of what largeneſs ſoever) was capable of Diviſions; whereas 'tis hardly poſſible for any unarmed eye well to diſtinguiſh any Angle much ſmaller then that of a minute: and where two objects are not farther diſtant then a minute, if they are bright objects, they coaleſs and appear one, though I confeſs, if they be dark objects, and a light be interpoſed, the diſtance between them ſhall be viſible, though really much leſs then a Second; and yet notwithſtanding, my firſt aſſertion ſtands good; for though a bright object, as a candle or light at a diſtance, or a Star, or the like, can be ſeen by the eye, though its body do really not ſubtend an Angle of one third, yet it proceeds from a radiation (that is, from reflection and refraction together) in the air and in the eye, whereby the body thereof is repreſented to the naked eye ſome hundred times bigger then it really is. That this is ſo, any one that will but carefully examine will find it true.

It was, I doubt not, their extraordinary deſire and care to be exact, that cauſed them to make their Inſtruments ſo large, and to ſubdivide them to ſuch an exactneſs, as to diſtinguiſh, if poſſible, to Seconds; And I queſtion not but that they uſed their utmoſt indeavour in directing the ſight to the object: but ſince the naked eye cannot diſtinguiſh an Angle much ſmaller then a minute, and very few to a whole minute, all their charge and trouble in making and managing large Inſtruments, and in calculating and deducing from them, was as to this uſe in vain. Hence I judged that whatever mens eyes were in the younger age of the World, our eyes in this old age of it needed Spectacles; and therefore I reſolved to aſſiſt my eyes with a very large and good Teleſcope, inſtead of the common ſights, whereby I can with eaſe diſtinguiſh the parts of an object to Seconds: and I queſtion not but that this way may be yet made capable of diſtinguiſhing much more curiouſly, poſſibly even to ſome few Thirds. This invention removed that grand inconvenience which all former obſervations were ſpoiled with: but there remained

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mained yet further this difficulty, How to make an Inſtrument large enough for this purpoſe, that I might be aſſured did not ſhrink, nor warp, nor ſtretch ſo much as to vary a Second; for ſuch is the nature of all Materials that can be made uſe of for Inſtruments of the bigneſs I deſigned this, that 'tis almoſt impoſſible to make a moveable Inſtrument that ſhall not be ſubject to a variation of divers Seconds: It was therefore my next inquiry where I might fix this Archimedean Engine that was to move the Earth. For the doing of which, I knew 'twas in vain to conſult with any Writer or Aſtronomer, having never then heard of any perſon that had ever before that time had any thoughts thereof: and when I firſt propounded it to the *Royal Society*, 'twas look'd upon as a new thought, and ſomewhat extravagant, and hardly practicable, until upon hearing my explication, and the various wayes how it might be reduced into practiſe, it was at length judged poſſible, and deſirable to be tryed. I propounded therefore to them the ſeveral ways that it was poſſible to be performed, and what method was to be obſerved in every one of them, and ſomewhat of the conveniencies and inconveniencies in each of them; for having ſeriouſly meditated upon the Inquiry, I quickly thought of many expedients for the doing thereof. As firſt, I had thoughts of making uſe of ſome very great and maſſy Tower or Wall that were well ſetled, or of ſome large Rock or Hill whereunto I might fix my Glaſſes, ſo as to take the exact altitude of ſome eminent Star near the Pole of the Ecliptik, when at its greateſt height, at two differing times of the year; to wit, about the Summer and Winter Solſtice, to ſee if poſſibly I could diſcover any difference of altitude between the firſt and ſecond obſervation. But to accompliſh this (beſides the vaſt difficulty there would have been to have meaſured ſuch an Angle to the accurateneſs requiſite, if at leaſt it were deſired to have the Angle of altitude to Minutes and Seconds, which ought alſo to have been repeated as oft as any obſervation had been made for fear of ſetling or ſwelling, &c.) I was deſtitute of ſuch a convenience near my habitation; beſides, had I had my wiſh, I found that 'twas lyable to an inconvenience that would wholly overthrow my whole deſign, which I knew not well how to avoid: Namely, to that which hath hitherto made even the very beſt

best obſervations of Parallaxes ineffectual and uncertain, the *refraction of the Air* or *Atmoſphere*, which though it could have been but very little at the greateſt altitude of the Pole of the Ecliptick, yet it might have been enough plauſibly to have ſpoiled the whole obſervation, and to have given the *Anti-copernicans* an opportunity of evading the Arguments taken from it, eſpecially upon the account of the differing conſtitution of the Atmoſphere in *June* and *December*, which might have cauſed ſo much a greater refraction of the ſame altitude at one time then another, as would have been ſufficient to have made this obſervation ineffectual for what it was deſigned. Adde to this, that it would have been no eaſie matter to have ſet the Glaſſes or Teleſcope exactly againſt the Meridian, ſo as to ſee the higheſt altitude of any Star near the Pole of the Ecliptick diſtinctly to a Second.

The like difficulties I found if obſervations were made of the greateſt altitude of the Pole of the Ecliptick in *June* and *December*, or the leaſt altitude of the ſame in *December* and *June*. For beſides all the uncertainties that the Inſtruments, be they what they will, are liable to, the grand inconvenience of the refraction of the Air, which is enough to ſpoil all obſervations if it be intermixed with uncertainty, in the former is conſiderable, and in the later intolerable.

Having therefore examined the wayes and Inſtruments for all manner of Aſtronomical obſervations hitherto made uſe of, and conſidered of the inconveniencies and imperfections of them; and having alſo duly weighed the great accurateneſs and certainty that this obſervation neceſſarily required: I did next contrive a way of making obſervations that might be free from all the former inconveniencies and exceptions, and as near as might be, fortified againſt any other that could be invented or raiſed againſt it. This way then was to obſerve by the paſſing of ſome conſiderable Star near the Zenith of *Greſham Colledge*, whether it did not at one time of the year paſs nearer to it, and at another further from it: for if the Earth did move in an Orb about the Sun, and that this Orb had any ſenſible Parallax amongſt the fixt Stars; this muſt neceſſarily happen, eſpecially to thoſe fixt Stars which were neareſt the Pole of the Ecliptick. And that this is ſo, any one may plainly perceive if he

C 2 conſider

consider the annexed Scheme, *Fig.* I. where let S represent the Sun placed as it were in the center of the Planetary Orbs, A B C D an imaginary Orb of the fixt Stars of the first magnitude, whose center for demonstration sake we will suppose the Sun. Let ♈ ♋ ♎ ♑ represent the Orb in which the Earth is supposed to move about the Sun, obliquely projected on the Paper. Let ♑ represent the Earth in *Capricorn*, and ♋ the Earth in *Cancer*, let 1 2. 1 2. represent the imaginary Axis of the Earth, keeping continually a parallelism to its self, and let ♑ A F C D ♋ represent an imaginary Plain passing through the center of the Star at D in the Solstitial Colure, and the two centers of the Earth in ♑ and ♋, and C represent the Zenith point of *Gresham Colledge* at noon, when the Earth is in *Cancer*, and A the Zenith point of the said *Colledge* at midnight in the aforesaid Orb A B C D when the Earth is in *Capricorn*, 'tis manifest therefore that since the Poles of the Earth, the Poles of the Eclip-tick, and the Zenith points of the Earth at noon, when in *Cancer*, and at midnight, when in *Capricorn*, are all in the same Plain; and that the Axis of the Earth keeps alwayes its parallelism, and that the Angles made by the Perpendiculars of *Gresham Colledge*, with the Axes are alwayes the same, that the aforesaid Perpendiculars of the said *Colledge* shall be parallel also one to another, and consequently denote out two points in the abovesaid Orb A and C as far distant from each other as the parallel Lines A ♑ and C ♋ are, and consequently the point A shall be farther from the Star in D, and the point C shall be nearer to it, when in the Meridian near the Zenith of *London*, and consequently if the said Star be observed when in the Meridian of the place abovesaid, if there be any such difference considerable, it may be found if convenient Instruments and care be made use of for the observation thereof: and the difference between the Angle A ♑ D, and the Angle C ♋ D, will give the parallactical Angle ♑ D ♋ of the Orb of the Earth to the fixt Star D of the first magnitude. The same demonstration will hold *mutatis mutandis*, supposing the Star be not in the Meridian or Plain abovesaid, but in some other Meridian, as any one upon well considering the nature of the thing it self may easily prove, if the observation be made when the Zenith passes by the Star at midnight, and at mid-

mid-day. But the nearer the Zenith of the place of obſervation paſſeth to the Pole point of the Ecliptick, the better; The Angle of Parallax being ſtill the more ſenſible. Therefore the beſt place to compleat this obſervation were in ſome place under the Polar Circles, as in *Iſeland*, where the Zenith of the place at the times aboveſaid, muſt conſequently paſs at one time to the North ſide of the Pole of the Ecliptick, and at the other on the South ſide, and the Zenith of *March* and *Sept.* muſt paſs through the very Pole-point it ſelf. Now it falling out ſo, that there is no conſiderable Star in that part of the Heavens nearer the above ſaid Plain, and nearer the Zenith point of *Greſham Colledge* in that Plain, then the Bright Star in the head of the *Dragon*, I made choice of that Star for the object by which I deſigned to make this obſervation, finding the Zenith point of *Greſham Colledge* to paſs within ſome very few minutes of the Star it ſelf; the declination thereof according to *Riccioli* being 51°. 36′. 7″. and the Plain the Star and Pole of the World, making an Angle with the aforeſaid Plain but of 2°. 52. 36, the right aſcention thereof being according to *Riccioli* 267°. 7′. 24″.

And that this may be made a little plainer, let us ſuppoſe in the third *Figure*, the North part of the Heavens projected ſtereographical upon a Plain to which the Axis is perpendicular. Let p repreſent the Pole, e the Pole of the Ecliptick, l the bright Star in the head of *Draco*, and let a c c c repreſent an imaginary Circle deſcribed by the Zenith of *Greſham Colledge* among the fixt Stars in *June*, and b d d d a like Circle deſcribed by the ſaid Zenith in *December*, and e f f f a like Circle deſcribed as above in *March*, and g h h h in *September*. It is very evident that the true diſtances of the Zeniths in that part of the Meridian which is next the Pole of the Ecliptick, to wit, in the head of the Conſtellation *Draco*, ſhall be to the true diſtances of the ſaid Zeniths in that part which is furtheſt from the ſaid Pole, to wit, near the conſtellation of *Auriga in conſequentia*, as the ſign of 75 degrees to the ſign of 14°. 54′, and the variation of the Zeniths, or the Angle of Parallax here at *Greſham Colledge*, to the Angle of Parallax in *Iſeland*, or any other place under the Pole of the Ecliptick, or Artick Circle is, as the ſign of ſeventy five to the ſign of ninety or the *Radius*.

us. This will be very evident if we conſider in the ſecond Scheme; AB to repreſent the Diameter of the great Orb: AC and BD the perpendiculars of *Iſeland*, or ſome other place under the Polar Circle. GA, HB the perpendiculars of *Greſham Colledge* in *Draco*: and LA, MB the perpendiculars of the ſame place to the Solſtitial Colure near *Auriga*, the ſeveral diſtances CD, GH, IK, LM, will be as the ſigns of 90°|75°|66°.30'|14°.54'|. to wit, as the Lines or Cords A B. A O. P B. Q B.

I might have made obſervations of the diſtances of the tranſits of our Zenith from any other Star as well as from this of *Draco*, and the ſame Phenomena might have been obſerved, taking care to make one of the obſervations when the Star is in the Zenith at midnight, and the other when the ſame Star is in the Zenith at noon or mid-day; and upon this account when I next obſerve, I deſign to obſerve the tranſits of our Zenith by *Benenaim*, or the *ultima caudæ urſæ majoris*, it being a Star of the ſecond magnitude, and having almoſt as much declination as *Greſham Colledge* hath latitude. The principal dayes of doing which will be about the 4 of *April*, when our Zenith paſſeth by the ſaid Star at midnight, and the 7 of *October*, when it paſſeth by it at noon or mid-day: the reaſon of all which will be ſufficiently manifeſt to any one that ſhall well conſider the preceeding explanation.

This Star I would the rather obſerve, becauſe as it is placed ſo as that the Parallax thereof will be almoſt as great as of the Pole of the Ecliptick in *Iſeland*, or under the Artick Circle, ſo it being a Star of the ſecond magnitude, and conſequently perhaps as near again as one of the fourth, the Angle of Parallax will be near about twice as big, and the Star it ſelf much more eaſie to be ſeen in the day time. This will be very eaſie to be underſtood, if we conſider in the firſt Scheme the differing diſtances of the Orb *A B C D*, in which we may ſuppoſe the Stars of the ſecond magnitude to be fixt, and of the Orb *α β κ δ*, in which we may ſuppoſe the Stars of the fourth magnitude, and a b c d in which we may ſuppoſe thoſe of the third magnitude, and A B C D in which we may ſuppoſe thoſe of the firſt; for if the Stars are further and further removed from the Sun, according as they appear leſs and leſs to us, the parallactical difference found by obſervation muſt neceſſarily be be leſs and leſs, according as the obſervation is made of leſs and leſs Stars.

The reaſons then why I made choice of this way of obſerving will be eaſie to any one that ſhall conſider that hereby, firſt, I avoid that grand inconvenience wherewith all ancient and modern obſervations have been perplext, and as to Parallax inſignificant, and that is the refraction of the Air or Atmoſphere. How great an inconvenience that was is obvious, ſince 'tis certainly much greater at one time then another, and never at any certainty; and ſecondly, 'Tis not equally proportionable, for ſometimes the refraction is greater at ſome diſtance above the Horizon, then in or nearer to the Horizon it ſelf, and ſometimes the quite contrary, which I have very often obſerved; and this to ſo exorbitant a difference, as to confound all Hypothetical Calculations of Tables for this purpoſe. This ariſeth from the uncertain and ſudden variations of the Air or Atmoſphere, either from heat and cold, from the thickneſs and thinneſs of Vapours, from the differing gravity and levity, from the winds, currents, and eddyes thereof, all which being not ſo well underſtood by what way, and in what degree, and at what time they work and operate upon the Air, muſt needs make the refraction thereof exceedingly perplext, and the reduction thereof to any certain theory fit for practice, a thing almoſt impoſſible. Now if we are uncertain what part of the obſerved Angle is to be aſcribed to refraction, we are uncertain of the whole obſervation as far as the poſſible uncertainty of refraction. Let me have but the liberty of ſuppoſing the refraction what I pleaſe, and of fixing the proportional decreaſe thereof according to the various elevation of the Rayes above the Horizon; I will with eaſe make out all the viſible Phenomena of the Univerſe, Sun, Moon, and Stars, and yet not ſuppoſe them above a Diameter of the Earth diſtant. Now in this obſervation there is no refraction at all, and conſequently be the Air thicker or thinner, heavier or lighter, hotter or colder, be it in Summer or Winter, in the night or the day, the ray continually paſſeth directly, and is not at all refracted and deflected from its ſtreight paſſage. In the next place, by this way of obſerving I avoid all the difficulties that attend the making, mounting, and managing of great Inſtruments: For I have

have no need of Quadrant, Sextant, or Octant, nor of any other part or Circle bigger then a Degree at moſt; nor have I need to take care of the diviſions and ſubdiviſions thereof, nor of the ſubſtance whether made of Iron, Braſs, Copper, or Wood, nor whether the parts thereof ſhrink or ſwell, or bend or warp, to all which the beſt Inſtruments hitherto made uſe of, have been ſome wayes or other lyable. And notwithſtanding the vaſt care and expence of the noble *Ticho* about the making, fixing, and uſing his great Inſtruments; yet I do not find them ſo well ſecured from divers of theſe inconveniences, but that they were ſtill ſubject to ſome conſiderable irregularities. Nay, notwithſtanding the ſeemingly much greater curioſity and expenſe of *Hevelius*, and his infinite labour and diligence in the compleating and uſing of his vaſt Apparatus of Aſtronomical Inſtruments, I do not find them ſo well ſecured, but that ſome of the cauſes of errors that I have before mentioned, may have had a conſiderable effect upon them alſo; eſpecially if they were ſuppoſed to meaſure an Angle to ſome few Seconds, as I ſhall hereafter perhaps have more occaſion to manifeſt. Now, if the Inſtruments of *Ticho* and *Hevelius*, (who had certainly two of the moſt curious and magnificent Collections of Aſtronomical Inſtruments that were ever yet got together or made uſe of) were ſubject to theſe uncertainties, What ſhall we ſay of all that other farrage of trumpery that hath been made uſe of by moſt others? We ſee therefore the neceſſity of the conjunction of Phyſical and Philoſophical with Mechanical and Experimental Knowledge, how lame and imperfect the ſtudy of Art doth often prove without the conjunction of the ſtudy of Nature, and upon what rational grounds it was that Sir *John Cutler*, the Patron and Founder of this Lecture, proceeded in joyning the contemplation of them both together.

The next thing was the Inſtrument for the making of this obſervation, ſuch a one as ſhould not be lyable to any of the former exceptions, nor any other new ones that were conſiderable. To this purpoſe I pitched upon a Teleſcope, the largeſt I could get and make uſe of, which I deſigned ſo to fix upright, as that looking directly upwards, I could be able certainly to obſerve the tranſits of any Stars over or near the

the Zenith, and furnishing it with perpendiculars and a convenient dividing Instrument, I should be able not only to know exactly when the Star came to cross the Meridian, but also how far it crossed it from the Center or Zenith point of *Gresham Colledge*, either towards the North, or towards the South. All which Particulars, how I performed, I shall now in order describe, and this somewhat the more distinctly, that such as have a desire to do the like, may be the more ready and better inabled to proceed with the same.

First then (finding a Tube would be very troublesome to the Rooms through which it past, especially if it were placed pretty far in the Room, and that one wanted so free an access as was necessary if it were planted nigh the wall, and that there was no absolute necessity of such an intermediate Tube, supposing there were a cell to direct the eye fixt to the Eye Glass, and that there were some short cell to carry the Object Glass in at the top, so as to keep it steady, when raised upward or let downwards, the light in the intermediate Rooms not at all hindring, but rather proving of good use to this purpose for seeing the Mensurator) I opened a passage of about a foot square through the roof of my lodgings (see the Fourth *Figure*) and therein fixt a Tube a a perpendicular and upright, of about ten or twelve foot in length, and a foot square, so as that the lower end thereof came through the Ceiling, and was open into the Chamber underneath: This Tube I covered with a lid at the top q, housed so as to throw off the rain, and so contrived, as I could easily open or shut it by a small string n o p, which came down through the Tube to the place where I observed. Within this perpendicular Tube a a, I made another small square Tube b b, fit so as to slide upwards and downwards, as there was occasion, and by the help of a skrew to be fixt in any place that was necessary: Within this Tube in a convenient cell c, was fixt the Object Glass of the Telescope (that which I made use of was thirty six foot in length, having none longer by me, but one of sixty foot, and so too long to be made use of in my Rooms) the manner of fixing which was this: The Glass it self was fixed into a cell or frame of Brass, so exactly fitted to it, that it went in stiff; and to fill up all the Interstitia's, there was melted in hard Cement; this cell had a

D small

ſmall barr that croſſed under the center of the Glaſs, or the aperture thereof; in which barr were drill'd two ſmall holes at equal diſtance from the middle of the Glaſs, thro gh which the upper ends of the two perpendiculars d d were faſtned; and in the fixing this braſs cell or frame into the ſquare Tube that was to ſlide up and down, care was taken to make the barr lye as exactly North and South as could be, though that were not altogether ſo abſolutely neceſſary to this obſervation. Theſe perpendiculars d d faſtned to the barr hung 36 foot and better in length, and had at the lower ends of them two balls of lead e e as big as the Silks could bear, by which the loweſt parts of this Inſtrument were adjuſted, as I ſhall by and by explain. But firſt, I muſt acquaint the Reader, that I opened a ſo perpendicularly under this Tube a hole r r a foot ſquare in the floor below, which with ſhutters could be cloſed or opened upon occaſion; by this means I had a perpendicular Well-hole of about forty foot long, from the top of a to the lower floor s s. Upon the ſecond floor s s I fixed the frame that carried the Eye-glaſs and the other Apparatus fit to make this obſervation. I made then a Stool or Table, ſuch as is deſcribed in the ſame Fourth *Figure* i h h i, having a hole through the top or cover thereof h h, of about nine inches over; the middle of which I placed as near as I could perpendicularly under the middle of the Object Glaſs in the cell above, and then nailed the frame faſt to the floor by the brackets i i, that it could not ſtir; underneath the cover of this Table I made a ſlider g g, in which was fixed in a cell an eye Glaſs f, ſo as that I could through the eye Glaſs moved to and fro, ſee any part of the hole in the Table that I deſired, without ſtirring the ſtool from its fixneſs. This was neceſſary, becauſe many Stars which were forerunners of this Star in *Draco*, and ſerved as warning to prepare for the approaching Star, went pretty wide from the parallel that paſſed over our Zenith; by this means alſo I took notice of the Star it ſelf, at above half a degree diſtance from the Zenith to the Eaſt, and ſo followed the motion of it with my eye Glaſs, and alſo with my meaſuring Clew, and at the ſame time told the Seconds beat by a Pendulum Clock, and ſo was very well prepared to take notice of all things neceſſary to compleat the obſervation, but might have been otherwiſe ſurpriſed

prised by the ſuddain approach and ſwift motion of the ſaid Star. The meaſuring Inſtrument or Menſurator was a round thin plate or circle of Braſs, delineated in the Seventh *Figure*, the aperture a b of which was about nine inches over, croſſed in the middle by two very ſmall hairs a b and c d, which ſerved to ſhew the Zenith point at e, by which the Star was to paſs; there were alſo two other ſmall hairs f g and i h drawn parallel to that which was to repreſent the Eaſt and Weſt line, that paſt under our Zenith, theſe cut the Clue that repreſented the Meridian, or North and South Line at the places k and l, where the perpendicular points were made by the two long plumb lines: This Inſtrument was produced on the ſide a to n, n e being made fifteen times the length of e m, ſo that e m being one inch and two thirds, e n was twenty five inches: at n the line n e was croſt by a rule of about $3\frac{1}{2}$ foot long o p, which from the point n was divided each way into inches and parts, each inch being ſubdivided into thirty parts, which ſerved to determine, though not preciſely, the Seconds on the line c d, for a minute of a degree to a thirty ſix foot Glaſs, being very near one eighth part of an inch, and this eighth part, by the help of the Diagonal, being extended to two whole inches upon the three foot Rule o p, it became very eaſie to divide a part of c d, which ſubtended a minute into ſixty parts, and conſequently to ſubdivide it into Seconds. Now though the ſixtieth part of an eighth of an inch be very hardly diſtinguiſhable by the naked eye, yet by the help of looking through the Eye-glaſs placed in the cell, and ſo magnifying the Objects at the Menſurator more then ſixteen times, 'tis eaſie enough to diſtinguiſh it. But to proceed, I had one ſmall arm m t in the Menſurator, to which the Diagonal thred was faſtned at the point m, which ſerved for the more nice ſubdiviſions into Seconds; The other Diagonal thred which was faſtned at u, ſerved for ſuch obſervations where ſo great niceneſs was not ſo neceſſary, diſtinguiſhing only every four Seconds. The points where theſe Diagonal threds were faſtned, were exactly over the line a b, and the diſtances e m and e u were an inch and two thirds, and five inches.

There is ſomewhat of niceneſs requiſite to the fixing theſe Diagonal threads (which is very material) at m and u, and that

D 2 is

is that there be a ſmall ſpringing ſlit to pinch the hair faſt exactly over the line a b, ſo that the point of its motion may be preciſely in the ſaid Eaſt and Weſt line, and not ſometimes in it, and ſometimes out of it, which it is apt to be, if the Diagonal line be fixt in a hole, and move round in it.

This was the Menſurator by which I meaſured the exact diſtance of the Stars from our Zenith: it may be alſo made uſe of for the meaſuring the Diameters of the Planets; for the examining the exact diſtances of them from any near approaching fixt Stars; for meaſuring the diſtances of the Satellites of *Jupiter* and *Saturn* from their diſcks, for taking the diameters and magnitudes of the ſpots of the Moon, and for taking the diſtances of approaching Stars, and for many other menſurations made by Teleſcopes or Microſcopes, if it be ſo placed as to be in the focus of the Object Glaſs and Eye Glaſs. I could here deſcribe at leaſt thirty other ſorts, ſome by the help of ſcrews, others by the help of wedges, ſome after the way of proportional Compaſſes, others by wheels, others by the way of the Leaver, others by the way of Pullies, and the like; any one of which is accurate enough to divide an inch into 100, 1000, 10000 parts if it be neceſſary; but I muſt here omit them, they being more proper in another place, and ſhall only name one other, becauſe I ſometimes made uſe of it in this obſervation, which is as ſimple and plain as this I have deſcribed, and altogether as accurate; but for ſome accidental circumſtances in the place where I made my obſervation, was not altogether ſo convenient as the former. This Menſurator then is made thus: take a Rule of what length it ſeems moſt convenient for the preſent occaſion, as two, three, or four foot long, repreſented by a b in the Eighth *Figure*, divide this into 100, 1000, 10000 equal parts; with what accurateneſs 'tis poſſible, between the points a b. On the top of this Rule, at each end fix two croſs pieces g h and e f, then from the two croſs pieces e f and g h, ſtrain two very fine and even clues, as Silkworms clues, curious ſmall hairs, or the like, ſo as that they croſs each other at n, and be diſtant at o and p, an inch, or any other certain meaſure deſired. Let this Rule, bezelled on each ſide, ſlip in a frame between two cheeks q and r, upon the top of which ſtrein another ſmall hair as s t. This frame muſt be faſtned to the Teleſcope,

leſcope, ſo as s t may lye in a due poſition to the Eye Glaſs of it. Now in the time of obſervation the frame q r being faſtned to the Teleſcope as above, by ſliding the Rule a b to and fro, you give upon the line s t any length deſired, which is noted out by the line s t upon the rule; for if o p be put one inch, then x y will be $\frac{494}{1000}$ of an inch, and if o p be the ſubtenſe of 10 minutes, then x y will be the ſubtenſe of 4|94; this is ſo plain, ſimple, and eaſie, that as any ordinary Workman will be able to make it, ſo I doubt not but every Reader will, without more application, underſtand both the deſcription and uſe thereof. I ſhall return therefore to the deſcription of the former Menſurator.

The next thing then is the way of fixing this Menſurator, ſo as to ſet the threads in their due poſture, that is Eaſt and Weſt, and North and South, and that they cut each other under the middle of the Glaſs. This laſt was that which had the moſt of difficulty in the whole Experiment. For the performing of this, I removed the ſlider underneath the Table that carried the Eye Glaſs, and alſo the Menſurator, and ſuffered the plumb lines to hang down through the aperture of the Table, and that the Balls might come the ſooner to their perpendicularity, I ſuffered them to hang into a veſſel of water, deep and wide enough, that they might not touch either ſide or bottom.

This expedient of hanging the plumbets in water I mention, becauſe without it 'tis not to be imagined how much time is loſt by expectation of the ſettlement of the ſaid perpendiculars, and how very apt they are to be made to vibrate by the little imperceptible motion of the Air, and by any ſmall hair or other impediment how apt to be put out of their perpendicularity: which by the way makes me very fearful that all common Inſtruments have hitherto been lyable to very great errors, by the unaccurate hanging of their plumb lines, being made for the moſt part to hang and play againſt the ſide of the Inſtrument. By this means they would ſoon come to hang perpendicularly, and be ſo detained when in that poſture; not being apt to be ſtirred by the motion of the Air, or their own ſwing; and whilſt thus ſteady, I fixed two ſmall arms of Braſ, ſuch as are deſcribed in the Seventh Figure by z z, z z, which had ſmall holes at the extreams, with a ſmall ſlit on the ſide to admit

admit or omit the plumb line as there was occasion ; one of these is more at large described in the Sixth *Figure*. Now the plumb line being let into the middle of this, I did with all the accurateness I could so fix the said arm, that the plumb line past exactly through the middle of the hole y. When I was sufficiently satisfied that the plumb line past exactly through the middle of the trying arms, I fixed those arms z z, z z, and removed the plumb lines, then I laid the Mensurator l l in the Fourth *Figure*, upon the surface of the Table, and took great care that the crosses k and l in the Seventh *Figure*, lay exactly under the middle of the holes in the arms, which having done by the help of certain screws, I fixt the Mensurator fast to the Table, and prepared for the observations, putting in the slider g g in the Fourth *Figure*, that carried the cell f, and lying down upon a Couch (k of the Fourth *Figure*) made purposely for this observation, I could look directly upward, and with my left hand move the Cell and Eye Glass so as to find any Star which passed within the hole of the Table, and at the same time with my right hand I could move the Diagonal thread (r m of the Seventh *Figure*) so as to find exactly how far distant from the Zenith e, either Northwards or Southwards, the Stars past the Meridian d c, and giving notice to my Assistant to prepare, he upon the sign given took notice exactly by a Pendulum Clock to the parts of a Second when the said Stars past, and also took notice what division the Diagonal thread m r cut upon the Rule o p.

With all these difficulties I was forced to adjust the Instrument every observation I made, both before and after it was made, which hath often made me wish that I were near some great and solid Tower, or some great Rock or deep well, that so I might fix all things at once, and not be troubled continually thus to adjust the parts of the said Instrument; for whoever hath that opportunity will, I question not, especially if the lines of his Mensurator be made of the single clues of a Silkworm, with much ease discover plainly a change of the distance of Stars of the greater magnitude from the Zenith, in a much shorter time then six moneths. This variation also will be much more easie to be discovered, if instead of a thirty six foot Glass, there be made use of one of four times that length,

length, to wit, one of one hundred fourty four foot; and if inſtead of a Tower ſome deep and dry Well be made uſe of, ſuch as I have ſeen at a Gentlemans houſe not far from *Banſted Downs* in *Surry*, which is dugg through a body of chalk, and is near three hundred and ſixty foot deep, and yet dry almoſt to the very bottom: For ſuch a one is much leſs ſubject to any kind of alteration, either from the ſettling towards this or that ſide, which moſt Towers and high Buildings, whether new or old, are lyable to: This alſo is ſafe from bending and ſhaking with the wind, which I find the ſtrongeſt Houſes, Towers, and Walls, if of any conſiderable height, are apt to do, nor would the wind have any power to ſwerve the perpendiculars, which 'tis almoſt impoſſible to prevent in high Buildings above ground. But this I can only wiſh it were performed, but cannot hope to have any opportunity of Doing it my ſelf. But certainly the diſcovery of the obſervation will abundantly recompenſe thoſe that have the curioſity to make it.

Having thus reſolved upon the way, and prepared the Inſtruments fit for the obſervation, I began to obſerve the Tranſits of the bright Star in the head of *Draco*; and alwayes both before and after the obſervation, I adjuſted the Menſurator by the Perpendiculars, that I might be the more certain of the exactneſs of the Inſtrument; for I often found that when I came to examine the Inſtrument, a day, or two, or three, or more, after a former obſervation, that there had been wrought a conſiderable change in the Perpendiculars, in ſo much as to vary above a minute from the place where I left them, which I aſcribe chiefly to the warping of the Tube that roſe above the roof of the Houſe, finding ſenſibly that a warm day would bend it conſiderably towards the South, and that a moiſt Air would make it bend from the quarter of the wind: But yet I am apt to think there might be ſomewhat alſo of that variation aſcribable to the whole Fabrick of the Roof, and poſſibly alſo to ſome variation of the Floors; but yet I never found theſe variations ſo ſudden, as to be perceptible in the time of a ſingle obſervation, finding alwayes the preceding and ſubſequent adjuſtings to anſwer.

The firſt obſervation I made was the Sixth of *July*, 1669. when I obſerved the bright Star of *Draco* to paſs the Meridian Northwards

Northwards of the Zenith point of the Menſurator, at about two Minutes and twelve Seconds.

The ſecond obſervation I made was upon the Ninth of *July* following, when I found it to paſs to the Northwards of the ſaid Zenith or croſs of the Menſurator near about the ſame place, not ſenſibly differing.

The third obſervation I made upon the Sixth of *Auguſt* following; then I obſerved its tranſitus North of the aforeſaid Zenith, to be about two Minutes and ſix Seconds.

The laſt obſervation I made upon the One and twentieth of *October* following, when I obſerved it to paſs to the North of the Zenith, at one Minute and about 48 or 50 Seconds.

Inconvenient weather and great indiſpoſition in my health, hindred me from proceeding any further with the obſervation that time, which hath been no ſmall trouble to me, having an extraordinary deſire to have made other obſervations with much more accurateneſs then I was able to make theſe, having ſince found ſeveral inconveniencies in my Inſtruments, which I have now regulated.

Whether this Zenith ſo found out upon the Menſurator, be the true Zenith of *Greſham Colledge*, is not in this inquiry very material (though that alſo I deſigned to examine, had not an unhappy accident broken my Object Glaſs before I could compleat the obſervation) for whether it were, or were not, it is certain that it alwayes had the ſame poſition to the true Zenith, the Object Glaſs and Perpendiculars having not been in all that time removed out of the Cell, whence if the ſaid Object Glaſs were thicker upon one ſide then upon the other (which is very common and very ſeldome otherwiſe) and conſequently deflected the ray towards the thicker ſide, and ſo made the Perpendicular of the Menſurator to lye on that ſide of the true Perpendicular, that the thicker ſide of the Object Glaſs reſpected, yet it being alwayes ſo if the tranſitus of the Star varied from this falſe Perpendicular, it muſt alſo vary from the true one. The manner how I deſigned to examine and find out the true Perpendicular, is this, which is the way alſo of adjuſting of Teleſcopical ſights, as I ſhall afterwards have occaſion to ſhew. Having marked the four ſides of the Glaſs, the North with N, the Eaſt with E, the South with S, and the Weſt with W, about the firſt of of *June* I begin to obſerve and meaſure the true diſtance of ſome remarkable fixt Star, as of this of *Draco* from the Zenith found one night when the ſide N of the Glaſs ſtood North. Then I change the ſide of the Object Glaſs, and put the North ſide Southwards, and the South, Northwards, and obſerve the Tranſitus of the ſame Star the next night, and note down the ſame; the third night following I put the Eaſt ſide or E North, and obſerve the tranſit of the ſame Star over the Meridian; and the fourth night I put the Weſt ſide or W North, and obſerve the tranſit of the ſaid Star. Now by comparing all theſe together, it will be very eaſie to deduce what the falſe refraction of the Object Glaſs is, and which way it lyes, and conſequently to regulate the apparent Zenith by the true one. But this only by the by.

'Tis manifeſt then by the obſervations of *July* the Sixth and Ninth: and that of the One and twentieth of *October*, that there is a ſenſible parallax of the Earths Orb to the fixt Star in the head of *Draco*, and conſequently a confirmation of the *Copernican* Syſtem againſt the *Ptolomaick* and *Tichonick*.

Before I leave this Diſcourſe, I muſt not forget to take notice of ſome things which are very remarkable in the laſt obſervation made upon the 21 of *October*. And thoſe were theſe. Firſt, that about 17 minutes after three a-clock the ſame day, the Sun being then a good way above the Horizon, and ſhining very clear into the Room where I lay to obſerve, and having nothing to ſcreen off the rayes of light, either in the Room where I was, or in the next Room through which I looked, I obſerved the bright Star in the *Dragons* head to paſs by the Zenith as diſtinctly and clearly as if the Sun had been ſet, though I muſt confeſs it had loſt much of the glaring brightneſs and magnitude it was wont to have in the night, and its concomitants were vaniſht: The like I found it divers other dayes before, when I obſerved it, the Sun ſhining very cleer into both the aforeſaid Rooms, which by the way I ſuppoſe was the firſt time that the fixt Stars were ſeen when the Sun ſhin'd very bright, without any obſcuring of its light by Eclipſe or otherwiſe. And though we have a great tradition that the Stars may be ſeen with the naked eye out of a very deep Well or Mine in the day, yet I judge it impoſſible, and to have been a meer fiction, without any ground: For the being placed at the bottom of a Well doth not at all take away the light of the Atmoſphere from affecting the eye in and near the Axis of viſion, though

E indeed

indeed the ſides thereof may much take off the lateral rayes; but unleſs the radiation of the falſe rayes of the Star be brighter then that of the Air, the true rayes from the body are ſo very ſmall, that 'tis impoſſible the naked eye ſhould ever be affected by them. For in the ſecond place, by this obſervation of the Star in the day time when the Sun ſhined, with my 36 foot Glaſs I found the body of the Star ſo very ſmall, that it was but ſome few thirds in Diameter, all the ſpurious rayes that do beard it in the night being cleerly ſhaved away, and the naked body thereof left a very ſmall white point.

The ſmalneſs of this body thus diſcovered does very fully anſwer a grand objection alledged by divers of the great *Anti-copernicans* with great vehemency and inſulting; amongſt which we may reckon *Ricciolus* and *Tacquet*, who would fain make the apparent Diameters of the Stars ſo big, as that the body of the Star ſhould contain the great Orb many times, which would indeed ſwell the Stars to a magnitude vaſtly bigger then the Sun, thereby hoping to make it ſeem ſo improbable, as to be rejected by all parties. But they that ſhall by this means examine the Diameter of the fixt Stars, will find them ſo very ſmall, that according to theſe diſtances and Parallax they will not much differ in magnitude from the body of the Sun, ſome of them proving bigger, but others p oving leſs; for the Diameter of the parallactical Circle among the fixt Stars, ſeems to exceed the Diameter of the Star almoſt as much as the Diameter of the annual Orb of the Earth doth that of the Sun. And poſſibly longer and better Teleſcopes will yet much diminiſh the apparent bulk of the Stars by bringing fewer falſe rayes to the eye that are the occaſion of the glaring and magnifying of the ſaid bodies. It may for the preſent ſuffice to ſhew that even with this Glaſs we find the Diameter of this Star conſiderably ſmaller then a Second, and the Parallax we judge may be about 27 or 30 Seconds. It will not therefore be difficult to find many Stars whoſe Diameters ſhall be leſs then a two hundredth part of this Parallax, as poſſib'y upon more accurate obſervation this very Star may be found to be. Now we find that the Diameter of the Orb of the Earth is but two hundred times bigger then the Diameter of the Sun in the Center thereof; and therefore if the parallactical difference be found to be two hundred times more then the viſible Diameter of the Star, the Star will prove but of the ſame magnitude with the Sun. This

This Discovery of the possibility and facility of seeing the fixt Stars in the day time wher the Sun shines, as I think it is the first instance that hath been given of this kind, so I judge it will be a discovery of great use for the perfecting Astronomy; as first, for the rectifying the true place of the Sun in the Ecliptick at any time of the year; for since by this means 'tis easie to find any Star of the first, second, or third magnitude at any time of the day, if it be above the Horizon, and not too near the body of the Sun: And since by a way I shall shortly publish any Angle to a Semicircle in the Heavens, may be taken to the exactness of a Second by one single observator: It will not be difficult for future Observators to rectifie the apparent place of the Sun amongst the fixt Stars to a Second, or very near, which is one hundred times greater accurateness, then has hitherto been attained by the best Astronomers. The like use there may be made of it for observing any notable appulse of the ☽, ♃, ♄, ♂, and ♀, to any notable fixt Star that shall happen in the day time, which may serve for discovering their true places and parallaxes. The Refractions also of the Air in the day time may by this means be experimentally detected.

I should have here described some Clocks and Time-keepers of great use, nay absolute necessity in these and many other Astronomical observations, but that I reserve them for some attempts that are hereafter to follow, about the various wayes I have tryed, not without good success of improving Clocks and Watches, and adapting them for various uses, as for accurating Astronomy, compleating the Tables of the fixt Stars to Seconds, discovery of Longitude, regulating Navigation and Geography, detecting the proprieties and effects of motions for promoting secret and swift conveyance and correspondence, and many other considerable scrutinies of nature: And shall only for the present hint that I have in some of my foregoing observations discovered some new Motions even in the Earth it self, which perhaps were not dreamt of before, which I shall hereafter more at large describe, when further tryals have more fully confirmed and compleated these beginings. At which time also I shall explain a System of the World differing in many particulars from any yet known, answering in all things to the common Rules of Mechanical Motions: This depends upon three Suppositions. First, That all Cœlestial Bodies whatsoever, have an attraction or gra-

E 2 vitating

vitating power towards their own Centers, whereby they attract not only their own parts, and keep them from flying from them, as we may obſerve the Earth to do, but that they do alſo attract all the other Cœleſtial Bodies that are within the ſphere of their activity; and conſequently that not only the Sun and Moon have an influence upon the body and motion of the Earth, and the Earth upon them, but that ☿ alſo ♀, ♂, ♄, and ♃ by their attractive powers, have a conſiderable influence upon its motion as in the ſame manner the correſponding attractive power of the Earth hath a conſiderable influence upon every one of their motions alſo. The ſecond ſuppoſition is this, That all bodies whatſoever that are put into a direct and ſimple motion, will ſo continue to move forward in a ſtreight line, till they are by ſome other effectual powers deflected and bent into a Motion, deſcribing a Circle, Ellipſis, or ſome other more compounded Curve Line. The third ſuppoſition is, That theſe attractive powers are ſo much the more powerful in operating, by how much the nearer the body wrought upon is to their own Centers. Now what theſe ſeveral degrees are I have not yet experimentally verified; but it is a notion, which if fully proſecuted as it ought to be, will mightily aſſiſt the Aſtronomer to reduce all the Cœleſtial Motions to a certain rule, which I doubt will never be done true without it. He that underſtands the nature of the Circular Pendulum and Circular Motion, will eaſily underſtand the whole ground of this Principle, and will know where to find direction in Nature for the true ſtating thereof. This I only hint at preſent to ſuch as have ability and opportunity of proſecuting this Inquiry, and are not wanting of Induſtry for obſerving and calculating, wiſhing heartily ſuch may be found, having my ſelf many other things in hand which I would firſt compleat and therefore cannot ſo well attend it. But this I durſt promiſe the Undertaker, that he will find all the great Motions of the World to be influenced by this Principle, and that the true underſtanding thereof will be the true perfection of Aſtronomy.

LONDON,

Printed for *John Martyn*, Printer to the *Royal Society*. 1674.

ANIMADVERSIONS

On the first part of the

MACHINA COELESTIS

Of the Honourable, Learned, and deservedly Famous

Astronomer

JOHANNES HEVELIUS

CONSUL OF

DANTZICK;

Together with an Explication of some

INSTRUMENTS

MADE BY

ROBERT HOOKE, Professor of Geometry in *Gresham College*, and Fellow of the *Royal Society*.

LONDON,

Printed by *T.R.* for *John Martyn* Printer to the *Royal Society*, at the *Bell* in St. *Pauls* Church-yard. 1674.

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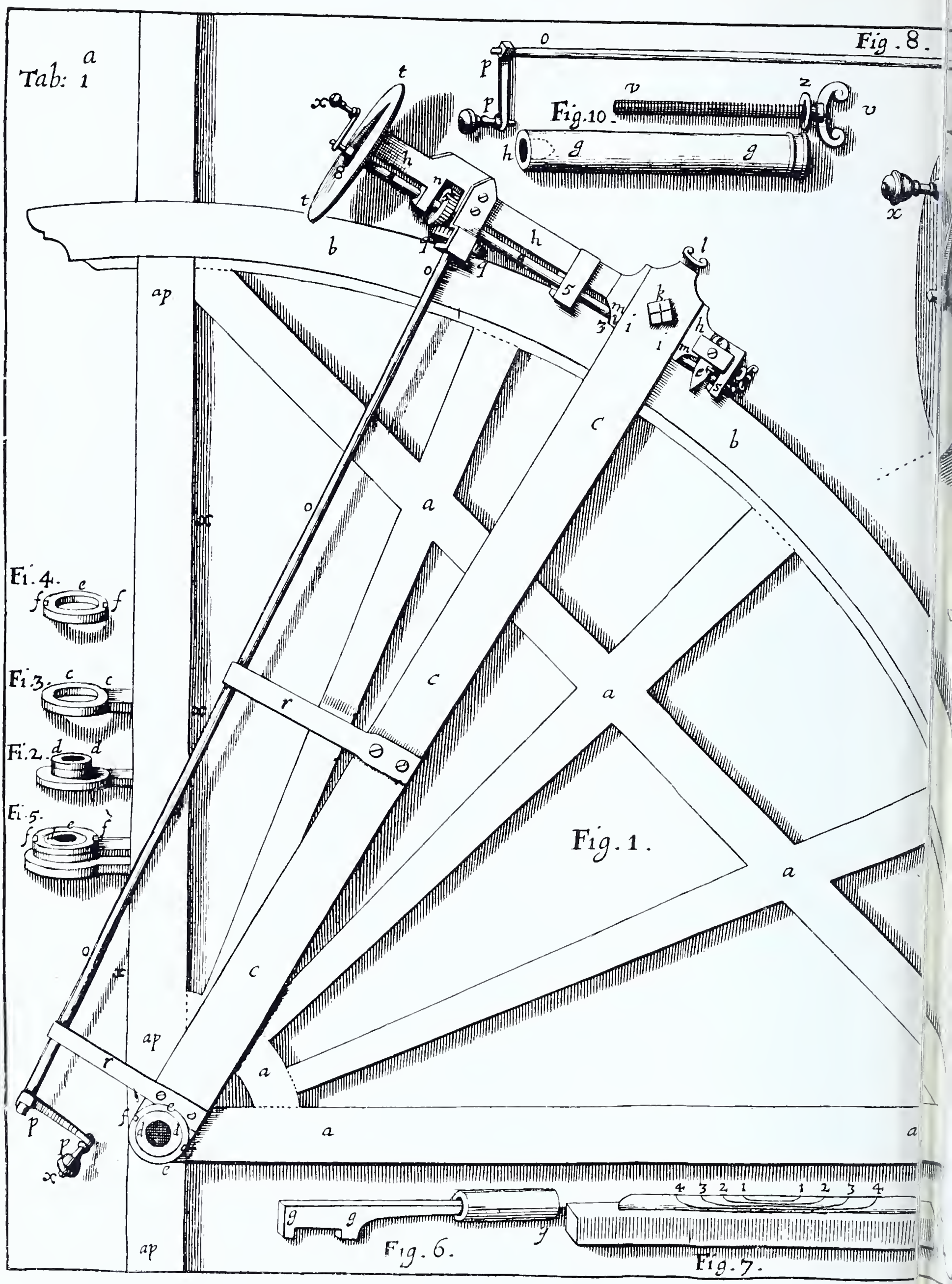
Hevelius



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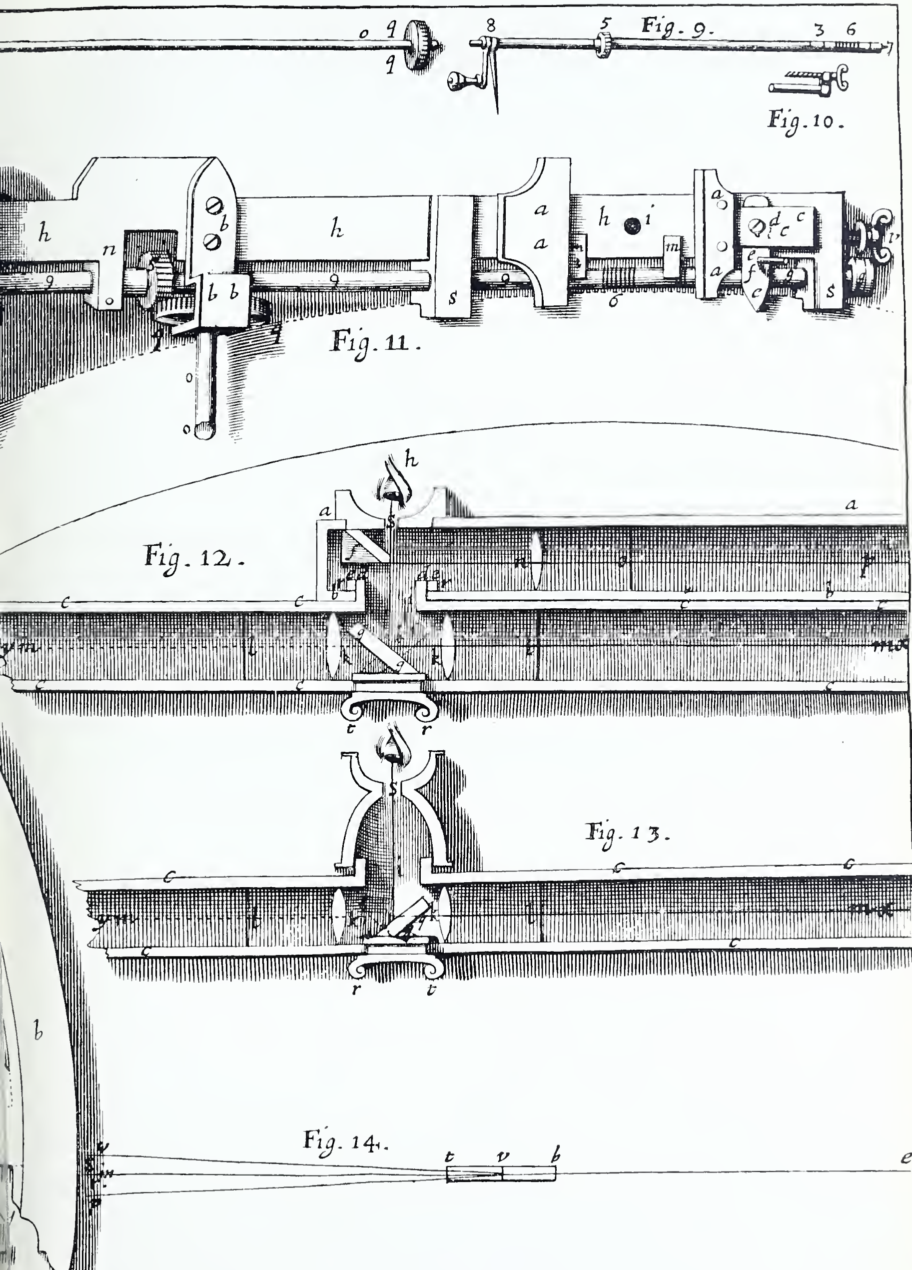
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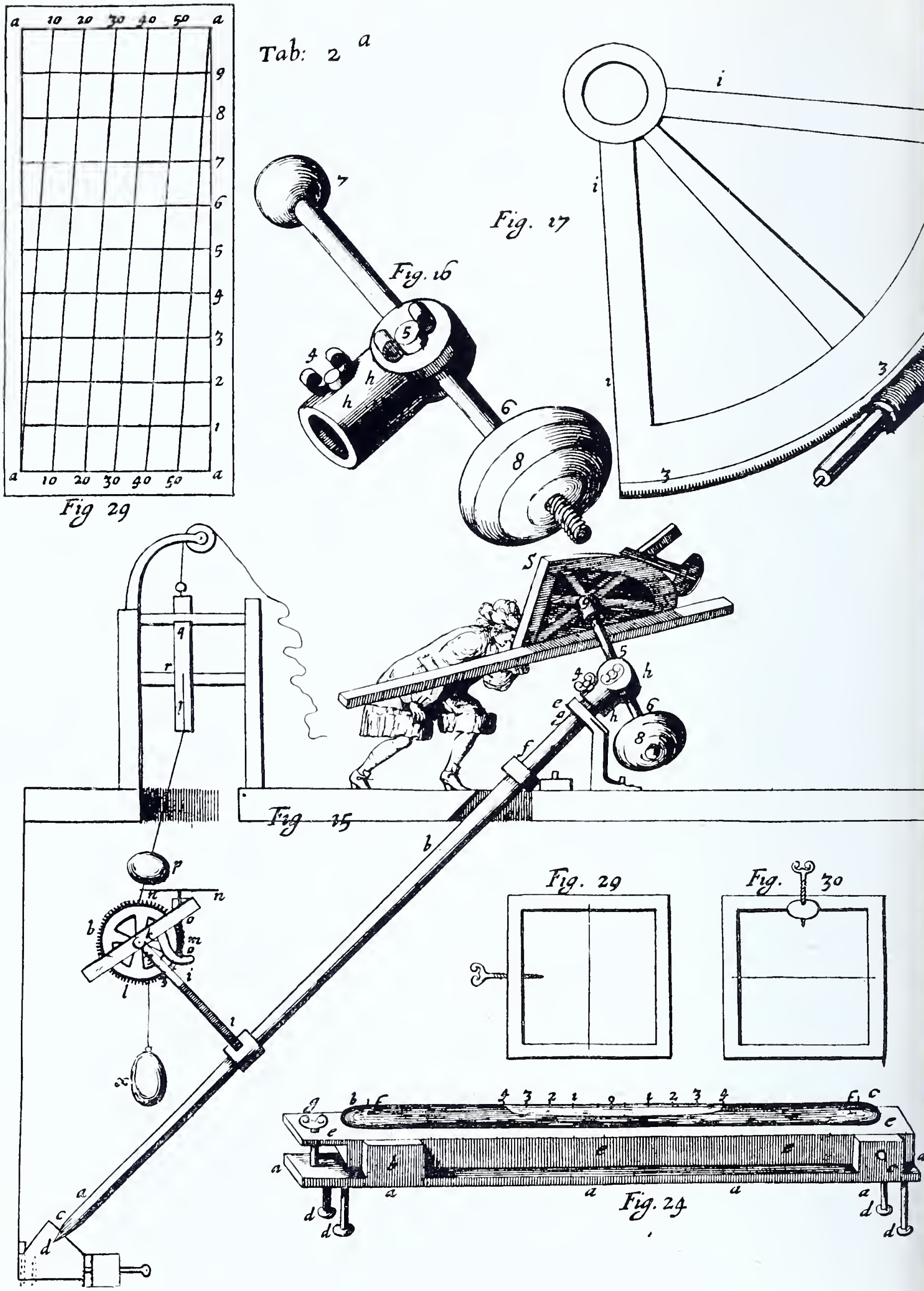
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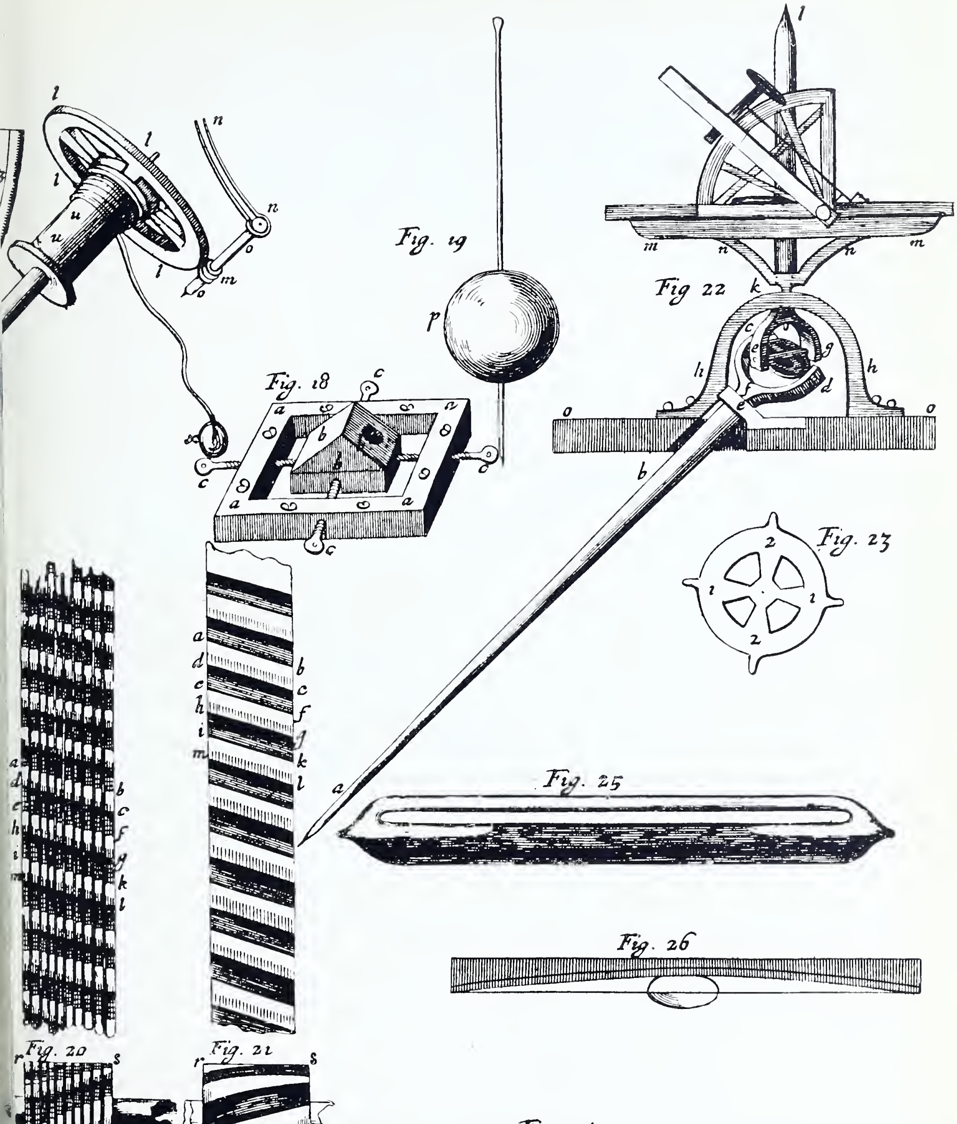
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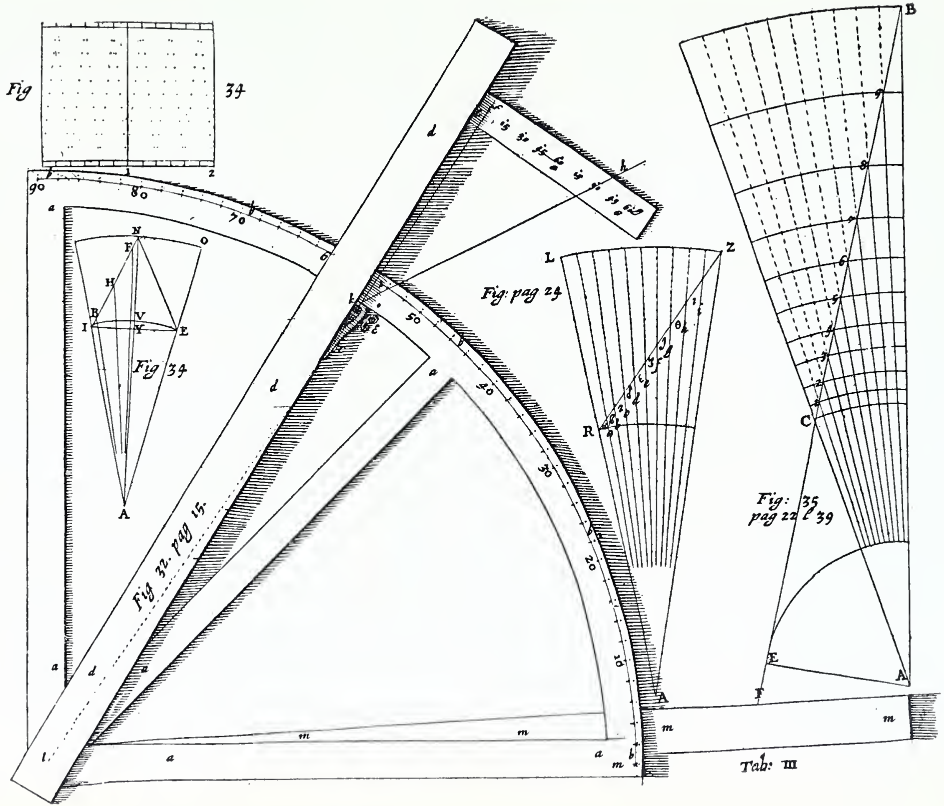
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SOME
ANIMADVERSIONS
On the first Part of
HEVELIUS
His *MACHINA COELESTIS*, &c.

HAVING lately perufed a Difcourfe of *Hevelius*, newly publifhed, entituled, *Johannis Hevelii Machina Cæleftis, pars prior Organographiam five inftrumentorum Aftronomicorum omnium quibus Autor hactenus fidera rimatus & dimenfus eft accuratam delineationem & defcriptionem, plurimis Iconibus æri incifis illuftratam & exornatam exhibens*, &c. and finding it a Difcourfe about practical and mechanical Knowledge, and of that kind wherein Geometry feems to be more then ordinarily concerned; I thought it might not be ungrateful to my Auditory, (*nor improper to the Subject of Sr.* JOHN CUTLER'*s Lecture, which is partly Mechanical and partly Phyfical*) to confider a little the Contents thereof: And fomewhat the rather too, becaufe having heretofore communicated to him fomewhat of this Subject, which I had occafion to read in this place in one of my former *CUTLERIAN* Lectures, I find he hath made fome Animadverfions and reflections thereupon.

A I

I find then that this excellent Perſon hath been for the moſt part exceedingly circumſpect, to find out the inconveniences and difficulties that do accrew to the beſt Obſervators, even with the beſt inſtruments, and has not been leſs induſtrious to find out ways to obviate and overcome them; In the doing of which, he ſeems not to have ſpared either for labour and vigilancy, or for any coſt and charges that might effect his purpoſe, for which he hath highly merited the eſteem of all ſuch as are lovers of that Science: But yet if he had proſecuted that way of improving Aſtronomical inſtruments, which I long ſince communicated to him, I am of opinion he would have done himſelf and the learned World a much greater piece of ſervice, by ſaving himſelf more then $\frac{9}{10}$ of the charge and trouble, and by publiſhing a Catalogue ten times more accurate. For though I doubt not in the leaſt but that he hath by his own extraordinary diligence, care and coſt, corrected ſeveral miſtakes and errors committed by the aſſiſtants of the Noble *Ticho*: yet I am not ſatisfied that his Inſtruments are capable of making Obſervations more accurately then thoſe of *Ticho*, though 'tis poſſible they may do it with ſomewhat leſs trouble and inconvenience. For firſt, I find that thoſe of *Ticho* were as large as thoſe of *Hevelius*, and conſequently were capable of as accurate and minute diviſions, and of as long and convenient Sights. Secondly, I find that the Sights made uſe of by *Hevelius* are the very ſame, at leaſt not at all materially differing from thoſe of *Ticho*, being only naked Sights, made by a ſlit and edge, ſerving only to regulate the direction of the naked eye, but no ways capable of aſſiſting the eye to diſtinguiſh more accurately the object. Thirdly, I find that though the way of Diviſion made uſe of by *Hevelius*, be a very ingenious invention, and that which is Geometrically true and certain, yet if we conſider the great difficulty there is in Mechanically performing it, we ſhall find it not much preferrable, if altogether as good as that of *Ticho*. And 'tis plain enough that *Ticho* himſelf was not ignorant of it, though his particular reaſons why he made no more uſe of it, we certainly know not: 'Tis very probable, becauſe he thought it not altogether ſo accurate, as that he did make uſe of. For ſomewhat to this purpoſe he ſays himſelf, in the ſecond Book of his Obſervations of the Comet of

1577.

1577. pag. 461 *Hanc graduum in singula minuta*, meaning the Division by Diagonal Lines; *& etiam horum in dena scrupula secunda subdivisionem in omnibus meis machinis Astronomicis usurpo, eo quod illam multis ab hinc annis exquisitissimam expertus sum. Licet enim ejus demonstratio in Rectilineis parallelogrammis proprie conveniat, nihilominus arcualibus etiam in tam exili interstitio quod à recta linea insensibiliter differt, citra omne erroris vestigium convenienter applicatur.* 'Tis true, *Ticho*'s Objection against this way of Division by Diagonals is material, as to a Geometrical accurateness, but his Answer to it is altogether as material, that though it be not exactly true, yet it doth *insensibiliter differre*, and so long as the error is not discovered by sense, there can be no error committed in observation; and indeed the whole matter both one way and the other is insignificant, and but a vain curiosity to endeavour to divide an instrument into seconds, or parts smaller then a minute, for *I* shall by and by shew that the eye can hardly distinguish minutes in the object: But were such niceness of Division of any use, 'tis easily enough to be done to Mathematical truth; for as *I* shall anon shew, there is a certain distance of each of the parallel Circles, which being given, the straight Diagonal Lines will divide the degree, by the intersection with those parallel Circles, into exactly equal parts, which would have better answer'd *Ticho*'s Objection, had he known it, which *I* wonder, *I* confess, how he could over-see, since he seems to have spent many thoughts on the matter; but this only by the By, because *I* shall speak more at large of it afterwards. But he proceeds to this other way of Divisions, which he, as well as *Hevelius*, ascribes to *Nonnius*, whereas the other that he approves of came first from *England*, as it appears by a passage in another Book of his, where he discourses somewhat of the same Subject.

Altera Divisio ad clarissimi Mathematici Petri Nonnii *in Libello de crepusculis propositione tertia imitationem per plures quadrantis arcus introrsum descriptos, & diversimodè subdivisos procedit; etsi autem in hac ipsa apprime ingeniosa* Nonnii *inventione aliquid Auctuarii loco expeditius à nobis additum est, ita ut exterior arcus in plurimas partiunculas dividatur, neq; is ordo aut numerus arcuum sese introrsum concomitantium quem ille præfinivit*

A 2 *sed*

ſed multo expeditior & perfectior obſervetur ; (*I* am apt to think he knew this very way, and here hints it :) *Tamen quia hæc ſubtilitas cum ad praxin deventum eſt plus habeat laboris quam fructûs, neque id in receſſu præſtet quod prima fronte pollicetur, ut alibi plenius oſtendemus, idcirco apud nos dudum in uſu eſſe deſiit.* From which words, and alſo from what he ſays in his firſt Book of the new Star in 1572. pag. 671. ſpeaking of the compariſon between theſe two ways of Diviſions, to wit, *Sit cujuſcunq; velit ingenioſa certe & apprime utilis eſt diſtributio, quam & ego poſtea arcualibus graduum diviſionibus in quadrantibus ſextantibus & armillis, non inconcinnè aut infrugiferè applicui. Licet enim demonſtratio ejus in ſolis rectilineis ſuperficiebus ad unguem ſe habeat ; tamen cum quinorum vel denorum minutorum ſpatium in circumferentiis majuſculis à rectilineo inſenſibiliter differat, hic quoq; ejus uſus ſatis commodus & ratus eſſe poterit, multoq;* Nonniana *plurimorum arcuum intricata & difficili ſubdiviſione expeditior aptiorq; deprehenditur.* From his Diſcourſe I ſay in theſe two places, and from ſeveral others diſpers'd up and down his Works, which 'twould be too long now to quote, 'tis evident that *Ticho* was not ignorant of this way of Sub-diviſion, ſo much applauded by *Hevelius*, invented by *Petrus Nonnius*, and promoted by *Ticho* himſelf ; and yet we ſee he prefer'd that way of Diagonals, firſt made uſe of in *England* by the moſt skilful Mathematician *Richard Cantzler*, before it, rejecting the one and making uſe of the other in all his Inſtruments. But either of them will do well enough if the Diviſions be done with great circumſpection and care, and inſtruments of the ſize of thoſe larger ones both of *Hevelius* and *Ticho*, are capable of Diviſions ten times more accurate then are needful for common Sights, be they never ſo long, without making uſe of either *Ticho*'s or *Hevelius*'s way of Diviſion, the eye being unable to diſtinguiſh a ſmaller Angle. To what purpoſe therefore is it to make the Diviſions ſo fine, or any one part of the inſtrument or obſervation more accurate then another ? ſince the power of diſtinguiſhing by the naked eye is that which bounds and limits all the other niceneſs, and whatever part is more curious then that can equalize, is of no ſignificancy. For inſtance, in taking the altitude of a Star, it would be but labour loſt to diſtinguiſh by the Diagonals, or

otherwiſe

otherwiſe to Seconds, whilſt in the mean time you are not certain that the Plumb-line is true to a minute, or whilſt you are not able to direct the Ruler, bearing the Sights to a greater certainty then to that of a minute. And the like might be ſaid of the extraordinary curioſity in any two parts, and the failure in any third, that is eſſential to an obſervation ; as fruitleſs it is to calculate to ſeconds, when the obſervations are not true to minutes, or to be certain by the Sights and Diviſions to ſeconds, and uncertain in the Plumb-line to minutes.

There is therefore one thing in *Hevelius* his Inſtruments, that though they be never ſo large, never ſo accurately divided, of never ſo choice and convenient materials, and never ſo tractable for uſe, and never ſo skilfully and induſtriouſly uſed, will notwithſtanding make them all equal as to uſe, with one of about two or three foot radius of mettal with *Ticho*'s Sights and Diagonal Diviſions, which is occaſioned by the limited power of diſtinguiſhing by the naked eye.

Something to this purpoſe *I* communicated to *Hevelius* in the year 65. and hoped that *I* might have thereby ſomewhat aſſiſted him in his great and laborious Work, firſt by eaſing the eye, and next by making it capable of diſtinguiſhing more exactly, *I* having hinted to him the way how to reform and obviate that inconvenience by Teleſcopical or Perſpective Sights, as alſo the way of making inſtruments of much leſs bulk, to do ten times more then 'twas poſſible to do with the largeſt inſtruments made the common way. *In* anſwer to which he returns me this Diſcourſe, in a Letter to the Royal Society, in the year 65.

MODUS ille obſervandi per Teleſcopia adminiculo Sextantis vel Quadrantis, videtur mihi vix adeò tutus, quam vulgaris, ſi pinnacidia rectè ac juſtè ſint affixa. Hæc enim ſunt immobilia ; Teleſcopia verò nullâ ratione adeò firmiter affigi poſſunt ut loco haud dimoveantur ; etiamſi omni diligentiâ juxta methodum deſcriptum per totum Horizontem experiundo ſint ſemel collocata. Adhuc quàm arduum ſit, eâ ratione verum eorum locum indagare, ſatis ſuperq; expertus ſum ; ſic ut vix videam, an alicui

circa

circa restitutionem Fixarum Planetarumq; adminiculo esse possint; in majoribus scilicet illis distantiis capiendis: In minoribus, largior, posse aliquid præstari; sed an Instrumenta, unius Spithamæ radio instructa, elaborari possint multò exactius, quàm optima quævis, vulgares Dioptras habentia, licet 60 pedum radio elaborata, nollem adhuc asseverare. Multa namq; in Theoriâ videntur certissima, quæ in praxi satis longe nonnunquam à vero recedunt. Si quis mihi certas observationes quarundam distantiarum & quidem Fixarum, circa Eclipticam & Æquatorem existentium, illis ipsis Instrumentis, Dioptris Telescopicis instructis habitas exhiberet: (utpote distantiam Lucidæ ♈ à Palilicio; Palilicii à Polluce; Pollucis à Regulo; Reguli à Spicâ ♍; Spicæ ♍ à Boreal. sinist. manus Serpentarii; Boreal. sinist. manus Serpentarii ab Aquilâ; Aquila à Marcab; & Marcab à Lucidâ Arietis) vellem protinus de rei illius certitudine & meum quale quale judicium ferre; sed antequam eas observationes obtineam, judicium suspendo. Interea utiq; fateor; si quis adminiculo minoris cujusdam Instrumenti observationes corporum Cælestium peragere potest, multò sane illum esse feliciorem, variis de causis, eo, qui per majora id præstare allaborat. Rationes dividendi Instrumenta, diversæ quidem mihi probè cognitæ sunt; easq; etiam in usum transtuli; num autem sint eadem quas Clarissimus Dominus Hookius *novit, ac invenit, me prorsus latet: Si illi non adversum est, rogo, ut precipuas communicet, ego ut meas intelligat rursus studebo.*

Since which time *I* have not sent any other description of instruments, save that of the manner of making and using a Tube for a 60 foot Glass, which *I* am much pleas'd to find he makes use of, and should gladly have communicated any thing further, if *I* had not found they were esteemed insignificant. *It* did much trouble me, *I* confess, that *I* could not prevail with him to make use of Telescopical Sights at least, since with less trouble he would have afforded the World Observations, and a Catalogue of the Stars, ten times more exact. And *I* am the more sorry to find that he hath proceeded to finish his *Machina Cælestis*, by instruments not more accurate then those of *Ticho*, and that he still remains in the same opinion of Telescopical Sights, and other improvements of instruments. For pag. 293. of this first Part of his *Machina Cælestis*, speaking concerning Sights,

Sights, he ſays, Poſſibly ſome may wonder that *I* do not make uſe of Teleſcopical Sights, ſince they are by ſome accounted better and more accurate, inſomuch that there is one in the World hath proceeded ſo far, as to ſuppoſe Teleſcopical Sights to be ten, twenty, thirty, nay forty times more accurate then the common Sights; and that 'tis poſſible to make an inſtrument of a Span Radius to do more with Teleſcopical Sights, then an inſtrument of 60 foot with the common Sights. 'Twould be a thing of much moment could it be done, and not to be valued by money, but many things do ſeem true in the Theory, which do not anſwer upon Experience. You may perceive by comparing this ſlender Refutation with his Letter before, who he means by the Aſſertor of Teleſcopical Sights. But *I* am troubled he ſhould think them ſo ſlight as not to deſerve one tryal in ſeven years time, eſpecially ſince by explaining the manner of making uſe of them much in the ſame ſenſe with that which *I* ſent him, he ſeems to have underſtood enough of the way to have made uſe of it if he would. As to his Objection, That the Glaſſes are apt to be broke, and the Pins or Threads are apt to be bent and broke, there is not the leaſt colour for it, for they cannot without much labour and deſign be broken or put out of order, but if they were, it might as well be ſaid, that the Plumb-line of any of his inſtruments may be broken, or his Sights bended, and the like, and therefore thoſe inſtruments were not to be uſed. But theſe Objections *I* ſhall not urge againſt his inſtruments, nor a great many other *I* could produce of leſſer moment, but only this one which is very fundamental, and cannot any ways be helped but by the help of Glaſſes, and that is, 'Tis impoſſible with Sights made after *Ticho*'s or *Hevelius* his way, to diſtinguiſh any diſtance in the Heavens leſs then half a minute, or thirty Seconds, and hardly one of a hundred can diſtinguiſh a minute.

And this being proved, what will become of all the machinations and contrivances for greater inſtruments, to ſhew the Diviſions of ſingle or double Seconds? May not ſingle minutes, nay half minutes, by the help of Diagonal Diviſions, be ſufficiently diſtinguiſhed in an inſtrument of three foot Radius? What need is there then of all the other cumber? Certainly any one that will but try with the one and the other inſtrument, will

will find himſelf able to do as much with an inſtrument of three foot, as with one of threeſcore, ſince the eye cannot diſtinguiſh a leſs Angle, at leaſt none that *I* have yet met with hitherto. Who is there that by his bare eye can diſtinguiſh any of the Teleſcopical ſpots in the Moon, though ſome of them are above a minute in Diameter? As for inſtance, Who can ſee Mount *Sinai*, ſo call'd by *Hevelius*, which is a bright ſpot in a dark field, and conſequently muſt appear near two minutes in Diameter to the naked eye? Or who can ſee the *Palus Mareotis*, or the *Lacus niger*, which are two dark ſpots in light fields, and each more then a minute in Diameter? Now if the eye cannot diſtinguiſh a ſmaller object then appears within the angle of half a minute, 'tis not poſſible to make any obſervation more accurate, be the inſtrument never ſo large.

Now that any one may preſently ſatisfie himſelf of the truth of what *I* aſſert, concerning the limited power of the naked eye, as to the diſtinguiſhing of Angles; Let him take a ſheet of white Paper, and thereon draw two parallel Lines, as O O, and P P, in the 28th. Figure, at four or five inches diſtance, then draw as many other ſmall lines between them at right angles to them, and parallel one with another, as he think convenient, as a a, b b, c c, d d, e e, f f, g g, h h, i i, *&c.* and let them be drawn diſtant from each other an inch, then let him alternately blacken or ſhadow the ſpaces between them, as between a a and b b, between c c and d d, between e e and f f, between g g and h h, between i i and k k, between l l and m m, *&c.* leaving the other alternately white, then let him expoſe this Paper againſt a Wall open to the light, and if it may be ſo that the Sun may ſhine on it, and removing himſelf backwards for the ſpace of $287\frac{1}{3}$ feet, let him try whether he can diſtinguiſh it, and number the dark and light ſpaces, and if his eyes be ſo good that he can, then let him ſtill go further backwards and backwards from the ſame, till he finds his eyes unable any longer to diſtinguiſh thoſe Diviſions, there let him make a ſtand, and meaſure the diſtance from his eye to the aforeſaid Paper, and try by calculation under what Angle each of thoſe black and white ſpaces appears to his eye, for by that means it will be manifeſt how ſmall an Angle his eye is capable of diſtinguiſhing, and beyond which it cannot reach: Which being

being once known, he hath a Standard, by which he is able to limit the bigneſs and exactneſs of his Inſtruments, if he make uſe of common Sights, beyond which all magnitude and curioſity is not only uſeleſs, but of much detriment upon many accounts.

This is that Conſideration which I could wiſh had occur'd both to *Ticho Brahe* and to *Hevelius*, eſpecially to the latter, who hath ſo earneſtly endeavour'd to out-do the former, and for the accompliſhment thereof, ſeems to have ſpared no charge, labour, or endeavour he was able to expend. I hope at leaſt that this publick notice will for the future engage all ſuch as ſhall attempt this Work, to be as ſollicitous about aſſiſting the Eye in the diſcovery of the parts of the Object, as of diſtinguiſhing the Diviſions of the Inſtrument, for the doing of the one without being able to reach the other, will avail nothing.

Thoſe therefore that deſire or need Inſtruments to make Obſervations to Seconds, muſt take another courſe then any that I know yet deſcribed. 'Tis true indeed, That Altitudes of the Sun may be taken, with the Sights commonly uſed for that purpoſe, to what accurateneſs is deſired, if the Inſtrument be large enough, becauſe the Image of the Sun being tranſmitted by the upper Sight through a ſmall round hole, is repreſented within a Circle upon the lower Sight, and by means of the eyes approaching near that Sight, 'tis poſſible by Inſtruments large enough, to arrive at the accurateneſs of a Second, in Obſervations made of that kind. And ſomewhat of this may be done alſo by the Moon, when very bright and clear, but in all the other celeſtial Bodies it has never yet been done.

But then if we compare even this way with that of Teleſcopes, *cæteris paribus*, we ſhall find it much ſhort, both as to clearneſs and diſtinctneſs, and therefore even here alſo Teleſcopical Sights are to be preferred, as I ſhall ſufficiently manifeſt hereafter more at large, when I come to deſcribe my own Inſtruments for this purpoſe; for I doubt not but to make it ſufficiently plain, That by the help of an Inſtrument I have contrived, of three foot Radius, I will be able to make all Obſervations whatſoever, ten times more accurate, excepting thoſe of the Sun, then any one can make with the largeſt Inſtrument,

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ſtrument, deſcribed either by *Ticho* or *Hevelius*, and to manage the ſame with a quarter the trouble, clutter, and *Apparatus* neceſſary to either of theirs, and to make the Diviſions as accurate and ſenſible as can be deſired.

For the doing of which, I will ſhew, Firſt, How to make the Plain of the Inſtrument, that it ſhall not be ſubject to bending or warping, and yet be ſo light as to be eaſily manageable. Secondly, How to make the Diviſions on that Inſtrument, ſo as to diſtinguiſh certainly and exactly to Seconds, without any trouble, or wearying the ſight. Thirdly, I will ſhew how to make the Sights of that Inſtrument, ſo as to diſtinguiſh the parts of the Object to Seconds, if need be, even by thoſe who cannot diſtinguiſh to Minutes with common Sights, certainly, and without fallacy or error. Fourthly, How to make the Sights, ſo as to ſee two Objects, though never ſo far diſtant, with one glance of the eye. And Fifthly, I will ſhew how to adjuſt the Perpendicular, ſo as to ſet it exactly upright and plain to a Second, ſo that if it meets with a diligent, accurate, and experienced Obſervator, it will ſerve to make as curious Obſervations as are hitherto deſirable. Sixthly, I will ſhew a way how to fix this Inſtrument, either for taking Altitudes or Azimiths, ſo as to be manageable with the leaſt trouble imaginable, for Obſervations of that kind, and to be always ſteady and fixt in any Perpendicular poſture, to whatever Azimith it be apply'd. Seventhly, I will explain an exact way for fixing the Inſtrument, ſo as to take the Diſtances of any two Stars, or celeſtial Object, and ſeveral other contrivances of the like nature. But of each of theſe hereafter, after I have examin'd over the ſeveral particulars mention'd by *Hevelius*, in his Deſcriptions of the Inſtruments and Contrivances made uſe of by himſelf.

To paſs by then his long Preface, and the Diſcourſe of Inſtruments in general, which he hath premiſed in the firſt Chapter; I ſhall proceed to an examination of thoſe Inſtruments of his own, which he doth more fully and particularly deſcribe.

The firſt of which kind I find to be a Quadrant of Braſs, which he deſcribes in the ſecond Chapter, and begins with that firſt, as being an Inſtrument which he leaſt eſteem'd, and which

which at length he made no uſe of, though for many Reaſons I think of a quantity big enough, to be as good, nay better, then any he made uſe of. But of that anon.

This Braſs Quadrant was of three foot Radius, and ſo well fitted with croſs Bars, and ſtrengthned, that it was not ſubject to warp or bend; it had alſo a convenient Pedeſtal, and was made eaſie to be removed from place to place; it was ſuſpended by a Cylinder placed on the back-ſide, in the Center of Gravity of the Quadrant, and could by this means more eaſily be moved to and fro to take any Altitude, then that way of *Ticho*'s, who fixt his Cylinder at the upper corner: But it hath this of inconvenience that *Ticho*'s hath not, namely, That the Plumb-Line or Perpendicular will be longer before it ſettle, and the Inſtrument ſomewhat more apt to warp The Sights of it are the ſame with that of *Ticho*, and indeed the beſt of Common Sights, now commonly every where made uſe of in Inſtruments of that bigneſs, but far inferior to thoſe which are made of Glaſſes, as I ſhall afterwards prove.

The way of Sights which he deſcribes, *pag.*98. for taking the Altitude of the Sun, is very good, but yet far inferior to one fitted with the Object-Glaſs of a Teleſcope, though he had omitted the Tube, for he might thereby have enlarged the hole of the upper Sight to what bigneſs he pleaſed, and conſequently have made the image of the Sun as bright as it ſhould be thought convenient, and that without any manner of Penumbra, if the lower Sight were placed at the due diſtance of the Focus of that Object-Glaſs. And therefore I do wonder at his carefulneſs to inform his Reader aright, for fear he ſhould underſtand a Teleſcope by the Tube he made uſe of, to keep off the adventitious light from the lower Sight, ſaying, *pag.* 99 *Per Tubum autem mi Lector non intelligo Teleſcopium aliquod lentibus inſtructum, ſed plane nudum ex charta conſtructum Tubulum*, as if he had ſome dread of making uſe of Glaſſes in any of his Sights. Whether it were, that he ſuppoſed Glaſſes to have ſome hidden, un-intelligible, and myſterious way of repreſenting the Object, or whether from their fragility, or from their uncertain refraction, or from a ſuppoſed impoſſibility of fixing them to the Sights, or whether from ſome other myſterious cauſe, which I am not able to think of or imagine, I cannot

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tell. Sure *I* am, that none of these *I* have named, are any thing at all considerable Objections against their use, and I have been so fully satisfied of the exceeding great use, nay absolute necessity of them in curious and exact Observations, that I do assure him there is not, nor can be any considerable Objection against them, which cannot easily be answer'd, nor any inconvenience, which cannot with ease be obviated and rectified; of which *I* shall say more hereafter.

The Divisions of it were made wholly by himself, with extraordinary labour and curiosity, insomuch that he says, he could not only distinguish each minute of a Degree, but almost every quarter of a minute, sufficiently accurate for his Common Sights, if he could have only distinguished every half minute, and indeed much more then most mens eyes are able to reach. He seems to have been at infinite trouble and pains, to perform the Divisions made by the help of Diagonals, cutting parallel Circles, a way made use of by *Ticho*, and now so commonly known, that *I* think *I* need not spend time in the Explication thereof; only *I* must take notice, That whereas he supposes these Circles to be equally distant, he ought to have placed their Distances according to the Proportions of the differences of the Secants of some ten minutes,next successively following one another in some Degree of the Quadrant, which is easie to determine, from the Distance of the two extream or bounding Circles; of which more hereafter.

Now though the Circles ought not according to the strict Rules of Geometry, to be equally distant from each other, as *Hevelius* seems to suppose, yet *I* confess, unless the space wherein these Circles lye be very large, and the parts of a Degree that are to be distinguisht, very small, there is no necessity of so curiously distinguishing those unequal Distances, but they may serve well enough for use, if they be taken equal, as *Hevelius* supposes, and indeed much more accurate, then 'tis possible to distinguish the Object by the bare eye; and therefore *I* shall not need to insist upon the further Explication thereof, especially because when *I* come to shew a more accurate way of Sights, *I* shall also shew a much more accurate way of Division, then either of those two of *Ticho Brahe*, or this set down by *Hevelius*, which is much the same with one of those

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which was 100 years ſince made uſe of by *Ticho*, and deſcribed, and is by him attributed to an Engliſh Mathematicians.

But becauſe this induſtrious and careful Perſon put himſelf to the trouble, of making and examining the Diviſions himſelf, I could heartily have wiſht he had thought upon ſome ſuch way as this, which I here deſcribe, and call a Compendium of Diagonal Diviſions, it being a way, whereby as $\frac{89}{90}$ of the trouble is ſaved, in performing the manual operation thereof, ſo I judg it to be much more certain, exact and plain, then the other way of Diagonals. My Reaſon for the firſt is plain, The Diviſion of one Degree ſerving for the whole ninety: And my Reaſons for the ſecond are, Firſt, Becauſe it is much plainer to be diſtinguiſhed, then by the help of the edge of a Ruler, lying over the Diagonals, one being able to ſee but one part of the Diagonal. And Secondly, I think it much better then a ſmall fiducial Thread, which is very apt to be bended and broken, if it lyes cloſe to the Superficies of the Diagonal, and if it lyes at a diſtance, a skew glance of the eye will much alter the ſeeming interſection of the Diagonals, which in this way are both prevented. The way then in ſhort is nothing but this; Take a thin piece of clear Looking-glaſs Plate, well ſmoothed and poliſhed on both ſides, and large enough one way to cover the whole breadth of the Rim of the Quadrant, on which the Diagonals were to be made, and the other way to cover two or three Degrees, (this I do the bigger, that the ſides of the Arm may not ſhadow or darken the Diviſions and numbrings.) Suppoſe a a a a in the 29th. Figure, Plate 2. to repreſent ſuch a Plate, upon this Plate deſcribe with great care a Degree of the Quadrant you would have divided, and compleat it with all its parallel Circles and Diagonals, as you would have done any one Degree upon the Quadrant, and if the Rim of the Quadrant be very broad in proportion to its Radius, you may by the Table of natural Secants or Tangents, ſet the parallels at their due Diſtances, but if the Rim be narrow, 'twill be ſufficiently accurate to make their Diſtances equal. Theſe Diviſions muſt be done with Compaſſes, pointed with ſmall Diamant Points, in the manner of thoſe wherewith Glaſiers cut their Glaſs. The Glaſs being thus divided and lined, number the Diagonals,

Diagonals, and place it in the Frame of the Ruler, with the lined ſide next the Quadrant, ſo that moving it to and fro, the ſide of the Glaſs may immediately touch the Braſs Rim of the Quadrant. This Braſs Rim muſt be divided into 90 equal parts or Degrees, and at each Diviſion ſtraight Lines drawn from the Circumference towards the Center, the whole breadth of the Limb, (at leaſt as much as is made uſe of for the Glaſs-Plate, for the breadth of the Diagonals) the Frame to carry this Plate is a convenient Cavity, left in the moveable Arm of the Quadrant, the whole manner of which will be better underſtood by the Delineation thereof, to which I ſhall therefore refer the Reader. The Diſtances of the parallel Circles if unequal, may be eaſily ſet down true, according to the numbers of natural Tangents or Secants, with a pair of Compaſſes, contrived like Beam-Compaſſes, but having its Points to be ſet at any diſtance, deſired by the help of a Screw, moving upon one ſide of the Beam, which I may have occaſion to deſcribe elſewhere more properly, and therefore will here omit it.

Next, If this way had not pleaſed, I could have wiſhed he had known this following, which is altogether as eaſie, and as Geometrically true, which I have contrived, and have made ſmall Inſtruments thereby to ſhew very minute Diviſions, very eaſily and very plainly. I ſtrike then upon the Limb of the Quadrant I would divide, being firſt made exceeding ſmooth and plain, a Circle very fine, and as lightly as poſſibly I can, ſo it be but diſcernable, and by the help of a very large Quadrantal Dividing Plate of ten foot Radius, I divide the ſaid Quadrant in the faint Circle above-mention'd, into 90 parts or Degrees, then by a peculiar contrivance of a very curious Point that ſtrikes with a Spring, which I deſcribe in another Diſcourſe, the ſaid Degrees are marked upon the Plate by curious, ſmall, round and deep holes, theſe are by another Line without it, which is divided and figured the Common way, diſtinguiſhed and numbred by Figures, according to the Common manner. Then for the ſub-Diviſions, I make a ſmall Hold-faſt by a Screw, which is fixed on to the moveable Arm of the Quadrant, this ſerves to hold the end of a Diagonal Hair, the other end of which is ſtrain'd over the Supplementary Degree, till it lyeth directly over ſome prickt-Hole of the curious

ous Diviſions, on the Limb of the Quadrant, this gives me the ſub-Diviſions of the Quadrant, to what accurateneſs I deſire. The Supplementary Degree is a Degree of a very large Circle, put on upon a ſmall Rule, fixed on to the ſide of the moveable Arm, whoſe Magnitude and Diſtance is found by this Proportion, as the Diſtance between the end of the ſmall Hold-faſt and the pointed Circle, is to the Radius of that Circle, ſo make the Diſtance between the ſaid End and the Supplementary Circle to the Radius of that Circle. This will be more plain by a Scheme.

Let a a a in the 30th. Figure repreſent a Quadrant, b b b a very fine Circle, ſtruck on the Limb of the Quadrant, from the Center l, which by a large Quadrant of 10 foot Radius, I divide into Degrees, and by a ſpringing Point ſtrike ſo many ſmall Points, and number them to 90. beginning at m, and numbring towards i. Let d d repreſent the moveable Arm, c c the hold-faſt, fixed upon the ſide of that Arm, which by a ſmall Screw pincheth and holds faſt a very fine Hair at k, ee the ſmall Ruler fixed at right Angles, with the Line l k f, in this Line (through the Points l and k) I take a Point, as f, and through f I ſtrike a part of a Circle f g, whoſe Center is ſomewhere in the Line f k l produced, which I find by reſolving this Proportion, as k i is to l i, ſo will k f be to the Radius of the Supplementary Circle f g, which will fall ſomewhere in f k l produced, towards l, then take a Degree of that Circle, which will extend from f to g, and divide it into as minute Diviſions as are neceſſary, and number them from f to g. Now to find what Angle the Sight d d maketh with the Sight m m, I ſtrain the Hair h k, till I find it lye over the next Diviſion Point towards the right hand, and obſerve in the Ruler e e, what part of a Degree is there marked, and on the Circle b b b, what Degree is marked, the ſum of both which gives me the true Meaſure of the Angle d d l m. But this only by the By, and I will not now further enlarge on the Explication thereof, deſigning it for another Diſcourſe, where I ſhall deſcribe various, Mechanical and Practical ways, of accurately dividing Lines, into any aſſignable number of equal or proportional parts.

To proceed then where I left off, to the examination of the

the Inſtruments of *Hevelius*, I find that together with the Braſs Quadrant I was ſpeaking of, he deſcribes two Contrivances about it; The firſt is, How to ſet it preſently to an upright, without the trouble of turning the Screws in the Pedeſtal, which is plain enough, and ſo much the better; but it hath this of inconvenience, that it muſt be altered for every Azimith, which is a very great one, and which by another way altogether as eaſie and plain, may be avoided; of which more hereafter.

Another Contrivance about this *In*ſtrument, is a ſmall Screw, for moving it and keeping it ſteady in any poſture in the ſame Azimith, which is convenient enough, but will not perform what he afterwards ſuppoſes it capable of, as *I* ſhall afterwards ſhew.

The ſecond *In*ſtrument, which in the third Chapter, *pag.* 102,103, *&c.* 108. he deſcribes, is a Sextant of Braſs, of three foot Radius, carefully made, and divided with the ſame care and after the ſame way as the former. The Sights alſo are much the ſame, only whereas in the Quadrant he makes uſe of a Plate, with parallel edges for the Sight that is at the center, and furtheſt from the eye; in this he makes uſe of a Cylinder, which way alſo *Ticho* made uſe of 100 years ago, and hath been ever ſince made uſe of. The other Sights next the eye are the ſame with the former: There is nothing ſingular in the Pedeſtal, nor in the Ball and Socket, only 'tis ſomewhat bigger then ordinary. His way of moving and fixing the Rule of it is convenient enough, and the ſame with his *In*ſtrument for moving and fixing his Quadrant, but 'tis not capable of performing what he promiſeth for it.

The third *In*ſtrument, which in the fourth Chapter he deſcribes, is a Sextant of *I*ron, of four foot Radius, to be managed only by one Obſervator, by putting the Center next the eye. The whole *In*ſtrument is little differing from the former, ſave only that the Cylinder at the Center which is here next the eye, is cover'd with another hollow Cylinder, which is voluble and convertible about the former, and carries two ſmall Slits for the Sights, which performeth the ſame as the other Sights, but nothing more, and as the Author himſelf affirms, is not ſo accurate for uſe as the other Sextant, where there

there are two Obſervators, and therefore was ſeldom made uſe of by him. But I ſhall anon ſhew a way by which one Obſervator alone ſhall be able to take any Diſtance to a Semicircle with much more accurateneſs and conveniency then any two Obſervators can; and therefore will be an Inſtrument of the beſt uſe for Aſtronomical and Nautical affairs, for the perfecting both which I deſign it.

The fourth Inſtrument, which in the fiſth Chapter, from *pag.* 114, *&c.* to 123. he deſcribes, is a Quadrant of ſix foot Radius, whoſe Frame was all made of dry Oak, but the Limb, Sights, Sockets, *&c* were made of Braſs, divided ſo as to ſee every quarter of a Minute diſtinctly, the Sights the ſame as in the firſt Quadrant, and the way of ſuſpending it not much differing, ſave only, whereas in the former the Pedeſtal was moveable, in this it is fixt, which is much better. And the Inſtrument is kept in an *Æquilibrium*, by the help of counterpoiſes hung at the end of a ſtring, and caſt over a Pully, as is more viſible by his Deſcription. But this (as all other wooden Inſtruments do) he found to ſhrink and warp, and conſequently to loſe its exactneſs, and therefore he made little or no uſe thereof, but laid it aſide, and made himſelf better of Braſs.

The fifth Inſtrument deſcribed in the ſixth Chapter, from *pag.* 123. to 132. is a Sextant of Wood of ſix foot Radius, made in all particulars like the former Sextant of Braſs of three foot; nor has it any other contrivance about it conſiderable, ſave only a reſt made to ſlip up and down for the Obſervators to reſt their Elbows upon. But this Inſtrument alſo he found to be vitiated by the ſhrinking and warping of the Wood, and therefore he laid that by alſo, and ſeldom made uſe of it.

The ſixth Inſtrument is a large Octant of Wood of eight foot Radius; this is made exactly according to the Form of *Ticho*'s Octant, and ſerves for taking any Diſtance not exceeding 45 degrees. The Sights near the eye are made exactly as the former, but moveable, ſo as to ſlip upon the Limbs of the Octant; the Diviſions of it are performed by Diagonals as before, and gives a greater niceneſs of Diviſion then the Eye is capable of diſtinguiſhing in the Object, and therefore of little uſe.

And thus far the Author proceeded in *Ticho*'s way. But finding theſe Inſtruments which were made for the moſt

C part

part of Wood to be ſubject to faileur, he aſpired to get better Inſtruments made all of Braſs or Iron, and wholly laid aſide the reſt as altogether uſeleſs. And I cannot but very much approve of his Judgment in ſo doing, for certainly *cæteris paribus* Inſtruments, well made of Braſs or Iron, are much to be preferred before the beſt of Wood. But yet neither are all manner of Wooden Inſtruments to be rejected; nor are all ſorts of Metalline Inſtruments free from error, though 'tis confeſſed, if they be made and uſed with skill, they ſuffer not any conſiderable or ſenſible variation. Firſt, I ſay, Wooden Inſtruments may be ſo contriv'd as very near to equalize thoſe of Metal, the Joynts and Plates for Diviſions only being made of Metal, they being very eaſie to be rectified before, and examined after every time of uſing. Such a one was contrived by Sir *Chriſtopher Wren*, being two ſquare Wooden Tubes or Teleſcopes, joyn'd together at the end next the Object by a Joynt of Braſs, and the Angle made by the opening of them, meaſured by a ſtraight Rule equal to half the Radius, divided by Diagonals into 5000 equal parts, which will by the help of a Table of natural Signs or Subtenſes, ſhew the parts in Degrees, Minutes, and Seconds, of which I think I acquainted *Hevelius* ſome years ſince. Next Braſs and Metalline Inſtruments, if they be not very carefully fortified againſt it, are more apt to bend then even thoſe of Wood. And the beſt way I have found to ſecure them true and plain in all poſtures, is to lay them on a Table or Frame of Wood, well fortified underneath againſt bending, and by the help of ſmall Screws in ſeveral parts of the Inſtrument to adjuſt it upon that Frame; the whole Table and Quadrant being ſo counterpois'd, as to be eaſily moveable and fixt in any poſture. But *Hevelius* is pleas'd, as I ſaid before, wholly to lay aſide all manner of Wooden Inſtruments as uſeleſs, and to indeavour the obtaining of Inſtruments of Braſs or Iron. *Nam* (ſayes he pag. 136.) *cùm longâ experientiâ probe tandem didicerim, multo ſecurius eſſe ex ſolido prorſus metallo obtinere Inſtrumenta, tum quo majora & ampliora eo eſſe accuratiora & abſolutiora, adhæc prioribus admodum Tichonicum conſtructis plurima deeſſe quibus ditari merito deberent, & quod iiſdem de cauſis omnino neceſſum ſit, ut parte corrigerentur & meliorentur, tam quà eorum materiam ſtructuram commotionem facilitandam diviſionem quam alia*

alia diversa subsidia & adminicula, quo sic aptius, exquisitius, promptius, minorique labore, &c. ac temporis dispendio possent Astris exponi observationésque peragi. Idcirco omnem curam atque operam pro tenui ingenii mei facultatúmque mearum modulo à Deo concesso (reliqua sublimioribus ingeniis atque ampliori fortunâ Viris, sive posteritati nostræ relinquens) adhibui: quo minora, tam lignea universa ab Astris planè removerem, atque in ejus locum ex puro solidóque metallo, organa mihi compararem: & quidem ejusmodi, quæ insigni amplitudine essent conspicua, simul commoditate egendi, simul aliquanto accuratioribus adhuc divisionibus, ad paulò subtiliores observationes obtinendas gauderent. His Reasoning indeed is very good, that since he had from much and long experience learn'd, that Instruments of Wood after *Ticho*'s manner, were not to be trusted to by reason of their warping and shrinking, and consequently that Instruments of solid Metall were much to be preferred before them, and also that the larger the Instruments were, the more exactly they could be made and divided, and that the more easie they were to be moved, and the more steddy and sure they were to be fixt in any position, the more convenient they were for use, he had therefore rejected all those Instruments which he had made after *Ticho*'s way; and had indeavoured to procure for his own use such as were compleat, both for their matter and form, having caused them to be made of Mettal that which could not be subject to the inconvenience of warping, swelling, or shrinking, with the variety of Weather, or length of Time: And likewise of such a bigness as was capable of receiving more nice and curious Divisions; and in the dividing them had found such contrivances, and used such diligence, that they were more then ordinarily true and exact. As far as he has gone on with these Designs, he seems to have been even profuse in his expences, and exceeding bountiful of his own care, labour, and diligence; but I could have wish'd heartily that it had been some other way imploy'd. Those Instruments which he chiefly laboured to perfect, he professes to be Quadrants, Sectants, and Octants, after *Ticho*'s manner, rejecting all other Instruments of whatsoever Figures, whether *Radii*, *Astrolabs*, *Zodiacal* or *Æquinoctial Rings*, *Parallactical Instruments* or *Hoops*, as more troublesome, and less accurate. But whether he hath in this his

C 2 choice

choice been rightly advised, I shall hereafter have more occa-
sion to examine when I come to describe an Apparatus of Instruments necessary for such a one as designs to promote and perfect the knowledge of the Cœlestial Bodies and their motions; wherein I shall shew that of some Instruments rejected by him, there is a use absolutely necessary.

The Instruments therefore that he begins with are three small Quadrants of Brass; the first of two foot, the second of eighteen inches, and the third of one foot Radius. Each of these Instruments, he sayes, were made somewhat larger then common Quadrants, to wit, of an arch of 110 degrees, which is to no other end, but only in order to shew the subdivisions of each degree of the Quadrant, by the help of a new invented Perpendicular of Brass wherewith each of them was furnisht. This Invention is by him highly extoll'd for most excellent and usefull; and to that end is made use of for the division of all his other Instruments, both great and small. Hear what he sayes of it: *Quiscunque hujus rei* (to wit, the new way of subdividing the degrees of the Quadrant) *primus fuerit repertor, sublimes profecto cogitationes exercuit, hoc ipso ad congruentem effectum deducendo, & inter præstantissima inventa meritissimo refertur, quod etiam minora Instrumenta remotis omnibus transversalibus Lineis, in singula minuta eorúmque particulas minimas subdividi liceat.* He seems indeed both here, and elsewhere in many other places of his Book to be highly possest with admiration of the sublimity, subtilty, and extream usefulness of this invention, and seems very much concern'd that the Author thereof should not certainly be known, but dares not father it upon any one positively. He sayes that one *Benedictus Hedreus* in a Work of his which he published *Anno* 1643. about the new and accurate Structure of the Geometrical Astrolab, describes it; but he gathers that he was not the Inventor himself, but rather that he got both this Invention and the whole Quadrant, which he describes out of the Observatory, or rather Repository of *Ticho Brahes* Instruments, for that it seems *Ticho* was the Inventor of this way of division; and yet, as I noted before, he prefer'd the way by Diagonals much before it, whatever Reason *Hevelius* had to be of a contrary Judgment. What this way is I shall by and by explain. But in the mean time I am sorry

ſorry to find *Hevelius* joyning with *Hedreus* in the Opinion or Demonſtration, as *Hevelius* calls it, that the Sub-diviſions by Diagonals is not capable of a Geometrical demonſtration,eſpecially in leſſer Inſtruments, which have need of many Circles. I confeſs I underſtand not their meaning nor reaſoning,nor why it ſhould be leſs demonſtrable in leſſer then in greater Inſtruments; ſince 'tis very eaſily demonſtrable both in greater and leſſer Inſtruments, and as Geometrical as any other way of Diviſion whatſoever: the Diagonal Line being alwayes a piece of a Tangent Line, that is to ſay, the ſpaces between the Parallel Circles upon the Diagonals are alwayes to be in proportion to the difference of ſome Tangent Lines, and the different diſtance of thoſe Circles from the Center are alway in proportion of ſome Secants: And the way of finding what thoſe Tangents or Secants are, and conſequently what muſt be thoſe Diſtances of the Parallel Circles I mentioned briefly before, and ſhall now more fully demonſtrate. From which I will make it evident, that the Theory was not as *Hedreus* and *Hevelius* have ſuppoſed, uncapable of Calculation or Mechanical Demonſtration.

But firſt give me leave to ſhew you what way *Ticho Brahe* made uſe of to demonſtrate, or rather to find out the true Angle unto each equal Diſtance, which I find ſet down at the latter end of his Mechanicks, as a Supplement to the reſt. *Diviſionis puncta habentis tranſverſalia modus talis eſt, ut* 34 *exprimit figura in qua ſingula denominata per Lineolas in decem interſtitia æqualia diſcriminatum punctis notata ſunt, ſicque regula fiduciæ quodcunque horum inter obſervandum tranſiens ipſum minutum gradus, quòd quærebatur promit aut aliquotam ejus partem, prout ab hoc vel illo puncto removeri diſcernitur. Ut vero hoc etiam demonſtratum hic addam ob ſciolos fortè quoſdam qui ea quæ non ſatìs capiunt carpunt ſic habe.*

In Figura 34. *Sit* A *centrum Inſtrumenti ejuſque Semidiamiter* A O, *aſſumitur autem* O I, *Particula in qua diviſio iſta per lineas tranſverſas fit ea proportione quæ eſt* 1 *ad* 48. *qualis in meis Inſtrumentis ut plurimum uſurpatur. Cúmque* A I *ponatur partium* 10000000000. *integri canonis majoris Rhetici, erit earundem* O I 208333333 *utpote pars quadrageſima octava radii Arcus* I E *ſit* 20'. & I V. 10'. *horum ſinus* 29088779 Y I. *Sinus autem ſecundus*

corundem

eo undem 42308. V Y. *qui additus* N V *quod æquale est* O I *facit* N Y 208375641. *In triangulo igitur* NYI *ad* Y *rectangulo nota sunt duo Latera* N Y *&* Y I. *quare datur basis* I N 21039620 8. *una cum angulo* NIY 82. 3'. 10''. 47'''. *cui additus* YIA 89°. 50'. *conficit* N I A. 171°. 53'. 10''. 47'''. *Basis verò* N I *in triangulo rectangulo* N V I *dividatur in decem partes æquales ut conveniant uni minuto* 21039621 *representatæ per* I B. *Moxque in triangulo obliquangulo* BIA. *dantur duo latera* IB *&* IA. *radius, una cum angulo* BIA, *qui idem est cum* NIA 171°. 53'. 10''. 47'''. *prius reperto: quare innotescit angulus* IAB 1'. 1''. 7'''. *qui tantummodo* 1'. *esse deberet, ità ut major sit saltem* 1''. 7'''. *differentia sanè insensibili: similiter si* F I *assumatur, noven particularum erunt eæ* 189356587 *habebimusque rursus triangulum* F I A *in quo dantur duo latera* FI *modo dictum una cum radio* I A. *&* *angulo* F I A *ab iisdem comprehenso velut antea exurgitque angulus* I A F 9'. 1''. 6'''. *qui debebat esse* 9' *exactè deficiente in ultimo minuto* F N. 1''. 6'''. *Porrò ut circa medium idem tentetur quod nunc apud extremitates fecimus inveniuntur eadem qua antea primo Angulus* I A H 5'. 3''. 6'''. *abundans* 3''. 6'''. *Secundo Angulus* N A H 4'. 56''. 55'''. *deficiens* 3''. 5'''. *Patet itaque quod maxima differentia, sive adjectiva, sive ablativa in hac pragmatia proveniat minimum quid ultra* 3''. *quam subtilitatem visus acumen discernere in quocunque tandem instrumento nullatenus sustinet, quæ etiam per se otiosa est, quare frustra nodum in Scirpo quærunt, si qui hanc nostram satis accuratam distributionis formam cavillari præsumant.* By which 'tis evident that *Ticho* underſtood an inequality, and what it was, and that it was inſenſible, and ſo not to be regarded. Now 'tis to me very wonderful indeed that *Ticho* having thought of a way of calculating this inequality, ſhould not think of an eaſie expedient of reforming it by putting the Parallel Circles at unequal, but their due proportionate diſtances. And 'tis much more ſtrange that *Hevelius* ſhould ſtill affirm it to be a way not Geometrical: For to any one that conſiders this proportion, the inclination of a Diagonal Line being given to find the true diſtances of the Parallel Circles that ſhall divide any aſſignable part thereof in any proportion aſſigned: Nothing can be more eaſie: and for more expedition uſe may be made of the Table of Natural Tangents which is ready calculated to hand. For inſtance: Let B C repreſent a Diagonal Line ſubtending an angl gul of 10'. at the Center A, produce the ſaid Line BC to F, and let fall a Perpendicular, from the Center A to E. Suppoſe then the Angle at B to be one Degree, then is B E the Tangent of 89°. to the Radius A E. and E C is the Tangent of 88. 50'. and the differences between the Tangents of 88 50, 88, 51. 88, 52. 88, 53. 88, 54. 88, 55. 88, 56. 88, 57. 88, 58. 88, 59. and 89. gives the Diſtances of the ſeveral Circles, C. 1 2 3 4 5 6 7 8 9 B. deſired.

Since the Reading of this Lecture, Dr. *Wallis* hath alſo deſcribed another way of finding theſe Diſtances, which he hath communicated in a Letter to *Hevelius*, and I have prevailed with the ſaid Doctor to permit it to be here printed, being very ingenious and accurate, and proceeding by a differing method.

Dr. *Wallis* his Letter to *Hevelius*.

—SED & eſt cur communi omnium Literatorum nomine rebus præſertim cælicis addictorum reddam gratias, tum ob immenſos in tanto apparatu ſumptos erogatos, tam prætioſum conquirendo ſupellectilem Aſtronomicam, graphice hic deſcriptam, tum ob indefeſſos labores, inſomnes noctes dieſq; occupatiſſimos cæleſtis acquirendis obſervationibus impenſos; quarum vim ingentem, Theſaurum ſupra aurum & margaritus prætioſum erudito orbi jam ante dederis, plura daturus indies, verum non eſt ut ſperem me verbis æquare poſſe tua merita, qui ex privato penu ſumptos plane Regios erogaſti; onuſq; ſuſcepiſti non infeliciter, Herculeis Humeris (ne Atlanteis dicam) formidandum.

Operis partem maximam jam evolvi, miratus inibi tanta molis Inſtrumentorum ingenioſiſſimum regimen, & ſubtiliſſimam diviſionum adminiſtrationem, cum pari diligentia conjunctam in Regulis & Dioptris ſolicite curandis, & quidem ſi hoc deeſſet reliquus in caſſum cæderet labor; quippe exiguus & vix evitabilis in Regulis aut Dioptris error, totum Inſtrumentum vitiaret, omneſq; inficeret obſervationes, ſed ſingulis immorari non licet, unum tamen eſt quod attingam breviter, nempe diviſiones per Lineas Diagonales, circulos in limbo concentricos oblique ſecantes. Hanc dividendi methodum jam diu receptam, ipſe retines & quidem merito, circuloſq; hos concentricos æqualibus intervallis disjunctos habes, quod quamvis in exiguorum aut etiam mediocrium Inſtrumentorum limbis

limbis latioribus aliquid erroris possit inducere in tuis tamen tantæ amplitudinis Instrumentis cum limbis exiguæ latitudinis (quod & tu recte mones) nihil quicquam erit discriminis quod in sensus occurrere possit. Hac tamen occasine libet hic subjicere, quod ea de re jam olim (circa A. 1650. *aut* 1651.) *meditatus sum, atq; apud adversaria mea nunc reperio: nempe si quis vellet minoris Instrumenti limbum latiorem Lineis Diagonalibus sic dividere, quibus intervallis oporteat concentricos illos circulos disponere ut angulos invicem æquales designarent illæ cum tranversali intersectiones calculo Trigonometrico determinare.*

Divisio arcus in limbo quadrantis (aliusvé ejusmodi Instrumenti) per circulos concentricos & rectam Diagonalem, sit latitudo limbi (RL=) L, *Radius circuli intimi* (AR=) R, *extimi* (AZ=AL=) L+R=Z *continentes angu'um* (RAZ=) A. *dividendum in partes quotlibet æquales (quarum numerus* n) *rectis* a,b,c, *&c. (quarum longitudo quæritur) facientibus ad* RZ *diagonalem, angulus* $\alpha, \beta, \gamma, \delta$, *&c. adeoque angulus* RA a$=\frac{1}{n}$ A, RA b$=\frac{2}{n}$ A. RA c$=\frac{3}{n}$ A, *&c. sitque* ARZ=O & AZR=V. *Datis ergo crucibus* R, Z *cum angulo contento* A. *(adeoque reliquorum summa* O+V) *inveniuntur reliqui* O *obtusus* V *accutus.) Nam* Z+R. Z−R :: *Ita tangens* $\frac{O+V}{2}$. *tangentem* $\frac{O-V}{2}$ & $\frac{O+V}{2}+\frac{O-V}{2}=$O. *deinde cognitis angulis* O & $\frac{1}{n}$ A *(adeoque reliquo* α) *cum trajecto latere* R *habetur latus* a. *nempe sin* a. R :: *sinus* O. a. & *pari*

$$\textit{modo ex cognitis.}\left\{\begin{array}{l} O\ \frac{2}{n}A. \\ O\ \frac{3}{n}A. \\ O\ \frac{4}{n}A. \\ \&c. \end{array}\right\}\textit{habentur}\left\{\begin{array}{l} b \\ c \\ d \\ \&c. \end{array}\right.$$

Praxis sit R=1. L=0, 2. Z=1, 2. A=10′. *ergo* O+V= 179° 50′. $\frac{O+V}{2}$=89°. 55′. *tum* Z+R=2, 2. Z−R=0, 2. :: $\frac{O+V}{2}$=687, 5488683. 62, 5044427= *tang.* $\frac{O-V}{2}$ *cui respondet angulus* 89°. 5′. 0″. 17‴. *proxime. Ergo* $\frac{O+V}{2}+\frac{O-V}{2}$=O =179°. 0′. 0″. 17‴. *fere cujus sinus* 0, 0174511. *nempe idem qui sinus* 0°. 59′. 59″. 43‴.

Deinde secandus sit A in 10 *partes quarum quælibet sit* 1′. *quæruntur igitur* a,b,c,d,e,f,g,h,i, *nempe.*

Sin.

			1.00000=R	
*Sin.*α(0° 58′ 59″ 43‴)	0.0171603.	R=1 :: *Sin* O=0.0174511.	1.01694=a.	1694
*Sin.*β(0 51.59.43.)	0.0168694.	R=1 :: *Sin* O=0.0174511.	1.03448=b.	1754
*Sin.*γ(0 56.59.43.)	0.0165780.)	0.0174511(	1.05264=c.	1816
*Sin.*δ(0 55.59.43.)	0.0162877.)	0.0174511(	1.07144=d.	1880
*Sin.*ε(0 54.59.43.)	0.0159969.)	0.0174511(	1.09091=e.	1947
*Sin.*ζ(0 53.59.43.)	0.0157060.)	0.0174511(	1.11110=f.	2019
*Sin.*η(0 52.59.43.)	0.0154152.)	0.0174511(	1.13206=g.	2196
*Sin.*θ(0 51.59.43.)	0.0151243.)	0.0174511(	1.15383=h.	2264
*Sin.*ι(0 50.59.43.)	0.0148335.)	0.0174511(	1.17647=i.	2353
			1.20000=Z.	

Praxis altera sit R=1. L=0,1. Z=1,1. A=10″. *ergo* $\frac{O+V}{}$= 179, 50. $\frac{O+V}{2}$=89°. 55′. *cujus tangens* 687, 5488693, & 2, 1. 0, 1 :: 687, 5488693. 32, 7404223½= *tang.* 18°. 15′. 1″. 57‴¼ $\frac{O-V}{2}$ *ergo* $\frac{O+V}{2}+\frac{O-V}{2}$=O=178°. 10′. 1″. 57‴¼. *cujus complementum ad semicirculum* 1°.49′.58″. 2‴¾. *cujus sinus* 0,0319827. *ergo*

			(1.00000=R		
Sin α=1°.48′.58″.2‴¾)	=316920)	319827	(1.00918=a	918	16
*Sin.*β 1.47.50.2¾)	=314013)	319827	(1.01852=b	934	17
Sin γ(1.46.58.2¾)	=311103)	319827	(1.02803=c	951	19
*Sin.*δ(1.45.58. ¾)	=308198)	319827	(1.03773=d	970	19
*Sin.*ε(1.44.58.2¼)	=305290)	319827	(1.04762=e	989	18
	302343)		(1.05769=f	1007	20
	299475)		(1.06796=g	1017	21
	296567)		(1.07843=h	1068	21
	293660)		(1.08911=i	1089	
	290752)		(1.10000=k	=Z	

Hactenus adversaria, ubi duos casus expendimus, nempe cum latitudo limiti ponetur pars quinta & pars decima Radii brevioris, & angulus dividendus 10 *minuta prima tanta fere ἀκρίβεια, quantum feret vulgaris canon Trigonometricus: & quidem ultima unitas in ambiguo est nunc justo major nunc justo minor. Radium autem (ut ego soleo) facio* L *(non ut plerumq; sit* 10000000.) *quo omnes multiplicationes & Divisiones per Radium faciendæ præcidantur: Adeoq. sinus habeo pro partibus decimalibus, quibus itaq; cum opus est, ciphras præmitto quo de unius integri loco constet.*

D *Simili*

Simili proceſſu utendum erit mutatis mutandis ſi latitudo limbi ſumatur in alia quavis proportione ad Radii longitudinem. Sed commodius erit (ad vitandam moleſtiam toties quærendi partem proportionalem) ut ſumatur angulus O *commodæ magnitudinis (juſtis minutis primis determinandæ abſq; annexis ſecundis tertiiſve) atq; ita quæratur Radii maximi* Z *longitudo, eodem modo quæ Reliquorum* a, b, c, *&c. puta ſi in praxi poſteriori ſumpto ut prius* R=1 *& angulo* A=10′ *ſumatur angulus* O *non qui illic prodit* 178, 10′, 1″, 57‴$\frac{1}{4}$ *ſed potius* 178. 10′. *cujus complementum ad duos Rectos eſt* 1.50′. *hujus ſinus in ipſo canone habetur* 0,0319922 *& reliquorum item* α, β, γ, δ, *&c. ſinus ſimiliter ibidem habebuntur, ut una tantum diviſione opus ſit pro ſingulis exhibendis ipſaque Radii* Z *Longitudo habetur non quidem preciſe ut prius* , 1, 1 ; *ſed proxima (quæ itaque ſumenda erit)* 109996 *nempe.*

			(1,00000=R	
Sin. α (=1.49)=317015)	319922		(1.00917=a	917.17
β =1.48)=314108)	319922		1.01851=b	934.18
&c.	311200)		1.02803=c	952.17
	308293)		1.03772=d	969.19
	305385)		1.04760=e	988.19
	302478)		1.05767=f	1007.20
	299570)		1.06794=g	1027.20
	296662)		1.07841=h	1047.20
	293755)		1.08908=i	1067.21
Sin. κ(=1.40)−290847)	319922		1.09996=k	1088.
				=Z

ſimiliter omnino res ſuccedit ſi ſumptis Radiis R L *cum angulo* A *quæramus* V *& Radios intermedios, aut ſumpto Radio* L *cum angulis* A V *quærantur* R *& Radii intermedii.*

Verum ſi limbi latitudo ſit Radii non niſi pars trigeſſima quadrageſſima, quinquageſſima aut adhuc minor, atq; angulus dividendus non quidem 10 *minuta prima ſed totidem ſecunda, aut minor adhuc, ſubtilior res eſt quam ut canon vulgaris Trigonometricus hic adhibeatur ; & quæ omnem ſenſum fugit, ipſiq; circuli concentrici diſtantiis æqualibus quantum ſenſu poſſumus diſtinguere invicem disjuncti : quippe unius pollicis pars milleſima nedum decies aut centies milleſima minor eſt diſcrepantia quam ut ſenſu percipi poſſit. Sed nimius ſum in re levi felicem itaq; exeuntem annum tibi comprecatus longa ſequentium ſerie contrivandum, valere jubeo.*

But

But to proceed. In the next place I think it will be ſufficiently plain, to any one that ſhall try both the ways, that the Diviſions are by Diagonals much eaſier diſtinguiſhed by the eye, then by this way ſo applauded by *Hevelius*, and therefore I cannot chooſe but conclude with *Hevelius*, (pag. 140.) though to a quite differing end and ſenſe: *Sunt igitur ſplendidiſſimæ tantum ſpeculationes mentisq; ideæ quæcunq; de* Nonianis *vel* Hedrianis *Diviſionibus proferuntur.* But becauſe perhaps there may be ſeveral perſons that have not yet peruſed this Book of *Hevelius*, nor that of *Benedictus Hedreus*, printed in 1643. nor *Ticho*'s Mechanicks, of a much longer ſtanding, and thence may perhaps not ſo well underſtand what this way of ſub-dividing is; give me leave a little to explicate it, and ſhew you plainly what it is.

The way then as it is deſcribed by *Ticho Brahe*, and aſcribed by him to *Petrus Nonius*, that excellent Spaniſh Mathematician, who publiſht it in his learned Book, *de Crepuſculis*, ſuppoſing it alſo to have been heretofore uſed by *Ptolomy*, but (as *Ticho* is of opinion) without much reaſon, is this; *Ut ducantur intra extremum quadrantem alii minores numero* 44. *ſucceſſive ſeſe comitantes, quorum extimus in* 89. *ſequens in* 88. *tertius in* 87. *& ſic deinceps donec ad ultimum & intimum perventum fuerit qui* 46. *portiones habebit.* To which Deſcription publiſhed in his *Mechanica*, he adds in the ſecond Book, *de Mundi Ætherei recenſioribus Phenomenis*, pag. 461. *Altera Diviſio ad Clariſſimi Mathematici* Petri Nonii —— *imitationem per plures quadrantis arcus introrſum deſcriptos & diverſimode ſubdiviſos procedit. Etſi autem in hac ipſa imprimis ingenioſa* Nonii *inventione, aliquid auctuarii loco expeditius à nobis additum eſt, ita ut exterior arcus in plurimas portiunculas dividatur; neque is ordo aut numerus arcuum ſeſe introrſum concomitantium, quem ille præfinivit ſed multo expeditior & perfectior obſervetur, tamen quia hæc ſubtilitas cum ad praxin deventum eſt plus habeat laboris quam fructus, neq; id in receſſu præſtet quod prima fronte pollicetur, ut alibi plenius oſtendemus, idcirco apud nos dudum in uſu eſſe deſiit.* [See more of this, pag. 62. *Epiſtolarum Aſtronomicarum.*]

From which way of Diviſion, this of *Hevelius* (which he aſcribes to *Hedreus*, but is more properly aſcribable to *Pierre Vernier*, as I ſhall afterwards ſhew) is ſomewhat different,

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and possibly might be the same that *Ticho Brahe* contrived to compendifie that of *Nonius*.

The way then is this, described by *Hevelius*, pag. 141: *Quadrantes contractiores ita à me sunt adornati, ut limbos eorum tantum in integros & semigradus distinxerim; quæ ut hæc distinctio non nemini admodum rudis videatur, sufficit tamen affatim commonstrandis singulis minutis primis; dummodo perpendiculi ex centro appensi extremitas limbum stringens in certas particulas sit subdivisa, imo quod magis de quo non nemo sane mirabitur, non solum hæc rudior limbi subdivisio sufficiens exhibendis singulis minutis primis sed etiam pro denis quinis quinetiam singulis secundis in majoribus organis videlicet nostrum Instrumentum directorium adhibeas. Oportet ut inferior illius pars curiosissime & levissime sit limata & levigata, ut limbum totum æquabilissime quidem tangat, sed nullibi nimis adhæreat; tum quovis loco liberrime pendeat atq; divisionis tam quadrantis quam perpendiculi observator rite discernere valeat. Dividitur autem istud perpendiculum hac ratione, si videlicet spatium 31 semigraduum in limbo perpendiculi accuratissime denotes; idq; primum in tres æquales partes, rursum quamlibet trientem in decem dividas; atq; ita obtinebis spatiola paulo admodum ampliora quam spatiola unius semigradûs, quia intercapedo 31 partium in 30 transmutata necessario fiunt modice ampliores. Attamen si divisiones perpendiculi ad limbum quadrantis accedant circa extremitates perpendiculi, discrepantiola illa divisionum ab invicem vix ac ne vix cognoscitur; circa medietatem vero perpendiculi satis evidenter. In medio limbo perpendiculi & divisionum parvulus index & quidem inter 15 & 16 spaciolum constituitur pro discernendis integris & semi gradibus; quos accurate dictus index indicat, quando totum spatium perpendiculi in 30 partibus divisum in ipso limbo quadrantis spatium 31 partium exquisite subtendit. Ea tamen expressa lege si totum Instrumentum absolute ab omni parte sit constructum; quando vero iste index pauxillum promotior existit integro aliquo vel semigradu certissimum est indicium, observationi minuta quidem adhærere aut integro aut semigradui adnumeranda, si index huic vel illi vicinior est. Cognoscitur autem minutorum numerus ex eo, quando lineola aliqua divisionum in perpendiculo cum una aliqua in limbo quadrantis prorsus in unam eandemq coincidit rectam. Nunquam enim nisi unica lineola in perpendiculo cum altera in quadrante, si exquisite peracta sunt*

ſunt omnia omnino concurrit. In iſto igitur utriuſq; lineolæ concurſu ubi una eademq; videlicet conſtituitur linea eſt terminus ipſorum minutorum vel integro gradui vel ſemi gradui adhærentium.

This ſame way is alſo made uſe of by *Hevelius*, for the Diviſion of all his larger Inſtruments, as well as for the Diviſion of this ſmaller, by fixing it upon the Perpendicular, as he afterwards mentions, *cap.* 15. *pag.* 307. where he alſo gives a fuller deſcription of it, to which I refer the Reader.

The way indeed is exceeding ingenious, and very much improved by *Hevelius*, but yet at the very beſt it is very difficult, both to make the Diviſions, and much more difficult to diſtinguiſh them, as may be plainly enough ſeen even by that very Specimen publiſhed by *Hevelius*, in the firſt and ſecond Figure of the Plate T. eſpecially if it be viewed with a magnifying Glaſs or Lens; and I do wonder that *Hevelius* did not all this while think of making uſe of a Lens, to make the Diviſions and Diſtinctions appear more plain, without which Seconds are not to be diſtinguiſhed, by thoſe kinds of Diviſions even in an Inſtrument of 10 foot Radius, and by the help of it they may be made and diſtinguiſhed, in Inſtruments of a quarter that bulk, as he may find, if he pleaſe to make uſe of the ſhalloweſt Object-Glaſs of that Microſcope which he had from *London*; he may, I ſay, by looking upon the Diviſions of the firſt and ſecond Figures of the Table T. with his Microſcope, plainly detect how far thoſe Diviſions are ſhort of accurateneſs, and how many faults and inequalities the naked eye and unmachined hand do commit.

It is therefore one of my ways for dividing and diſtinguiſhing Diviſions, to make uſe of one, two, or three Lenſes, whereby not only the eye is very much eaſed, but the judgment is very much augmented, and the hand directed, as I ſhall afterwards explain, when I come to ſhew ſome particular ways of making Diviſions.

But becauſe this *Benedictus Hedreus*, from whom *Hevelius* affirms he received this invention of dividing the Limb of the Quadrant, was not ſo ingenuous as to confeſs that he received this invention from another, and becauſe perhaps the Book being ſmall, may have been long ſince loſt and forgotten, having accidentally

accidentally met with one, I ſhall acquaint *Hevelius*, that one *Pierre Vernier* (as he calls himſelf) *Capitain & Chaſtelain pour ſa Majeſté au Chaſteau Dornans, Conſeiller, & General de ſes Monnoies au Conté de Bourgongne*, printed at *Bruſſels*, by *Francis Vivien*, 1631. (to wit 12 years before *Hedreus*) a Treatiſe in French, which he calls, *La conſtruction l'Uſage & les Proprietes du quadrant nouveau Mathematique, comme auſſi la construction de la table des ſinus de minute en minutes ſucceſsivement par un ſeul maxime. De plus un abregé deſdicts tables en une petite demi page avec ſon uſage : & finallement la methode de trouver les angles d'un triangle par la cognoiſſance des coſtez & les coſtes par les angles ſans l'ayde d aucune table.* In which he hath at large and very plainly deſcribed this way of dividing the Quadrant, to what accurateneſs is deſired, and pretends it to be, as poſſibly it was, an invention of his own.

But to return where I left to *Hevelius* his Diviſion on the Quadrant by the help of the Braſs-arm, I ſay, againſt this way, beſides what I have already mention'd, I have a ſecond Objection, and that is, that it requires a moſt exceeding great curioſity and care to make that Metal Pendulum or Plumb of Braſs, ſo as to be exactly of equal weight and make on both ſides of the ſuppoſed middle Line, for if it be not ſo, it may eaſily vary not only ſome Seconds, but even ſome Minutes from its exact Perpendicularity, and if ſo, 'tis to little purpoſe all the former curioſity about Subdiviſions.

Thirdly, The Perpendicular ought alwayes to be kept very clean from Duſt, for if a little more Duſt ſettle on the one ſide then on the other, the Perpendicularity will be vitiated, and all the curioſity elſe about the Obſervation will be loſt.

Fourthly, If the Pin on which this Braſs Perpendicular hangs be not of ſome bigneſs, it may eaſily warp, or bend; and if it be of a conſiderable bigneſs, it will not move eaſily, and conſequently the Plumb will not hang tender, but ſtiff; in both which caſes it can be of no uſe in the World for Aſtronomical Obſervations. Further, if it hang looſe upon the Center, which it muſt do to hang tender, then there will lye as material an Objection againſt it, for its not moving true upon the Center of the Inſtrument; and therefore upon the whole matter I conclude it to be an Invention indeed of great ſublimity and ſubtleneſs

ſubtleneſs,but of little or no uſe for Aſtronomy,to which *Hevelius* applies it. He had much better therefore have been content to have followed *Ticho Brahe*,and made uſe of a common Plumb Line and Diagonal Diviſions, where there is occaſion for them, for that is true and practicably capable of exhibiting the Subdiviſions of a Degree,as Minute,as are neceſſary to commonSights.

In the next place, before he leaves the Deſcriptions of theſe three ſmaller Quadrants, he mentions an Invention of his whereby he fixes the Quadrant in any altitude, and eaſily moves it ſteadily into any poſture deſired by the help of Screws This Invention of his own contrivance he doth indeed very highly applaud, inſomuch that he believes no good Aſtronomical Obſervations can be made without it. But he muſt pardon me if I am not altogether of his mind; I grant indeed the thing is exceedingly convenient, in compariſon with any yet uſed, if it be well made, and that the way of applying it to the Quadrant be very facil and eaſie. But 'tis not alway ſo neceſſary, but that Obſervations may be as conveniently made without it, as I ſhall afterward ſhew, in the Deſcription of the moveable Axis, for continuing the Inſtrument in the Plain of the Object, whether a Diſtance or an Altitude be to be taken.

In the next place he proceeds to deſcribe his large Quadrant of Braſs adjuſted ſo as to take Altitudes and Azimuths, of which he makes a full and particular deſcription; but the moſt conſiderable thing that is new in it is, that inſtead of a Screw uſed by *Ticho* for lifting and moving the Arm with Sights, he makes uſe of two Lines poys'd with Plumbets, by the pulling of this or that of which he is able to raiſe or ſink the Ruler with Sights, all the reſt of the contrivance being to make it ſtand perpendicularly in any Azimuth, which I think may be done to greater certainty with leſs trouble, by a way I ſhall afterwards ſhew: As an Eſſential part of this Inſtrument, he takes occaſion to give the deſcription of the Turret or Obſervatory which he built for it, and the ſeveral contrivances about it, which I now omit.

The uſe he made of this Inſtrument was for the taking the Meridian Altitudes of the Sun, of which he affirms to have taken a very great number, eſpecially ſuch as were of principal

pal uſe for the regulating the motion of the Sun: Such as the Solſtitial and Æquinoctial Altitudes, of which I hope we may expect an account in the ſecond and third Part of his *Machina Cæleſtis*. I know not to what exactneſs he hath proceeded in taking his Meridian Altitudes of the Sun; but had he proceeded in the way by Teleſcopes, he might have taken all his Altitudes of that kind to a ſingle Second, with great eaſe and certainty.

And upon this occaſion I hope it will not be unacceptable to my Aſtronomical Reader to hint a very expeditious and exceeding accurate way of making a Catalogue of all the viſible, as well as the moſt conſiderable Teleſcopical Stars of the Heaven. For the doing of which there will not need a tenth part ſo much time as for the other wayes that have already been made uſe of, and yet will very much exceed them all in accurateneſs and certainty. The way then in ſhort is nothing but this: Let there be made a very large mural Quadrant, or rather Semicircle, of 30 foot Radius, fixed exactly in the Meridian againſt a Wall made of ſquared Stones, well joynted and cramped together, and ſetled on a foundation very firm and ſolid, to prevent all manner of ſhaking and ſwarving. Let the rim of this be made of Braſs Plates, ſtayed in their due poſture by cramps or bars of Iron fixed in the Wall, by running them with Lead: then having divided this Semicircle into 180 Degrees, and ſubdivided each Degree by the help of Diagonals, on a flat and well poliſht Plate of Glaſs, according to the way I before deſcribed into Minutes and Seconds: adapt to it a 30 foot Teleſcope, ſo that the Tube ſhall not warp, nor the Glaſſes deviate out of their true poſture; the Focus of the Object Glaſs make to be exactly upon the edge of the Braſs Limb, ſo that by the help of the Eye-glaſs, which is a deep Convex, the punctual place or altitude of a Star to a quarter of a hairs breadth, even to Seconds of a Minute, may be diſcover'd: the trouble of dividing this Quadrant will be no more then of one of an ordinary ſize, the ſubdiviſion of one Degree ſubdividing and examining all the reſt. The way of making the Tube of the Teleſcope ſo as not conſiderably to bend, may be done ſomewhat after that way of ſtiffning the Tubes of very long Teleſcopes, which I communicated to *Hevelius*, and you will find at at large deſcribed in this Treatiſe of *Hevelius* : Save only, that inſtead of Ropes which I firſt made uſe of, I rather commend ſo many Braces of Wood. Now though notwithſtanding all the Diligence that can be this way uſed, the Tube do ſomewhat bend in the middle, yet it can be of no manner of ſignificancy as to the vitiating the Obſervation; ſince firſt, the Object Glaſs always ſtandeth in the ſame poſture as to the Center, and ſecondly, the Focas thereof is exactly in the edge of the Limb.

Further, to prevent the inconvenience of looking up or in any other uneaſie poſture by the help of a reflex Metal one may always look Horizontally, that is, perpendicularly to the plain of the Wall or Mural Quadrant. And to prevent the trouble and labour of moving or lifting the Tube by the help of a long yard poyſed upon Centers on a Frame before the ſaid Inſtrument, both the Tube & Arm for the Sight, and the Seat on which the Obſervator ſits, may be counterpoiſed, ſo that by turning a Windle, he may eaſily raiſe himſelf with the Tube to any poſture deſired. The Object Glaſs is juſt before the Center, and the Eye Glaſs looketh directly on the Diviſions of the Limb, and there is nothing to ſtrain or ſtir the Inſtrument it ſelf, nor can the warping of the Tube, if there ſhould be any, have any effect on the Obſervation: Of this I may ſay more on another occaſion. By this means (in one Nights Obſervation) the Declinations of ſome hundreds of Stars may be taken to a Second by one ſingle Obſervator, having only one or two Aſſiſtants to write down the Obſervations as faſt as made. And at the ſame time the right Aſcenſion of every one of them may be taken by the help of a very accurate Compound-circular Pendulum Clock, which I ſhall elſewhere deſcribe, denoting even to of a Second of time the appulſe of the Star to the Meridian: There needs indeed great exactneſs in every part of this Apparatus, and 'twill not be done without a conſiderable charge, and much labour and diligence in the performance thereof; but if we compare it with the methods and wayes that have been hitherto uſed, we ſhall certainly find that the Obſervations will be near 30 times more accurate, the charge not a quarter, and the labour not near a tenth part ſo much as in other wayes made uſe of by *Ticho* and *Hevelius*. And though it may be objected againſt this way (which indeed may be much more ſo againſt

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any other) that the refraction of the Air will considerably vary the Declination of such Stars as are very far South, yet since the same Instrument affords a way beyond any in the World for the discovering the several Refractions of the Air at several Altitudes above the Horizon, to the accurateness of a Second, by taking the Altitude of such Stars as never set in the North, in the greatest and least Altitude above the Horizon ; a Table of such Refractions will easily rectifie the Declination of the other Stars to as great accurateness. This Subject doth deserve a much larger and more particular Description of every Branch thereof, and the Incouragement of some Prince, whose Name and Honour will thereby be Registred among those glorious Celestial Bodies to all Posterity, and the succeeding Learned World will be obliged to celebrate his memory. But I fear this Age will hardly yeild another *Alphonsus*, another *Ticho*, or another *Hevelius*, who have not spared to expend their utmost Indeavours in performing this task, though by other methods. But leaving this for another time, I shall proceed.

In the third place then he goes on to describe his great Horizontal voluble Brass Quadrant, of which he says, he does not believe that ever the like was made by any, if the splendid *Apparatus* and the whole Fabrick thereof be consider'd. It is in Diameter six foot and an half, and serves, as he affirms, to take Altitudes to Seconds; but yet he is necessitated to allow, that it is short both of *Ticho*'s large wooden Quadrant, and of his large mural Quadrant ; nor do I see any reason why *Ticho*'s mural Quadrant should not take Meridian Altitudes somewhat more accurately, since I believe his Sights every whit as good, and his Divisions altogether as exact ; what he might fail in diligence, I cannot say. I do believe this Instrument to be an exceeding good one of the kind, and that he hath from much practice and experience found out many contrivances, in order to the making it convenient to make Observations, and he hath not spared for cost, pains, study and industry, for the compleating thereof ; but still whether he be arrived to the greatest perfection, or to so great as to take Altitudes to Seconds, seems to me very dubious, and if he made use of the Sights before-described, wholly impossible. For first, a Degree upon the Limb is but about $\frac{4}{5}$ of an inch, and consequently a Minute is but

but the 50th. part of an inch, and a Second but the 3000th. part of an inch, which he that can diſtinguiſh with his naked eye, hath better then I, or I fear, any man now living. Short-ſighted men, I grant, can do much toward the diſtinguiſhing very minute Diviſions, by being able to bring the Object very near the eye, but the moſt ſhort-ſighted muſt be yet very much ſhortned by Glaſſes, before he will be able to diſtinguiſh the 3000th. part of an inch, and when he hath diſtinguiſhed it, which he may poſſibly do with a Microſcope, how will he diſtinguiſh of the Penumbræ, which is not certain even to a Minute? And though it may be ſaid, it is the ſame, round the Circle, and the Circle is the true bigneſs of the Sun, ſo that if a Circle of a bigneſs, anſwering to the Diameter of the Sun, and the Diſtance of the lower Sight from the upper be deſcribed on the lower Sight, it muſt bound the Limb of the Sun, and that conſequently it will be eaſie to diſtinguiſh when that Circle is perfectly fill'd with the figure of the Sun, admitted through the hole in the upper Sight. I anſwer, That this ſeems very probable and eaſie, and is indeed believ'd and aſſerted ſo by Optical Writers: But yet 'tis quite otherwiſe; for not to mention that there is confeſſed by all, that the Penumbra of this Circle muſt be as big at leaſt as the Diameter of the hole above, through which it is trajected, which cannot be leſs then a Minute; I ſay, that experience doth demonſtrate that it is quite otherways, and that the Limb of this Image painted on the lower Sight is terminated with a Penumbra, which is ſometimes five or ſix times bigger then the Diameter of the hole, and which is yet ſtranger, the ſmaller the hole be, the bigger is the Penumbra, and the bigger (to a certain Degree) the leſs, but there is no bigneſs which will take it off quite, and the Diameter of the Sun that way taken, is ſometimes bigger and ſometimes leſs then it ought, and that to a very conſiderable quantity: Of which, and ſeveral other very ſtrange proprieties of Light, I ſhall hereafter ſay more on another Subject.

But to proceed. That he hath made this Inſtrument his chiefeſt, you may perceive by his pathetical deſcribing thereof; for he ſays of it, *pag.* 184. *Ad commodiorem hujus quadrantis uſum, tot ac tot adminicula recens excogitata atq; huic organo applicata fuere, ut neſciam à quibus primum inchoare debeam. Imo*

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etiamsi vel maxime velim, nullo tamen modo omnia & singula adeo perspicue vel delineare vel describere potero, ut universi præprimis qui similia haud ipsimet oculis usurparunt quævis recte ac plane intelligant, quinetiam credas velim utut aliis sunt attentiores atq; hujus rei bene gnaros, aliquoties sane hocce Instrumentum visuros antequam dimidiam tantam partem debite animadvertant ac plenissime comprehendant. Quippe & verum fateor nec ipse ego, licet singula ex meo solo cerebro prodierint ac confecta fuerint, possem adeo distincte tibi eum sub aspectum ponere nisi mihi hocce organum sub oculis assidue versaretur. Nihilotamen minus dabo operam, ut quantum fieri poterit, dilucide omnia proponam, reliqua veri exercitatis cæli metaloribus ulterius rimanda & perquirenda committ am, &c.

And so he proceeds with the Description of this Quadrant, and the *Apparatus* about it, and first, he tells us of the weight of this Instrument, that it was 80 *l.* Next, of the shape of the Turret in which it was fixt, which is indeed very convenient and ingenious, it being so contrived, as to be voluble or convertible upon Truckles, having one only side open, and inclos'd on all sides else, so that neither the Observator nor the Quadrant was much expos'd to the injury of the weather, which is indeed of no small use in Astronomical Observations. But this may be done many other ways also. He tells us further of the admirable and prodigious use of Screws, in order to the setting and fixing the Quadrant. Next, As to the giving a motion to it, in order to follow the Sun and fixed Stars in their diurnal motion. Thirdly, As to perform all the Subdivisions of a Degree, not only into Minutes but into single Seconds. To all which I say first, As to the use of the small Hand-screws, I do grant, that in some cases they may have their conveniency, as to the moving and staying the Instrument. But then since he is fain to make use of two Screws, whereby both the hands must be imploy'd to manage these Screws, I judge them too troublesom for that use, and that there is a much better way, whereby the Quadrant being once set into the Azimuth of the Stars, it shall continue to be so, and to move along with it, without any trouble to the Observator, so long as the Observator hath occasion to have it remain so, which (that I may hint that only now by the By) is a small

ſmall Automaton, which ſhall continue it for many hours exactly in the Azimuth of the Star deſired, of which more hereafter.

Next, Whereas he affirms this way capable to ſhew Seconds as well as Minutes, I grant it may be capable; but then I muſt further affirm, that he hath not at all ſhewed how that can be done, nor is it indeed feaſible in his way, for he ſhews us not any way how to ſet it, that is, fix it certainly to any Degree: Now if he be not ſure in the fixing it exactly to a Second, upon that Degree where he would begin his Diviſion, 'tis a vain thing to be ſo accurate in the other Dimenſion, for he cannot be more certain, (let him be never ſo curious in the Subdiviſion with his Screw) then he is certain in the firſt fixing of his Screw to the Degree, for whatever he varies from the Degree in the ſetting, he varies at leaſt as much in the Subdiviſions, and conſequently unleſs that be ſome ways taken care of, which I do not find, 'tis a nicety without uſe.

To conclude therefore, I ſay, the Frame of this Inſtrument is extraordinary good, and by the help of ſome additions, as to the Sights, Diviſions, Perpendicular and Erection, might be made as good as need be deſired for any uſe in Aſtronomy, and 40 times better then what it is now made and deſcribed by *Hevelius*, or then any I have yet heard of to be made in the World. But as it is, it is not more exact then the large Inſtruments of the Noble *Ticho Brahe*, which he uſed 100 years ſince, and much ſhort of his mural Quadrant, for taking Meridional Heights.

He proceeds to the Deſcription of his new and large Braſs Sextant of ſix foot Radius: The Sights and the Diviſions thereof are in nothing differing from thoſe of the Quadrant, nor do I find any thing very conſiderable in the Deſcription thereof; it was made uſe of by two perſons in the ſame manner as the former Sextant, and like that of *Ticho*; but what grand inconveniences do attend that way of Obſervation, I ſhall afterwards ſhew, when I come to explain how one perſon alone may be able to do it with leſs trouble by half, and ten times more exactneſs.

But by the way, I cannot but take notice of what *Hevelius* ingeniouſly confeſſes, of the great difficulty there is in taking the

the Diſtance of fixt Stars from the Moon, which is from nothing elſe but the imperfections of his Common Sights, and all that difficulty vaniſhes, if the Sights be made another way. Next, He ſeems to make it a much more difficult buſineſs, to take the Diſtance of the Sun from *Venus*, when ſhe is ſeen in the day-time; but by a way I ſhall hereafter ſhew, it will not only be eaſie to take the Diſtance of the Sun in the day-time from *Venus*, but from *Mars*, from *Jupiter*, nay, from ſeveral of the fixt Stars.

I ſhall paſs by therefore his *Apparatus*, which ſeems very great and chargeable, ſince I ſhall elſe-where ſhew a ſingle, plain way, without any trouble or perplexity, how the matter may be quite otherwiſe ordered, much to the advantage of the Obſervator.

As to what he aſſerts of his extraordinary care, diligence and pains, in dividing and examining the truth of his Inſtrument, I do no ways doubt it, but that he hath proceeded as far as it was poſſible for one to do in that way he made uſe of, but might have ſaved much of it, if he had thought of the way by Diagonals on Glaſs, which I have already deſcribed. Yet I ſhould have been very glad to have ſeen the Diſtances, which he mentions to have taken of eight fixt Stars near the Ecliptick, to wit, *Lucidæ Arietis & Palilicii, Palilicii & Pollucis, Pollucis & Reguli, Reguli & Spicæ, Spicæ & in manu Serpentarii, in manu Serpentarii & Aquilæ, Aquilæ & Marchab, Marchab & Lucidæ Arietis*, and that to ſo great exactneſs, as not to miſs one ſingle Second in the whole Circle of the Heavens, taken at eight Obſervations. For to me indeed it ſeems one of the greateſt affirmations I ever met withal, and not leſs then humanely impoſſible, were there no Refraction in the Air, and did all the Objects ſtand ſtill in the Horizon, but the Refraction of the Air, were it much leſs then it is granted by all, would neceſſarily cauſe a variety of a great number of Seconds. And I durſt undertake to demonſtrate it to any, as plainly as any Geometrical Propoſition, that it was wholly impoſſible for him, with all or any of the Inſtruments he hath deſcribed, to make any one of theſe Obſervations, to the certainty of 30 Seconds, whence if that uncertainty be 8 times multiplied, it will follow, he cannot be certain in the whole Circle to 240 Seconds,

Seconds, or 4 Minutes, which how much it is differing from one single Second, any one may judg.

I had many other things to have added, which have occurr'd to me in the perusing of *Hevelius* his Book. but I will say no more at present by way of Objection, having, I fear, wearied the Reader, with shewing him my doubts and scruples, especially about the imperfection of that way of Sights and Divisions made use of by him: Only, to make my Reader some mends for his patience, I shall describe a short *Apparatus*, which I have contrived for this purpose, and in the doing thereof, shall be as plain and brief as possible the matter will bear.

Since the reading these Lectures, the Author having been acquainted, that some considerable Objections had been made against the certainty and accurateness of his Instruments, and that I had affirmed it impossible to perform what he had promised in his Book, he returns his Sentiments thereof in a Letter to Mr. *Oldenburg*, to this effect:

——*Cæterum percipio vestrates non omnes mihi adstipulari in isto Dioptrarum negotio, de quibus in machinæ meæ cœlestis Organographia tractavi, verum etiamsi Cla.* Hookius *& Cla.* Flamstedius *aliiq; plane aliter sentiant, experientia tamen quotidiana me edocuit atq; etiamnum docet, rem longe aliter se habere in magnis illis organis, quadrantibus scilicet sextantibus & octantibus imprimis quadrantibus Azimuthalibus aliisq; quadrantibus regulis constructis, quæ nempe adeo procliviter commoveri & inverti (dum Dioptræ Telescopicæ examinantur) imo nullo modo possunt, ut quidem Instrumenta illa trium quatuorve pedum perpendiculo constructa. Rei cum primis in eo consistit, quod nullam plane observationem suscipere possint suis Dioptris Telescopicis nisi prius denuo eas examinent ac rectificent; in quo tamen examine variâ viâ, tum jugiter utut studiosissime illud suscipiatur hallucinari datur. Adhæc in quadrantibus Azimuthalibus, octantibus & sextantibus, qua ratione examen istud adeo accurate nunquam non haud magno negotio temporisq; dispendio institui possit, profecto nondum capio, vix mihi persuadeo ullibi adhuc ullum aliquem magnum quoddam Instrumentum 6 vel 9 pedum utpote sext. octant. vel quadrantem cum regula vel quadrant. Azim. cum pinnacidiis Dioptricis construisse, eumq; ad cælum felici aliquo successu adhibuisse, & quicquam solide observasse; si tentasset ac per annos aliquot observationibus*

vationibus continuo invigilasset sine dubio aliter sentiret. Hoc negotium enim non solum in eo consistit quod stellæ aliquanto distinctius conspiciantur (quanquam fixæ ab eo qui visu pollet & exercitatus est æquè bene nudis oculis discernantur) sed an Instrumenta ab omni parte correcte commonstrent, an pinnacidia Telescopica Instrumentis toties ad quasvis observationes rite imponi & tuto conservari queant; de quibus quidem id omni tempore æquè præcise fieri posse valde dubito. Quare Clarissimos illos viros humanissime rogatos volo nisi jam possideant ejusmodi vastissima organa utpote sext. octant. & quadrant. Azim. Dioptris Telescopicis munita, eaq; cælo continuo admoneant, suspendant judicium paululum, donec longa annorum serie experti fuerint haud fuisse multoties egregie elusos. Nam ex una alterave observatione quadrant. aliquo leviori perpendiculo gaudenti obtentâ, res hæc non est decidenda, sed si quis per 10 & amplius annos assidue observaverint, tum ab ovo seriam stellarum restitutionem per distantias susciperit, poterit quædam certiora in medium hac de re proferre. De reliquo satis mirari nequeo, eas omnes qui ejusmodi Dioptris Telescopicis gaudent, nondum locorum suorum, elevationem poli ubi degunt & observationes peragunt, quantum sciam recte & omnino pracise determinasse & stabilivisse. Hucusq; enim ad aliquot minuta integra Parisiis elevatio poli nondum est definita, alii quippe eandem observationem 48. 49' alii 50'. alii 51', alii 52', alii 53', alii 54', 55' imo ampliorem adhuc statuerunt: sicuti legere est ex discertatione Petri Petiti de latitudine Lutetiæ, sed nolo in his prolixius esse; ad observationes ipsas provoco, tempus aliquando docebit quorum observationes universas accuratiores fuerint, si modo nonnulli censuram suam eo usq; rejicere possent. Nam video aliquos inter quos etiam Cl. Flamsteditis invenitur, prout ex Epistola ad Cassinum apparet, jam judicium de nostris qualibus observationibus tulisse, priusquam illas adhuc viderunt examinarunt vel quicquam de iis cognoverunt. Nolo quidem vanus esse rerum mearum jactator, nec unquam mihi imaginatus sum rem in omni isto negotio circa scilicet restitutionem stellarum fixarum acu omnino tetigisse vel tangere posse. Sed hocce penitus mihi imaginor si totum istud negotium Dioptris Telescopicis suscepissem, quod non solum plurimos annos examinibus trivissem, sed spe sine dubio varia via (de qua hic non est discerendi locus) cecidissem. Exinde gratulor mihi me ad eam sententiam nondum transiisse, ac me mea methodo universa perfecisse se quicquid præstitum

præstitum Dei beneficio erit: an nihil amplius (ut putat Clarif. Flamstedius) *quam hactenus & quousq; progressum fuerit, liberum erit cuiq; cum deinde viderit judicium suum exponere quinetiam integrum erit alium novum integrum catalogum superadditis tot ac tot centenis novis fixis, hactenus neglectis alia ratione construere. Verum nondum video an curâ hæc molestissima, tædiosissima ac laboriosissima, quæ non nisi multorum annorum vigiliis suscipi & peragi potest, aliquem adhuc serio tangat. Unam aut alteram stellam ope Telescopii vel Dioptrarum Telescopicarum, dum præcipuas ac majores fixas earumq; intercapedines supponimus correctas ad debitum locum deducere, tum nonnunquam distantias nonnullas stellarum capere hæc ludicra sunt; sed omnes conjunctim secundum longum & latum restituere, tum ductu continuo singulis serenis diebus ac noctibus, tam altitudinum solarium quam reliquarum stellarum observationibus operam dare, easq; orbi exponere ut pateat motuum harmonia atq; Instrumentorum certitudo, hoc artis hoc laboris est. Quando observationes 20 vel 30 annorum spatio continuatas ab utraq; parte aliquando habebimus, nimirum tam quæ Dioptris Telescopicis quam quæ solummodo nostris ex cælo deprompta sunt res omnino clarior erit. Interea quilibet fruatur suo ingenio, ac sua ratione pro libitu rem tentet. Honorificum nobis omnibus erit pro modulo nostro à Deo concesso, rei literariæ incrementum varia via promovere.*

To this Letter of *Hevelius* I have this to anfwer, That the Author neither hath, had, nor can have any experience, to fhew Telefcopical Sights not to be as good as the Common, or that they are lefs applicable to large Quadrants, Sextants, Octants, or Azimuth Quadrants, or to any other Quadrants furnifhed with Rules, and fo fixt, that they cannot be eafily inverted, or turned, then they are to Quadrants or Inftruments of 3 or 4 foot Radius. Nor is his Reafon againft them of any validity, that no Obfervation can be made, without a repeated previous examination and rectification of the Sights, in which, fays he, notwithftanding all the care and diligence, there is a Reafon of failure and miftake. For firft, I fay, There is lefs need of rectifying the Inftruments or Sights, after they have been once adjufted, then of Inftruments with Common Sights, all things being perfectly fixt, and fo ftrong

F as

as not easily to be stirred or removed. I now begin to fear, that he hath not a true notion of the manner of performing the same, otherwise he would never have propounded such an Objection; and indeed he seems to say as much in the following words, *Qua ratione examen illud omni tempore commode & sine magno temporis dispendio institui possit profecto nondum capio.* Though I am very sorry that he should be so: for first, I thought I had about 9 years since, explain'd to him the way, when I exhorted him by all means to the use thereof; at least if he had not understood it thereby, I should, upon his desire, have sent him a more ample and particular Description thereof, or have procured an Instrument of that kind made and fitted for him here. But I fear, he had been some ways or other prepossest or prejudiced against them, before I writ first unto him concerning them, at least before he writ that Answer, which I have before printed in the 5 and 6 Pages, for thereby it appears, that he was then of the same opinion he seems now to continue of. And whereas he thinks, that no tryal hath ever been made of Telescopical Sights, to a large Instrument of 6 or 9 foot, I do assure him, (and I mis-remember. if I did not then acquaint him with as much) that I had then by me several, and particularly one of Sr. *Christopher Wren*'s invention, furnished with two Perspective Sights of 6 foot long each, which I made use of for examining the motions of the Comet, in the year 1665. And if the same thing can be better done with a Quadrant of 6 inches Radius, then he can perform with one of 6 foot the common way, I think he might have concluded at least, that the same thing would be 10 times better done in one of 6 foot Radius, made after the same manner; of this, I am sure, I gave him then an account. Now it is not with these kinds of Instruments, as it is with Common Instruments, where 'tis not possible to make any better then one may be made of 3 foot Radius, because that is capable of Divisions, accurate enough to reach the power of the naked eye; but Instruments with Telescopical Sights, are capable to be made to distinguish minutes, seconds, nay single thirds, if they be proportionably augmented. Nor is there any need that a man must make 7 years tryal of an Instrument, before he can be certain of the greater excellency thereof, for I can be as certain with

with 3 or 4 times viewing an Object through a Telescope, and with my naked eye, that I can see it better, and distinguish many more and much smaller parts in it through the Telescope, then I can with my naked eye, as I could be, supposing I had been viewing it 20 years together. But yet I must assure *Hevelius*, my experience hath not depended upon 3 or 4 tryals only; I cannot choose but wonder why he should be of that opinion, who hath not been less exercised in the use of the Telescope, then any at present in *Europe*: Possibly indeed his Telescopes were not altogether so good as now they are made, yet sure I am, he saw more with them then any one can see without them, as will sufficiently appear by his Phases of the Moon, *Jupiter* and *Saturn*. But I hope he will not wonder at me, though I do now venture to affirm, without staying 10 years or more to make Observations, that I can do more with a Quadrant, Sextant or Octant, of 1 foot Radius, furnished with Telescopical Sights and Screws, then can possibly be done with any other Instrument, furnished only with Common Sights, though 10, 20, 30, nay threescore foot Radius; nor does it at all follow, that the Latitude of *Paris* is not yet exactly known, because Monsieur *Petit* was ignorant of it; but it rather shews, that Observations made with Common Sights, (such as I suppose Monsieur *Petit*'s Instruments and others, before the publishing of his Book were) are no ways capable of certainty to a minute or two.

But I have done, and am sorry I have been forced to say so much in vindication of Telescopical Sights; and that in the doing thereof, I have been necessitated to take notice of the imperfections, that are the inseparable concomitants of Instruments made with Common Sights. Nor should I have published these my thoughts, had I not found them so highy decryed by a person of so great Authority, fearing that thereby other Observators might have been deterr'd from making any use of them, and so the further progress of Astronomy might have been hindred. Nor would I willingly be thought to depretiate or undervalue the Works and performances of a person, so highly meriting the thanks of all the learned World, both for his great and liberal expence, and for his vast pains, care and diligence, in the performing a Work so highly usefull to

F 2 Astronomy

Aſtronomy and Navigation, and of ſuch infinite tedium, trouble, labour and coſt, to the undertaker. I do not in the leaſt doubt, but that it will be a Work worthy ſo excellent a perſon, of perpetual eſteem and fame, and much preferrable to any thing yet done of the like kind in the World, and that he hath gone as far as it was poſſible for humane induſtry to go with Inſtruuments of that kind, and that his Inſtruments were as exact, and compleat, and fit for uſe, as ſuch Inſtruments with Common Sights could be made, and that he hath calculated them with all the skil and care imaginable, and deliver'd them with all the candor and integrity. But yet I would not have the World to look upon theſe as the bound or *non ultra* of humane induſtry, nor be perſwaded from the uſe and improvement of Teleſcopical Sights, nor from contriving other ways of dividing, fixing, managing and uſing Inſtruments for celeſtial Obſervations, then what are here preſcribed by *Hevelius*. For I can aſſure them, that I have my ſelf thought of, and in ſmall modules try'd ſome ſcores of ways, for perfecting Inſtruments for taking of Angles, Diſtances, Altitudes, Levels, and the like, very convenient and manageable, all of which may be uſed at Land, and ſome at Sea, and could deſcribe 2 or 3 hundred ſorts, each of which ſhould be every whit as accurate as the largeſt of *Hevelius* here deſcribed, and ſome of them 40, 50, nay 60 times more accurate, and yet everyone differing one from another in ſome or other circumſtantial and eſſential part. And that this may not ſeem altogether ſo ſtrange, I will aſſure them, that I have contrived above 20 ways for dividing the Inſtrument, each of them as much diſtinct from each other as this of *Hevelius*, and that of Diagonals, and yet every one capable of as great certainty and exactneſs at leaſt, and ſome of them 100 times more. I have above a dozen ſeveral ways of adjuſting the Perpendicularity or Horizontality of Inſtruments, all as exact as the common Perpendicular, and ſome of them very much more, even to what accurateneſs ſhall be deſired, and yet each of theſe very differing one from another. I have as many differing kinds of Sights, for improving, directing, adjuſting and aſcertaining the Sight, ſome of which are applicable to ſome particular uſes, but ſome for all, by means of which that part alſo may be be improved to what accuratenefs is defired. I have various ways of fixing thofe Inftruments, and appropriating them for this, that, or the other particular ufe. I have various mechanical ways for making and working the feveral parts of them with great expedition and certainty, which is a knowledge not lefs ufeful then the knowledge of the theory and ufe of them when made, there being fo very few to be found in the World that can or will perform it. I have a mechanical way of calculating and performing Arithmetical operations, much quicker and more certainly then can be done by the help of Logarithms, which compleats the whole bufinefs of meafuring Angles. Thefe I mention, that I may excite the World to enquire a little farther into the improvement of Sciences, and not think that either they or their predeceffors have attained the utmoft perfections of any one part of knowledge, and to throw off that lazy and pernitious principle, of being contented to know as much as their Fathers, Grandfathers, or great Grandfathers ever did, and to think they know enough, becaufe they know fomewhat more then the generality of the World befides: *Reptat humi quicunq; vult, Cœlo reftat itur, Cœlo tentabimus ire.* Let us fee what the improvement of Inftruments can produce.

And now to make my Reader fome amends for his patience, I fhall give a Specimen or two, of each of the feveral parts that belong to the perfecting of celeftial Inftruments: And this I fhall do, in the Defcription of an Inftrument for taking all manner of Angles and Diftances in the Heavens, which if increafed in bulk, is capable of as great accuratenefs, as the Air or Atmofphere will ever permit celeftial Obfervations to be made. Its perfection confifts in feven feveral particulars. 1. In the Sights, which are fuch as may be made to difcover the minuteft part difcoverable in an Object, they do no ways ftrain the eye, and are fit for all Sights, whether fhort-fighted or old, *&c.* 2. In the Divifions, which are fuch as will diftinguifh the Angle, as minutely as the Sights will diftinguifh the parts or Objects. 3 In the Sights, being fo contrived, that with one glance of the eye, both the Objects though a Semicircle diftant, are at once diftinguifhed and feen together. 4. In the method of fetting it exactly perpendicular to a Second,

cond, if need be. 5. In its fixation and motion, it being ſo fixed and moved, that if once ſet to the Objects, it continues to move along with them, ſo long as 'tis neceſſary to continue, or be very certain of any Obſervation. 6. In its not being difficult to be made and adjuſted, and its not being without induſtry and deſign put out of order, and its being preſently, and with all imaginable eaſe rectified and again adjuſted. 7. In its not being very chargeable. Firſt, For the Sights, They are no other then plain Teleſcopes, made with two convex Glaſſes, an Object and an Eye-Glaſs, of what length and charge ſhall be thought moſt convenient, fixed into ſquare Boxes or Tubes of Iron or Braſs, and having croſs Clews at the Focus, made with very fine Hair, or ſilk-Worms Clews. One of theſe is fixed upon the ſide of the moveable Bar or Plate of the Quadrant, the Object-Glaſs of which is next the Rim, and the Eye-Glaſs is next the Center. The other of theſe is fixed upon the ſide of the Quadrant by ſeveral Screws, and care is taken to keep it from bending or ſagging. This Tube is made of twice the length of the former, and hath at each end an Object-Glaſs, each of them of the ſame length with the former, and hath two Eye-Glaſſes in the middle, the manner of ordering which I ſhall ſhew by and by under the third head.

But firſt I ſhall explain the manner of fitting a Teleſcope for a Sight. Let a a b b in the 12th. Figure repreſent a Tube, in which let p repreſent the part toward the Object-Glaſs, whoſe Focus is at o, and let n repreſent the Eye-Glaſs, whoſe Focus alſo is at o, let s repreſent the point, where the eye being placed, the whole Eye-Glaſs n will be enlightned and fill'd with the Object, then make a ſmall Tube about an inch in length, and of ſuch bigneſs as it will juſt ſlide within the hollow of the Tube a a b b, and croſs the Cavity of that ſtrain two very fine Hairs or ſilk-Worms Clews, which may croſs each other in the Center of the Cavity, by the means of which Box, the ſaid croſſing Clews or Hairs may be moved to and fro, till they are exactly placed in the very Focus both of the Object-Glaſs and Eye-Glaſs, for if they be not there, the moving of the eye to and fro over the hole at s, will make the Threads ſeem to move upon the Objects, but if they be exact-

ly

ly in both the aforesaid Focus's, the moving of the eye will not at all make the said Threads seem to move upon the Object, but they will appear as steady and fixt to the Object, as if they were strained and fastned to it. And though they are exceeding small, even as small as the Web of a Spider or Silk-Worm, they will appear very big and distinct, and much plainer and bigger then a Thread in the Common Sights, at the further end thereof, will to the naked eye, though above 100, nay 1000 times the bigness, which at the first glance will sufficiently discover the vast advantage these kind of Sights have above the Common ones. Nor is this way of Sights at all confined, but may be made to distinguish the smallest part of the Object desirable, even the parts appearing to the naked eye, under the Angle of a single second or third of a Degree, which is some hundred of times more curious then the naked eye can distinguish, without the help of them, for the Telescope can be made longer, and the Eye-Glass can be made deeper, and according as the Telescope is longer, and the Eye-Glass deeper, so will the Object appear bigger, and more minute parts be distinguished, the power of the eye being increased proportionably to the length of the Object Glass, and the charge of the Eye-Glass, and the goodness of them both. Now as Sights this way made, are capable of the greatest accurateness desirable, so they are so appropriated to the eye, that they no ways strain it, for they may be so ordered, as to make all those parts that are to be distinguished, to appear to the eye under the Angle of 3 or 4 minutes, which most eyes are able well to distinguish, without using too much attention or straining to discover them. This is no small convenience, to one that is to make many Observations one after another, for the eye by too much attention is apt to be suddenly weary'd, and it doth very much harm and weaken the Sight, to endeavour to distinguish parts so small, as appear to the eye under the Angle of a minute, very few eyes being able to reach it at all, and most others not without much difficulty and endeavour. 'Tis further considerable upon this account, that 'tis fitted for all kinds of Sights: For a short-sighted person, the Eye-Glass may be made to slide a little nearer the Cross in the Focus; and for an old or decayed Sight, the Eye-Glass may be moved

moved a little longer or further off from the ſaid Croſs or Focus; for a dim Eye, the aperture of the Object-Glaſs may be augmented, and the Eye-Glaſs made ſhallower, or of a leſs charge; and for a weak, tender and curious Eye, the charge of the Eye-Glaſs may be augmented, and the aperture of the Object-Glaſs made leſs. And according to the ſeveral conſtitution of the Obſervators eyes, the manner of Sights may be accommodated, which the other Common Sights without the help of Glaſſes, are no ways capable of.

The ſecond thing wherein the perfection of this Inſtrument conſiſts, is the way of making the Diviſions, which I think, is far beyond the Common way, both for the certainty and eaſe of making, and ſecondly, for the plainneſs and certainty of it, in being diſtinguiſhed; nor is it capable of leſs accurateneſs for meaſuring, then the Sights are for diſtinguiſhing. And it excels all the Common ways of Diviſion in theſe particulars: 1. That it is made certain and not by gueſs, we being not at all to depend upon the care, credit and diligence of the Inſtrument-maker, in dividing, graving or numbring his Diviſions, for the ſame Screw makes it from end to end, as you will ſee by and by. 2. That the Diviſions are not at all difficult to be diſtinguiſhed, and there is no uncertainty in the Fabrick, nor can there be any reaſon of miſtake, there being nothing to be looked after, but the Numbers expreſſed in Figures at large, ſufficiently plain to any one that can read the Print of a large Church-Bible. It excels the Common ways thirdly, upon the account of its Compendium; for whereas by *Ticho*'s or *Hevelius*'s way, the Inſtrument muſt be made of 150 foot Radius at leaſt, eaſily and certainly to diſcover and diſtinguiſh Seconds, in this way it may be made to do it within the compaſs of 3 foot Radius. And whereas in either of their ways, even in an Inſtrument of 150 foot Radius, the Diviſions are not eaſily diſtinguiſhed and diſcover'd without the help of Glaſſes, in this way they are made ſo eaſie and plain, that a man cannot miſtake, that is able by his naked eye to diſtinguiſh Decimals of an inch. Now that this is ſo, as I affirm, the Reader will eaſily underſtand, if he conſiders, firſt, that the bigneſs of a minute is hardly half an inch, in an Inſtrument of 150 foot Radius, and conſequently the bigneſs of a ſecond is but $\frac{1}{120}$ of an inch,

inch, which to a good eye is but barely a viſible point at the beſt advantage, and to moſt eyes is not diſtinguiſhable without much difficulty, and to very many not at all without the help of Glaſſes. Now though *Hevelius* pretends to be able to do much by the help of the new way of *Nonnius*, *Vernier*, or *Hedreus*, yet if he conſiders what I have now ſaid, he will be of much another mind, a Radius of 10 foot being but a 15th. part of one of 150, and conſequently every 120th. part of an inch, being no leſs then 15 whole Seconds. At leaſt, I am ſure, he will be convinced that his own is not true, if he look upon that Specimen of it which he hath printed in his *Machina Cæleſtis*, in the Plate T. with a moderately magnifying Glaſs, as I hinted to him before. He will further underſtand the truth of my Aſſertion, if he conſiders in the next place, that by the help of the Screw, I am able to make the bigneſs of a Minute as much as I pleaſe; for ſince in an Inſtrument of 5 foot Radius, a Degree is ſomewhat better then an inch, 'tis eaſie enough to underſtand, that there may be 30 Threads of a Screw in the length of an inch, and conſequently there will be but 2 Minutes to fill up the whole Circle of the Index-Plate, and conſequently if the Circle be 7 inches Diameter, the Circumference will be almoſt 22 inches about, and conſequently the bigneſs of a Minute not leſs then 11, and the bigneſs of a Second not much leſs then the 5th. part of an inch. Now the Index-Plate e in the firſt and 11th. Figures, ſhews exactly the number of Revolutions, and the Hand 8 in the ſame Figures, ſhews the parts of a Revolution, and both theſe in Characters large and diſtinct enough; and therefore the certainty and truth of this Aſſertion cannot be further doubted.

The way then for theſe Diviſions is this: Make a Frame of a Quadrant of hammer'd Iron, after the manner expreſſed in the firſt Figure, and in the Center thereof fix or raiſe a hollow Cylinder, whoſe hollow may be about a 40th. part of its Radius, and whoſe convex part may be about a 30th; leave this ſtanding above the Plain of the Quadrant about $\frac{1}{90}$ part of the Radius, let the out-ſide of this Cylinder be made as exactly round as 'tis poſſible to be turned or wrought, then make a Ruler or Plate, with a round hole in it at one end, turned, groun'd and fitted exactly about the above-mention'd Cylinder, and as

G long

long as you design the Telescope for the Sights of the Quadrant, this by a Screw on the top thereof must be kept close and steady upon the said Cylinder: Upon the end next the Limb is to be fitted a Socket or Frame with Screws, to carry the Screw-Frame steady and firm, according to the contrivance exprest in the first and 11 Figures; this Plate must be filed or bended at that part of it which touches the Limb of the Quadrant, so as to lye obliquely to the Plain of the Quadrant, and to be parallel to the Plain of the Frame which carries the Screw, and upon the part beyond the Limb must be fixt with a Screw k, the Frame h h h, which carries the Screw 9 9 9, and the Index Plate t t; the contrivance of this Frame h h, is to keep the Screw 9 9 9 close against, and very steady to the Limb of the Quadrant, and is moved to and fro upon the Limb of the Quadrant b b b, by the help of the Screw turning upon and against the edge of the Quadrant; and this Screw by reason of its distance from the center and eye, (the reason of the placing of which in that place you will understand by and by) being too far off to be reached by the hand, is turned by a small Rod of Iron, o o o in the first and 11 Figures lying by the side of the Ruler or Plate, which hath a small Wheel q q, at the end next the Limb, by which the Screw is turn'd round with it, and hath a small Handle or Windle p p next the Center, by which it is made convenient to be so turned round. Upon the end of the above-mention'd Screw-Frame h h, is fixed a round Plate t t, which is divided into 1, 2, 3, 4, or 5 hundred equal parts, according as it is in bigness, and as it shall be thought convenient, which Divisions are numbred and marked accordingly, serving to shew what part of a Revolution is made of the aforesaid Screw; for the end of the Screw 9 9 9 coming out through the middle thereof, and a Hand 8 being fastned upon the said end, every turn of the Screw doth make a Revolution of the Index upon the said Plate; and consequently the motion of the arm made by one turn of the Screw, is actually and sensibly divided into 1, 2, 3, 4, or 5 hundred equal parts, which is so exceeding exact, and withal so Mathematically and Mechanically true, that 'tis hardly to be equallized by any other way of proceeding. This Description will be much better understood by the

the Explication of the Figure, and the ſeveral parts thereof.

Let a a a a a, &c. repreſent the Frame of the Quadrant, conſiſting of 5 Bars, radiating from the Center, ſteadyed all of them by a Quadrantal Limb, and a ſtraight ſubtending Chord Bar; this whole Frame is to be made of very good Iron, partly welded and partly ſodered together with Braſs; the breadth of the Bars may keep the ſame Proportions expreſs'd in the Figure, and the thickneſs may be about 180 part of the Radius in large Inſtruments. In the Center of this, out of the ſolid Bar, is to be raiſed a Cylinder, as d d, expreſſed above more plainly in the 2d. Figure; the out-ſide of this Cylinder is to be turned and wrought, as Founders do their Stopcocks, as exactly as poſſibly it can be, and the end of the Iron Plate or moveable arm c c c c, ſhaped as is expreſſed in the 3d. and firſt Figure, muſt be bored and wrought upon it very well, ſo as they may turn exactly true, evenly and ſmoothly, without any manner of ſticking or ſhaking, which a good Workman will eaſily perform. This arm being put on the Cylinder, is ſcrewed down faſt by the help of a Screw-Plate, expreſſed in the 4th. and firſt Figures by e e, which hath two notches in it ff, by means whereof a Handle g g in the 6th. Figure, doth readily ſcrew and unſcrew it, as there is occaſion. Between this ſcrew'd Plate and the hole of the Plate c c c c, is a thin Braſs Plate, let on upon an 8 ſided part of the Cylinder, that ſo the turning of the Plate c c c c, may not have any power to unſcrew the Plate e e, which otherwiſe it is very apt to do. Why this Center is thus made, and a hole left in the middle thereof, you will ſhortly underſtand more plainly. Upon the Iron Limb of the Quadrant laſt mention'd, is ſcrew'd and rivetted a Limb of fine Braſs, firſt caſt into that ſhape, and then very well hammer-hardned and filed, repreſented in the Figure by b b b b: This, as I ſaid, by many holes drilled through the Iron and the Braſs, is ſcrewed and rivetted upon the iron Limb, ſo as about half an inch in a Quadrant of 5 foot Radius doth over-hang the iron Limb, and the ends thereof extend a conſiderable deal longer then the Quadrant, the reaſon and uſe of which you will by and by underſtand, when I give the Deſcription of the Screw-Frame.

G 2 The

The edge of this Brafs Limb muft be, by the help of the Plate c c c c, and a File or Plain, cut very exactly round, to anfwer the Center of the Quadrant, and the upper fide thereof muft be plained exactly fmooth and flat, upon which Plain-fide the Loop-holed Plate c c c c muft move, as is vifible in the Figure. This Plate at i i muft be wrenched or wreithed, fo that the Plain thereof muft ftand parallel to the Plain of the Index-Frame, and by the wreithing of it at i i, as aforefaid, there is room left for the Screw to lye obliquely, without the Screws touching the aforefaid Plate, or grating againft it. The reafon why I put the Screw obliquely to the Plain of the Quadrant is, that that part of the Thread which toucheth the edge of the Limb, may be exactly at right Angles, or perpendicular to that Plain, and confequently that the Teeth upon the faid edge, may likewife be exactly crofs or perpendicular alfo, and confequently that no bending of the Rule c c c c, (to the end of which the Frame of the Screw is faftned) may at all vary the Angle, nor any unequal thicknefs in the Limb of the Quadrant, but that the turning only of the Screw fhall produce a variation, and that exactly proportionate to the number of Revolutions, and the parts thereof, fhew'd by the Index.

The way to know exactly what the obliquity of the Screw ought to be, to make the Teeth upon the Limb perpendicular, is to number how many Threads of the Screw there are in a known length, and what the Compafs of the faid Screw, or the Cylinder out of which it is made is, and multiplying the faid Compafs by the number of Revolutions into a Product, the Proportions of that Product to the known length, will give the obliquity of the Screw, the Product being the Radius, and the known length the Tangent of obliquity, thus; Suppofe in the length of 4 inches, there be 83 Threads of the Screw, and that the Compafs of the Cylinder of the Screw be |92 Centefms of an inch, I multiply the 92 by 83, the number of Revolutions, and it giveth me 76|36, that is 76 inches, and 36 Centefms of an inch, making this Product the Radius, and the known length, *viz.* 4 inches, the Tangent of the obliquity of the Thread of the Screw to the Axis thereof, or of the Axis of the Screw to the Plain of the Quadrant. The demonftration monſtration of this is ſo plain, that I need not inſiſt upon it, for the length of the Thread of the Screw is the Secant, the Compaſs of the Cylinder is the Radius, and the bigneſs of the Thread, or the Diſtance between two Threads, is the Tangent, in a right angled Triangle, and the Screw is ſuch a right angled Triangle, wound about a Cylinder, putting the Tangent thereof parallel to the Axis of the Cylinder, and conſequently in the Mechanical tryal of theſe Proportions, the more Threads are taken to make that compariſon or meaſurement, the more exact is the inclination found. The conſideration of which doth plainly ſhew, how exact a way of Diviſion this by the help of the Screw is, for the whole Quadrant is thereby reſolved into one grand Diagonal, the ſame with the Triangle, the length of the Thread upon the Compaſs of the Cylinder being the Diagonal, and the Diſtance of the two ends of thoſe Threads, in a Line parallel to the Axis, being the ſpace to be divided by it, and conſequently by augmenting the bigneſs or Compaſs of the Cylinder, and diminiſhing the Thread, you may augment the Diagonal in any Proportion aſſigned. Or by making the Hand or Index upon the end thereof, of double, treble, quadruple, decuple, &c. of the ſemi-Diameter of the Cylinder, out of which the Screw is made, you may duplicate, triplicate, quadruplicate, decuplicate, &c. the ſaid length of the Diagonal, in Proportion to the ſpace to be divided.

The next thing then to be deſcribed is the Screw-Frame, made of Iron, much of the ſhape repreſented by h h h, in the firſt and 11 Figures: This Frame, by the help of a Screw through the aforeſaid Plate, whoſe head is expreſſed by the round head k, is fixed on to the long Plate from the center, and by the help of the Screw l, is forced and kept down very cloſe, upon the edge of the Limb of the Quadrant; the Frame hath 4 Collers for the Screw-Pin to run againſt, which are indeed but half Collers, ſerving only to keep the Screw ſteady; two of theſe are made with moſt care, marked with m m, in the 11th. Figure againſt m i, doth reſt the Shoulder of the Screw Pin 3, which is kept cloſe home againſt it, by the Cylinder g g, in the 10 and 11 Figures; the ſharp Conical Point of this Screw 9 9, goeth into the Conical hole, at the end

end of the ſaid Cylinder g g g. The ſhape of this Cylinder, and the Screw by which it is forced againſt the end of the Screw 99, is repreſented in the 10th. Figure; 7 in the 9th. Figure repreſents the Conical Point; 3 the place lying againſt the Col er m i; 6 the Screw that moves upon the edge of the Limb of the Quadrant; 5 the Nut or Pinnion by which the Screw is turn'd by a Rod from the Center, expreſt alone in the 8th. Figure, but the manner how it lyes in the Frame, is expreſt by p p o o o in Fig. 1. o o o repreſenting the Rod; p p the Handle by which it is turned; q q the Nut or Pinnion that turneth the Pinnion 5 of the Screw; s r the Collers or Holes that hold it faſt to the moveable Plate or arm of the Quadrant; s s repreſenteth two ſmall pieces that clip the edge of the Limb, and ſerve to keep the Screw-Frame ſteady and true in its oblique poſture, and move equally on the Limb, by a ſtrong ſpringing of one ſide of it; t t repreſenteth the Index-Plate, which is divided into what number of parts are thought neceſſary, 1, 2, 3, 4, or 5 hundred parts, according to the bigneſs of the Thread of the Screw at 6, a greater Thread requiring a more minute Diviſion, and a ſmaller Thread requiring a more groſs. Theſe Diviſions are pointed at by the Index 8 at the end of the Screw, and the number of Revolutions or Threads are marked on the Limb of the Quadrant, and pointed at by the Tongue e e, upon the which is faſtned a ſmall Pin f, ſerving to carry a Lens over the Point of the Tongue, which maketh the number of Threads appear more plain and big: The manner of doing which upon the Frame of the Screw, is ſo eaſie, that I ſhall not ſpend more time in the Explication thereof, and the manner of making the whole Inſtrument, will be eaſie enough to any ingenious Workman; but if any perſon deſire one of them to be made, without troubling himſelf to direct and overſee a Workman, he may imploy Mr. *Tompion*, a Watchmaker in *Water*-Lane near *Fleetſtreet*; this perſon I recommend, as having imploy'd him to make that which I have, whereby he hath ſeen and experienced the Difficulties that do occur therein, and finding him to be very careful and curious to obſerve and follow Directions, and to compleat and perfect his Work, ſo as to make it accurate and fit for uſe.

By

By the help of theſe Indices, 'twill be eaſie and plain to ſee how many Revolutions of the Screw, and what parts of a Revolution make a Quadrant of a Circle, and conſequently 'twill be eaſie to make a ſmall Table, which ſhall ſhew what parts of a Quadrant, divided into Degrees, Minutes and Seconds, will be deſigned by the Revolutions, and parts of the Revolutions of the Screw. As for inſtance, If I find that 1600 Revolutions and |912 make a Quadrant, then 17|788 Revolutions make a Degree, and |296 Milleſms of a Revolution make a Minute, and about 5 Milleſms make a Second, thence 'twill be eaſie to find (if you obſerve) an Angle to contain 294|358, that is, 294 Revolutions, and 358 Milleſms of a Revolution, that the Content of that Angle in Degrees, Minutes and Seconds, is 16 Degrees, 32 Minutes, and 47 Seconds, which is plain enough, and much leſs ſubject to miſtake, then the common way made uſe of. I ſhall therefore proceed to

The third particular, wherein this Inſtrument excels all others, and that, is, That one Obſervator with a ſingle glance of his eye, at the ſame moment doth diſtinctly ſee, that both the Sights of the Inſtrument are exactly directed to the deſired Points of the two Objects, and this, though they be removed by never ſo great an Angle, nay, though they are oppoſite to each other directly in a Line. This, I queſtion not, will by all that know any thing of Inſtruments, or celeſtial Obſervations, be accounted one of the greateſt helps to ſuch Obſervations, that was ever found out. For whereas other Inſtruments require two Obſervators, for taking a Diſtance in the Heavens, and *Ticho* generally made uſe of four, amongſt which there was neceſſary ſo unanimous a concurrence in their readineſs and certainty, that the failure of any one ſpoyl'd all the reſt, and made the Obſervation become uncertain and of no uſe; and ſuch Inſtruments as were contrived for one Obſervator, were accompany'd with ſo great difficulty, in the adjuſting to both the Objects, being both in a continual and ſwift motion, and but one to be ſeen at once, that they were generally left off and diſ-uſed, there being ſo vaſt a trouble and fatigue of looking now upon one, then upon another, by many repeated tryals, and ſo many new ſettings of the Inſtrument to the Objects in motion, before the Sights could be adjuſted, beſides the great

great uncertainty at the beſt, of ſeveral Minutes of the truth In this way, the Obſervator has no farther trouble, then firſt, to ſet the Plain of the Quadrant in the Plain of the Objects, and by the Screw to move the arm of his Inſtrument, till he perceive both the Objects to toucheach other, in thoſe Points he would meaſure the Diſtance between. That this is ſo, he will eaſily perceive, when he underſtands the method of ſo adapting two Teleſcopes, that by looking in at one ſmall hole in the ſide of one of them, he will be able to ſee both thoſe Objects diſtinctly to which they are directed, how much ſoever ſeparated. The way then of doing it is in ſhort this.

Joyn them together at one end, by a hollow Joynt that has a hole through it, about $\frac{3}{4}$ of the hollow of the Tubes, prepare two ſquare Tubes of Wood, Braſs, Iron, *&c.* of what length you pleaſe, and directly againſt the Center of this hole in the Joynt, make a ſmall hole, about the bigneſs of the blackeſt part or pupil of the eye, ſo as the eye looking in at that hole, may ſee perpendicularly into the lower Tube, then obliquely place two pieces of reflecting Metal, very well and truely poliſht, ſo as to reflect the Axis of both thoſe Tubes, perpendicular or at right Angles, which is by fixing the Plain of the Plates, inclined to the ſaid Axis, in an Angle of 45 Degrees, let the upper reflex Plate reach from the upper ſide of the Tube, ſo low as to touch the Axis or middle of the Tube, and let the lower extend over the whole Tube, from the top to the bottom, and from one ſide to the other. Theſe will be known to be duely placed, if looking in at the ſmall hole againſt the Center of the Joynt, the two round holes of the Tube do appear to the eye to coalleſce into one, and that the eye ſees directly through the lengths of them both alike. Then into theſe Tubes fit two Teleſcopes, with convex Eye-Glaſſes, and croſs Threads for Sights in their Foci, that they may be both of them at due diſtance from the eye, looking in at the ſide-hole, then opening thoſe Tubes upon the ſaid Joynt to any Angle, and looking in at the ſide hole, you ſhall plainly diſtinguiſh at once both the Objects, that are brought into the Tubes directly, and reflected up to the eye.

That this may be the plainer underſtood, I ſhall add a Delineation thereof *in plano*. Let

Let a a b b in the 12th. Figure repreſent the upper Tube, and c c c c the lower Tube, and let d d repreſent that part of the Joynt, which belongs to the lower Tube, at one end, by which they are joyn'd together, and can be open'd in the manner of a Sector. Let i repreſent the hollow or center of this Joynt, which communicates the Cavities of the two Tubes. Let e e repreſent that part of the ſaid Joynt which belongs to the upper Tube, being only a hole through the lower ſide, big enough to incompaſs the Cylinder d d of the lower Tube; and let r r repreſent a Plate ſcrew'd or pinn'd on, to keep the parts of the Joynt together inſtead of rivetting. Let s repreſent the hole in the ſide, by which the Eye h is to look in, and f the reflex Mettal in the upper Tube, reaching only half way the Tube, and g g the reflex Mettal in the under Tube, reaching over the whole Cavity; then will n o and p repreſent the Eye-Glaſs, Sight-Threads, and Object-Glaſs of the upper Tube, and k l and m the ſame parts in the lower, and whatever Angle the Tubes make to each other, whilſt they open upon the before-mention'd Joynt, the Eye h looking in at s, will ſee directly by the Axis of them both, and ſee the Sight-Threads diſtinctly croſſing the Points of the Objects, whoſe Diſtances are to be meaſured.

Theſe being thus explain'd, I ſuppoſe, it will be no difficult matter for any man to conceive, how theſe may be apply'd to the above-deſcribed Quadrant; for 'tis but ſuppoſing c c, the upper ſide of the under Tube in this Figure, to repreſent a p a p, the fixt ſide arm of the Quadrant, and d d the Joynt of this, to repreſent d d the Joynt of the Quadrant, and b b the under ſide of the upper Tube, to repreſent c c c the moveable arm of the Quadrant, and applying two Tubes to theſe parts, and fitting them with reflecting Plates, Eye-Glaſſes, Sight-Threads, and Object-Glaſſes, at due Diſtances, the whole will be performed.

Theſe Tubes thus fitted, will ſerve to take any Angle leſs then a Quadrant, to what exactneſs is deſired, but for bigger Angles, the Contrivance muſt be ſomewhat varied, the Deſcription of which I ſhall now add.

Let either of the two Tubes for the Sights, be made of double the length of the other, that is, let it be as long behind the

H   Center

Center as before it, and make the Reflex-Glaſs, that it may be turned round, and reflect the Ray exactly backwards, as before it did forward, then fix into this other half of the Tube a Teleſcope-Sight, in all things fitted, adjuſted, and like the other two, then adjuſt them, that they may look forwards and backwards in the ſame like, which being done, the Reader will eaſily underſtand how any Angle may be taken, even to the extent of two right ones: For 'tis plain enough, that the two Tubes I firſt deſcribed, apply'd to the Quadrant, will meaſure any Angle to a Quadrant or right Angle; and 'twill be as eaſie to underſtand, how by the help of the Reverſe-Tube, any Angle between a Quadrant and two right Angles may be meaſured.

To make this a little plainer to the Reader, let c c c c c in the 12th. Figure repreſent the under Tube or fixed Sight, s the hole or Eye-cell, t r a round piece carrying the reflex Mettal g g; this is made to turn round, and the reflecting Mettal g g being fixed to it within the Tube, is carried round alſo with it. Let s i k l m x repreſent the Ray paſſing forwards by the Eye-Glaſs, Thread-Sight, and Object-Glaſs; then this round piece t r being turned and made r t, as in the 13th. Figure, is repreſented, and with it the reflecting Mettal g g, here marked q q, being turned alſo: the Line s q k l m y will repreſent the Ray reflected, and paſſing backwards by the reflex-Mettal q q, Eye-Glaſs k, Thread-Sight l, and Object-Glaſs y.

The meaſure of the Angle is found by the ſame Apparatus or Screw-Plate; for as much as the Screw-Plate would ſhew the Angle leſs then a Quadrant, if the fore-part of the Tube were uſed, by ſo much is the Angle more then a Quadrant, if the reverſe or back part of the Tube be uſed; and the ſame reaſon of the accurateneſs and certainty for the one, is good for the other, without being lyable to any manner of Objection or Inconvenience.

It remains therefore now only to ſhew, Firſt, How theſe two Perſpective or Teleſcope Sights, placed within the ſame Tube, may be made to look exactly forwards or backwards in the ſame Line. And ſecondly, How they ſhall be adjuſted to the Teleſcope, fixt upon the moveable arm of the Quadrant, ſo ſo as to know when the Diviſion-Angle begins, and when they are open'd to a Quadrant, right Angle, or 90 Degrees; for unleſs theſe be aſcertain'd, and fixt to as great a meaſure of accurateneſs, as the contrivance of the Screw is capable of dividing, or the Teleſcope-Sights are capable of diſtinguiſhing, or the Perpendicularity aſcertain'd, all the pains, care, induſtry, and curioſity, beſtow'd about the other, are of no uſe.

Firſt then, For fixing the Thread-Sights of the two Teleſcopes within the ſame Tube, ſo as to look directly forward and backwards, care muſt be taken, that every one of the four Glaſſes, that is to ſay, the two Object-Glaſſes, and the two Eye-Glaſſes, muſt be ſo ſteadily and ſecurely fixt into the Tube, that they cannot by any means be ſtirr'd or removed; the manner of doing which, I ſuppoſe, ſo exceeding eaſie, that I need not ſpend time in deſcribing a way to do it. Next, Sufficient care muſt be taken of the ſtiffneſs of the Tubes, that they may not warp or bend. Thirdly, One of the Thread-Sights muſt be fixt as firmly and ſecurely as the Glaſſes, and ſo, that the croſſing of the Threads may be, as near as poſſible, in the Axis of the Object and Eye-Glaſs, the other Thread-Sight muſt be left free, till by ſeveral tryals it be found to ſtand exactly in the ſame Line with the firſt; the manner of doing which, I ſhall now deſcribe.

There being two Threads which croſs each other, the one Perpendicular and the other Horizontal, care muſt be taken, that both theſe lye exactly in the ſame Lines with the Horizontal and Perpendicular Threads in the other Sights; and in order thereunto, there muſt be two Frames of Braſs, repreſented in the 29 and 30 Figures of the 2d. Plate, of the bigneſs of the hollow of the Tube; theſe muſt have groves made in the Tube fit to receive them, in which they may by the help of Screws be moved, and made to ſlide to and fro, as there is occaſion, for their adjuſting. Next, They muſt lye ſo cloſe together, that the Hairs may touch each other. And thirdly, They muſt croſs exactly in the Focus of the Object and Eye-Glaſs. One of theſe Frames muſt carry the Perpendicular Thread, and by a Screw in the ſide of the Tube, muſt be moveable to the right or left ſide, as there is occaſion; the other

H 2 Frame

Frame muſt carry the Horizontal Thread, and by a Screw in the top of the Tube, muſt be made to riſe or fall in the Tube, as there is need. The Mechanical Fabrick of which is ſo eaſie, that, I hope, I need not ſpend time in the further Deſcription thereof, but refer the Reader to the 29 and 30 Figures.

Theſe things being thus done, from the top of ſome Turret, or any other Station, where two oppoſite places at a conſiderable diſtance, as half a mile, or a mile or two, can be plainly ſeen, find out two Points, which, at the firſt looking through your Glaſſes, you find to be ſhewn out by the Croſſes of the Thread-Sights, then note thoſe Points very diligently, that you may be ſure to find them and know them again, when you have removed the Glaſſes; this done, turn the ends of the Tube, and (if you were looking Eaſtwards and Weſtwards) turn that part towards the Eaſt which before looked Weſtwards, and *vice verſâ*, and find out the two Points you ſaw in the former Obſervation, then directing that part that hath the fixt Threads, to the Point that was ſeen before by the moveable Threads, find out the other Point, which you will be ſure to ſee within the compaſs of your Eye-Glaſs, and obſerve how far the croſs Threads are now removed from it, either Northwards or Southwards, upwards or downwards, then, as near as you can, by your judgement half that Difference, and by the Screws move the Frames, that the Threads may ſtand in the middle between the two Points, then take notice again of the Points ſhewn by the Threads, and turn the Tube again: Do this ſo many times, till you find upon converting the Tubes, that you ſee the ſame Points to be marked by the Croſſes of the Thread-Sights, with which end ſoever you look on them, and then the Tube will be exact and fit for uſe.

The reaſon of this adjuſting will be ſufficiently plain, to any one that ſhall conſider the 14th. Figure: Where let v repreſent the middle of the Tube t u b, or the place of the Eye, and let w repreſent the Object ſeen Weſtwards, and e the Object Eaſtwards, at the firſt view; then keeping the middle of the Tube exactly upon the ſame Point u, turn the end of the Tube t towards the Eaſt, and the end b towards the Weſt, and find out firſt the Eaſtern Object e, and finding the other

other Crofs to direct now to the Point p, and not to w, divide the Diftance between the Point w, and the Point p, as exactly as you can, in half, which if you chance to hit exactly at firft, it will be the middle Point m, but if you do not, but you rectifie it only to r, then by the next turning of your Tube you will find s, where you muft again rectifie to half the Difference between s and r ; now the Difference being grown yet lefs, you will a 3d. or 4th. time fet it fo exactly, as to fee the Points m and e, which lye in the ftraight Line with the Center of the double Tube.

The 4th. thing wherein this Quadrant exceeds the Common, is for its accuratenefs for taking Altitudes; and this is done by the help of a Water-Level, for adjufting the exact Perpendicularity thereof. This Level may be made and fixed fo exactly, that any Obfervator may be fure of the Level of his Inftrument to a Second or two. The Level it felf is nothing but a fhort Tube of Glafs, about 6 or 8 inches long, Hermetically fealed at each end, and filled with a Liquor that will not freeze nor grow foul with ftanding.

The Glafs, as near as can be gotten, fhould be Cylindrical and ftraight, it being the better the nearer it be to a ftraight, provided it have a fenfible bending or fwelling in the middle, the gibbous part of which fhould be fet upwards, and a proper Cell and Box made for it of Brafs.

This Glafs is to be filled almoft full of diftill'd Water, to which about a 3d. part of good Aqua-fortis or fpirit of Niter hath been put, to keep the fame from freezing, and alfo from growing foul, then carefully fealed up Hermetically, and p a-ced in its Box of Brafs, and with hard Cement fixed into the fame, which by Screws is fixed to that fide of the Quadrant, that is to lye Horizontal.

The Brafs Box being thus fixed to the right fide of the long fixt Tube ap ap ap, and underneath the Quadrant, fo as not to hinder the free movement of the arm c c c, as at x x ; the next thing to be done, is by it to fet the Quadrant truly Horizontal, which is thus performed.

Setting the fide a p a p a p Horizontal, and the Limb of the Quadrant upwards, and looking in at the Center, take notice of two Objects in the Horizon oppofite to each other,

obferve

obſerve the limits of the bubble of Air on the top of the Liquor, on each ſide of the middle of the Level, and make a mark, then turning the ends of the Quadrant, ſet it, till the ends of the bubble ſtand as in the former Obſervation; then look again at thoſe Objects in the Horizon, and find what the difference is between theſe oppoſite Objects, and thoſe in the former Obſervation; then halve the difference between them as near as you can, and by your eye ſet the Sights to the middle between them, by inclining the Quadrant, then by the Screw that rectifies the Level, ſet the Glaſs-Level ſo, that the ends of the bubble may be equally diſtant from the middle, and convert the Quadrant again, and ſee if the ends of the bubble ſtanding at the ſame marks, the two oppoſite Teleſcope-Sights do ſee the ſame Objects, for if ſo, you are aſſured of the perfect Horizontality of the Sights, upon the fixt arm of a p a p; but if you do not find it to direct to the ſame Objects, continue examining and converting, till you find it perfect.

Now this way of Perpendicular being ſubject to the inconvenience of heat and cold, which doth rarifie and condenſe the Liquor, and conſequently make the bubble of Air leſs or more, care muſt be taken, to mark all the varieties of thoſe kinds of the bubble, that are cauſed by the degrees of heat and cold, which you may thus eaſily effect.

Reduce the Liquor in the Tube of the 24th. Figure, by the help of Ice and Salt, to as great a degree of cold as you can, then by the method newly directed, ſet the Quadrant Horizontal, and mark the two ends of the bubble with 44, then by gently applying heat to the ambient Air, warm likewiſe the Water, and obſerve the expanſion thereof at both its ends, and mark them on the Glaſs with the point of a Diamant, as 33. 22. 11. 0 0. which being done, it will be exceeding eaſie at any time, to adjuſt the Quadrant to any accurateneſs deſired, by being careful to ſee, that the two ends of the bubble be proportionably extended, as to 0 0. 11. 22. 33. 44, &c. or to any intermediate ſpace.

The Contrivance of faſtening and adjuſting this Level to the Quadrant or other Inſtrument, will be very eaſily underſtood, by the Delineation thereof in the 24th. Figure.

Let

Let a a a a repreſent the Frame or Plate of Braſs, which by four Screws d d d d, is fixed to the Tube, as before. This Plate hath 4 upright Cheeks, b b, c c, between which the Braſs Box e e e e, (into which the Cylindrical Glaſs-Level f f, is fixed with hard Cement) is held ſteady, without any manner of ſhaking. This Braſs Box, at the end of it near the right hand, hath a Pevots, which are fitted exactly into 2 ſmall holes in the Cheeks c c, and at the other end next the left hand, hath a ſmall Screw-Pin g, which holds it down faſt to the bottom Plate, and keeps it from riſing out from between the Cheeks b b, which a very ſtrong Spring lying underneath it, between the Plate a a, and the Box e e, would otherwiſe force it to do. By this Screw the Level is to be adjuſted to the Sights of the Quadrant, by the way I juſt now deſcribed, and being once thus adjuſted and fixed, 'tis not eaſily put out of order, without moving or altering the Screw g, which may eaſily be prevented by too Contrivances.

The Reaſon of the accurateneſs of this kind of Level, will be eaſily diſcover'd, if we conſider, that the upper part of the Tube being very near to a ſtraight Line, is conſequently either a part of a Circle of a very great Radius, or of ſome irregular Curve, very near of the ſame nature with a Circle, as to this buſineſs of Levelling, and conſequently a Degree of the ſame will be proportionably large, and the flexure of the Tube may be made of a Curve of ſo large a Radius, that every Second of Inclination may cauſe a change in the Level of a very ſenſible length.

This can hardly be performed by the ordinary way of Plumbets, without hanging from a vaſt height, which is not practicably to be performed, without almoſt infinite trouble, expence and difficulty, and when done, can be of no uſe in the World, as any one will grant, that conſiders the vaſt Apparatus that is requiſite to obviate the great unſteadineſs of Buildings, the motion of the Air, and a multitude of other incumbrances.

Now the Curvature this way made may be a portion of a Sphere of 1000 foot Radius, or more, if it be deſired, and conſequently a Minute of the ſame will not be leſs then $\frac{29}{100}$ of a foot, and every Second will be almoſt half a Centeſm of a foot,

foot, which is ſufficiently diſtinguiſhable to the naked eye. So if the Glaſs Cylinder be 9 inches long, it may contain two whole Minutes of ſuch a Circle between f and f, and one between 4 and 4, and conſequently the ſaid Glaſs may be ſet Horizontal to the certainty of a Second, which is hardly to be aſcertain'd any other way.

But there remains yet one great Difficulty, how to be able to make ſuch a Curviture, for though the thing be true in theory, yet is it not without ſome trouble, put in practice. Very few Glaſs Canes are ſo conveniently bent, as is deſirable, and 'tis as difficult to find them true ſtraight.

To prevent this, If Glaſs Canes be uſed, there muſt be much care taken, and many tryals made, for the finding what pieces, and what ſide of thoſe pieces will be moſt fit for this purpoſe, for our Glaſs-Houſe Workmen know not yet a way, certainly to draw them of this or that curviture or ſtraightneſs, nor are they eaſily ground into a ſtraightneſs or curviture by the Glaſs-grinder afterwards, though that can be done with ſome trouble. But diligence and tryal will quickly find ſome piece or other, that will be ſufficiently exact for any tryal, among thoſe which are only drawn at the Glaſs-Houſe. I made uſe of one of another form, ſuch as is deſcribed in the 25th. Figure, which I found to do exceeding well, the dark part repreſenting the Water, and the lighter part the Air. This was made of two Glaſſes, drawn in diſtinct Pipes at the Glaſs-Houſe, but joyn'd together in the Lamp, and the upper part of the larger or under Tube, was incurvated with its convexity downwards, ſo that the Water touched the middle part, and the bubbles of Air at each end thereof, communicated together by the ſmall Pipe above. I tryed alſo another way, by which I was more certain of the truth of the Curvity, and could make the Curvity of a greater Circle: This was by a long piece of a Looking-Glaſs-Plate, ground very ſmooth and poliſhed, which by the help of Screws I bent upon the circular edges of a braſs priſmatical Box, and cemented the ſame very tight, with hard and ſoft Cement; this Plate had a hollow Channel ground in it the length thereof, which ſerv'd to keep the bubble in the middle. By this means, 'tis not difficult to bend ſuch a Plate, into the Curviture of a Circle of 50, 60, 60, 100, 1000 foot Radius, and the Braſs Box can eaſily be made to fill or empty, as there ſhall be occaſion for the uſe thereof, ſo that the Bubble may be at any time left, of what bigneſs ſhall be deſired. It will be convenient alſo to varniſh the in-ſide of this Braſs Box with Lacker-Varniſh, very thick and cloſe, both to keep it from ruſting, and alſo to preſerve it from being corroded by Aqua-fortis, whenſoever there ſhall be occaſion to put it in, for the cleanſing the inward tarniſh and foulneſs of the Glaſs-Plate. This Curvity of the upper ſide of the Level may be made, by grinding the under ſide of ſuch a long Plate of Looking Glaſs, upon a Convex Glaſs-Tool of 50, 60, 100, 1000 foot Radius, and poliſhing the ſame accordingly of that Figure: The Curvity of the ſaid Plate is expreſs'd in the 26th. Figure. Now what by this way may be done with Water and Bubbles of Air, the ſame may be done with the ſame Glaſſes turned upſide-down, by the help of an exactly round and poliſht Cylinder or Globule of Glaſs, Chryſtal, Cornelian, Agate, or other exceedingly hard and cloſe Stone, after the manner repreſented in the 27th. Figure, for the Ball or Cylinder will naturally roll to the loweſt part of the Concavity, and there ſtand. But in the doing of this, great care muſt be taken, that the Globule be exactly round and poliſht, and that the Concavity of the Plate be as ſmooth and well poliſht, and that they be both very clean and free from duſt, otherwiſe the Cylinder or Globule will be apt to ſtand in a place where it ſhould not, and conſequently produce conſiderable errors.

And here I cannot omit to take notice of a very curious Level, invented by Sr. *Chr. Wren,* for the taking the Horizon every way in a Circle. Which is done by a large Concave, ground and poliſht on a very large Sphere, and the Limb of it ground and poliſht on a flat, for by placing the ſame Horizontal, and rectifying it by a ſmall quantity of Quick-ſilver, poured into the Concavity thereof, 'twill be eaſie, by looking by the flat poliſht Limb, to diſcover the true Horizon. The only inconvenience I find in it is, that the ☿ hath ſome kind of ſticking to the Glaſs, but a ſmall Chryſtal Bowl, I ſuppoſe, may remedy that inconvenience, and make it fit for uſe.

The 5th. thing wherein this Inſtrument is made to excell

I others,

others, is in its easinesses to be adjusted to the Objects, and in this, that being once adjusted, the whole Instrument is so order'd, as that it will remain constant to those Objects, though they are moved. The want of this is so great an inconvenience, in all other Instruments hitherto made use of, that almost all Observations have been thereby vitiated. And *Hevelius*, to prevent and obviate this, hath found out many Contrivances, but they are such, as though they do it in part, yet 'tis but in part, and that with much trouble and inconvenience. I need not spend time to shew, how many inconveniences his way by 4 several Hand-Screws, to be managed by 2 Observators at the least, is subject to; they are indeed so many and so great, that it was not without very good reason, that he so often appeals to experience, for the truth is, there was great need of long practice and much experience, to be able to make an Observation in that way well, the removal of every one of those Screws, having an influence upon every one of the other, so as no Screw could be turn'd, but the whole Instrument was put out of its due situation, and both the Objects being continually in motion, the whole Instrument was to be rectifi'd every moment. There was therefore necessary so great a judgement and dexterity, to manage every one of those Screws, that without an acquired habitude and handiness by long practice and experience, nothing could be done to any certainty, nay, not even to that little accurateness that the common Sights are able to reach. But this, though it were a very great unhappiness to *Hevelius*, that he was not furnished with better Contrivances, yet it no ways tends to his dispraise, for his most extraordinary and indefatigable care, pains and industry, is so much the more to be admired, esteem'd and honour'd, and will be so much the more, by such as have by experience found the difficulty, of making any one Observation certain in that way.

But that he or any other, that hath a mind to make further Tryals and Observations, may be freed from this intollerable trouble and difficulty, I have thought of this following Instrument, by means whereof the Quadrant being once adjusted, and set to the Objects, will continue to be so, for as long a time as shall be desired, without at all requiring the help of any one hand of the Observator, though he be but one.

My

My way then in ſhort is this: I make an Axis of very dry and ſtrong Dram-Fir, of a bigneſs thick enough for its length, to defend it from bending; at the lower end of this, I fix into the middle of it, (well bound and hoop'd about with Iron) a Center or Point of Steel, very well turn'd, hardned and ſharp, which is to move in a conical hole fit to receive it, of as good and well hardned Steel; at the other end of this Rod, I fix another piece of Steel into the middle thereof, that, immediately contiguous to the Wood, hath a Neck very well turn'd and hardned, a little tapering from the Wood outward, which is to be moved in a Collar fit for it, as I ſhall ſhew by and by; and at a convenient Diſtance from the ſaid Neck, as at ſomewhat more then half the Radius of the Inſtrument, is made a Cylindrical Neck, fitted with a Collar of Braſs, with a Joynt, and other Apparatus, large enough to carry the Table and Inſtrument firm and true, without ſliding or yielding in its Socket, after it be once ſet. This Axis by the Collar and conical hole below, I place parallel to the Axis, which by ſome tryals is eaſily enough adjuſted; about the Cylindrical Neck, at the upper end of this Axis, is a Socket of Braſs faſtned with a Screw, which Socket claſpeth in a Joynt, a ſhort Arm, which hath at one end a Ball that is fitted into a Socket, that is fixed under the Table and Frame of the Quadrant, and at the other end a Counterpoiſe of Lead, to ballance the weight of the whole Apparatus, about the Quadrant, upon the middle Line of the long Axis, then the Table and Quadrant is rectifi'd, ſo as to lye in the Plain of the two celeſtial Objects, whether Planets or fixt Stars, and by the ſmall Screws in the Sockets it is fixt in that Plain. What further adjuſting is requiſite, is done by the help of ſmall Screws in the Quadrant it ſelf, which are eaſily enough conceiv'd without Deſcription. The Table being adjuſted to the Plain of the Objects, with the Quadrant on it, and all counterpois'd pretty near by the poiſes underneath the Table, and the fixed Sight directed to one of the ſaid Objects, the ſaid Table and Inſtrument continues to be in that Plain, ſo long as is deſired, without any farther trouble to the Obſerver, though the Objects continually change their places, and the fixt Sight remains directed at one of the Objects, till the other can be found by the moveable Sight. To effect which

I 2 motion

motion of the Table and Instrument, a Watch-work is fitted to the Axis, so as to make it move round in the same time, with a diurnal revolution of the Earth, and consequently to keep even pace with the seeming motion of the fixt Stars; the manner of doing which is thus: About some part of the Axis, where 'tis most convenient for the Room in which 'tis to be used, fix an Octant of a Wheel of 3 foot Radius, let the Rim of this be turn'd true to the Centers of the Axis, and cut the edge thereof into 360 Teeth, there being so many half minutes of an hour in the 8th. part of a whole Revolution, though these minutes and hours which respect the fixt Stars, will be considerably shorter then the solar hours; then fit a Worm or Screw to these Teeth, that one revolution of the Worm being made in $\frac{1}{2}$, a minute may move one Tooth forward; the revolution of the Worm is adjusted by a circular Pendulum, which is carried round by a Flie, moved in the form of a one wheel'd Jack, from a swash toothed Wheel, fastned upon the shank of the Worm or Screw above-mention'd; the weight that carries round this Wheel must hang upon the shank of the Worm, and must be of about a 3d. or 4th. part of the weight of the Quadrant and Table, that it may carry it round steadily and strongly; and the circular Pendulum must be so order'd, that the Observator may at any time of his Observation either shorten or produce the length thereof, so as to make it move quicker or slower, as there shall be occasion, which is done, by sliding the hole upon which the Pendulum makes its conical motion, a little higher or lower, without lifting up or letting down the Pendulum, or else by winding up the Thread of the Pendulum a little shorter, or letting it down a little longer, by the help of a Cylinder, above the hole or apex of the Cone, in which the Pendulum is moved.

This whole Contrivance will be somewhat better understood by a Delineation. Let a b then in the 15th. Figure represent the Axis of Fir or Iron, c the conical Point at the bottom, d the conical center or hole in which it is to move, e the Collar above, in which the tapering Neck of the iron Par f is to be moved. The Axis of this is to be placed as exactly as may be, parallel to the Axis of the Earth: at the end or head of the Iron f g, is fitted a Socket h h, with a Screw 4, which will

will fix it to the head in any poſture. This Socket h h in the 15 and 16 Figures, hath a large Joynt to be ſtiffned by a Screw 5, in which Joynt is moved a ſtrong Bar of Iron, about 4 foot in length, to wit, 2 foot on each ſide of the Joynt, the one end 6 hath a large weight or counterpoiſe of Lead 8, which ſerveth to counter-ballance the whole weight of the Frame and Inſtrument upon the other, and can be ſcrew'd either nearer to or farther from the Joynt, as there ſhall be occaſion for poiſing; at the other end of the Iron is a large Ball of Iron 7, to which is fitted alſo a Socket of Braſs 9, with a Screw to fix it and move it, as there ſhall be occaſion. This Socket is faſtned under the middle of a Table s s, upon the plain ſide of which the Quadrant is to lye. Upon ſome convenient part of this Axis is fixed an Octant or Sextant of a Circle, repreſented in the 15th. Figure edge-ways, and in the 17th. Figure broad-ways, by 3 3 i i, whoſe circular edge 3 3 is cut into Teeth, as before is directed; unto theſe is adjuſted a Worm or Screw k, which is the Axis or Arbor of the Wheel l l l; this Wheel is moved round by the weight x, whoſe Line is coiled round the Barrel u u, and with it it turneth round the Flie n n, by the help of a Screw m, fixed upon the Arbor o o, in the manner of the Flie of a one wheel'd Jack; this Flie moveth circularly the Pendulum p p, in the 15th. and 29th. Figures, which is ſhortned or lengthned, by ſlipping up and down the Cylinder q q, the Thread of the Pendulum being faſtned at r.

I ſhall not now ſpend any more time in the Explication of the making or contriving the circular Pendulum, reſerving it for another opportunity and Diſcourſe, wherein I ſhall ſhew ſeveral uſeful Contrivances and Inventions about the ſame, and particularly about this and ſome other Experiments of motion, which was the cauſe of the Invention thereof by me long ſince, in the year 65. Upon which occaſion, I cannot but take notice of a Publication, made by *Chriſtianus Hugenius Zulichemius Conſt. F.* in his Book call'd, *Horologium Oſcillatorium ſive demotu Pendulorum ad Horologia aptato demonſtrationes Geometricæ*; containing a ſhort Deſcription of a circular Pendulum with ſomewhat about the Explication of it, without naming me at all, as concern'd therein, though I invented it, and brought it into uſe in the year 1665, and in the year 1666, I com-

communicated it to the Royal Society, at their publick Meetings, both as to the Theory and Practick thereof, and did more particularly explain the Isocrone motion of the Ball of a Pendulum, in a parabolical Superficies, and the Geometrical and Mechanical way of making the same move in such a Superficies, by the help of a Paraboloeid, which I caused also to be made and shew'd before the same Society, upon several days of their publick Meeting, where besides many of the Society, were divers strangers of forreign parts. This many of the Royal Society can bear me witness, and the publick Registers thereof do testifie and make appear, and I was told by Sr. *Robert Moray*, that he did then write to Monsieur *Zulichem* concerning the same. But of this more hereafter, when I examine some other things in that Book, about finding the descent of heavy Bodies, and of finding the Longitude of places, and publish some more certain and practicable ways of doing them.

This puts me in mind of publishing an Invention, which I made and produced before the Royal Society, in the same year 1666, much about the same time that I produced the Theory and Experiment of the circular Pendulum compleat, which I call'd the perfection of Wheel-work, as being indeed founded on a principle capable of the greatest perfection can be imagined. It is in short, First, To make a piece of Wheel-work so, that both the Wheel and Pinnion, though of never so small a size, shall have as great a number of Teeth as shall be desired, and yet neither weaken the Work, nor make the Teeth so small, as not to be practicable by any ordinary Workman. Next, That the motion shall be so equally communicated from the Wheel to the Pinnion, that the Work being well made, there can be no inequality of force or motion communicated. Thirdly, That the Point of touching and bearing, shall be always in the Line that joyns the 2 Centers together. Fourthly, That it shall have no manner of rubbing, nor be more difficult to be made then the common way of Wheel-work, save only that Workmen have not been accustomed to make it.

First then, If there be a certain number, and no more of Teeth required to be made in a small Wheel, then must the Wheel and Pinnion consist of several Plates or Wheels, lying one besides the other, in the manner they appear in the 20th. Figure.

Figure. Where ſuppoſe it be required, that the Wheel ſhall have 1000 Teeth, and the Pinnion 100, and yet that the Teeth both of the Wheel and Pinnion have ſufficient ſtrength: take 10 Plates all of equal bigneſs and thickneſs, and by 2 or more Screws fix them firmly together, as if one Wheel, cut this Wheel into 100 Teeth, and compleat it, then fit the middle hole upon the round neck of an Arbor, then unſcrew the Plates, and place them in ſuch order, that the Teeth may gradually follow each other, much after the manner as is expreſt in the 20th. Figure, (though it be there very ill expreſt, by reaſon of the miſtake and failure of the Graver) and with ſuch ſteps, that the *laſt* Tooth of one Degree, may within one ſtep anſwer to the firſt Tooth of the next Degree. I call the 10 Teeth comprehended within the lighter part, a b c d, or e f g h, or i k l m, a Degree of Teeth in ſteps, and d c f e, or h g k i, are Degrees of Notches between the Teeth, and the Tooth b c, which is the laſt towards the right hand, ſhould have been placed within one ſtep as low as e h, the firſt of the next Degree on the left ſide, (though it be much otherwiſe here graven) whence all the inequality in the touching, bearing or rubbing, in a Wheel-work thus well made, would be no more then what could be between the 2 next Teeth in one of the Degrees, which would be much leſs then a 10th. part, of what muſt neceſſarily happen in a Wheel of one Plate of 100 Teeth only.

Secondly, If it be deſired, that the Wheel and Pinnion ſhould have infinite Teeth, all the ends of the Teeth in the Degrees of the 20th. Figure, muſt by a Diagonal ſlope be filed off, and reduced to a ſtraight, as in the 21, which may indeed be beſt made by one Plate of a convenient thickneſs, which thickneſs muſt be more or leſs according to the bigneſs of the ſloped Tooth. And this is to be always obſerved in the cutting thereof, (though it be otherwiſe and very falſly expreſt in the 21 Figure) that the end of one ſlope Tooth on the one ſide, be full as forward as the beginning of the next Tooth on the other, that is, that the end b c of one Tooth on the right ſide, be full as low as e h, the beginning of the next Tooth on the left ſide, (though by the Gravers miſtake it be here quite otherwiſe expreſt.) I ſhall not ſpend more time in explicating the Pinnions, r s t u, r s t u, of the 20 and 21 Figures, which are to anſwer

ſwer the Teeth of the Wheels, they being plain enough to any perſon a little verſed in Mechanicks, and becauſe the further and more full Explication of the form and reaſon of this and other Wheel-work, is compriſed in another Diſcourſe, which I may afterwards publiſh.

But to proceed where I left at this Digreſſion, to the finiſhing of the Deſcription of the Inſtrument for moving the Quadrant, ſo as alway to reſpect the Object. The conical hole, in which the end of the Axis is to move, may be made after the form expreſſed in the 18th. Figure, where a a a a repreſents an iron Frame ſcrew'd faſt to the Floor, b b b b the iron piece, containing the conical ſteel hole, c c c c 4 long Screws, by which the piece is moved and fixed in any part of the ſpace, included within the Frame a a a a ; this by a ſtrong ſpringing Frame underneath, is kept down cloſe to the Superficies of the Floor, and cannot in any wiſe totter or ſhake. There is no great difficulty in the Contrivance, and therefore I ſhall proceed.

In the next place then, having ſhew'd the way how to keep the Inſtrument, in the Plain of two Objects that are to be obſerv'd, I ſhall ſhew, by what means a Quadrant may be kept always Perpendicular, and in the Azimuth of the celeſtial Object. And this I do, by a ſmall addition to the former Contrivance ; that is, Let a b in the 22 Figure, repreſent the Axis deſcribed in the former Contrivance, accommodated with all the Contrivances of the moveable Center below, of the Clock-work of the circular Pendulum, to keep it moving equally round in the middle, and of the Collar e above. But unto the ſmall Neck f muſt be joyn'd a ſemi-circular piece of Iron c d, with a Center-hole in each arm at c and d, to receive the Pevots i i, of the circular piece of Iron x, in the 22 and 23 Figures ; upon the ſecond Floor o o, muſt be ſtedfaſtly fixed a Bow or Frame of Iron h h, which muſt have a hole through it, exactly over the middle of the Plate x, this is to be a Collar for the Neck k, of a perpendicular Axis l k, which by means of a moveable Center fixed in the cieling, in which the Point l moves, may be exactly adjuſted to a Perpendicularity ; to this Axis at right Angles is fixed a Frame m m, ſteadied by the Brakets or Braces n n ; upon this Frame the

the fixed Sights of the Quadrant, are laid and adjusted to an exact Horizontality, and the Plain of the Quadrant being once adjusted to the Plain of the celestial Object, will by the circular Pendulum moving the Axis a b, in an equal motion with that of the Object about the Axis of the Earth, be always kept in the Plain of the Object, whose Azimuth and Altitude is to be observed. Now the motion of the under or inclining Axis a b, is communicated to the perpendicular Axis l k, by means of the circular Plate x, in the 22 and 23 Figures, for the semi-circular Arms c d of the lower Axis, taking hold of the Points 1 1 of the Plate x, and the semi-circular Arms of the upper Axis, taking hold of the Points 2 2 of the said Plate, the perpendicular Axis is moved in a proportionate motion with the inclining Axis a b, which Proportion is Geometrically and strictly such as it ought to be, to keep the Plain of the Quadrant exactly in the Azimuth of the celestial Object, as any one never so little versed in Geometry, will easily find; and I shall hereafter more at large demonstrate, when I come to shew, what use I have made of this Joynt, for a universal Instrument for Dialling, for equalling of Time, for making the Hand of a Clock move in the Shadow of a Style, and for performing a multitude of other Mechanical Operations.

The next thing I have to explain, is the way of finding how many Revolutions of the Screw, and what parts of a Revolution go to make a right Angle, or 90 Degrees upon the Quadrant. For the doing of which, I must, in a place where I can have a good Prospect for a semi-Circle, first direct both the Sights of the Telescopes directly at the same Object, and the same Point thereof, and then rectifie the Indices to 0, or the beginning of the Divisions; then I turn the Screw, till as near as I can measure with Compasses, the moveable Telescope hath moved a Quadrant, and through the three Telescopes take notice of three Points in the Horizon, that is to say, two Points exactly opposite one to another, in respect of the Center of the Quadrant, and a third pretty near the middle between them, in the same respect, which I further adjust thus; I shew'd before how I rectifi'd the fixed Sights, so as to look exactly forwards and backwards, which being accordingly done, I observe the supposed right Angle, with the moveable Sight on the Quadrant,

K and

and with the Sight fixt on the Quadrant looking forwards, and note diligently the two Objects pointed at; then without moving the Screw, or moveable Arm upon the Quadrant, I find those Objects through the moveable Sight, and the fixt Sight, looking backwards, and directing one of the Sights exactly to one Point, I observe, how much the other doth vary from the other Object, either by being within it or without it; then I half that Difference, as near as I can judge by my Sight, and move the moveable Sight by the help of the Screw, so as to respect the middle Point: Then I observe this second found Angle, by the fixt Sight looking forwards, and by the moveable Sight, and see whether there be any Difference, and if I find any, as near as possible, I adjust it again, to half this last Difference, and so continue to examine and adjust, till I am certain, that the Angles on each side of the moveable Tube, between the same and the Sights, looking forwards and backwards, are equal to each other, and consequently are both right Angles, or Quadrants of a Circle. Which when I have found, I observe, by the Indices on the Screw-Plate and Limb, how many Revolutions, and what part of a Revolution, the Screw hath been turned to open that Angle; this Number I set, as the Number answering to 90 Degrees, and dividing that Number into 90 equal parts, I have the Numbers that belong to every Degree, and dividing the common Difference between them into 60 parts, I find the Numbers answering to the Minutes of the Quadrant, and dividing the common Difference between the Minutes into 60 parts, I easily make the Numbers answering to the Seconds; but these will be needless, for subducting the next Number, less then it in the Table from the Number observed, you have the Degree and Minute, and some Number perhaps over, which may presently be found by one small Table of the common Differences of Seconds. See page 55.

Here methinks I hear some object possibly, That the Divisions on the Quadrant, do not exactly correspond to the Divisions made on the Plate. I answer, That in part they do, and in part they do not. First, They concur, in that all the Divisions made by whole Revolutions, shew exactly the same by the Indices, that they do upon the Quadrant. Secondly, I say, in part they do not, that is, the parts of any single Revolution,

lution, are not exactly and Mathematically the ſame pointed out by the Index, upon a Ring equally divided, that are made upon the Limb of the Quadrant. But yet, I ſay, they are ſenſibly equal even to the ſenſe, aſſiſted by a 60 foot Teleſcope, and conſequently need no manner of rectification; but yet if any one will be ſo curious and nice, he may make the Diviſions on the Index-Ring, according to the proportion of the Differences of the Tangents, that are ſubtended within half the compaſs of the diſtance of the two next Threads. As ſuppoſe in the above-mention'd inſtance, half the Diſtance of two Threads be the Tangent of three Minutes, or thereabout; if we examine any large Table of Natural Tangents, we ſhall find the Differences between the Minutes themſelves, even till ſix Minutes, (which is much more then double three) doth not differ above one or two parts of a thouſand thouſand, which is 1000 times more nice, then our Sight, even with Glaſſes, can arrive to, much leſs then will be the difference between the Differences of the Seconds; and therefore it will be a niceneſs meerly notional, and of no uſe, and as ſuch, ought to be omitted, and the plain and equal Diviſions made uſe of, they being as to all ſenſe true and perfect, and proper Diviſions, though as to curioſity of Theory and Calculation, unequal.

Now I have done, poſſibly ſome may ſay, To what purpoſe all this curioſity? To which I anſwer, That though poſſibly in many common caſes 'tis of but little value, yet I conceive in general, that it is of infinite value, to any that ſhall deſign to improve Geography, Aſtronomy, Navigation, Philoſophy, Phyſicks, *&c.* And to inſtance in ſome particulars, I conceive,

Firſt, That one uſe of this Inſtrument, may be for taking the exact Refraction of the Air, from the Horizon to the Zenith; by which we ſhall be able not only to rectifie all Obſervations, and clear them from Refractions, which in ſome Obſervations, eſpecially thoſe of Parallax, is abſolutely neceſſary, but it may give us a new means to judge of the qualities and conſtitutions of the Air, as to the ſeaſons of the year, and the temperature of the weather, which are to ſucceed. For 'tis moſt certain, that there is as great a variety in the re-

K 2 fractive-

fractiveneſs of the Air, as there is in the heat and cold, gravity and levity, dryneſs and moiſture, rarefaction and condenſation thereof, and ſometimes when none of thoſe do ſeem at all to be ſenſibly alter'd, its refractiveneſs hath been very much varied, which change does ſeem to proceed from ſome alterations in the upper Regions thereof, far removed from the Superficies of the Earth, and is ſometimes many days in deſcending and fermenting, as it were deeper and deeper; into the lower Regions of the Air, before it deſcend ſo low as the bottom thereof next the Earth. But of this much more in another place.

A ſecond uſe is for regulating the places of the fixt Stars, as to their Longitudes and Latitudes, and Diſtances from one another, eſpecially thoſe within the Zodiack, by which we ſhall in a ſhort time be able to judge, whether thoſe Bodies that we account ſo fixt and conſtant, do not vary their Poſitions one to another, which I have very good grounds to believe they do.

A third uſe of this Inſtrument, is for regulating the places of the Planets, by their Appulſes to thoſe fixt Stars, ſo that not only Aſtronomy will be perfected, but the Longitude of places upon the Earth, (a thing ſo highly advantageous for Trade and Navigation) will of conſequence follow, which without ſuch an Inſtrument as this, is in vain expected from the Heavens.

A fourth uſe of this may be for ſtating the exact Latitude of places to a Second, whereby we ſhall quickly know, whether thoſe Latitudes do vary, as well as the variation of the Loadſtone, which hath been conjectur'd, not without ſomewhat of probability, but is hardly to be determined, without ſome ſuch accurate way of Tryal, as this Inſtrument is capable of performing.

A fifth uſe of it may be, for examining what influence the approach or receſs of the other Planets have upon the Earth, as to its Periodical motion, and what influence the Earth hath upon them as to theirs; for I have good ground to believe, each of theſe to have influence upon one another, and to cauſe ſuch motions, as have hitherto much confounded all Aſtronomical Hypotheſes and Calculations: Of which I ſhall ſay more on another occaſion. A

A ſixth uſe may be for meaſuring the quantity of a Degree upon the Earth; the beſt Experiment of that kind, that is yet publick to the World, is that of Mr. *Norwood*, made between *London* and *York*: But if we examine with what Inſtruments he made it, we ſhall find, that he was not certain in either of his Latitudes to a Minute, and conſequently could not be certain of the quantity of the Earth, anſwering to his ſuppoſed mark to two miles, and conſequently it could not be made the common ſtandard of all meaſure. But by the means of this Quadrant, all Latitudes may be certainly taken to a Second, and conſequently the error in 150 miles, cannot be more then the 30th. part of a mile, and conſequently a foot, or yard, or rod, this way ſtated, cannot vary above a 6000 part of its length, which is ſufficiently accurate for a univerſal and common ſtandard of all meaſure and quantity, to which all other meaſures in the World ſhould be referr'd and proportioned. This was the occaſion of the contriving and making thereof; His Sacred Majeſty having commanded me to ſee that Experiment accurately performed, and to give Him a true Account thereof, which had been before this performed, had not my indiſpoſition of health prevented.

A ſeventh uſe may be for meaſuring the Diſtance between two places, exactly in a ſtraight Line. This it will perform to admiration, by the exactneſs of taking the Angles, if ſome length be exactly meaſured at the place that is to be the Object, inſomuch that 'tis hardly poſſible, by any other means in the World, to come to that exactneſs, nay, though there were a continued Plain extended between the two places, whoſe Diſtances are to be found, and the ſame were carefully meaſured with Chains, Rods, or Wheels. By this means the Diſtance of a Ship on the Sea, can be found more exactly, then any other way whatſoever, by one or two Stations, and a multitude of Philoſophical Tryals under this Head, which are not practicably to be done with any tolerable accurateneſs, by other ways.

An eight uſe may be for taking the exact Diameters of the Sun, Moon, and Planets, even to a Second, and the Diſtance of the ſmaller appearing Planets from the fixt Stars, near adjoyning. Now becauſe for this Deſign, it may perhaps ſeem a little too cum-

cumberſom, and by reaſon of its ſhort Tubes, ſomewhat too ſmall, I have therefore contrived an Inſtrument of 6 times the length or radius, which will take in an Angle of about 5 Degrees, and yet take in the whole Angle by one glance of the eye, and determine the meaſure thereof to leſs then a Second. I have likewiſe invented and made a new Helioſcope, by which the Body of the Sun may be look'd on as inoffenſively to the eye, as a ſheet of white Paper; of great uſe for ſuch, as will make Phyſical Obſervations of that glorious Body. Theſe I will in ſome enſuing Papers deſcribe.

A ninth may be for exactly taking the Level, for the conveyance of a River or Water from place to place; and under that Head of performing infinite of Philoſophical Experiments, which can hardly be try'd by any other way in the World, about the Refractiveneſs of the Air near the Earth, whereby diſtant places ſometimes appear, and ſometimes diſappear, under the Horizon. By this means alſo the Rotundity of the Earth may be truely found, vaſtly ſurpaſſing any thing performed by the beſt Levels yet known. To this we may add, the height of Hils, if their diſtance be known, or their diſtance, if their height be known.

I could have enlarged upon theſe, and have named divers others; but deſigning it only as an Anſwer to ſuch, as may captiouſly put ſuch a Queſtion, I ſhall rather leave the pleaſure of finding them, to ſuch as ſhall really ſeek them, to be aſſiſted thereby in their own undertakings.

F I N I S.

Errata.

PAg.2.l.13.r.$\frac{0}{10}$ p.6.l.14.r.*aquilæ*.p.13.l.3.r.*Mathematician*. p.15.l.11. r.*Fig*.32. p.13. l.28. r.*Fig*.31. p.18.l.39. r.*ſtructuram*. p.21.l.26. r.*dena minuta*. p.21.l.27.r.*diſcriminatim*. p.22.l.3.r.*Fig*.35. p.28.l.34. r.*quædam*.p.32.l.21.r.*ſhaking*.p.33.l.8.r.*focus*.p.39.l.28.r.*res*.p.40.l.11. r.*admoveant*. p.40.l.39.dele *ſe*.

A
DESCRIPTION
OF
HELIOSCOPES,

And ſome other

INSTRUMENTS

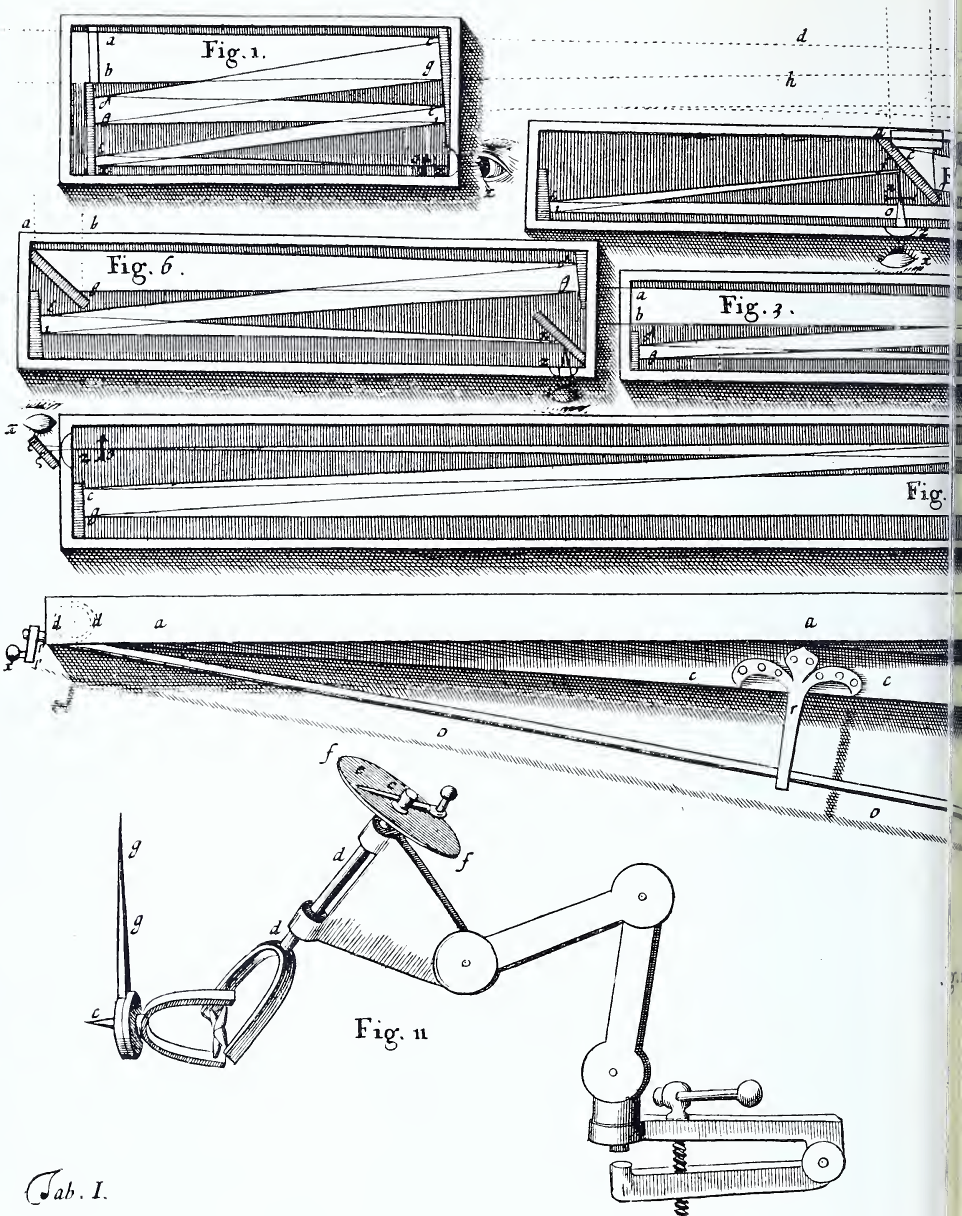
MADE BY

ROBERT HOOKE,
Fellow of the *Royal Society.*

Hos ego, &c.
Sic vos non vobis——.

LONDON,

Printed by *T. R.* for *John Martyn* Printer to the *Royal Society*, at the *Bell* in St. *Pauls* Church-yard, 1676.



Tab. I.

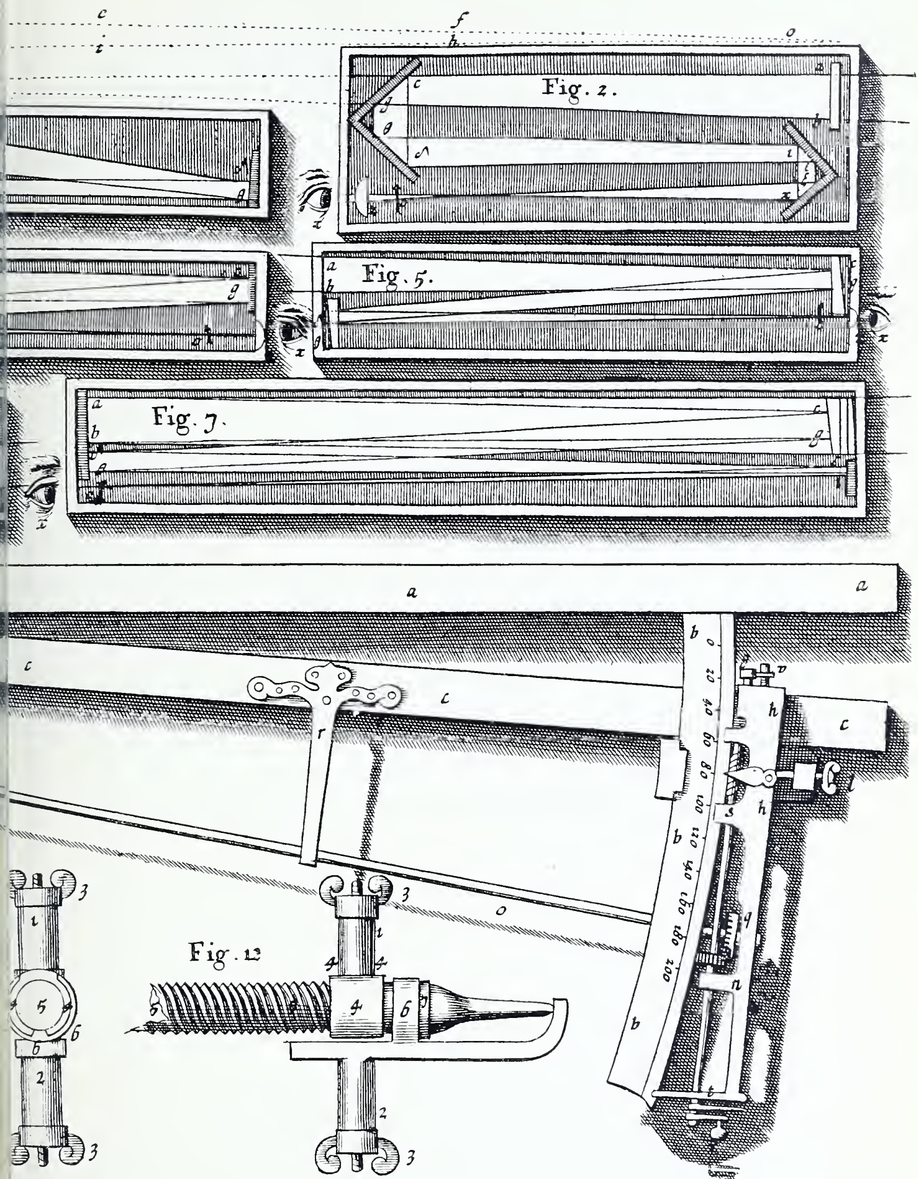


Fig. 2.
Fig. 5.
Fig. 7.
Fig. 12

A DESCRIPTION OF HELIOSCOPES, And ſome other INSTRUMENTS.

THE neceſſary avocations of buſineſs, and the urgent importunity of ſome, for the ſpeedy publication of my Animadverſions, made me conclude them in the Eleventh ſheet, without ſtaying to Explicate ſeveral things which I deſigned to go along with them. But having now retrieved a little more of leaſure, both for *Delineation* and *Deſcription*, for a further elucidation of what I have ſaid, I ſhall make it my third Attempt, to explain:

Firſt, *A* Helioſcope *to look upon the body of the Sun, without any offence to the Obſervers eye.*

B Secondly,

Secondly, *A way of ſhortning reflective and refractive* Teleſcopes.

Thirdly, *A way for uſing a Glaſs of any length, without moving the Tube.*

Fourthly, *An Inſtrument for taking the Diameters of the* Sun, Moon *and* Planets, *or for taking any other Diſtances, to five or ten Degrees, to the certainty of a Second.* Two of theſe I promiſed in the 78*th.* or laſt page of my *Animadverſions*, and the other fall in as analogous to them.

Fifthly, *An Inſtrument for deſcribing all manner of* Dials, *by the tangent projection.*

Sixthly, *The uſes thereof*;

1. *For adjuſting the Hand of a Clock, ſo as to make it move in the ſhadow of a* Dial,*whoſe ſtyle is parallel to the Axis*: Or,
2. *In the Azimuth of any Celeſtial Body*, that is, *in the ſhadow of an upright, or any other way inclining Style, upon any plain.*
3. *For making a Hand move according to the true aquation of Time.*
4. *For making all manner of* Elliptical Dials, *in Mr.* Foſter's *way, &c.*
5. *For communicating a circular motion in a Curve Line, without any ſhaking*: And for divers other excellent purpoſes.

And firſt, *For a* HELIOSCOPE *which ſhall ſo take off the brightneſs of the Sun, as that the weakeſt eye may look upon it, at any time, without the leaſt offence.* My contrivance is, By often reflecting the Rayes from the ſurfaces of *black Glaſſes*, which are grownd very exactly, flat, and very well poliſhed, ſo to diminiſh the Radiations, that at length they become as weak and faint as thoſe of the Moon in the twilight, ſo that one may with eaſe, and very much pleaſure, view, examine and deſcribe the phaſe of the Sun, and the *macula* and *faculæ* thereof, if any ſuch happen to appear when the Obſervation is made, and it gives a good opportunity of diſcovering them, before we have any advertiſement thereof from others. The reaſon of which will be ſufficiently plain to ſuch as conſider, how great a quantity of the rays of Light

is

is loſt by every reflection, and that every reflection doth duplicate, triplicate, quadruplicate, quintuplicate, &c. the firſt proportion of loſs. For Inſtance:

Suppoſe I have a *Helioſcope* made of an Object Glaſs, an Eye Glaſs, and four Reflecting Glaſſes, and that, by the firſt reflection, I loſe $\frac{3}{4}$ of the Direct light, I affirm there will remain but $\frac{1}{256}$ part of the Direct rays of the Sun, which can fall upon the eye at the laſt, for if every reflection doth loſe $\frac{3}{4}$ of its Rays, and reflect but $\frac{1}{4}$, and that quarter loſeth $\frac{3}{4}$, and reflects only $\frac{1}{4}$ of its received Light, there will remain but $\frac{1}{16}$ part of the whole, and if this ſixteenth part loſeth three quarters of its Rays, and reflects only a fourth, it will follow, the remainder will only be $\frac{1}{64}$ part of the whole, and if that be once more reflected, the Ray will return but with $\frac{1}{256}$ part of its firſt light.

This, although it be obvious, and eaſie enough now it is known, yet I do not find that any Perſon hath yet had thoughts of applying it to this uſe. The generality of Obſervers have hitherto made uſe of, either ſome very opacous and thick Glaſſes next the Eye, whether of red, green, blew, or purple Glaſs; others have diminiſhed the Radiation, by covering the Glaſſes with a very thick and cloſe coat of the ſoot of a Lamp; others, by caſting the figure upon a piece of white Paper, whence 'tis reflected to the eye; Others have contracted the Aperture into a leſs circle, and thereby let in leſs Light, and ſo make uſe of one ſingle Ray inſtead of a pencil of Rayes; Others have expanded the figure of the Sun, by the help of Eye Glaſſes, into a circle of ten, twenty, or an hundred times its Diameter. But none of all theſe waies do come near this which I now deſcribe by the help of three, four, or more Reflections, as any one upon trial will very plainly diſcover.

Firſt, As to the coloured Glaſſes, I cannot at all approve of them, becauſe they tinge the Rayes into the ſame colour, and conſequently take off the truth of the appearance, as to Colour; beſides, it ſuperinduces a hazineſs and dimneſs upon the Figure, ſo that it doth not appear ſharp and diſtinct. The ſame inconvenience is alſo produced by Monſieur *Hugenius*'s way, of covering the Glaſs with the ſoot of a

B 2 Lamp,

Lamp, though not to so great a degree. The Figure on paper, or a smooth white surface is not magnified enough, nor the difference of shadows so very distinct, though that doth very well, if the surface be very smooth, and the Object be magnified by a Hand Glass. That by the contracted Aperture is the worst of all, by reason of a certain propriety of Light not taken notice of yet by Optick Writers, the edges of Objects seeming ragged, of which I have hinted somewhat in my *Animadversions, pag.*35, and shall shortly say much more, the whole ground of *Opticks* depending thereon.

The way of expanding the figure of the Sun by the Eye Glass, to me seems the best of all the rest, but that is apt to vitiate the Figure, to super-induce somewhat of Colour, and doth not give the smallest distinctions of lights and shadows, without somewhat of colour, and somewhat of hazinesse and dimness.

The Glasses of this HELIOSCOPE may be made either by *refracting* or *reflecting* Spherical Glasses. The best way for taking in a large Angle, is, the using refracting Glasses, both for the Object and Eye Glasses; but the best way for taking in a small part, and for avoiding haziness, dimness, and colours, is, by Reflection, either in part, or in whole; that is, either to make the Object Glass only by way of *Reflection*, and the Eye Glass by that of *Refraction*, or, both the Object-glass and Eye Glass also by reflection, and to have no refraction at all. The several waies of doing which I have represented in the adjoyning Table, wherein I have expressed ten several waies of placing the several Glasses, so as to be fit for the use designed.

The first way represented in the first Figure, is, a sixty foot Object-Glass, contracted into a twelve foot Tube, by the help of four several Reflecting-plates placed between the Object-Glass and Eye-Glass. The Experiment of doing which, I produced and shewed before the *Royal Society*, at divers of their publick Meetings at *Arundel house*, in the year 1668, and it remains upon their Register.

This (as I then shewed) would be of exceeding great use in all manner of *Perspectives* and *Telescopes*, if we could find a good material that would make the Reflections very strong and

and full. And that would not be ſubject to loſe its Figure, which all our ſpecular Mettals are very apt to do; for, by it, 'twould be poſſible to contract the Tubes for long Glaſſes into very ſhort lengths, and ſo make them of eaſie uſe and manage.

This I attempted with ſeveral ſorts of Mettal, made with ♃, ♀, ♂, *Antimony* and *Arſenick*, but moſt of theſe compound Mettals I found to be very ſpongy, and conſequently in the laſt poliſh to receive, though a very glaring poliſh, yet ſuch as did much confound the Object by a kind of hazineſs, eſpecially if *Putty* be uſed to glaſe it, and, for this purpoſe, *Putty* muſt not in any wiſe, that I yet know of, be uſed, it being ſo very apt to round off the edges of pores or ſcratches, which does much contribute to the hazineſs and confuſion of the Object.

If I made uſe of Glaſſes foil'd with *Quickſilver*, which I found to give much the beſt *reflection*, yet I found this inconvenience, that a conſiderable part of the Ray was loſt, by the double reflection at the unfoil'd ſuperficies of the Glaſs. The firſt from the ſurface of the Glaſs before it entred; this, as it weakned the Ray, ſo mingling with the other reflection that came from the bottom, it created ſome kind of hazineſs and confuſion, if the two ſuperficies of the Glaſs were parallel, but if they were not parallel, it ſuperinduced ſomewhat of Colour, unleſs it were helped by a contrary refraction in a ſecond Reflecting-glaſs, after the manner of that which is delineated in the fifth Figure, where let *a b* repreſent the Object-Glaſs, *c g* the firſt Reflecting-plate, whoſe thinneſt ſide is to *c*, and *d θ* the ſecond Reflecting-plate, whoſe thinneſt part is towards θ, which doth thereby take off the firſt Refraction of *c g*, and deſtroy the Colours ſuperinduced by the firſt. The Ray alſo was weakned much more from the ſecond reflection it ſuffered at the unfoil'd ſuperficies of the Glaſs, from the reflection of the Air, or *æther*, which is much ſtronger than that of Glaſs, at its re-entring into the Air. Beſides this, I find that the ſubſtance of moſt Glaſs is ſo imperfectly mixt, that there is in the very beſt much of veinyneſs and inequality of Refraction in the parts thereof, and thence, though there

there were no viſible vein appearing in the body of the Glaſs, and though both the ſurfaces thereof were very truly figured and poliſhed, yet there was ſome kind of dimneſs ſuperinduced upon the Objects, by the rays paſſing through thoſe Glaſſes. But this was not in all, for I found ſome that did very well anſwer my expectation, and I am very apt to believe, that if a pot of Glaſs were made on purpoſe, by a way I know, the body thereof might be made perfectly clear, uniform, and tranſparent, without blebs, veins, or ſands, which, when I have leaſure and opportunity I deſign to experience farther But this only by the by, in relation to the ſhortning the Tubes of Teleſcopes for the *Moon*, *Planets*, and other Objects, becauſe it is not at all to our preſent purpoſe of making a *Helioſcope*, where we make uſe only of the reflection of the firſt ſuperficies of the Glaſs, and where our main aim and deſign, is, the loſs of the ſtrength and brightneſs of the Rays, and not for preſerving the ſtrength and briskneſs of the Rays, or augmenting them. And therefore for this uſe, the beſt material I have yet met with, is, black Glaſs, black Marble, and Glaſs of *Antimony*. For theſe ſubſtances being very dark and opaque, do reflect but a very ſmall part of the Raies that fall upon it, and none of thoſe that penetrate into it, eſpecially if they be thick; and being of a very hard and permanent ſubſtance, are capable of receiving a very curious and exact poliſh, and qualified ſufficiently to retain and keep it, without receiving injury from the Air, or ordinary wiping.

But in the making of theſe Glaſſes for Long *Teleſcopes*, very great care and diligence muſt be uſed to make them of a true flat, and ſo much the more, by how much the nearer they are placed to the Object-Glaſs, and the further from the Eye-Glaſs; a little errour at a great diſtance from the eye being vaſtly magnified to the eye at that diſtance, whereas a greater becomes inſenſible, if it be near the eye. Let *a b*, in the firſt, repreſent a ſixty foot Glaſs, whoſe focus is at *o*; let *a c d e f o*, and *b g h i k o*, repreſent the two ſide Rayes of the pencil of light, this Pencil, by the four Reflecting ſurfaces (γη, δθ, ει, ζκ) is broken into five ſhorter lengths (ηδ anſwering to *c d*, γθ to *g h*, δε to *d e*, θι to *h i*, εζ to *e f*, and ικ to *i k*, and laſtly,

laſtly, ζo and κo, to $f o$ and $k o$) as will be ſufficiently plain to any one that will but conſider the Scheme.

By this way four fifths of the length of the Tube is taken away, which is the moſt that can be taken away by four Reflections, every reflection running the whole length of the Tube, a leſſer part of the length may be taken away in any proportion aſſigned, as in the ſecond contrivance, deſcribed in the ſecond Figure, two thirds are taken off, when the ſame Letters anſwer to the Object-Glaſs, Eye-Glaſs, the flexures of the ſide Rays of the Pencil, and the Reflecting-plates that make thoſe flexures. The third and fourteenth Figures repreſent the Tube ſhortned by two or three reflections, and ſo ſerves to ſhorten the Tube by two thirds only. Theſe are of uſe for a very ſtrong Eye and with a ſmall aperture of the Object-Glaſs, and when the Sun is near the Horizon, or its light is a little diminiſhed, by a Fogg, thin Clouds, or the like.

If it be thought more convenient to have this long Tube to lie alwaies Horizontal, and conſequently, that there ſhould be no need of having a Pole or Engine to raiſe the Tube: It may be framed ſomewhat like that in the fourth Figure, where the ſame Letters anſwer to all the parts above-mentioned, or elſe like that in the ſixth Figure, the Letters of both which being the ſame with the former, will eaſily explain them.

Now in all theſe, and 20 other contrivances of this nature, with one, two, three, or four Reflecting-plates which may be preſently thought of, the ſight is directed exactly at the Sun, ſo that there will be little difficulty of finding it after the Glaſſes are fixt to their due lengths and poſitions.

I explained alſo at the ſame time to the *Royal Society*, at their publick Meeting at *Arundel-houſe*, ſeveral other waies of facilitating the uſe of very long Glaſſes, for other Objects in the heaven, by the help of one Reflecting plate only, and that was by a Tube fixed, either perpendicularly, horizontally, or obliquely, for it mattered not whether as to the ſeeing the Object in any part of the Heaven, ſuppoſing other circumſtances hindred not, and the object could be as eaſily found as by the common *Teleſcopes* of the ſame length. But of theſe elſewhere.

Theſe contrivances with four *Reflections*, may be made uſe of of by ſuch whoſe ſight is weak, but ſuch as can endure it ſomewhat brighter, and would ſee the parts more ſtrong, may make uſe of one of three Reflections only, like that of Fig. 14. which doth beſt ſuit my eye.

Next, this *Helioſcope* may be made by *Reflection* only, without any *Refraction*, and that may be done either in the manner of that in the ſeventh Figure, when *a b* repreſents a concave ſurface of a black Glaſs, whoſe focus is *o*, which, for Inſtance, we will ſuppoſe at the diſtance of forty foot, *c d* repreſents a clear plate of Glaſs of two flat ſurfaces, which are made not parallel but a little inclining, ſo as the reflection from that ſide which is furtheſt from the concave may be caſt another way, and not fall at all upon the third Reflecting-plate *ε ζ*, and becauſe the wedg like form of this tranſparent plate of Glaſs, *c d*, will cauſe a refraction, and conſequently a coloration of the Ray; therefore there muſt be another wedg-like Plate exactly as may be like the former, which at ſome diſtance, as at *m p*, where the reflection will not come to fall upon the Plate, *ε ζ* muſt be ſo fixed that the thinneſt part of this may lie juſt upon the thickeſt part of *c d*, and the thickeſt of this over the thinneſt of that, by which means both the falſe *reflections* and *refractions* will be removed. From *ε ζ* the Rays are reflected to *γ θ*, and from *γ θ* to *o* the focus, and ſo through the lens, *z*, to the eye *x*. This I take to be the beſt by *Reflection*; but it may be twenty other waies contrived, which I ſhall not now ſpend more time in deſcribing, it being ſo eaſie a matter from the conſideration of theſe I have mentioned, to make an hundred other variations of the principle.

To this *Helioſcope* may be fitted Inſtruments for meaſuring the *Maculæ*, *faculæ*, and *Nebulæ*, viſible in the body of the Sun, as alſo the ſpaces paſſed by them in a day, two, three, ten, &c. together with the variation of their Figures and Magnitudes; but the diameter of the body of the Sun will be better taken by the following Inſtrument. And by reaſon that it will be often neceſſary to draw their figures more exactly, the Engine that I have deſcribed in my *Animadverſions*, in the 67, 68, and 69 *pages*, may be made uſe of to keep the *Helioſcope* alwaies directed at the body of the Sun, which will be no ſmall eaſe to an Obſerver, that is to delineate the figures on Paper.
When

When the brightneſs and radiation of the *Moon*, *Venus* or *Jupiter*, do ſomewhat offend the eye, they will preſently loſe their beards and look very diſtinct, if one reflection from glaſs be made uſe of in the *Teleſcope*.

Another Inſtrument I promiſed to deſcribe, is, for taking any ſuch Diameters *tranſits*, or diſtance to the certainty of a ſecond Minute, by which more may be done for the finding the Parallax of the ſuperiour Planets, and the Longitude on the Earth, then hath been ever yet done by all the Inſtruments that have been uſed in the World.

1. This is made exactly, in all particulars like the *Quadrant*, as to its hollow centre, Screwd-limb, Screw-frame, and long Rod to turn the Screw from the Centre; and that the Screw-frame may be kept down the truer, upon the edge of the Limb, there ſhould be made a ſmall Arm to claſp behind the inward limb of the Inſtrument, after the manner repreſented in the 8*th*. *Figure* by *w*, by which means the Screw will be kept cloſe, ſteady, and eaven to the outward edge of the Limb. The Letters in this 8*th*. *Figure* being the ſame with thoſe of the 1 and 11*th*. *Figures* of the *Animadverſions*, and repreſenting the ſame parts, need no further explanation.

2. Inſtead of this Screw upon a circular Limb, a Screw may be made to move upon a ſtraight Limb, or Ruler; the end of which muſt move upon Centres or Rowlers, the centres or axes of which Rowlers muſt be exactly in the ſame line, when both the Perſpective-ſights are adjuſted to the ſame Object, and the diviſions began. The ſame thing may be done by a ſtraight Screw, in the manner of a pair of dividing *Compaſſes*, where the ſame care muſt alſo be had, that the axes of the Rowlers muſt be exactly in the ſame line, and the ſides of the Incompaſſing-ſcrew, being made of ſteel, muſt be made to ſpring about the long Straight-ſcrew; this long Screw muſt be made of ſteel of half an inch of diameter at leaſt, if it be made 18 inches long, and 'twill be beſt to ſcrew it with a ſmall thred, otherwiſe it will be apt to be moved out of a *ſtraight* by ſcrewing a large thred; and the thred, whether greater or leſs, muſt be made by degrees with a pair of cutting-ſtocks, that may be ſet cloſer every time of ſcrewing.

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The manner of contriving the Centres and Sockets may be ſeen in the 12 and 13 *Figures*, where the 13 repreſents it in an end-way Proſpect, and the 12 in a lateral or ſide-Proſpect; 1 is the Rowler of the upper Tube, and 2 of the under, 33 the Screws to faſten them in the holes, 44 the incompaſſing or Socket-ſcrew which ſpringeth cloſe to the Cylinder, 5, 6 the Cylinderical ſmooth Socket which guides the Cylindrical-ſcrew, ſo as to make its Axis paſs exactly over the center of the Rowler 22, and which, by means of a Ring 7 on the ſcrew, keepeth the pointed-end thereof 8 againſt the ſtay or portance 9; 'tis not difficult how to make a Dividing-plate, and an Hand or Index thereunto, nor how it may be turned from the centre of the two Tubes by a long Rod, as in the *8th. Figure*; nor vvill it be difficult, after it is known by Obſervation, how many Revolutions, and vvhat part of a Revolution anſwers to five whole degrees, to calculate a Table of *Subtenſes*, which ſhall ſhew vvhat part thereof goeth to make the *ſubtenſe* of every Minute and Second of the ſaid angle.

3. The ſame thing in the year 1665, I performed by a Rowler, rowling upon the limb of the *Quadrant*, by the help of two Wires vvhich vvere coyled about thoſe Rowlers, and the ends thereof were faſtned upon the limb of the *Quadrant*; for, by a large index on the end of this Rowler, I was able to move the arm of the Inſtrument to any fifth Second of the *Quadrant*, vvith great eaſe and certainty.

I alſo at the ſame time made another Frame with a ſtraight Screw, vvhich opened to five degrees only, vvith Tumbrels or Rowlers like a pair of dividing *Compaſſes* (after the ſame manner vvith this I have newly deſcribed, for taking *Diameters* or *Diſtances* to five degrees) and by the help of very curious Lines drawn upon a ſmooth Glaſs-plate, and Points very curiouſly made at every five degrees on the limb of the *Quadrant*, or Inſtrument on vvhich it vvas fixt, and the help of a very deep *Plano convex lens*, vvhoſe plain ſide vvas turned downwards towards the Plate, and the convex ſide towards the eye, the ſaid Frame vvas moveable from five degrees to five degrees, upon the whole limb of the *Quadrant* or Inſtrument, by vvhich Inſtrument I could vvith great eaſe actually and accurate divide an angle into every five Seconds, and

and consequently take any angle to the accuratenefs of five Seconds; for, removing the Frame to the next division, lefs than the Angle defired, and then by the Glafs, fixing one of the Arms that had the plate, exactly over the hole or point of divifion, by the Screw the remaining part of the Angle could be exactly meafured.

As to the method of *dividing* any of thefe, the beft vvay vvill be to meafure upon fome Plain 1000, 1500, or 2000 foot in length, by two Rods of twenty foot long a piece, or elfe by Wires ftrained vvith vveights, the vvay of which I fhall fhortly defcribe: Beginning from the very centre of the Inftrument, and at the end thereof, to fet up fo many Deal-boards joyned to the end of each other in a ftreight line, or elfe to ftrain a pretty big Line, vvhich fhall cut the meafured line of *diftance* from the center of the Inftrument at Right-angles, and then by a Table of natural *tangents*, according to the *diftance* from the centre of the *Quadrant*, put as *Radius*, to fet and mark off upon thofe *Boards* or *Lines* the divifions of Degrees and Minutes, by *Compaffes* or *Rules*, as exactly as may be, and mark them accordingly, that the *Degrees* may be diftinguifhed very plainly from the *Minutes*: Then having adjufted the Inftrument, fo as to fee the beginning of thofe *Divifions* through both the Tubes at once, to fet both the *Indices* to *o*, or the beginning of the *divifions*, then keeping the undermoft of the two Tubes fixt to the fame place, fo as ftill to refpect the fame point or beginning of the Divifions upon the *Boards* or *Line*, by the help of the Rod to turn the *Screw* or *Rowl*, till you find the upper Tube to refpect the firft *minute*, and then the firft *degree*, and fo till you fee the laft minute of the five vvhole *degrees*, or vvhatever Angle elfe you defign it to take in; then (for the firft and third way) reckon how many vvhole Revolutions, and vvhat part of a Revolution goeth to make up that vvhole Angle, and fubdivide the fame by a fmall Table into *Minutes* and *Seconds*, and you vvill prefently find by the Trial, that you vvill be able to divide to a ftrange accuratenefs upon thofe Boards, by the help of your *Tubes* and *Screw*, even at the diftance of 1000, 1500, or 2000 foot, and even almoft to equalize the Divifions by your *Compaffes*, when at the very Boards. And by this you

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may eaſily examine, whether your Inſtrument doth make the *ſub-diviſions* exactly or not, which will be a great confirmation of the certainty and truth of your Inſtrument. But for the ſecond way, by ſtreight Screws, the Table of Sub-diviſion into *degrees*, *minutes*, and *ſeconds*, muſt be proportioned according to the length of *Subtenſes* anſwering to the *Radius*, which is the diſtance of the centre of the *Rowlers* from the centre of the Inſtrument.

Now, becauſe in an Inſtrument of this bigneſs it will be ſomewhat troubleſome to turn the whole Angle by the help of the *Screw* upon the Limb, vvhich I find alſo is ſomewhat troubleſome in the Inſtrument of three foot *Radius*, vvhen the Angle is large, therefore for preventing of that trouble, and to be able immediately to open the Inſtrument to the *Angle* deſired, or very near it. The *Screw l* (in the firſt *Figure* of my *Animadv.*) at the end of the moveable Arm, is made, by unſcrewing, to draw off the long Screw from touching the threds on the Limb, which being done, the Arm is at liberty to be moved to any part of the *Quadrant*, when by returning the Screw *l*, the Screw-frame and Screw is brought down again to take hold of the Threds of the Limb of the Inſtrument. The only care to be taken in this action, is, that neither the Index *e e* be at all moved out of its poſture to the Index-frame *h h*, nor the Index 8 be moved at all about the rod of the Screw 999. It matters not at all though the Screw-rod 999 be turned round or moved, ſo as it be done by the Rod *o o o*, and the handle thereof *p p*, or by the ſmall handle *x* at the end of the Screw-rod, and that the Index 8 being very ſtiffly fixt to the ſaid Rod, be moved round with it by the ſame motion, without varying its poſition to the Rod; for being again brought down by the return of the Screw *l*, to take hold of the Threds of the Limb, into which it muſt be ſteadily guided by hand, the Index *e e* will ſhew upon the Limb the number of *Threds* or *Revolutions* from the beginning, and the Index 8 will ſhew what part of a Revolution there is to be joyned to it.

I hope I ſhall not need to ſpend time to explicate, how the Centre of theſe *Tubes* are to be made, nor how the Glaſſes and Thred-ſights are to be fixt, nor need I much to ſhew, how the *Tubes* may be ſtiffned to keep them from warping very much;

much; A ſmall matter of *warping* not creating any ſenſible errour, I am not much concerned to prevent.

If it be deſired to make the *Screw* leſs, and only long enough to ſubtend one whole *degree*, which is enough in Inſtruments of fifty or ſixty foot *Radius*, it may be done by a ſtraight Screw very well, if care be uſed, which will very exactly take *Diameters* and *Tranſits* to a ſingle Second.

Another thing I promiſed further to explain, was, the contrivance of the *Arms* and *Joynt*, mentioned in *page* 73, as a Univerſal Inſtrument for deſcribing all manner of *Dials*. *For adjuſting the Hand of a Clock, ſo as to make it move in the ſhadow of the Style of a Dial*, that is, *in the Plain of the right aſcenſion of any Point, of the Ecliptick, or of the Heaven; or* ſecondly, *in the ſhadow of a perpendicular, or inclined Style: For dividing and deſcribing all manner of* Ellipſes *in any* Analematical *projection*; and alſo, *For making all manner of Elliptical Dials in Mr.* Foſter's *way. For communicating a round motion through any irregularly bent way, without ſhaking or variation*, and the like.

Firſt, The Inſtrument for deſcribing all manner of *Dials* by the *Tangent projection*, muſt be made in this manner, deſcribed in the 11*th. Figure*, in which there are two Axes or rods of Wire that are joyned together by a Joynt, which from the applicability of it to, and fitneſs for all kinds of motions and flexures, I call a *Univerſal Joynt*. One of theſe Rods *b b*, is, by the help of a Frame *a a*, placed perpendicularly over the centre of the Dial, the ſharp or pointed end thereof *c* being ſunk into the Centre, about which it is to be moved according as it ſhall be guided by the motion of the ſecond Rod or Axis *d d*. This ſecond Rod or Axis, is, by its Frame, to be moved and ſet ſo as to be parallel to the *Axis* of the World; then the Hand *e e* of this laſt being turned to the hour of Twelve on the Plate *f f*, the Hand of the firſt *g g* will point out upon the Dial-plain, the Meridian or Twelve of Clock Line.

And ſo for deſcribing any manner of *Dial*, you have nothing to do but to find the *Subſtile*, and the altitude of the *Stile* above the Plain, and to put the Axis in its due ſcituation accordingly, that is, parallel to the *Axis* of the Earth; and then

then by the Plumbet at the end thereof to rectifie the Meridian or Twelve of clock point: For then, by turning round the Axis or Rod *d d* by the handle, till you see the Index *e e* on the Axis to point at those Hours, halfs, quarters, or minutes you have a mind to take notice of in your Dial; by the second Index *g g*, you are directed to the true corresponding point in the Plain of the Dial it self. But in such Dials as are in or near a Polar-plain, it will be convenient to make use of a small Thred to extend from the Cross, till it touch the Plain in the several hours, halfs, quarters, minutes, *&c.* The Arms of the Joynt in this Operation are to be so fixed, that the axis of the Plate may cross the axis of the Rod at right Angles.

The *Universal Joynt* for all these manner of Operations, having not had time to describe the last Exercise, I shall now more particularly explain. It consisteth then of *five* several parts, each of which I shall describe in the 9 and 10 *Fig.*

The two *first* parts are, the Rods and Axes *A* and *B*, on which the Semicircular Arms are fastned, which are to be joyned together so, as that the motion of the one may communicate a motion to the other according to a proportion, which, for distinctions sake, I call *Elliptical* or *Oblique.*

The two *next* parts are, the two Semicircular Arms *C C* and *D D*, which are fastned to the ends of those Rods, which serve to take hold of the four Points of the *Ball*, *Circle*, *Medium*, or *Cross* in the middle, *X*; each of these pair of Arms have two Centre-holes into which the sharp ends of the *Medium* are put, and by which the Elliptical or oblique proportion of Motion, is steadily, exactly, and most easily communicated from the one Rod or Axis to the other. These Centre-holes I call the *Hands.*

The *fifth* and last thing, is, the *Ball*, *Round-plate*, *Cross*, or *Medium X* in the middle, taken hold of by the hands both of one and the other pair of Semicircular Arms, which, for distinctions sake, I henceforth call the *Medium*, and the two Points 1 1, taken hold of by the Hands of the Axis, I call the *Points*, and the other two Points 2 2, taken hold of by the second pair of Arms, I call the *Pivots.*

First,

First, for the *Rods*, they may be made of what bigneſs you think fit, according to the uſe for which you deſign the Inſtrument. The only care to be taken in the making of them, is, firſt that they may be exactly Cylindrical in thoſe parts that move in Collers, and ſecondly, that the Axis or middle line of them do cut each other exactly in one point, which point muſt not vary upon any alteration or change of the Joynt by bending the angle they make with each other, more or leſs, nor with the inclination of the Semicircular-arms to any deſired obliquity, nor with the rotation or turning round of the whole Inſtrument. They require therefore a very dexterous, and a very knowing Artiſt, to make them as they ought to be, to perform their motion with exactneſs. Let *a b* then repreſent one of thoſe Rods, and *c d* a ſecond, which are turned exactly cylindrical within the Collers *e f g* and *h*, and theſe Collers are ſo diſpoſed and fixed on ſome frame, that the middle line or axis of both theſe Cylinders may cut each other in the point *e*; if then both their necks and collers be wrought true and exact, the Axis or middle lines of them will alwaies cut each other in the ſame point, howſoever they be turned round within their Collers; nor muſt this point *i* be varied, howſoever thoſe two Axes are inclined to each other, ſo that though *c d* be inflected to *l m*. or *n o*, and ſo make either an obtuſer or acuter Angle, yet the point *i* muſt be the centre of the Medium, where both the Axes concur and cut each other.

Secondly, The *Semicircular-arms* may be made of what bigneſs, thickneſs, or ſtrength, the occaſion for which they are deſigned ſhall require; that is, if they are only to carry the Hand of a Clock in the ſhadow of a Common Dial, whether made after the *Orthographical*, *Stereographical*, or *Horological* projection; or if they are by an Annual motion to ſhew the motion of the Sun in the Eclipt ick, or the æquation of Time, a very ſmall ſtrength is ſufficient; but if they are for carrying round a great *Quadrant*, ſuch as that I have heretofore deſcribed, there they muſt be made ſtronger and more ſubſtantial. Care alſo muſt be had, that the inclining the Arms to any angle may not vary the centre of the Ball or Croſs out of the point, where the two Axes cut each other. Both theſe Arms

Arms are to be made ſo as to be inclined to any angle; that is, that the Axis of the Medium, taken hold of by the Arms of Iron, may be made to incline to the axis of the Rod, on which they are in any angle deſired, and being ſet to that Angle, to be ſteadily fixed, which may be done by a pin, ſcrew or wedge; the way I make uſe of for the *Azimuth-*Inſtrument, deſcribed in the 73 *p.* of my *Animadverſions*, is this which is delineated and explained in the *9th. Fig.*, where *G* repreſents a ſocket of Braſs, movable cylindrically round about the end or neck *B*, of the Axis or Rod *BB*, the ſame with *a b*, in the 22 *Fig.* of my *Animadverſions*, and fixable in any poſture deſired, by help of a ſide Screw *h*, ſuch as is very commonly made uſe of for moſt Inſtruments that are fixed upon the end of a three legg'd Staff, and is commonly called a *Cylinder* and *Socket*; this Socket of Braſs hath a ſmall Rod of Iron, *k*, fixed into it at *k*, which is near the middle of its concave part, through this Rod there is made a ſmall eye or hole, and through that hole a wedge-like pin *m* being thruſt, ſerves to keep the Semicircular Iron-arms *C C*, ſteady and fixed in any poſture they ſhall be rectified to. The Semicircular-arms *C C*, are to be made of very good Iron, or rather Steel, and to have a channel or grove quite through the middle of one of them, and extending the whole length of a quadrant of a Circle, namely from *n* to *o*, becauſe, according to the variety of occaſions, it may be varied to any point between *n* and *o*; and 'tis to be obſerved, that the Iron-rod *k* muſt be ſo far fixed out of the axis of the Socket *g*, as *n* is diſtant from *i*, or *o* from *p* the middle of the Iron-arms between *i* and *i*, that ſo when there is occaſion, the Centre-hole or hands *i* may be moved to *p* and faſtned. At *q* muſt be made a Joynt in the Semicircular-arms, ſo that when the end *n* of the Arms is fixed in or near *k*, the other arm *C* may fall back from the point *i*, otherwiſe the circular motion, in many caſes, cannot be continued quite round, and communicated from one Rod to the other, by help of the Medium or Plate *x*. The ſeveral pieces of this Joynt, as they are apart and diſtinct, you may ſee in the *9th. Figure*, and as they are joyned all together fit for motion you may ſee in the *tenth Figure*, to which alſo the deſcription of every part is adjoyned in words referred to by the help of *Literal marks*, which

which, I hope, will make it ſufficiently plain to any Artiſt to underſtand.

Thirdly, The medium Ball or Croſs *X*, muſt be made of a bigneſs ſuitable to the Arms and Cylinders, and great care muſt be had that all the ends, points, or handles, lie exactly in the ſame plain, and that they be all equally diſtant from their Center, at leaſt, that any two oppoſite ones be ſo made, becauſe it is not abſolutely neceſſary that they ſhould be ſo all four, though in moſt caſes it be beſt; and farther, the Handles or Pivots ought to be exactly round, conical, or cylindrical, and the middle lines of them to cut each other at right angles, or upon a ſquare; and in general, that all things about the ſaid Joynt be ſo contrived and wrought that the Axis of the two Rods may alwaies cut each other in the centre of the medium Croſs or Plate, and that the ſaid Centre, whatever change happens to the Joynt, may alwaies keep exactly in the ſame very point, without any alteration.

The ſhape of this *Medium* may be either, a Croſs whoſe four ends hath each of them a Cylinder, which is the weakeſt way, 'tis deſcribed in the 9 and 10*th. Figures* by the Croſs *X*; or ſecondly, it may be made of a thick plate of Braſs, upon the edge of which are fixed four Pivots, which ſerve for the handles of the Iron-arms to take hold of; this is much better than the former, but hath not that ſtrength and ſteadineſs that a large Ball hath, whieh is the way I moſt approve of, as being ſtrong, ſteady, and handſome; theſe are delineated in the aforeſaid *Figures*, by *X x*, and *X x x*.

If it be an *Elliptical Dial* to be deſcribed by the *Orthographical projection*, the former way for deſcribing *Tangent Dials*, gives the lines that divide the Ellipſis of the *Equinox* in its true proportions: and if you would have the Lines that divide the Ellipſis of either Tropick, or of any other parallel Circle, you muſt rectifie the Semicircular Arms *C C* of the Axis *B B*, to the degree of the declination of that Parallel, and them proceeding as before, you have the Lines which from the aforeſaid Circle divide the Ellipſis of that *Parallel* accordingly. *Perpendiculars* alſo, let fall from the ends of the Croſs I I, give the true Ellipſis in the *Orthographical projection* anſwering to that Parallel.

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These *Lines* thus found, are the true *azimuth Lines* of the points or divisions of that Parallel, and are this way traced out exactly, without any trouble of Calculation, which for some purposes, in *Surveying*, *Navigation*, *&c.* are of very great use, as I shall afterwards shew.

The Universality of this Contrivance, for resolving almost all *Spherical Questions*, makes it of very great use in *Navigation*, if it be adapted as it ought to be, especially for the Common Sea-mans use, who, with a very few Rules, will be able immediately to find the *hour*, and *azimuth* of any point in the Heaven, sufficiently accurate for most Observations that can be made at Sea; of which more hereafter.

For making the *Hand* or *Index* of a Clock move in the shadow of the *Style*, made upon the Face of the Dial, and exposed to the Sun, this *Joynt*, being made to joyn the arbor of the Wheel that goeth round in twenty four hours, with the arbor of the hand, performeth it without any other *Wheel* or *Pinion* in the Dial or Face part of the Clock; if the Arbor of the Clock that should have carried the Hand round in twenty four hours, be made to have the same inclination to the plain of the Dial that the Axis hath, whether parallel to the Axis or not, it matters not at all, so that the Hand be rectified accordingly as it ought to be, and that the *Style* of the Dial ariseth from the centre of the Dial, out-through which the Arbor is produced for carrying the Hand, and placed in its Parallel respect to the Axis, as it ought to be for a *Tangent Dial.* For the shadow-Line of the Axis upon the plain of the Dial, being alwaies carried round the centre of a Dial in a plain, which passeth through the *Axis* or *Style*, and maketh equal progressions about it in equal spaces of Time, and unequal progressions upon the Dial-plain, according to the proportion of Inclination, and the whole Revolution being performed in twenty four hours, and the Hand of the Clock upon the Face of the Dial being alwaies moved in a plain which passeth through the Arbor of the Clock, and maketh equal progressions in equal spaces about the said Arbor, but unequal progression about the Centre of the Dial, according to the differing Inclinations: And those Inclinations being both in the Sun-Dial and Clock-Dial the same, it will follow, that the Hand

Hand of the Clock must alwaies move in the shadow of the *Style*, if the Hand be once rectified to the true Plain, and the Axis or Arbor make its Revolution as it ought to do in twenty four hours.

If it be further desired, for the ease of taking *Azimuths* and *Altitudes*, that the Arm of the Azimuth quadrant that is once adjusted to the *Cælestial Object*, should, by the aforesaid Joynt or Instrument, be kept alwaies respecting and following the said Object in its *Diurnal motion*, it may be very easily performed by the help of a small *perpendicular Ruler*, whose lower end is Joynted into either of the Arms 1 1, of the circular Plate *X*, in the 22 and 23*d*. *Figure* of my *Animadversions*, and the upper end joynted into the movable Arm, at the same distance from the Centre of the Quadrant that the lower end is from the centre of the Plate *X*, and that the centre of the *Quadrant* be set exactly perpendicular over the centre of *X*; but then the divisions by the help of the Screw cannot be made use of, because the Clock-work it self is to turn and move the Arm: But it may be done by any *Quadrant*, where the minute Divisions are performed by the help of *Diagonals*. For the Arms of the Circular-plate 1 1 being alwaies moved in the superficies of the Cone described, by the radiation from the Cœlestial Object to the centre of the Plate *X*, that is to say, the Line that passes through the Centre of the said Plate, and through the two Points 1 1, being alwaies directed to the Cœlestial Object, if the Arm of the Quadrant be moved perpendicular over it, and parallel to it, that also must be alwaies directed to it. And hence it may very easily be conceived, how the aforesaid Semicircular Arms may be readily and certainly rectified to any *Cælestial Object*; that is, by fixing *Telescopes* or Common-sights upon the Circular-plate, so as the Axis of them may be parallel to the Line through 1 1, and loosing the Screw *h* to rectifie it to the Object by the sight, and then immediately to fix it in the said posture by the aforesaid Screw; the Clock-work of the said Instrument having been before that put into motion. The reason of all which will easily appear to any one that throughly considers, that all Celestial Objects seem, by the diurnal motion of the Earth, to move equally from East to West about the Axis of it, and

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would all do exactly ſo, were they not ſomewhat varied by their own proper periodical revolutions, which though it doth indeed make a real difference between their velocities about the Axis of the Earth, yet that difference is but ſmall; and the ſame circular Pendulum will ſerve both for the *Sun*, *Moon*, *Planets*, and *Stars*, if at leaſt the Pendulum *p*, in the *fifteenth Figure*, be a little lengthened or ſhortened, by lifting up or letting down the Rod *q q*, in proportion as the Body *k* moves ſwifter or ſlower. And 'twill not be difficult to mark upon the Rod *q q*, the appropriated length of the Pendulum for the *Sun*, *Moon*, or *Stars*; but this only by the by.

If in the next place it be deſired, that the Hand of the Clock ſhould be alwaies carried round upon the face of the Clock, in the ſhadow of a Style perpendicular to that plain, by reaſon that the declination of the Sun daily varieth, the angles of the ſhadow about that Style varieth alſo, and conſequently the inclination of the plate of the Joynt to the Axis or Arbor muſt vary alſo, and that variation muſt alwaies be the ſame with the variation of the declination of the Sun, which is twenty waies mechanically performable in Clock-work, ſo that the motion ſhall be performed by the Clock-work alone, without touching it with the hand. All the other directions that are requiſite to adjuſt the Clock-work to ſuch a Dial, is, only to make the Arbor of the Clock-work to have the ſame inclination to the plain of the Dial, that the Axis of the Earth, or a line paralel to it hath; and rectifying the Hand into the true plain of the Axis, or Inclined-arbor, the equality of the motion of the Clock-work, according to the diurnal and annual motion of the Sun, we ſuppoſe alſo to be provided for.

If the Hand of the Clock be deſired to be moved in the ſhadow of any other ſtreight Style, howſoever inclined to the plain of the Dial, then muſt there be another Joynt like the former, added to the end of that Axis which was perpendicular to the plain of the Dial, and all the three Axes muſt be ſcituate in reſpect of the Plain, in which the Hand on the end of the laſt is to move, that the inclination of the ſaid Axes to each other, may repreſent the inclination of the Axis to the perpendicular axis of the Plain, and of that

that perpendicular Axis to the axis of the Style. Or, which is ſomewhat ſhorter, and may be made handſome enough, Let the two ends of the Hand repreſent the two points of the ſecond circular Plate or Globe, extended long enough to reach to the hour Circle, then let the axis of this ſecond Arm be placed in the axis of the inclined Style, and let the axis of equal motion, repreſenting the axis of the diurnal motion of the Earth, be placed with ſuch inclination to it, as the axis of the Earth hath to the oblique Axis or Style of the Dial, and the motion will be moſt exactly performed mechanically, and according to the truth of Geometry and Calculation.

Now, in all theſe motions, care muſt be taken, to provide that the inclination of the *declination* of the Sun from the Equinoctial, be expreſt by the ends 11, in the 22 and 23 *Figures* of the *ſecond Plate* of my *Animadverſions*, of the Croſs, taken hold of by the ſemicircular arms *c d*, upon the end of the firſt Axis; that is, that the ſaid arms may, by their revolution, make the line of the Croſs deſcribe ſuch a cone about the firſt Axis, as the motion of the Sun doth about the axis of the Earth, making the centre of the Earth the apex of that Cone; which will be done, if the ſaid ſemicircular Arms be moved, and ſet to the declination of the Sun for that day. Or, that an additional motion be added to the firſt Axis, that the Clock it ſelf may perform it. This may be done twenty waies eaſily enough, which I ſuppoſe will be ſufficiently obvious to any knowing Mechanick, and that without the help of Tooth-wheels or Pinions, which in works of this nature are in no wiſe to be made uſe of, by reaſon of their ſhaking and uncertainty, which I ſhall elſewhere deſcribe.

There is one only difficulty in this motion, and that is only in ſuch Objects as paſs over, or very near the Zenith or Nadir of the place, for in thoſe caſes, when the Object comes very near the Zenith, the obliquity of the motion of the one to the other is ſo very great, that the firſt Axis doth not move the ſecond without ſome difficulty: But to remedy this, the expedient is as eaſie, and that is, by having a little barrel about the perpendicular Arm, to carry it forward as far and as faſt as the firſt Inclined axis will permit it; which weight may

Appendix.

Concerning the Eclipſe of the Moon, *obſerved in* London.

J*Anuary* the firſt, 167$\frac{4}{5}$, being at Sr. *Jonas Mores* in the Tower of *London*, and making uſe of a Teleſcope of eight foot, and my pocket-Watch, whoſe ballance was regulated with ſprings, I obſerved the Eclipſe of the *Moon*, which began at about twenty minutes after five, the penumbra very much cheating the naked eye; for the Penumbra had darkned that ſide of the *Moon*, next the ſpot *Grimaldi*, about half an hour before, and grew darker and darker towards the edge where the *Umbra* entred, ſo that if the light of the *Moon* were diminiſhed either by reflection upon dark Glaſs, or looking through a ſmall hole, between a quarter and a third part of the *Moon* ſeemed eclipſed before the *Umbra* entred; but the Teleſcope diſcovered it plainly to be no true *umbra*, but *penumbra*.

This I note, becauſe ſuch Perſons as do not make uſe of a Teleſcope, but only of their naked eye, are very apt to be much deceived in their eſtimation of the beginning and end of the Eclipſe.

At 5. 48 we judged by the Teleſcope that the *Moon* was eclipſed ſix digits, or half; at 6. 19. the total Eclipſe began, when the Moon appeared of a very red colour, eſpecially towards that part of the Limb where the direct Raies left it, which was at the *Mare Criſium*, which is oppoſite to *Grimaldi*. Now the Skie being ſomewhat clearer, it being before hazy, with the Teleſcope I began to diſcover a great number of ſmall *Stars* about the *Moon*, which appeared yet much more conſpicuous, after I had taken off the apperture from the Object-glaſs, and amongſt the reſt, one ſeemed very conſpicuous, and lay in the way of the *Moon*, which I diligently watched and obſerved, that it was juſt covered by the *Moon* at $6^h. 47'. 30''$. the *Moon* firſt covering it with that part of it

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may be removed as ſoon as the Object is a little way paſt the Zenith.

The next uſe that may be made of this, is, for carrying the Hand of a Clock ſo, as alwaies to move over that point of the Ecliptick in which the Sun is, in a Stereographical projection of the Sphere upon the Plain of the Equinoctial, or in an Orthographical projection of the ſaid Sphere upon the ſame Plain, ſo as to expreſs thereby not only the differing right aſcenſions, but the anomaly alſo of the Suns motion in the excentrick of the Ecliptick. And by this means the Face of the Clock may be made by a Planiſpherical projection, to repreſent the motion of all the *Stars* appearing in any Horizon that is not too near the Equinoctial, their *Riſings*, *ſettings*, *culminatings*, *azimuths*, and *almicauters*; *Riſings* and *ſettings* of the *Sun*, the *lengths* of the *Days* and *Nights* and of the *Twilights* and *Dawnings*, and many other Problems of the Sphere. And, which is a conſequent of this, it may be made to ſhew the equation of Time, which is neceſſary to be made uſe of for ſetting a pendulum Clock by the Sun, the manner of doing which I muſt refer to another opportunity, as I muſt alſo the uſe of this Joynt, for *drawing Ellipſes*, *drilling* and *boring* of *bending Holes*, for *turning Elliptical* and *Swaſh-work*, till I publiſh my deſcription of a *Turning Engine*, capable to turn all manner of *Conical Lines*, and *Conoeidical*; all manner of *Foliage* and *Flower-work*, all variety of *Basket* or *Breaded-work*, all variety of *Spiral* and *Helical-work*, ſerving for the imitation of the various forms and carvings of all ſorts of *Shells*; for *cylindrical* and *conical Screws*; all variety of *Emboſſments* and *Statues*; all variety of *edged* and *Wheel-like work*; all variety of *Regularly ſhaped Bodies*, whether the five Regular bodies of *Plato*, or produced from thoſe by various ſections or additions, of which the variety is infinite; all variety of *bended Cylinders* or *Cones*, and thoſe whether round, in the manner of an *Oxes-horn*, or compreſſed and angular, like thoſe of a *Ram* or *Goat*; for all manner of *Swaſht-work*, *Compreſt-work*, *&c.* every of which principal parts hath a vaſt variety, and the compound and decompound principles have a variety almoſt infinite.

Appenwhich was almoſt perpendicularly under the centre of the *Moon*.

About three quarters of an hour after the total immerſion, the body of the *Moon* was exceeding dark, and almoſt unperceivable, being then near the centre of the *Umbra*, and afterwards the Eaſtermoſt or foremoſt part of the Limb of the *Moon* began to be inlightned, whereas before the Weſtermoſt Limb had been the brighteſt. This was alſo very notable, that that part of the *Moon* that was towards the North-Pole, a pretty while before the emerſion of the *Moon* out of the total Eclipſe, and even till the very emerſion, and ſomewhat after too, appeared inlightned with a much brisker light than any other part of the body, except that which was next the Limb where the light again entred. From what cauſe this ſhould happen, I know not; poſſibly it might be cauſed by a greater refraction of the Air near the North-Pole of the Earth, and I am much troubled, that I had not taken notice whether the like phenomenon had not happened to the body of the *Moon* before it had paſt the centre of the *Umbra*. It was very manifeſt, that there was a conſiderable quantity of light that kept that Limb of the *Moon* which was next the light, conſpicuous by the Teleſcope all the time of the total Eclipſe; and 'tis very rational to aſcribe it to the Raies of the Sun, refracted by the Air, or atmoſphere of the Earth.

I was very well pleaſed to obſerve the *Moon* to cover ſeveral ſmall *Stars* that lay in its way, but I kept no account of them, but only watched diligently when the *Star* that entred behind the *Moon* at 6. 47. 30. would come out again, which I found it to do at 7. 30'. ſeeing it at the very moment of time that it began to appear again. And it was alſo at the ſame inſtant diſcovered by Sr. *Jonas More*, who was expecting it with another Tube.

At 7. 58. the body of the *Moon* firſt emerged out of the *Umbra* at the ſpot *Grimaldi*, and ſoon after all thoſe ſmall *Stars* that were conſpicuous before about the body of the *Moon*, vaniſhed. However I had, before its firſt emerſion out of the ſhadow, taken a little draught of the ſmall Stars, according to their ſeveral poſitures and magnitudes, only by gueſs,

gueſs, that I might a week after, when the *Moon* was gone farther off, inquire what that Star was that had ſuffered ſo conſpicuous an Eclipſe, and that thereby I might the more certainly determine the true place of the *Sun* and *Moon* at that inſtant, which I found to be that in *Bayer*, touching the Ecliptick, in about 21°.40′. of *Cancer*. The *Umbra* ceaſed wholly at eight of the Clock and five minutes, though the *Penumbra* then poſſeſſed almoſt a third of the *Moons* Diameter, and laſted near half an hour after, before that ſide of the *Moon* was perfectly inlightened like the other.

There was one *Phenomenon* very remarkable, which I took more eſpecial notice of, as ſeeming to me very conſiderable for the determining that controverſie, whether the *Moon* have an atmoſphere or not, like that of the *Earth*? And that was, that after the *Moon* was entred wholly into the *Umbra* of the Earth, that part of the Limb of the *Moon* which was laſt enlightned, continued for a conſiderable while to have a very great brightneſs upon it, which extended on each ſide that part of the Limb, both northwards and ſouthwards, to about a quadrant of the *Moons* Limb, making a repreſentation almoſt of a New *Moon* about a day or two old, and as the body of the *Moon* was immerged deeper into the ſhadow, ſo this brightneſs or light grew fainter and fainter, but ſtill ſeemed to ſpread it ſelf very far upon the Limb of the *Moon* only, and not upon the body thereof. That which was ſpread into the body being much fainter and weaker, and ſeeming (as I before noted) to proceed from the refraction of the Atmoſpheres of the Earth. Nor was this only conſpicuous at the *Moons* entring into the total darkneſs, but as remarkable alſo at the *exitus* thereof out of the ſame, inſomuch that ſome of thoſe Perſons, who at the ſame time viewed the ſame with me, verily believed the *Moon* was not wholly eclipſed ſo ſoon as really it was, nor continued ſo long in that obſcurity, as very viſibly it did by the ſpace of two or three minutes. For I took eſpecial notice when this inlightning of the Limb began again to appear, and I obſerved its increaſe, and ſpreading about the Limb, till the very inſtant that the immediate light of the Sun touched the very extremity of the Limb it

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ſelf, which was indeed ſo very briskly bright and ſtrong, that it did not only ſoon make the other light diſappear, but alſo all the Teleſcopical Stars that were near to it, and towards the end alſo many of the more conſpicuous Stars, eſpecially ſuch as were not far from the body of the *Moon*.

Poſtſcript.

I Should have here taken leave of my Reader for this time, but that finding in the *Tranſactions* a paſſage inſerted out of the *French* Journal *de Scavans*, about the invention of applying a *Spring to the Ballance of a Watch, for the regulating the motion thereof*, without at all taking notice that this Invention was *firſt* found out by an *Engliſh*-man, and long *ſince publiſhed to the World*: I muſt beg the Readers patience, whilſt I, in vindication of my own right againſt ſome unhandſome proceedings, do acquaint him with the ſtate of this matter.

About *ſeventeen years ſince*, being very inquiſitive about the *regulating the meaſure of Time*, in order to *find the Longitude*, I did from an Art of Invention, or mechanical *Algebra* (which I was then Maſter of) find out and perfect this contrivance, both as to the Theory and Experimental verification thereof, of which I then diſcourſed to *divers of my Friends*, but concealed the *modus*.

About fifteen years ſince, to wit, in the year 1660, preſently after his Majeſties happy Reſtauration, I was in treaty with ſeveral Perſons of Honour (ſome of which are yet living, though *one* of them is *ſince dead*, but I have ſufficient evidence to produce in his own writing that he was one) for the diſcovery thereof, upon propoſed Articles of encouragement. This I can prove by *undeniable* Witneſſes yet living, and I have ſtill all the Papers, Articles, and Tranſactions of this matter by me, in their own hand-writing.

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In order to bring this Treaty to paſs, I was neceſſitated to diſcover ſomething of Invention about *meaſuring Time*, which was, this way of applying *Springs to the arbor of the Ballance of a Watch, for the regulating the vibrations thereof in all poſtures.* And this I did, to the end that I might gain ſomewhat of belief in thoſe Noble Perſons (with whom I was to treat) That I had ſomewhat more than ordinary, and was not one of the heard-of Pretenders to that Invention: which effect it had, and their Treaty with me had finally been concluded for ſeveral Thouſand pounds, had not the inſerting one Clauſe broke it off, which was, *That if after I had diſcovered my Inventions about the finding the Longitude by Watches, or otherwiſe (though in themſelves ſufficient) They, or any other Perſon ſhould find a way of improving my Principles, he or they ſhould have the benefit thereof, during the term of the Pattent, and not I.* To which Clauſe I could no waies agree, knowing 'twas eaſie to vary my Principles an hundred waies, and 'twas not improbable but that there might be made ſome addition of conveniency to what I ſhould at firſt diſcover, it being *facile Inventis addere.* And judging it moſt unreaſonable to be deprived of the benefit of my Inventions, in themſelves ſufficient, becauſe others might vary them, or any other ways improve them, of which it was very probable they would have no thought, if they had not the advantage of being inſtructed by my diſcovery, it having lain hid ſome thouſands of years already, as indeed the effect hath made evident and certain, there having been nothing done by any body elſe upon that matter for theſe fifteen years.

Upon this point our Treaty was broken off, and I concealed the farther diſcovery of any of the other more conſiderable parts of my Inventions, for the regulating of *Time-keepers*, as hoping I might find ſome better opportunity of publiſhing them together with my way of finding the *Longitude of Places*, for which I hoped to have had ſome benefit for all the labour, ſtudy, and charge I had been at for the perfecting thereof. Upon this I was told, *That I had better have then diſcovered all, ſince there were others that would find*

E 2 *it*

it out within ſix months; to which I anſwered, that *I would try them one ſeven years*; and it is now above twice ſeven, and I do not find it yet found out. Indeed Mr. *Hugens* hath made uſe of that part I diſcovered, and ſomewhat Mr. *Leibnitz* hath hit upon, but both of them are imperfect as I ſhall hereafter ſhew.

'Tis true, I was alarum'd by one of thoſe Perſons about two years after that, who told me, *That he had news that the* Longitude *was found out by a Perſon of Honour, by a way of carrying Mr.* Hugens'*s Pendulum-Clock, at Sea, by the help of a Ball and Socket, hung to the underſide of the Deck of a Ship.* Bnt having a deſcription of it, I preſently told that Perſon, That *that Invention would do mine no harm*; and indeed we experimentally found it uſeleſs to that effect not long after, upon a trial made of carrying the ſaid Clocks off to Sea in one of His Majeſties *Pleaſure-Boats,* in the year 1662.

The Invention indeed in it ſelf was ingenious, and did much more than what Mr. *Hugens* did expect, as I was then informed by the Right Honourable the Earl of *Kincardine,* the Author and perfecter of that part of the Invention. But wanting a little addition (which I concealed, and Mr. *Hugens* hath not got yet that I hear of) it failed of the effect that was expected. Notwithſtanding this, it was not long after publiſhed in *Low Dutch*, and preſently after in *Engliſh*; wherein what made for it was related, but what made againſt it was concealed, though they were both equally known.

But on the otherſide, all that I could obtain was a Catalogue of Difficulties, *firſt*, in the doing of it, *ſecondly*, in the bringing it into publick uſe, *thirdly*, in making advantage of it. Difficulties were propounded from the alteration of *Climates*, *Airs*, *heats* and *colds*, temperature of *Springs*, the nature of *Vibrations*, the wearing of *Materials*, the motion of the *Ship*, and divers others. Next, it would be difficult to bring it to uſe, for Sea-men knew their way already to any *Port*, and Men would not be at the unneceſſary charge of the *Apparatus*, and obſervations of the Time could not be well made at Sea, and they would no where be of uſe but in Eaſt and

and West *India* Voyages, which were so perfectly understood that every Common Sea-man almost knew how to Pilot a Ship thither. And as for making *benefit*, all People lost by such undertakings; much had been talkt about the *Præmiums* for the *Longitude*, but there was never any such thing, no King or State would ever give a farthing for it, and the like; All which I let pass.

At the earnest importunity of a Dear Friend of mine, since deceased, I did, in the year 1664, read several of my first *Cutlerian Lectures* upon that Subject, in the open Hall at *Gresham* Colledge, at which were present, besides a great number of the *Royal Society*, many Strangers unknown to me. I there shewed the ground and reason of that application of *Springs* to the *Ballance* of a Watch, for regulating its motion, and explained briefly the true nature and principle of *Springs*, to shew the Physical and Geometrical ground of them. And I explained above twenty several ways by which *Springs* might be applied to do the same thing, and how the *Vibrations* might be so regulated, as to make their Durations either all equal, or the greater slower or quicker than the less, and that in any proportion assigned. Some of these ways were applicable to lesser Vibrations, others to greater, as of 2, 3, 4, 5, 6. or what number of Revolutions were desired; the models of which I there produced, and I did at the same time shew wherein the aforesaid Sea-Clocks were defective.

All these particulars also were at several other times, at the Publick meetings of the *Royal Society*, discoursed, experimented, and several Models produced. I did also, at the earnest desire of some Friends, in the year 1664 and 1665, cause some of the said Watches to be made, though I was unwilling to add any of the better applications of the *Spring* to them, as waiting a better opportunity for my advantage.

Of all these things the Publisher of the *Transactions* was not ignorant, and I doubt not but Mr. *Hugens* hath had an account, at least he might have read so much of it in the History of the *Royal Society* as was enough to have given him notice

notice of it, for *page* 247 of that Hiſtory, amongſt other Experimented Inventions, there are recounted ſeveral new ways of *Pendulum Watches for the Pocket*, *wherein the motion is regulated by Springs, &c.* The account of the ſeveral ways was given *ſomewhat larger to the Learned Author of that excellent Hiſtory*, though he, as judging it more proper to his deſign, was pleaſed to give only this ſummary account. Mr. *Hugens* might therefore, if he had pleaſed, have mentioned the *firſt Inventer*, *Nam ingenuum eſt fateri*; as he might alſo that of the Circular Pendulum, which is mentioned in the ſame *page* of the aforeſaid Hiſtory.

But though he would not pleaſe to confeſs he knew my publiſhed Invention, yet *I am ſure* he hath manifeſted, that he knows *no more than what I had formerly diſcovered*, he having not in leaſt mentioned the othe Contrivance, which is the principal, and without which the firſt part of the Invention is but lame and imperfect, and doth but limp on one leg, and will ſome time hobble, and ſtumble, and ſtand ſtill. And the ſaid Watches will not be *tres Juſte*, nor ſhew the Longitude at Sea or Land, but, on the contrary, they will be ſubject to moſt Inequalities of motion and carriage, and with many of thoſe motions will be apt to ſtand ſtill, whatever to the contrary is affirmed in the *French Journal*, or in the *Engliſh Tranſactions*.

I forbear now to mention any further the carriage of the Writer of the *Tranſactions* in this Affair, and begging my Readers excuſe for this digreſſion, I ſhall conclude this Tract with a ſhort communication of the general ground of my Invention for *Pocket-Watches*, the number of particular ways being very great, which (that the true Lovers of Art, and they only may have the benefit of) I have ſet down in the *Univerſal* and *Real Character* of the late Reverend Prelate, my Honoured Friend Dr. *John Wilkins*, Lord Biſhop of *Cheſter*, deceaſed. In which I could wiſh, that all things of this nature were communicated, it being a Character and Language ſo truly Philoſophical, and ſo perfectly and thoroughly Methodical, that there ſeemeth to be nothing wanting to make it have the utmoſt perfection, and higheſt Idea of any

any Character or Language imaginable, as well for Philosophical as for common and constant use. And I have this further to desire of my Reader, who will be at the pains to decipher and understand this description, that he would only make use of it for his own information, and not communicate the explication thereof to any that hath not had the same curiosity with himself.

This I do, not so much to hinder the spreading of this Description here delivered, as to revive, and, if possible, bring into use and practice that excellent Design: It being a Character and Language perfectly free from all manner of ambiguity, and yet the most copious, expressive and significative of any thing or Notion imaginable, and, which recommends it most to common use, the most easie to be understood and learnt in the World. *See Table the third.*

To fill the vacancy of the ensuing page, I have here added a *decimate* of the *centesme* of the Inventions I intend to publish, though possibly not in the same order, but as I can get opportunity and leasure; most of which, I hope, will be as useful to Mankind, as they are yet unknown and new.

1. *A way of Regulating all sorts of* Watches *or* Time-keepers, *so as to make any way to equalize, if not exceed the* Pendulum-Clocks *now used.*

2. *The true Mathematical and Mechanichal form of all manner of* Arches *for Building, with the true butment necessary to each of them.* A Problem which no *Architectonick* Writer hath ever yet attempted, much less performed. abccc ddeeeee f gg iiiiiii llmmmmnnnnnooprr sssttttttuuuuuuuux.

3. *The true Theory of* Elasticity *or* Springiness, *and a particular Explication thereof in several Subjects in which it is to be found: And the way of computing the velocity of Bodies moved by them.* ceiiinosssttuu.

4. *A very plain and practical way of counterpoising Liquors, of great use in* Hydraulicks. Discovered.

5. *A new sort of Object-Glasses for* Telescopes *and* Microscopes, *much outdoing any yet used.* Discovered.

6. *A*

6. *A new* Selenoſcope, *eaſie enough to be made and uſed, whereby the ſmalleſt inequality of the Moons ſurface and limb may be moſt plainly diſtinguiſhed.* Diſcovered.

7. *A new ſort of* Horizontal Sayls *for a* Mill, *performing the moſt that any Horizontal Sayls of that bigneſs are capable of; and the various uſe of that principle on divers other occaſions.* Diſcovered.

8. *A new way of* Poſt-Charriot *for travelling far, without much wearying Horſe or Rider.* Diſcovered.

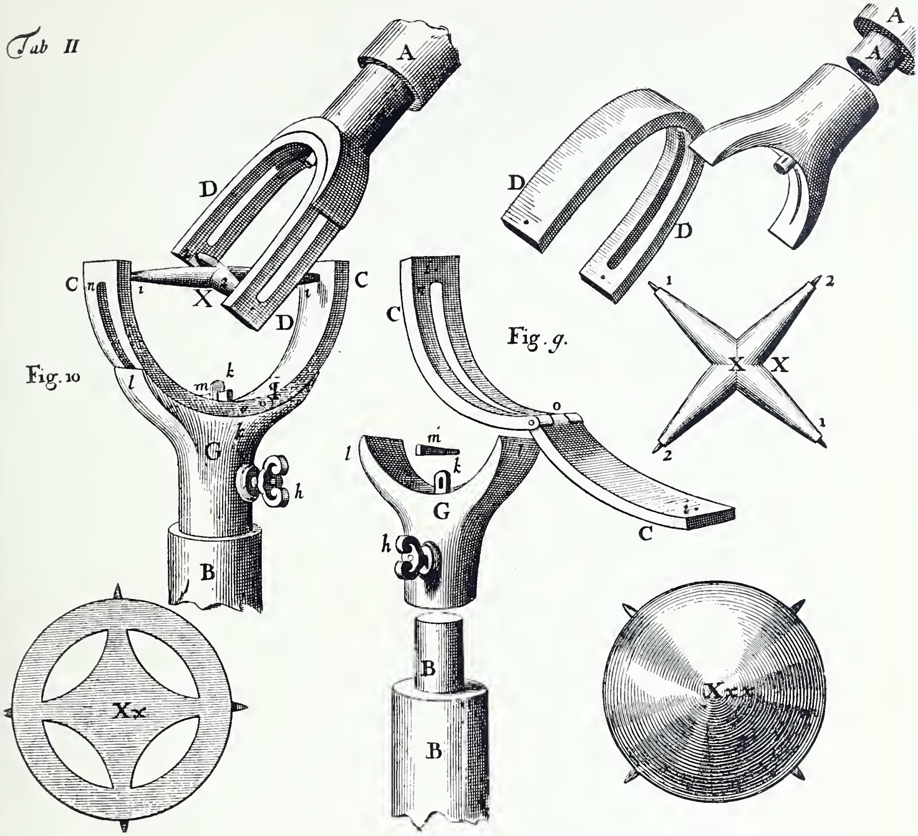
9. *A new ſort of* Philoſophical-Scales, *of great uſe in Experimental Philoſophy.* c d e i i n n o o p s s s t t u u.

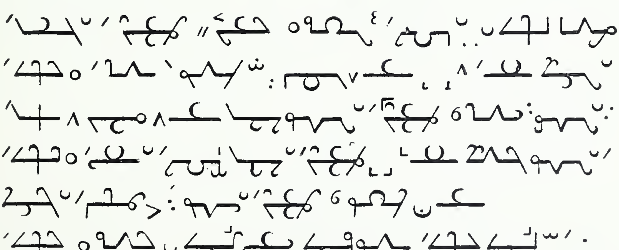
10. *A new Invention in* Mechanicks *of prodigious uſe, exceeding the* chimera's *of perpetual motions for ſeveral uſes.* a a a æ b c c d d e e e e e e g i i i l m m m n n o o p p q r r r r s t t t u u u u u.

a a e f f h i i i i l l n r r s s t u u.

F I N I S.

Tab II





Tabula.III

[LAMPAS]

LAMPAS:

OR,

DESCRIPTIONS

OF SOME

Mechanical Improvements

OF

Lamps & Waterpoiſes.

Together with ſome other

PHYSICAL and MECHANICAL
DISCOVERIES.

MADE BY

ROBERT HOOKE,
Fellow of the Royal Society.

LONDON,
Printed for John Martyn, *Printer to the* Royal Society, *a*
the Bell *in St.* Paul's *Church-yard.* 1677.

LAMPAS:

OR, A

DESCRIPTION

OF SOME

Mechanical Improvements

OF

LAMPS.

THe Hypotheſis of Fire and Flame I did about eleven years ſince publiſh in the 16. Obſervation *Pag.* 103, 104, and 105. of my *Micrographia*, which hath ſo far obtained, that many Authors have ſince made uſe of it, and aſſerted it; nor have I yet met with one conſiderable objection againſt it. It ſhall not therefore be my buſineſs at preſent to diſcourſe of, or farther explain that Theory, which any one upon a ſtrict inquiry into, I queſtion not, will find cauſe ſufficient to confirm him in, but rather to mention ſome pleaſant and beneficial uſes thereof, and to hint ſome Mechanical contrivances for the ſupplying the *Pabulum* Oyl or Spirit by the ſame Degrees by which it is conſumed in the flame of a Lamp, that great diſſolvent.

I do not here deſign to ſhew a way how to make a perpetual Lamp, that being a Chimera which my Hypotheſis of flame doth ſeem to deſtroy, for the diſſolvend muſt in time be diſſolved: But to ſhew a way how to make

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the Receptacle of a Lamp in ſuch manner as that it ſhall continue to ſupply the *Pabulum* to the flame equally and for a very long time till it be all conſumed. The conſideration of which Problem firſt put me upon the enquiry after a counterpoiſe for Liquors or Fluids, which is alſo of very great uſe in Hydraulicks, as I ſhall hereafter have occaſion to manifeſt.

This I can do by very many contrivances, depending from very differing Principles, all and every of which may be fitted ſo as to ſupply the Oyl or *Pabulum* of the Lamp in ſuch quantity, and after ſuch manner and proportion as ſhall be deſired. I ſhall now omit all the other ways of performing this effect, though divers of them are as much or more conſiderable than any of theſe I here mention. And having promiſed in the 32 Page of my deſcription ofŚlioſcopes to publiſh a Counterpoiſe for Liquors, I ſhall only explain ſeveral ways by the help of theſe Counterpoiſes to do whatſoever can be required, as to the manner and quantity of ſupplying Oyl to the flame.

The chief deſign of the Counterpoiſe in this inquiſition is to keep the Superficies of the Liquor (whether Oyl, Spirit of Wine, Oyl of *Turpentine*, or the like) whatever quantity there be in the Veſſel, always to the ſame height, ſo that the ſaid *Pabulum* ſhall always be equally diſtant from the bottom of the flame, and the Wick or flame being once placed at a convenient height or diſtance above the Superficies of the Oyl, ſhall not be deſerted by the ſaid Superficies till the whole quantity be conſumed; but it is as eaſie to contrive it, to ſupply it by decreaſing or increaſing degrees, which are conveniences that none of all the Lamps I have ever yet met with have had, that was tolerable for uſe. The moſt ingenious is that which is commonly known by the name of *Cardans* Lamp, as being publiſhed and very probably invented by *Cardan*, which doth in ſome manner ſupply the waſting and decay of the Oyl cauſed by the flames Conſumption. But then it is ſubject to a great many inconveniences,

veniences, which make it intollerable and difuſed : The firſt is, though it doth ſupply the defects of the Oyl to the Wick, yet it doth it not conſtantly and equally, but by ſtarts and gluts; for after the receptacle by the Wick is filled, the Superficies of the Oyl continues to ſink by degrees a conſiderable ſpace below the flame, before there be any more ſupply added from the great Magazine or Repoſitory, and till the Air can break in, (which it doth very unequally) ſo that there ſometimes comes down ſo great a quantity that the receptacle is over-filled, and the flame extinguiſhed, and theſe gluts are more unequal the bigger the Magazine be in proportion to the Receptacle by the flame, and the more the quantity of the Oyl be that is ſuſpended, and the more the Air ſpace be above the Oyl, and the more tenacious or ſluggiſh the conſtitution of the Oyl is.

The ſecond inconvenience of *Cardans* Lamp is that the Air is apt to rarifie it with heat, ſo as ſometimes to drive down ſo much Oyl as to overflow the receptacle, and choak the flame.

The third Inconvenience is, that the Wick by the ſinking of the oyl doth ſooner decay the flame, being ſometimes a little higher and ſometimes lower upon the Wick; for if the Wick riſe up into the hollow dead part of the Cone of the flame, the ſtreams and coals of the Oyl will be ſo caked together as to dead the flame and much to diminiſh the light and heat thereof, whereas if the Wick be but ſhort, and ſuffered only to go but a very little within the under-Superficies of the flame, it will not be ſo ſtopped and caked with thoſe feculencies. The reaſon of which is evident, for the flame, as I formerly proved, being nothing but the parts of the Oyl rarified and raiſed by heat into the form of a vapour, ſmoak, or ſteam, the free Air that incompaſſeth this ſteam keepeth it into a Cylindrical form, and by its diſſolving property preyeth upon or diſſolveth thoſe parts of it that are outwards and next to the Air, ſo as by the ſaid diſſolution it continueth the heat, and produceth the light which we obſerve; but

B 2 thoſe

thoſe parts of the body of ſteams that riſe from the Wick, which are in the middle, and not contiguous to the outward Air, are not diſſolved or turned into ſhining flame by the Air till they riſe towards the top of the Cone of flame where the free Air can come to reach, and ſo to diſſolve them, and thence gathering about the Wick in the Center of the Cone of flame they choak, clog, and quite ſtifle it that the flame will quickly go out. That this is ſo, any one may eaſily find if he examine the flame of a Lamp or Candle by the help of a piece of glaſs: For by the tranſparency thereof he will plainly perceive that all the middle of the Cone of flame neither ſhines nor burns but only the outward Superficies thereof that is contiguous to the free and unſatiated Air, and that the middle parts may be collected in the form of Soot, or very fine powdered coal duſt.

Take then a piece of Glaſs, whether Window-Glaſs, Looking-glaſs Plate, or the ſide of a Viol, it matters not, or, which is beſt of all, a thin Plate of *Selenitis* or *Muſcovia* Talk, and hold it Horizontally in the middle of the flame, ſo as to cut off the top or upper part of the Cone thereof, then preſently, before it be choaked with ſoot, look down upon it, and you ſhall plainly ſee that all the middle parts of the Flame and the Wick have no ſhining power or light at all; nor are they diſſolved by the Air, but remain in the form of Soot, but that only the Superficies or outſide of the ſaid Cone doth burn, ſhine, and conſume into and mix with the ambient Air.

In the ſame manner, if you hold the Glaſs or Selenitis perpendicularly, and apply the ſide of it ſo as to cut the flame *per axin coni*, that the Air cannot come to one ſide thereof, you may plainly perceive that the ſhining part of the flame is only that which is contiguous to, and preyed upon by the free and unſatiated Air, and that where that Air cannot come free without being glutted and ſatiated in its way, there neither the conſumption of the Oyl, nor the heat and light of the flame is produced, but only a ſooty, choaking, and ſtifling ſubſtance.

To

To make then the reaſon of the Phænomena obſervable about the laſting or ſtifling of the flame of a Lamp the more clear and eaſie to be underſtood and comprehended, give me leave to explain the manner of its production and continuation by a Scheme, delineation, and deſcription thereof.

Let A A then in the ſecond Table repreſent a body of Oyl, or any other combuſtible fluid ſubſtance, the Superficies whereof B B is Horizontal, and pretty near plain. [I ſay, pretty near, becauſe it is always either Concave, or Convex, more or leſs according to ſeveral circumſtances; to wit, the capacity and the nature of the Veſſel E E, in which it is contained; for if the Veſſel be ſmall, and that the Oyl hath a greater congruity with it than the Air, the Superficies of the Oyl will be very much concavated eſpecially towards the ſides of the Veſſel as at C C; but if the Veſſel be incongruous to Oyl, the Superficies will be Convex as at D D, the reaſon of which I have long ſince explained in another place.]

Let F F then in the third figure repreſent the Wick, which conſiſts of a great number of very fine Cylinders or hairs of Cotton f f f twiſted and laid very cloſe together, into, and between which the Oyl (having a very great congruity therewith) doth readily inſinuate it ſelf and adhere, and is by the preſſure of the Air (much greater without than between thoſe Cylinders or hairs) forced up to a conſiderable height between them, (as to the height of an inch and half, or two Inches) and if by any means the Oyl be taken out at the top thereof, the remaining part of the Oyl in the Veſſel will aſcend to ſupply the vacancy of the part drawn off, which is evident in Filtration. About the ſides of this Wick the Oyl will be ſure to aſcend, and the Superficies thereof will be concavated as at G G, becauſe unleſs there be a congruity between the Oyl and the Wick there will be no aſcent of the Oyl therein, and therefore that ſubſtance that the Oyl doth not readily adhere to cannot be a fit material for that purpoſe

Now

Now to this Wick thus filled with Oyl apply the flame of a Lamp or Candle, or any other ſubſtance extremely hot, as a glowing piece of Iron, Copper, or the like, and by this means the parts of the Oyl in the Wick will be very much heated, and expand themſelves in vapours into the contiguous Air by the ſteams h h h h h, and fill all the Ambient ſpace of the Air H H therewith, which vapours being very much rarified, and conſequently lighter than the incompaſſing Air, are by the greater gravity and preſſure thereof carried upwards by the Curve Lines h i k. Theſe at firſt guſh out of the Wick at Right Angles, but by the protruſion of the Air are quickly turned into a kind of Parabolick Curve h i k The motion of the Particles in which is ſwifteſt in k k, that is to a certain degree of Altitude. The motion of aſcent increaſing ſomewhat after the nature of the motion of deſcent in heavy bodies, I ſay ſomewhat in that nature, for if the aſcending bodies were uniformly lighter than the Ambient they would be the ſame, but becauſe the rarefaction and nature of them is varied by Circumſtances, therefore it hath but part of that Analogy.

To proceed then with the Explication: I ſay, theſe ſteams of the Oyl thus aſcending, if they are heated to a ſufficient degree of heat are preyed upon, and diſſolved or burned by the Ambient Air; which diſſolution hath this effect, firſt, that it produceth light; next, that it produceth heat enough to make the ſucceeding parts of the ſteams that ruſh out of the Wick and follow after it to be ſufficiently heated for diſſolution by the Air, the heat of which produceth the ſame operation upon a third, and that upon a fourth, and that upon a fifth, and ſo ſucceſſively ſo long as there are ſteams of Oyl to be diſſolved, and plenty of freſh and unſatiated Air to diſſolve. The action alſo of this diſſolution cauſeth heat ſufficient to raiſe up the ſucceeding parts of the Oyl into the Wick, and expand them into vapours, and ſo to make them fit to be further heated and diſſolved. It is further obſervable in the flame of a Lamp, that thoſe vapours that

that iſſue out of the Wick are by degrees diſſolved, and not all in a moment, for the parts of the flame that are lowermoſt about H have a kind of faint blew light until they come to I, where they ſeem to have their brighteſt and cleareſt light and heat, the ſaid vapours not being heated to that degree at their firſt breaking out that they afterwards acquire by the farther action of the Air upon them. At I they ſeem to be in their higheſt degree of diſſolution, and from thence upwards are made one with the diſſolving Air, ſo that they are not but by other means diſcernable to the eye of the obſerver; ſo that the ſhining part of this Conical ſhaped ſpace of the flame is only the outſide of the Cone, it being that part where the Ambient Air preys upon the aſcending eruptions of the Oyl, namely, where the Chain of ſmall Circles intercept the Curve lines of the motion of the aſcending eruptions.

This Figure and ſhape of the flame and vapours may be plainly ſeen by the help of a Metalline Concave placed at a certain diſtance and Poſition, and alſo by obſerving the ſhadow of the Candle caſt by the beams of the Sun upon a ſheet of white Paper, or white Wall, but that way of a Concave *ſpeculum* is incomparably beyond it, becauſe it doth ſo very plainly ſhew the form and manner of the ſteams riſing above i i i i, as about k k k k, &c.

The Air after it hath performed the action of Diſſolution, and is ſatiated and incorporated with the parts of the Oyl at i i i, aſcend by k k k, but ſhine not. All the ſteams or eruption of the vapours of the Oyl out of the Wick f f f ſhine not between the Wick f f and i i, but begin to be diſſolved, and to ſhine as they approach the freſh Air at ii, where the diſſolution is compleated.

The upper parts of the flame ſhine more than the lower, the parts having been heated to a much greater degree by the longer ſpace of paſſage they have had through the hot Concave part of the flame, and contiguous or very near to the glowing ſides thereof at i i i.

All the under parts of the Wick neither ſhine nor burn,

burn, but are as it were charkd by the extremity of the heat of the Conical Superficies of the flame, they are defended from burning at the bottom by the fresh access of new Oyl from the Vessel underneath; and the middle parts are defended from burning or shining by reason the Air cannot approach them before it be satiated at the Conical Superficies i i i by the dissolution of the steams of the Oyl it there meeteth with. But the upper parts of the Wick do burn and shine, if they be high enough, into the smaller part of the Cone of flame that the Air before it be satiated can reach at them. And if any part of the Wick fall into the said Conical and shining Superficies of the flame, it doth both shine and consume, and suffers the same dissolution into the Air as the steams of the Oyl, and if any part of this Wick be without this Conical Superficies at i i i, it is presently consumed and reduced to Ashes; as by many experiments differing ways made is very plainly visible.

This plainly gives the cause why knots and Tophus's do as it were grow to the Wick of the Lamp like so many Mushrooms on a rotten Tree, which as soon as they are removed out of the middle and dead part of the flame are immediately consumed by, and dissolved into the Air, and shine like a coal of fire, as being indeed nothing else.

Hence we may give a plain Reason why upon applying any cool Superficies very low into the flame of a Lamp, there is immediately condensed upon it a great quantity of soot, namely, that the middle parts of the Cone of flame, being nothing but a great number of oyly steams ascending, are not fired nor consumed by the Air, till they can come to be wrought upon by the free and unsatiated Air. Now if the Air be so intercepted that it cannot come at them, and the steams be cooled by the plates coldness that the Air is not able to prey upon or dissolve them for want of a preparatory heat sufficient, they must remain in the form of burnt Oyl, or Lamp-black.

I have been somewhat the longer and more particular in in this deſcription and explanation of my Theory of the flame of a Lamp or Candle, that ſo the Reader underſtanding the nature and cauſes thereof the more fully and plainly, he may the eaſier diſcover the inconveniences that may occur in the burning, heating, ſhining, duration, *&c.* thereof, and the ſooner and more readily and ſcientifically find a cure and prevention of thoſe inconveniences, which he that is ignorant of can but hoodwinked grope after, and at beſt can but hope poſſibly after long puzling himſelf in vain attempts and blind trials, nothing to the purpoſe, he may at length ſtumble upon that which had he been inlightned by the true Theory, he would have readily gone to at the firſt glance.

I could have further expatiated into the contemplation of this moſt admirable Phenomenon of flame, producing heat and light, the two moſt ſpirituous and moſt potent Agents in Nature, and the ways of Intending and Diminiſhing them, and the uſes that may be made of them, but that it is not my preſent deſign to annex a diſcourſe on thoſe ſubjects, which doth more properly belong to another Lecture I ſhall ſhortly publiſh. I ſhall therefore at preſent proceed only to ſhew ſome Mechanical contrivances for counterpoiſing Liquors in Veſſels, ſo as to keep them running or ſupplying a ſtream always with equal ſwiftneſs, whatever quantity there be of the ſaid Fluid; which as they are very convenient for perfecting Lamps for divers uſes, which they could not otherwiſe perform, ſo in Hydraulick they are of moſt admirable benefit for divers effects, hardly to be performed without them, as I ſhall hereafter manifeſt. But firſt, I will explain ſome few ways by which more conveniences may be obtained, and more inconveniences prevented in the uſe of Lamps for Chymical, Mechanical, and Philoſophical uſes than by this way of *Cardan*, or any other I have met with: For this I look upon as one of the Tools to be made uſe of in the Work-houſe or Elaboratory of Nature, without a good Apparatus of which, be the Workman otherwiſe

C never

never ſo well accompliſhed, he will never be able to produce any very conſiderable effect; and with them, even a Bungler otherwiſe, will, if well furniſhed, do wonders to ſuch as know not the means by which they are done.

It may poſſibly ſeem very ſtrange to ſome to hear, that by the flame of a Lamp Plants may be made to grow, bear Leaves, blow Flowers, ripen Seeds; that the Eggs of Fowls and Inſects may be hatched, and brought to life and perfection; that Metals, even the hardeſt, Glaſs, Stones, *&c.* may be almoſt in a moment melted, ſoftned, liquified, hardned, *&c.* that thouſands of ſeparations of conjoyned and naturally united bodies may be effected, and they reſerved diſtinct; and as many other bodies, naturally diſtinct, and very differing, may be united and compounded into Homogeneous mixtures, ſome ſcarce ſeparable afterwards; that Glaſs may be ſhaped and moulded like Wax; that almoſt all the ſenſible qualities of bodies may be increaſed, diminiſhed, annihilated, and created; and ſome alſo of the qualities inſenſible (otherwiſe than by the effects;) and yet even theſe, and many more, may be effected by this Tool or Inſtrument, if rightly uſed, as I could manifeſt if I had now time. But I ſhall not here any further expatiate on it, poſſibly I may hereafter but at preſent I ſhall only proceed to the deſcription of one ſort of thoſe Inſtruments which ſerve to ſupply the Oyl or *Pabulum* of a Lamp conveniently by any degrees, and in what quantity is deſired. This ſort doth depend upon ſome contrivance of Counterpoiſes for the Liquor in the Receptacle that is to feed the Lamp, and may be made uſe of in Hydralicks as well as Lamps to feed and continue any running ſtream any time deſired.

Theſe Counterpoiſes then of Fluids might be made to feed the flame of a Lamp equally for any time aſſigned, and conſequently would make a kind of Perpetual Lamp, but the *Pabulum* it ſelf will be ſome ways or other unapt for ſuch an effect; as Oyl hath a foulneſs whereby the Wick

Wick is choaked or ſtopped, ſo as that it will no longer aſcend in it; Spirit of Wine will in length of time evaporate and loſe much of its nature; and other Oyls have their ſeveral defects which make them uncapable of continuing the flame very long. But there are none of theſe that I have met with but may be in great meaſure avoided by the help of ſome Chymical or Mechanical contrivances, ſome inſtances whereof I ſhall hereafter give, which the Theory of Fire and Flame doth readily hint.

The firſt way then I ſhall now deſcribe is by a round Box, the inward Cavity of which is divided by a Diaphragm into two equal parts, and fitted with a proper Counterpoiſe, the Axis of whoſe motion lieth Horizontally. The contrivance of which will be more plainly underſtood by the Delineation thereof in the firſt place, where the ſecond Figure repreſents the whole Inſtrument, with its Globe, Frame, Pedeſtal, Socket, and lighted Lamp.

A repreſents the Pedeſtal or foot upon which the Inſtrument ſtands, which may be made of Silver, Braſs, Wood, or the like. B C D E F, the Frame faſtned to the Pedeſtal, and ſhaped in the form of a Snake, perforated at B and D to receive the Pivots or Gudgeons of the Lamp G H, and hollow from E to F to ſerve to convey the Oyl or Spirit of Wine from the end of the hollow Gudgeon H to the Wick I, to feed the Flame K; the hole at E to receive the end of the hollow Gudgeon; H is made a little tapering, and the end of the Gudgeon H is ground fit into it, ſo as to turn eaſily, and yet ſo true, as not to let any Oyl there leak out, the ſaid Gudgeon being kept cloſe home by the ſpringing of the Arm B; the Superficies of the Oyl or Spirit for the *Pabulum* is always kept by the motion of the ſaid Globe upon its Axis G H, exactly in the Line L M, untill it be all conſumed, which how it is done will be better conceived by ſhewing the contrivance of the inſide of the aforeſaid Globe, how the ſame is divided, how filled, and how counterpoiſed.

C 2 Suppoſe

Suppoſe then the aforeſaid Globe cut in ſunder by the middle Line or Circle N O, and diſcovering the Inſide or Cavity thereof to be repreſented in the firſt Figu re, where P A H R Z P repreſents the aforeſaid Circle, or half ſhell of the Globe ; O repreſents the middle of the hollow Gudgeon H, which is the Pole or Axis about which the ſaid Globe doth move. H O Z repreſents the Horizontal Line or Plain paſſing through the aforeſaid Axis ; P R the Perpendicular to that Plain. Let H Z then repreſent a Diaphragm or Partition of the ſame material with the Globe, by which the Concavity thereof is divided into an upper Hemiſphere H P Z O H, and into an under Hemiſphere H R Z O H. Let the under Hemiſphere be filled with Oyl, Spirit of Wine, *&c.* or the like fit material for a Lamp to burn ; and let the upper part be filled with ſome material of half the weight of the Oyl, Spirit, or other material, or becauſe that will be ſomewhat difficult to do, let there be a counterpoiſe of Lead or other ponderous matter fixed ſomewhere in the Line P O, ſo that the ſaid upper Hemiſphere ſhall have half the gravity of the under Hemiſphere upon the Center of motion O. I ſay, whatever quantity of the Fluid *Pabulum* is in the Cavity of the ſaid under Hemiſphere, the Superficies thereof ſhall always be in the Horizontal Line or Plain O Z, the counterpoiſed upper Hemiſphere keeping it always up to that height. For inſtance, ſuppoſing the ſaid Hemiſphere full, there is no doubt but that the under Hemiſphere being double the weight of the uppermoſt will be lowermoſt, and that Horizontal Line will lie Horizontally, ſince it is evident that the Center of gravity of the whole will be below the Center of motion O, and ſomewhere in the Line O R, which is Perpendicular to the aforeſaid Plain. Next, ſuppoſe ſo much of the aforeſaid Liquid *Pabulum* conſumed as to leave enough only to fill the ſpace C O Z B R C, and the Diaphragm be moved from its Horizontal Poſition H Z, and placed in the Oblique Poſition C O D. I ſay, the ſaid upper Hemiſphere C H A P D O C

CHAPDOC ſhall exactly counterpoiſe the ſaid under Hemiſphere CRBZDOC, ſo as the Superficies of Liquor ſhall be in the Horizontal Plain OZ. Make AP equal to PD, and draw the Line AOB through the Center O, it is manifeſt then that the Wedge COR of the Liquor doth counterpoiſe the Wedge ROB on the other ſide the Perpendicular, and that the Wedge POD of the upper Hemiſphere doth counterpoiſe the Wedge POA on the other ſide of the Perpendicular, ſo that neither of theſe have any prepollency to move the Globe out of this Poſture. Next, it is plain that the Wedge BOZ of the Liquor will be counterpoiſed by the Wedge AOC, which is double the bigneſs of BOZ, and conſequently of equal weight, the parts of the upper Hemiſphere being put of half the gravity or weight of the under Hemiſphere.

Next, ſuppoſe half the Oyl be conſumed, and there be only left enough to fill the quadrantal Wedge ZOR, I ſay, the Superficies thereof ſhall be in the Horizontal Line OZ; for ſince the upper Hemiſphere is half the weight of the under, the two quadrantal Wedges POH and HOR muſt neceſſarily counterpoiſe the quadrantal Wedge ROZ of the Oyl.

Thirdly, Suppoſe that more than half the ſaid Oyl or liquid *Pabulum* be conſumed, and that there be only left enough to fill the Wedge BOZ, I ſay, the counterpoiſing upper Hemiſphere now made the under, and placed in the Poſition AHCRBOA ſhall exactly counterpoiſe the ſaid Wedge of Liquor, ſo as that the Superficies thereof ſhall be in the Line OZ; for the Wedge ROB of the aforeſaid upper Hemiſphere doth counterpoiſe the Wedge COR on the other ſide of the Perpendicular, and the double Wedge AOH and HOC will counterpoiſe the Wedge BOZ.

Nor can the Superficies of the Liquor be any whit higher or lower than the Line OZ, for if it be any whit higher as at EF, the Liquor muſt neceſſarily overpoiſe the aforeſaid Wedge AOC, by all the weight of the

Liquor

Liquor contained in FGOZF. And if it be any whit lower as at IK, the Wedge KIB muſt be too light for the counterpoiſing Wedge AOC by the weight of the Liquor contained in the ſpace ZOTKZ, ſince I juſt now ſhewed that AOC did juſt counterpoiſe ZOB, which was the thing to be proved.

Now though in this Inſtance I have choſen to explicate I have made choice of a Globe, yet that form is not neceſſary, but it may be made of any Figure whatſoever that is turned upon an Axis or Poles, ſo as whereſoever the ſaid Figure be cut by a Plain to which the Axis is Perpendicular, the Superficies of the ſaid Figure ſhall deſcribe a Circle, the Center whereof is in the ſaid Axis, whether the ſaid Figure be a Cylinder Cone, or any other Conoeidical, mixt, or otherwiſe, regular, or irregular figure. Such as the Figures ABCDEFG, which repreſent the Section of the ſaid Veſſel through the Axis.

The ſecond way for the poyſing the Liquor, and keeping the Superficies thereof always to an equal height, is this :

Make a Concave Receptacle for the Oyl or Liquor of a Hemiſpherical, Semicylindrical, Semiconical, or of any other half-round hollow Figure, where the turned Figure is cut in two parts *per Axin*, and whereof the Axis is placed Horizontal, and the plain Section *per Axin* likewiſe Horizontally, ſo as it may be filled with any Liquor up to that Plain; and that the Liquor may not be apt to daſh, be ſhaken, or filter over, it will be convenient to extend the brims of that Receptacle ſomewhat above the half-Round, that there may be about half or three quarters of an Inch of ſpace above the Superficies of the Oyl vacant or empty. And that upon whatever Plain the foot ſtand, the Plain *per Axin* may ſtand Horizontal, it will be good to ſuſpend the Receptacle in the ſame manner as a Sea-mans Compaſs is ſuſpended, within a frame: Fix

Fix this Receptacle, or the Frame that is to keep the Receptacle, Horizontal upon a convenient Pedeſtal; and fit within the Hollow or Concavity of the Receptacle a half-round ſolid poiſe, turned of the ſame form with the hollow of the Receptacle, and cut exactly through the Axis in two equal parts. Let this ſolid poiſe be made exactly half the weight of the Liquor that is to be poiſed, and fit to it two Pivots or Pins at each end of the Axis, which may be exactly in the Poles of the half-Round, and fit to thoſe Pins make two holes in the Centers of the Ends of the Concave Receptacle, in which the Pins may freely move, and ſuffer the half-Round poiſe to move round within the hollow of the Receptacle, according as the quantity of the Oyl or Liquor is increaſed or diminiſhed. Fit to this Receptacle a neck and ſocket fit for the Wick and flame of the Lamp, and the ſame operation will be performed by this as by the firſt contrivance; to wit, the Oyl will be kept always to the ſame height in the Receptacle.

This will be eaſier underſtood by explaining a Deſignation thereof which is ſhadowed forth in the fourth Figure: Where

A A A repreſents a Pedeſtal, which may be made with three claws or toes to make it ſtand the ſteadier and evenner upon any Plain or Table.

B B repreſent one of the Semicircular Arms that are fix'd to the top of the Pedeſtal, this hath two holes in it at the ends or extremities, as at C is one, the other hole being in the other arm which goes behind the Globe, and therefore cannot be ſeen, is ſuppoſed to be Diametrically oppoſite to this at C. Theſe two holes are the Center holes in which two ſmall Pins or Centers, faſtned into two oppoſite points of the Hoop or Frame are made fit to move, by which means the ſaid Hoop is preſerved in an horizontal Poſition.

D D is this Hoop or Frame, which is made to incompaſs the Veſſel or Receptacle of the Oyl, and is ſhaped exactly like it. This is made ſtrong enough of Braſs, Iron,

Iron, Silver, or other material to bear the Receptacle, Poiſe and Oyl without bending, and hath, as I ſaid before, two Pins or Gudgeons at C, and oppoſite to it Diametrically, or Semicircularly, upon which the ſaid Hoop always hangeth Horizontally. It hath alſo on each ſide in the middle between the aforeſaid Pivots, two Centers as at F and E to receive the ends of the Axis of the Receptacle appearing at F and E, by which the ſaid Receptacle is always free to hang plumb or in its Perpendicularity, ſo as that the upper edge thereof at F F will always lie Horizontally.

One of theſe Pivots, namely, that on the Right hand is the Pipe to convey the Oyl to the Socket of the Lamp I, in which is fitted a Wick of Cotton to ſerve for the flame, K G G repreſents the Veſſel or Receptacle of Oyl, which is here deſcribed Hemiſpherical, that being the moſt capacious uniform Figure, but may be of any other, qualified as thoſe I mentioned in the firſt contrivance. The Brims of this are extended ſomewhat higher than a Semicircle, namely, to F F, to keep the Oyl from flaſhing or filtring over. This is always kept full with Oyl or other Liquor to the Horizontal prick'd Line L L, which paſſeth through the Center or Axis of its Cavity by the Counterpoiſe moved on the Center C.

H H H repreſents that Counterpoiſe which is made exactly half the weight of the Oyl or Liquor, and the Center of gravity of it muſt be ſomewhere in the Line M M; and it ought to be fitted as exactly into the hollow of the Receptacle as it is poſſible, that there may be left as little ſpace as may be between its convex ſides and the Concave of the Receptacle, but yet ſo much muſt be left that it may move very freely upon its Center C a whole Semicircle. This done, and the Receptacle being filled with Oyl, the ſame effect will follow as in the firſt contrivance, and the Demonſtration of it being much the ſame, I ſhall not now ſpend time to explain it. But rather proceed to the deſcription of a third way of keeping the Liquor counterpoiſed to the ſame level.

The

The third way then is:

Take any round Veſſel, whoſe Concavity and Convexity is turned upon an Axis, and ſuſpend that Veſſel upon two ſmall Pivots (but yet big enough to bear the ſaid Veſſel filled with Oyl, *&c.*) faſtned in the Poles of that Axis; and leave or cut open a ſixth part more or leſs as you pleaſe of the ſide thereof, that thereby any thing may be put into or taken out of the Cavity of the Veſſel; then poiſe the Veſſel exactly on thoſe Centers, that no ſide be heavier than the other; then fit into it a float of Braſs, Silver, Tin, Lead, *&c.* Convex on the under ſide, ſo as juſt to fill to the Cavity of the Veſſel. And on the upper ſide, Plain, or Convex, or any other convenient Figure, it matters not much. Make this float as heavy as you can at the bottom, and as light as may be at the top, but yet of ſuch weight as may well float upon the top of the Oyl, *&c.* Let one end of this be faſtned by a wire or ſtring, ſo as that end thereof may always touch that point of the Concave of the Veſſel to which it is tied, and that the reſt thereof may turn and follow the ſinking of the Oyl; and through the end of it, near the place where it is faſtned, let a Pipe go through it to receive the Wick, which Pipe hath no communication with the Cavity of the hollow float. This done, fill the Veſſel as full as convenient with Oyl, and light the Wick, and you ſhall find that as the fire conſumeth the Oyl, the Veſſel will turn upon its Poles and keep the Superficies of the Oyl always at the ſame diſtance from the flame that it was put at at firſt till the whole be conſumed.

This will be made more conceivable by a figure and explanation thereof, which therefore take as follows in the fifth figure.

A C B B repreſents a hollow Veſſel, the Cavity whereof is very exactly turned upon an Axis whoſe Poles are in P, the ſpace between A and B in the ſide thereof is left open into the Cavity of it. This Veſſel is ſuſpended

D upon

upon its Poles at P, ſo as to be free to move round upon them, and exactly poiſed as no one ſide thereof be heavier than another. To the hollow of this Veſſel is fitted a float D of Braſs, Latton, Silver, Lead, &c. whoſe underſide is made of a Convexity juſt fit for the Concavity of the Veſſel, as may be ſeen at K D I, and the upper ſtraight or Plain. Let this float be made ſomewhat lighter than the Oyl or Liquor on which it is to ſwim, ſo that a part thereof may float above the Superficies thereof. Let one end thereof E be faſtned to the ſide of the Veſſel a little below the Brim B; through the end of this float is put a Pipe and Wick h, for the flame i, then pouring in Oyl by the open ſide A Q B, fill the ſame till it carry the float up to touch the hollow of the Veſſel; then light the Wick, and you will find that the Lamp will conſume the Oyl, and this contrivance will continually ſupply it till the whole be conſumed, and the Poiſe be moved to touch the Concave of the aforeſaid Veſſel; for when the Veſſel is filled up to f g, the float D will touch at O and E, and the Cavity above f g being empty, the Veſſel will be as is deſcribed in the Figure, the open part A B being upwards. And as the flame conſumeth the Oyl, the ſide of the Veſſel B will deſcend downward towards B 1; and ſo by B 1, B 2, B 3, to B 4, where the whole quantity of Oyl will be conſumed, and the bottom of the float will touch the hollow ſide of the Veſſel; in all which gradual waſting of the Oyl the Superficies thereof will lie at the ſame diſtance below the upper ſide of the float D that it had at firſt, and conſequently at the ſame diſtance from the bottom of the flame. The reaſon of all which will be very eaſie to be underſtood by any one that ſhall ſeriouſly on this Delineation conſider that the float D muſt neceſſitate the Veſſel A C B to move on its Axis B according as its Oyl waſts, becauſe one end thereof E being faſtned to the brim of the Veſſel B, the other end O being looſe will as the Oyl waſts deſcend towards N, whence the end E muſt hang heavier on the brim B, and conſequently muſt move it down towards B, till the upper ſide

ſide fg of the float be reduced to a Paralleliſm with the Superficies of the remaining Oyl, and the end E have no gravitation on the brim B, which motion will be continued as the Oyl waſts, and the brim B will be moved downwards by the points B 1, B 2, B 3, to B 4. I ſhall not therefore ſpend any more time in the Geometrical demonſtration thereof, but proceed to explain a fourth way by which the Flame and Superficies of the Oyl keep always at the diſtance they were firſt put at.

The Fourth way then is, the making the Socket of the Wick to ſwim upon the top of the Oyl, ſo that the Socket may ſink as well as the Oyl, by reaſon it is ſuſtained by that, and by that only. The Veſſel or Receptacle is generally made of Glaſs, and it is beſt of a Hemiſpherical Figure, the light caſting it ſelf through the body of the Oyl as well as of the Glaſs. This is ſo plain and obvious, and ſo commonly uſed and practiſed, that I need not ſpend more time in the explanation or demonſtration thereof, but proceed to deſcribe a Fifth way.

The Fifth way then is much upon the ſame principle with the Fourth, but avoids ſeveral inconveniences to which that is ſubject: For whereas the Flame in the Fourth is neceſſitated to be within the capacity or the Receptacle in this Fifth, it may be at any diſtance, and ſo is made much more convenient to be come at, and to be dreſſed and trimmed. Take then a Veſſel of Glaſs, Cylindrical is beſt, as a Glaſs Bottle, and fit to it a Siphon, long enough to draw the Oyl from the bottom of the ſaid Veſſel, make the one end of this Siphon extend at what diſtance you think convenient for the placing the flame of the Lamp, and ſo order it that it may always draw from the Receptacle by its arms to feed the flame, which it will do if the end of the Siphon be made where the Socket of the Lamp is placed to return or bend upwards again. So that the Plain of the upper Superficies of the Oyl may cut that end of the Siphon where the flame is

D 2 between

between the top of the mouth of it next the Socket and the return thereof upwards; then by a counterpoiſe ſo ſuſpend this Siphon that it may follow the Oyl as it waſts, and fit into the return of the Siphon a Socket and Wick for the flame to be continued. A contrivance ſomewhat of this kind you have in divers Authors, and therefore I ſhall ſpend leſs time in the deſcription thereof. Let A A A A in the Sixth Figure then repreſent a large Cylindrical Viol of Glaſs through the mouth B of which the Cavity thereof may be filled with Oyl, and alſo the end D and float C of a convenient Siphon may be put in. This Siphon D D D P G muſt be made long enough that the float C may reach the bottom of the Veſſel when the Oyl is ſpent, and the other end thereof muſt be ſo curved that the knee of the Siphon P may be below the Superficies of the Oyl E F, and yet that the Socket H made for holding the Wick for the flame I may be ſomewhat above it, this Siphon D D D P G with its Socket and float ſhould be ſo counterpoiſed with a weight M, hung over a Pulley K, by a ſtring L, that the float may not ſink deep into the Surface of the Liquor, but ſwim as it were at the top. This done, if the Wick I be lighted, the Surface of the Oyl will be kept always at the ſame diſtance below the flame that it was firſt put at.

In the firſt, third, fourth, and fifth ways the flame of the Lamp deſcends equal ſpaces with the Superficies of the Oyl in the Veſſel, and therefore though for ſome uſes it be very convenient, as in annealings, where things are to be cooled by degrees, yet for many other it is not; Eſpecially in Lamp Furnaces, where the ſame heat is to be continued, and in ſome caſes gradually increaſed. For ſuch caſes therefore the firſt and ſecond ways will be very convenient. In ſome other caſes the ſixth and ſeventh ways, which do much the ſame thing.

The ſixth way then is this: Through an arm or Siphon (like the Branch of a Lamp hung againſt a Wall) fixed in any convenient place, the Oyl from the Receptacle is continually

continually and equally ſupplied to the flame of the Lamp by the raiſing of the Receptacle as faſt as the Oyl waſts, ſo as to keep the Superficies of the Oyl alway in the ſame Horizontal Plain. The Receptacle is raiſed by a Counterpoiſe hung upon a Fuſey, which Fuſey is a part of an Archimedean Spiral.

Let C C then in the ſeventh Figure repreſent the Receptacle for the Oyl, being a Cylindrical or Priſmatical Veſſel, of what Bigneſs or Length you pleaſe; to this by two Ears at L L faſten two Lines or Ropes K K, the ends of both which are faſtned to the Wheel or Pulley G, though one of them do run over the Pulley F. Fit into this Receptacle is made a Cylindrical or Priſmatical Plug A A, which is fixed in ſome convenient place, ſo as not to riſe or ſink, and through the middle thereof paſſeth a Siphon B B B, the one end whereof extended like the branch of a Candle or Lamp ſuſtains the Socket D for the Flame E, which is fed with Oyl through the Siphon B B B by the riſing Receptacle C C.

To the ſide of the Pulley G is faſtned a Fuſey H, made with very great care of one Revolution of an Archimedean Spiral, not beginning from the Center, but from ſome convenient diſtance from it, where the weight I hanging, may juſt counterpoiſe the Receptacle C C, when quite empty of Oyl, the other hanging counterpoiſe (Tangent to the largeſt part of this Spiral) muſt be ſo far diſtant from the Center of the Wheel G, that the ſame weight I may juſt counterpoiſe the ſaid Receptacle filled top-full of Oyl, and the Fuſey muſt be filed true to a Spiral, drawn with great care of one Revolution between thoſe two points. I ſay here of one Revolution, becauſe I have ſuppoſed the Wheel or Pulley G big enough, by one Revolution of it to draw up the Receptacle the whole ſpace it is to be raiſed; for if the ſaid Pulley be ſo ſmall as to require two, three, four, or more Revolutions, then muſt the piece of the Spiral between thoſe points be drawn of two, three, four, or more Revolutions proportionably, which being

being very Artificially and Mechanically performed, the Receptacle C C will be raiſed by the ſame Degrees by which the Oyl is conſumed at E, and the upper Superficies thereof ſhall always be in the ſame Horizontal Line MM. The Geometrical and Mechanical Reaſon of which being ſo very plain, I hope I ſhall not need to ſpend any more time in the explication thereof than only to ſay, that by means of the Archimedean Spiral-Fuſey the Power of the weight I upon the Pulley G decreaſeth in the ſame proportion as the weight of the Oyl in the Receptacle C C is diminiſhed by its conſumption.

The ſeventh way then is, by a Cylindrical or Priſmatical Plug fitted into a Cylindrical or Priſmatical Receptacle, and let down into it by a Counterpoiſe, hung upon a Spiral Fuſey, the Oyl is ſo raiſed in that Receptacle as always to ſtand Brimfull, or to the ſame Horizontal height till the whole Oyl be conſumed.

The contrivance of this way will be very eaſily underſtood by any one that ſhall peruſe the Delineation in the eighth Figure, and examine it by this following deſcription.

Let A A in the eighth Figure then repreſent a Cylindrical or Priſmatical Receptacle, ſtanding fixt upon a Table or Pedeſtal, from the ſide of which iſſues a hollow Arm or Branch B B, bearing the Socket for the Wick C, where the flame D is continued. Into the Cavity of this Receptacle is fitted a Cylindrical or Priſmatical Plug E E, big enough to fill the whole capacity thereof, and yet not ſo cloſe but that it may freely ſlip up and down the Cavity of the ſaid Receptacle without ſinking. Let this Plug be made conſiderably heavier than the Oyl of the Receptacle; that is, let the Counterpoiſe L, hanging upon the little Wheel M juſt reduce its gravity to be equal to that of the Oyl; then let the point I, where the Perpendicular toucheth the Spiral, be ſo far removed from the Center of the Wheel H, that the counterpoiſe

terpoiſe L may juſt take off its whole gravity, and ſuffer it to have no degree of gravity or preſſure downwards. Then draw the Spiral n o p according to the direction I gave in the former way, and the effect will be produced. The Geometrical and Mechanical Demonſtration of which is very plain to any one that ſhall conſider, that, As the Plug E E by ſinking into the Receptacle A A ſo far as to raiſe the Oyl to the Horizontal Superficies M M will loſe its gravity by the ſame Degrees by which it ſinketh into the Receptacle, and that is alway proportionable to the diminiſhing of the Oyl in the Receptacle by the flame: So the weight L will loſe its power upon the Wheel H, by the ſame degrees by which the Plug deſcendeth, by reaſon the Line by which it is ſuſpended becomes a Tangent to a proportionately ſhorter Radius of the Spiral, of the Rays of the Spiral.

I know indeed that both in this and the former Fuſey there lies an objection againſt the true form of the Spiral, becauſe the Line K K of the weight L doth not touch the Spiral in a point level with the Center, but in one ſomewhat above it, and in this latter ſomewhat beneath it; but though that be a ſeeming material one, yet as to practice it ſignifies very little. For firſt, it will not be difficult to prove that this may be Mechanically drawn true enough, that there ſhall be no ſenſible error, and if the error be not ſenſible, it is no error in practical Mechanicks. Next, were it the true Spiral, yet it would not be more Geometrically Delineated than this which is here required, and at beſt it would prove but a Mechanical approach, which is ſufficient for the effect to be produced by it.

Theſe two laſt contrivances do keep the flame of the Lamp always in the ſame place, and of the ſame ſtrength and fulneſs. But the ſucceeding ways, though they maintain the flame in the ſame degree of ſtrength and nouriſhment, yet by their motion upwards they may be made to increaſe, and intend the heat produced by them in the bodies poſited above them, which is of great

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uſe in many Chymical and Philoſophical Experiments.

The eighth way then is this: Make a Cylindrical or Priſmatical Receptacle for the Oyl exactly like the former, with its Arm, Socket, Wick, *&c.* and fit into it a Cylindrical or Priſmatical Plug, as in the former, that may be able to fill the ſaid Receptacle. Fix this Plug faſt into ſome Wall or Standard, ſo that it ſhall not be able to ſtir; Then by the help of two Lines faſtned to a Counterpoiſe at one end, and the other to the Ears of the Receptacle, ſo counterpoiſe the ſaid Receptacle that it ſhall have no weight or gravity downwards, but hang in a perfect equilibrium; I ſay, whatever quantity of Oyl there be in the ſaid Veſſel, the Superficies thereof ſhall always be in the Plain which is equal to the top of the Oyl when the Veſſel is filled as high as is deſired, which will very plainly appear to any one that ſhall examine and conſider well this following deſcription, and compare it with the Delineation of the Inſtrument in the ninth Figure, where A A repreſents a Receptacle for the Oyl of any convenient capacity, made Cylindrical or Priſmatical, to which is faſtned a hollow Neck or Arm B B for bearing the Socket C, to which through its Cavity (being made hollow) is conveyed the Oyl or *Pabulum* for the continuance of the Flame D; into this Receptacle fit a Cylindrical or Priſmatical Plug, ſo as it may pretty equally fill the ſaid Cavity of the Receptacle, yet not ſo as any ways to hinder the ſliding on upon it of the Receptacle. Let this Plug then be fixt by the top in any convenient place Perpendicularly, and ſetting the Receptacle underneath it, Counterpoiſe the ſame when filled up with Oyl by a Counterpoiſe I, which is faſtned to the two ſtrings F F F F, by which the Receptacle is to hang, which two ſtrings for their more eaſie ſliding to and fro move upon the two Pulleys or Truckles G G, that are fixed to the ſame frame to which the Plug E E is fixed; which being ſo adjuſted, as faſt as the flame D conſumeth the Oyl out of the Receptacle A A, the Counter-

Counterpoiſe I raiſeth the ſaid Receptacle on upon the Plug ſo far till the top of the Oyl be equal to the height it was at firſt counterpoiſed at, to which height it always keeps it till the whole be conſumed.

This laſt way of poiſing the Liquor or Oyl doth make the Superficies thereof run higher and higher as the quantity thereof is more and more conſumed, which for divers Expedients in Mechanicks, Natural Philoſophy, and Chymiſtry is of excellent uſe, as I may hereafter have opportunity to manifeſt upon many occaſions where I ſhall make uſe of them; and it would be, I fear, too tedious to the Reader to have them here enumerated.

But becauſe it may not poſſibly be ungrateful to him to have ſome uſes of this Principle here hinted, I ſhall now ſpecifie a few, and hereafter add many more, together with a great number of other Poiſes for Liquors which ſerve for very differing effects in their kinds, not leſs conſiderable, but rather ſomewhat more ſtrange, as being yet farther removed from the common practices and diſcourſes of Hydraulicks.

The firſt uſe then that I ſhall mention of this Liquor-poiſe ſhall be in Hydraulicks, *viz.* to make a Ciſtern of whatever bigneſs and depth is required to deliver all its water at the top, or ſo near unto it as it ſhall be deſired: By which means nothing of the Deſcent of the water falling into the Ciſtern is loſt, but without any labour or trouble the whole quantity of water that is delivered at the top into the Ciſtern is re-delivered again out of the Ciſtern at the top. This may be done by the firſt, ſecond, and ſeventh ways of poiſing Liquors; this, that, or the other, of which may be more convenient to this, that, or another effect or operation to be performed by it, which muſt be choſen and applied with judgment, according to the occaſion, and the circumſtances of it. Every of the three, though they all agree together in the producing the effect of keeping the Su-

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perficies of the water to the ſame Level, and there delivering it, have yet each of them their ſeveral proprieties, which maketh ſome one of the three more proper and adapted to one deſign than either of the other two, and each of the other two in ſome other effects and applications may be much more uſefully applied than the firſt. By this means the whole depth of the Ciſtern is gained, and all that water that was uſed to be delivered at the bottom is now delivered at the top, and conſequently gains the advantage of the Perpendicular height of the Ciſtern to be imployed, for any uſe, for turning an Automaton, or conveying the Stream farther, or to a higher level.

A ſecond effect performable by theſe Poiſes may be for delivering any quantity of water with an equal degree of ſwiftneſs, ſo as to continue an equal ſupply of water till the whole Ciſtern or Receptacle be emptied, the ſpending of the water in the Ciſtern not at all abating the ſtream without, the Counterpoiſe always keeping the Ciſtern full, and maintaining the current till the laſt. This may be uſeful for ſawing or grinding ſtones by an Engine; for gauging of Glaſs Tools, or grinding glaſſes by an Automaton, in all which caſes there is need of a conſtant and equal ſupply of water and ſand; as alſo for waſhing and Fulling of Cloth; it may alſo ſerve for various ſorts of Clepſydras, or meaſuring the quantity of time by the quantity of the current of water, as I ſhall by and by ſhew. And thirdly, for maintaining any ſlow and conſtant motion, as that of a Jack, or Clock; an Engine for continually ſtirring of a liquid body, or ſhaking, tumbling, and turning of dry Solids and powders, of which ſort there are a great number of uſes in Chymiſtry for the operations of Digeſtion, Calcination, Pounding, Grinding, Trituration, Searcing, and the like; which operations being certainly, evenly, and conſtantly performed by an Engine ſupplied by ſuch a ſtream of water will far exceed the ſame kind of of work done by the hands of men, eſpecially in ſuch operations where the Labour and Diligence is to laſt divers days and nights together without any intermiſſion, which are Requiſites not at all ſtrange to Chymiſtry, and which will weary the diligence of the beſt Laborant and his Attendants.

A third effect performable by theſe Poiſes is the making a perpetual and conſtant ſtream in imitation of that of a natural Spring or Fountain in the Earth. This may be done if the Ciſtern be once in twenty four hours recruted and ſupplied with a new acceſs of water from ſome Pipes, which is uſual enough here in *London*, and elſewhere, where there are Waterworks and Conveyances of water. For as the waſting of the water in the Ciſtern does no ways abate or diminiſh the ſtream of the water from the Ciſtern, ſo the new acceſs of other water for a ſupply to refill the Ciſtern does not at all accelerate it, but the ſtream remains equal; And hence, conſequently conſtant, and, as it were, perpetual.

A fourth effect is, the delivering any quantity of water to any degree of ſwiftneſs, and the whole quantity of the water by the ſame degree. This is performed by tapping the Ciſtern at any part of the depth thereof, for according as the Veſſel is tapp'd lower under the Surface, ſo will the motion of the water be ſwifter; and here the depths muſt be in a duplicate proportion to the Velocity deſired: As for inſtance, the Ciſtern being tapped with a hole of a quarter of an Inch bore, at the depth of an Inch below the Surface, is found to deliver a certain quantity of water in a minute; if it be deſired that through a Tap of the ſame bore there ſhould be delivered twice that quantity, the Ciſtern muſt be tapp'd at four Inches deep; and if thrice that quantity in the ſame time, it muſt be tapp'd at nine Inches deep; and ſo forwards, as is already demonſtrated by *Merſennus*, and other Authors. For ſince the preſſure of Fluids upon the parts thereof increaſe, in the ſame proportion

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with the depth below the Surface. And ſince the forces requiſite to accelerate motions muſt always be in duplicate proportion to the Accelerations, it follows, that the perpendicular depths of the Tap under the Superficies of the water muſt be always in duplicate proportion to the Velocities required.

The plainneſs and certainty of this truth in Hydroſtaticks, long ſince ſo fully and excellently demonſtrated by *Stivinus* of all Fluids, and ſo highly improved of late in the particular applications thereof by many more modern Authors, who have writ moſt learnedly and clearly thereof, as well as experimentally and practically, makes me much admire at the learned Doctor *More*, who in his *Enchiridion Metaphyſicum*, in the 11, 12, and 13 Chapters, and in a Book, newly publiſhed, called, *Remarks upon two late ingenious diſcourſes*, &c. does not only deny this Gravitation in the parts of Air, but of Water, quickſilver, and other Liquors. And inſtead thereof, to ſolve the Phenomena, would introduce into the World a Principle, which he terms an *Hylarchick Spirit*, which at command acts and performs whatſoever is neceſſary to ſolve all the Phenomena of Mechanical, Hydroſtatical, and, in a word, all Phyſical motions and effects.

In anſwer to whoſe Doctrine about Hydroſtaticks I ſhall only urge this one Experiment of the Velocity of the current of Fluids, tapp'd and running at ſeveral depths under the Superficies of that Fluid, which can no ways be ſolved by the Hylarchick Spirit, and we muſt be fain to come to the Mechanical and plain Rules of motion, and to allow every particular of that Fluid to preſs with its own gravity where ever placed. And this I will prove from his own words in his *Enchiridion Metaphyſicum*, pag. 113. where explaining very ingeniouſly the Hypotheſis of Gravitation of the parts of Fluids one upon another by the ſimilitude of ſix men ſtanding in a Line, and preſſing againſt a Wall, (which men he marks with A B C D E F, and the Wall with G) He ſays, that A the firſt man cannot preſs F the laſt againſt the Wall G, G, but by preſſing B againſt C, and C againſt D, and D againſt E, and E againſt F; nor can A preſs B againſt C, nor C preſs D againſt E, nor E preſs F againſt the Wall G, but at the ſame time it muſt be underſtood that B preſſes D towards F, and D preſſes F towards the Wall G, for A C and E, ſays he, are here put for *Des Cartes Materia Cœleſtis*, preſſing the parts of the water within the pores, and B D and F for thoſe parts of the water preſſing the bottom of the Veſſel. But, ſays he, that B preſſes D, and D preſſes F appears from this, that caſting out E and F, D doth run to the Wall G, and caſting out C D E and F, B alſo will run to the ſaid Wall. And ſo, ſays he, the ſtate of the matter would be if Gravity did proceed from the meer Mechanical motion imparted to the Terreſtrial parts of the Fluid by the *Materia Cœleſtis* of *Des Cartes*, to wit, the Elements would actually gravitate in their proper places. But ſince there is no ſuch thing, it is a ſure ſign that Gravity doth ariſe from a higher cauſe, which higher cauſe he elſewhere ſuppoſes to be an Hylarchick Spirit. This from ſo plain reaſoning is a ſtrange Concluſion, and contrary to all experience.

Now though, I confeſs, I ſuppoſe Gravity to be otherwiſe performed than as *Des Cartes* has ſuppoſed, yet do I believe his Suppoſitions ſo Rational and Ingenious, and ſo much above the Objections brought againſt them, and ſo much better than any other I have yet met with, as no wiſe to deſerve to be eſteemed *fœda deliria*, as the learned Doctor is pleaſed to term them, *pag.* 125.

It ſhall not be my buſineſs to defend *Des Cartes* Principles at the preſent, nor to ſet up any new Hypotheſis inſtead thereof, but only to urge this Experiment of the running of a Liquor ſwifter and ſwifter, according as the hole through which it runs is deeper and deeper placed below the Surface of the ſaid Liquor or Fluid, and that the Velocities of thoſe ſtreams are always in a ſubduple proportion to the Altitude of the Fluid above thoſe holes; whence it is evident, that the force that makes that Fluid run is always in the ſame proportion with

with the Altitude of the fluid parts above thofe holes; and confequently, that the motion of them is exactly according to the plain and obvious Rules of Mechanical motions. And confequently for the folving all the Phenomena of Hydroftaticks there is no need of any other Principles than the plain Mechanical Principles, which fuppofeth every Terreftrial Body to have a Gravity in it, which is always the fame, and always communicates its Gravity to the Terreftrial Bodies fubjected under it, and not only its own, but the Gravity of all other Bodies above it, which have communicated their Gravity to it, and that this Gravitation is always the fame, and acteth continually by continual repetitions indefinitely fwift. And that this gravitating or communicating of its weight, together with the weight of all other Bodies communicated to it, is no ways differing from all other communications or propagations of motion, which the Doctor muft confefs to be meerly Mechanical, if at leaft he will admit of any fuch thing as Mechanical motion. For I cannot conceive any Reafon why the Doctor fhould not allow for inftance the parts of a Cylinder of Lead to prefs upon one another as much when they are kept melted in an Iron Cylinder into a Cylindrical form part over part as when the Lead is cold and divided into feveral parts, and laid one over another in the fame form that they were kept in by the incompaffing Iron Cylinder. Since if the Iron Cylinder and melted Lead, and the Iron Cylinder and cold Lead be weighed, it will be found that they have both the fame weight or gravity downwards, and do communicate continually the fame force, preffure, indeavour, impetus, ftrength, gravity, power, motion, or whatever elfe you will call it to the Scale. And I fuppofe the Doctor will grant, that if the cold Cylinder of Lead, weighing ten pounds, be divided into ten fhorter Cylinders, that are each a tenth part of the whole, and do each weigh a pound alone, every one of the upper fhall gravitate upon every one of the lower; and that the tenth, with the other nine upon

upon it, ſhall preſs the Scale with ten pound weight, and conſequently, that the tenth doth not only communicate its own gravity of one pound, but the gravity of all the other nine above it, which is nine pounds; and, if the tenth be taken away, and the ninth be put to touch the Scale, with the other eight upon it, it is certain that the ninth will not only communicate its motion, or preſs the Scale with its own weight of a pound, but will communicate the motion to, or preſs the Scale with the weight of eight pounds more, or of all the eight Cylinders ſuperincumbent, and the like Ratiocination may be upon the eighth, ſeventh, ſixth, fifth, fourth, and ſecond, but the laſt will only preſs the Scale with its own weight, unleſs we take in the conſideration of the weight of the Air, which in this Ratiocination is not neceſſary. Since then I think it cannot be denied but that the whole ten ſtanding in a Cylinder one over another, the tenth is preſſed by nine, and preſſes with ten pound weight; the ninth preſſes with nine, and is preſſed with eight; the eighth is preſſed with ſeven, and preſſes with eight, and ſo onwards, and that the preſſure of the loweſt downward is always proportionable to the height of this Cylinder. Suppoſing theſe to be all melted in an Iron Cylinder, but kept in the ſame poſition and ſituation, and finding the whole to keep the ſame weight, why ſhould we not believe that each of thoſe parts will exert the ſame effects, as to gravity, on thoſe beneath it as the ſame parts cold, and in the ſame poſture did; ſince if the Cylinder of the Fluid be ſhortned by 1, 2, 3, or 4, tenths of its height, the ſame abatement of weight or gravity will appear. Having ſeriouſly peruſed all the Ratiocination that the Doctor hath produced, both in this late Book, and in his *Enchiridion Metaphyſicum*, I cannot find any convincing reaſon againſt it, but what ſeems grounded upon ſome pre-conceived Notions and Hypotheſes which I cannot underſtand; and I cannot ſee how he can avoid acknowledging this to be a Mechanical motion, if at leaſt he will allow any Mechanical

chanical motion at all, ſince it doth ſo perfectly, and in all circumſtances ſo exactly conform and agree with the Laws of Mechanical motion, that I do not know any difference, nor any one Phenomenon of Hydroſtaticks or Gravity but what may be clearly ſolved by the common Rules of Mechanicks.

But to paſs by all other Mediums to prove this Gravitation or preſſure of the parts of Fluids one upon another, I ſhall only inſiſt upon this one Experiment of the Velocity of Fluids, vented or running at ſeveral depths below the Superficies of that Fluid. In which it is obſervable, that the quantity of water running within a certain ſpace of time is always in a Subduple proportion to the height of the preſſing Fluid above the hole. That is, the quantities of water are in proportion to one another as the ſquare Roots of the ſeveral Altitudes. As for inſtance, it is the obſervation of *Merſennus* in his Hydraulicks, that a Tap of an Inch bore, four foot under the Superficies of the water will yield a pound or pint of water in 13 Seconds of time; now, if it be deſired to make the water run through a Tap of the ſame bore twice as faſt, that is, to yeild a quart or two pounds of water. This new Altitude muſt be made to the former Altitude, as the ſquare of two to the ſquare of one, that is, as four to one; whence it will follow, that the Altitude of the water above the Tap muſt be made ſixteen foot to make the Tap run a quart of water in 13 Seconds of time. And if it be deſired to have the Tap run a Gallon or eight pints in 13 Seconds, the proportion of the new Altitude to the firſt muſt be as the ſquare of eight to the ſquare of one, that is, as 64 to 1, whence the Altitude of the water muſt be 256 foot, and the like for any other quantity or Velocity deſired. As if it be deſired that the Tap ſhould only run half a pint in 13 Seconds, the Tap muſt be placed at one foot under the Superficies, which is a quarter of the former Altitude. Now this is exactly according to the General Rule of Mechanicks. Which is, that the proportion of the

the ſtrength or power of moving any Body is always in a duplicate proportion of the Velocity it receives from it; that is, if any Body whatſoever be moved with one degree of Velocity, by a determinate quantity of ſtrength, that body will require four times that ſtrength to be moved twice as faſt, and nine times the ſtrength to be moved thrice as faſt, and ſixteen times the ſtrength to be moved four times as faſt, and ſo forwards. This is moſt certainly true in the motion of Bullets ſhot out of Cannons, Muskets, Piſtols, Wind-guns, Croſs-bows, Spitting-Trunks, and the like; as likewiſe in the motion of Arrows ſhot with Bows or Balliſtæ; of Stones thrown by the hand, or with Slings; of Pendulums moved by Gravity or Weights; of Muſical Strings; of Springs, and all other vibrating Bodies; of the motion of Wheels, Flies, *&c.* drawn and turned by Weights or Springs; of the motion of Perpendicularly or Obliquely falling Bodies; and in a word, of all other Mechanical and Local motions, allowance only being made for the impediment of the Air or other Fluid Medium, through which the Body is moved. Now if the Doctor will contend for an Hylarchick Spirit to perform all theſe, he may plauſibly enough contend for it alſo in the Experiment of the Gravitation of the parts of Fluids one upon another.

We ſee then how needleſs it is to have recourſe to an Hylarchick Spirit to perform all thoſe things which are plainly and clearly performed by the common and known Rules of Mechanicks, which are eaſily to be underſtood and imagined, and are moſt obvious and clear to ſenſe, and do not perplex our minds with unintelligible Idea's of things, which do no ways tend to knowledge and practice, but end in amazement and confuſion.

For ſuppoſing the Doctor had proved there were ſuch an Hylarchick Spirit, what were we the better or the wiſer unleſs we alſo know how to rule and govern this Spirit? And that we could, like Conjurers, command this Spirit, and ſet it at work upon whatever we had occa-

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ſion for it to do. If it were a Spirit that Regulated the motion of the water in its running faſter or ſlower, I am yet to learn by what Charm or Incantation I ſhould be able to incite the Spirit to be leſs or more active, in ſuch proportion as I had occaſion for,and deſired ; how ſhould I ſignifie to it that I had occaſion for a current of water that ſhould run eight Gallons in a minute through a hole of an Inch bore ? If the Doctor ſhould tell me, that I muſt make the Tap at ſuch a depth under the Superficies of the water, and then the Hylarchick Spirit will make the water run as I deſire, I would then inquire how he comes to call that an Hylarchick, or matter-governing Spirit, which is rather commanded by matter, and ſubjected to its Laws, and is neceſſitated to act exactly according to the quantity and poſition of matter, by what means ſoever it be ſo placed ? This Principle therefore at beſt tends to nothing but the diſcouraging Induſtry from ſearching into, and finding out the true cauſes of the Phenomena of Nature: And incourages Ignorance and Superſtition by perſwading nothing more can be known, and that the Spirit will do what it pleaſes. For if all things be done by an Hylarchick Spirit, that is, I know not what, and to be found I know not when or where, and acts all things I know not how, what ſhould ſhould I trouble my ſelf to enquire into that which is never to be underſtood, and is beyond the reach of my Faculties to comprehend ? Whereas on the other ſide, if I underſtand or am informed, that theſe Phenomena do proceed from the quantity of matter and motion, and that the regulating and ordering of them is clearly within the power and reach of mans Induſtry and Invention ; I have incouragement to be ſtirring and active in this inquiry and ſcrutiny, as where I have to do with matter and motion that fall under the reach of my ſenſes, and have no need of ſuch Rarified Notions as do exceed Imagination and the plain deductions of Reaſons there from.

For what is clearer to be ſeen and tried by Experiment.

ment, and what more eaſie to be imagined and underſtood than that a Cylinder of water, or any other Homogeneous ſubſtance of twice the height ſhould have twice the gravity or preſſure: of thrice the height, thrice the preſſure: of ten times the height, ten times the preſſure: of 100 times the height, 100 times the preſſure, and conſequently, to imagine that as in all other Mechanical motion, four times the preſſure will double the Velocity, nine times the preſſure will treble it, ſixteen times will quadruple it, and 100 times will decuple it, and ſo forward; So in this Experiment the ſame preſſure will perform the ſame effect, and a proportionate preſſure a proportionate effect. And ſince we find that the effect does moſt exactly anſwer the Theory (as moſt certainly, evidently, and undeniably it doth) why ſhould we doubt of the cauſe which is ſo certain and Regular a Concomitant, that it is always preſent when the effect is performed? And where ever it is preſent, (if other Circumſtances hinder not) the effect certainly follows. I could have gone over all the other Ratiocinations of the Doctor for an Hylarchick Spirit to perform the effects which do clearly belong to Mechanical motions and powers, and are performed and regulated exactly according to the quantity and quality of matter, and according to the general and univerſal Laws of motion, and not otherwiſe. But that is not my preſent buſineſs, but rather to explain how this contrivance of Poiſes doth ſerve to make a Ciſtern or Veſſel to run any quantity of water required in any ſpace of time. And that to run the whole quantity either with an equal Velocity or ſtream, or by any deſired degrees to be accelerated or retarded from the beginning to the end, which for ſome occaſions in Mechanicks is of great uſe, and hath not been explained by any Writer of Hydraulicks hitherto.

I ſhould have here left this Digreſſion, but that I find a little further in the aforeſaid Doctors *Enchiridion*, to wit, in the nineteenth Chapter, in the fifth, ſixth, ſeventh

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and eighth Sections, continued from the 246. to the 256. Page, ſome Animadverſions upon an Explication of Colours which I did formerly publiſh in my *Micrographia*, from the confutation of which he endeavours to aſſert this *Hylarchick Spirit*. But in this he doth *Canere triumphum ante victoriam*, and ſeems to make very ſlight of that which he neither hath hitherto by all he hath ſaid in his *Enchiridion Metaphyſicum*, nor can by all other Arguments he can produce anſwer. For if the Doctor had pleaſed to have conſidered the Objections I made againſt the Hypotheſis of the Rotation of the *Carteſian Globuli*, with a little more ſeriouſneſs and deliberation, he would not, I conceive, have believed that one that underſtood the Objection would be ſatisfied with ſo ſlight and inſignificant anſwers, as he is pleaſed to make to them. His Anſwer then to the firſt Objection which I brought againſt this Hypotheſis, which was raiſed from Experiments made with thin plated bodies, producing colours, though the refracting Superficies were parallel, is no more but this: That it is not every ſecond Refraction of the Ray in a Parallelipiped that doth deſtroy the Rotation generated by the firſt, but only that which entring at one ſide, paſſeth through, and goeth out again with the ſame refraction it entered. In which caſe only, ſays he, the Rotation of the *Globuli*, generated in the firſt Superficies, is deſtroyed in the ſecond. But, ſays he, a Ray falling upon a Parallelipiped, and being reflected from the ſecond Superficies, ſuffers a double Refraction in the ſame Superficies, the one at entring, and the other at going out again; both which Refractions, ſays he, do promote the Rotation of the *Globuli* the ſame way. This he ſays very poſitively; but gives no reaſon for it. Nor indeed could he, ſince it is expreſly contrary to *Des Cartes* Principles, and to all the Phenomena of ſuch Parallel ſided bodies until they come to a certain degree of thinneſs: For if his Affirmation were true, then muſt all Reflections from the Quickſilver, or foil of Looking-glaſſes, eſpecially if a little oblique, make the Object ſpread,

ſpread, and become coloured in the ſame manner as Objects do which are look'd at through Priſmes. But this is contrary both to Experience, and the Laws of Reflection; for the Refractions in the Parallelipiped B are the very ſame with the Refractions in the Parallelipiped A, the Reflection at D making the Ray to be refracted at F, in the ſame manner as if it were refracted at G by G H, and the Parallelipiped were twice as thick, and conſequently the colour generated in E muſt be deſtroyed in F, and conſequently produce no colours, as really it doth not in plates beyond ſuch a thickneſs; whereas if the Refraction at F did promote the Rotation, as he affirms, then muſt the reflected Superficies I K not be Parallel to EF, but inclined to it with an Angle at L M. Then G N would repreſent F O, which is impoſſible, and contrary to the Laws of all reflection, as he might have underſtood if he had conſidered my Demonſtration about the Reflections of a Globe. Nor will the Doctors adding, *Sed de hac prima objectione non eſt quod ſumus adeo ſoliciti, cum ſit in materia magis incerta ac inequali cujus interna contextura videatur Globulorum motus variis modis poſſe mutari.* For ſince all tranſparent bodies whatſoever produce the ſame effect, that Subterfuge of ſuppoſing ſome ſtrange inviſible texture in the body of *Muſcovy* Glaſs, differing from that of other tranſparent bodies, will prove but a lame help, for this *interna contextura* muſt be common to all tranſparent Bodies. And why it ſhould do it at one time, and not at another, the Doctor doth no where ſhew, nor ſeems to underſtand.

Next, whereas in the ſeventh Section of the ſaid nineteenth Chapter he ſays, *Verum in materia illa idonea Gutta ſcilicet Pluvia, ſi nullus Demonſtrationis Scopo ſubſit error, actum eſt de Globulis Carteſianis. Sed videtur* (ſays he) *ingenioſus demonſtrator non ſatis intellexiſſe ſcopum quo collinneare debeat ipſius Demonſtratio.* To which I anſwer, that I perceive by the Learned Doctors endeavours to refute it, that he neither underſtood that, nor the Laws of Reflection and Refraction according

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to *Des Cartes* Hypothesis. *Neque enim satis erat probare (quod agnosco eum fecisse scite & eleganter) Refractiones in gutta pluvia ita fieri, ut si in duobus pellucidi Parallelipipedi Lateribus oppositis, factæ essent, sed oportebat præterea evicisse quod eodem modo refringatur radius in utrisque Locis quo in Parallelipipedo* A *refringitur, hoc est ut Radius* B C *quamvis oblique, perpetuo tamen currat versus eandem extremitatem tam in* F *quam in* D *Parallepipedi* A *puta versus extremitatem* E, *nam in hoc casu Rotatio ad* D *dissolvitur iterum ad* F *ut supra dictum est; sed Demonstratio Ingeniosi Micrographi huc non attingit; sed probat secundam refractionem in opposito Latere fieri ad modum refractionis in Parallelipipedo* C *ubi Radius* B N *primo refringitur in* D *& procurrens versus extremitatem* E *ibique inflexus pergit postea versus alteram extremitatem* G *& Refringitur in* F, *quæ refractio non diluit Rotationem prioris refractionis in* D, *quippe quod tendentia Radii sit in partem oppositam.* If the Learned Doctor had better consulted *Des Cartes* Doctrine, or the common Laws of Reflection and Refraction, he would have been of quite another mind, and would not so positively have asserted a Proposition so positively contrary to the Principles of *Des Cartes*, and all Experiments. For if what he affirms were so, then (as I urged before) according to *Des Cartes* Doctrine, and the Doctrine he would defend, the Image from a Looking-glass must be returned coloured, and the same also from a plain sided Prisme, where the refracting sides are Perpendicular or equally inclined, but contrary ways to the Reflecting Superficies. But this is contrary to Experiment, he must therefore once again consider how to find out a Reason why there is no colour generated, where, according to his Assertion, there is so great a refraction, and a doubly promoted Rotation made in both the refracting Superficies the same way, and both so much promoting the said Rotation of the Globuli. He might therefore, if he had pleased, have suspended his Conclusion. *Adeo ut Doctrina Cartesiana de Globulis eorumque Rotationibus nihil periclitetur ab hac Demonstratione*

tione quæ quamvis ſatis elegans ſit & concinna, debitum tamen ſcopum non omnino attingit, until he had a little farther conſidered the nature of Reflection and Refraction. Now, becauſe I find that the Learned Doctor is not the only perſon that hath not rightly apprehended this Theory, give me leave to explain a little more particularly the manner thereof: Suppoſe we then in the three Figures D E and F, that the ſpace between the two Parallel Lines a c and b d doth repreſent a Ray or Radiation of light; Not a Mathematical Line, but a Phyſical one of ſome Latitude, between which Lines is propagated a motion, or ſomething equivalent thereunto, which ſerves to produce the effect of light. This motion we ſuppoſe to be propagated by a Pulſe or Wave in all uncoloured Rays at Right Angles with the Line of Direction, but in coloured Rays more or leſs obliquely according to the greater or leſs refraction. We will ſuppoſe the ſtroke of the Pulſe to be the length of the ſpace between 1 and 2, or 2 and 3, or 3 and 4, *&c.* and conſequently, in a uniform medium the pulſe will continue the ſame, and the expanſion of it will be Perpendicular to the Line of Direction or progreſs; but when it comes to the Refracting Superficies c d, Obliquely the ſide of the Pulſe c touches the refracting Superficies firſt, and being propagated into the refracting medium by a longer and quicker Pulſe, it is propagated to 4 below c before the other ſide of the Pulſe touches the Superficies at d, the Pulſe therefore 4 4, 5 5, 6 6, *&c.* becomes Oblique to the tendency of the Radiation; and by the Superficies e f it is reflected by 7 7, 7 7, 7 7, till it touches the ſecond refracting Superficies g h; where it is obſervable, that the ſame ſide of the Ray that entred firſt the Superficies c d enters firſt into the Superficies g h, in the ſame manner as if it had proceeded on by the ſtraight Lines f m e l till it met with a Parallel Superficies l m to the firſt c d; for the Ray between the two Parallel Lines f h, e g, hath the ſame inclination and reſpect to the Refracting Superficies h g, that the Ray between f m and e l would have

have to the Superficies m l, ſuppoſing there were no Reflecting Superficies at e f. I ſhall not need, I hope, more particularly to demonſtrate every part of this Explanation, the very obſerving the Delineation of the Scheme being enough to make it plain to any one never ſo little verſed in Geometry, from which he will plainly perceive that what I endeavour to demonſtrate was really ſo, and that I did underſtand what ſcope my Demonſtration aimed at, ſo far as to hit the Mark, which was to ſhew that Colours were generated, where, according to *Des Cartes* own Principles, there could be no Rotation of the Globuli. Now, though the Learned Doctor would not admit of this Demonſtration to be ſufficient to do the work, yet he ſays, *Pag.252. Veruntamen diſſimulandum non eſt, non pauca me meapte opera excogitaſſe quibus pro perſuaſiſſimo habeo eorum motus & rotationes modis pure mechanicis ſemper fieri non poſſe.* And in proſecution of the deſtruction of this Rotation of the Globuli, which he hath hitherto ſeemed to defend, he adds four ſeveral Arguments, I ſhall not now ſtay to repeat them. But whoſoever will pleaſe to read what the Learned Doctor hath *ſuapte opera* excogitated againſt the *Carteſian* Hypotheſis, and ſet down in the 252, 253, 254, and 255. pages. And compare them with what I have ſaid in the forementioned place, to wit, at the latter end of the 60. and the beginning of the 61. pages of my Micrographia, may plainly find the Arguments brought by the Doctor do very little, if at all, differ from thoſe I there publiſhed.

I could heartily therefore have wiſhed that the Learned Doctor had made uſe of ſome other Mediums to prove the Exiſtence of an Hylarchick Spirit, and not have medled with Arguments drawn either from Mechanicks or Opticks; for I doubt, that ſuch as underſtand thoſe ſubjects well, will plainly ſee that there is no need of any ſuch Hylarchick Spirit; and if there be no need of it, but that all the Phenomena may be done without it, then it is probable that there is none there, for *Natura*

Natura nihil agit frustra. It had been much easier to have proved the existence of it by Arguments drawn from subjects we less perfectly understand, as from the generation, nutrition, vegetation, and propagating of Vegetables, and animal substances; for there the manner of the progress of Nature being infinitely more curious and abstruse, and further removed beyond the reach of our senses and understandings, one may more boldly assert strange things of this Hylarchick Spirit without fear of controul or contradiction, and from whence possibly it may never lie within the power of Reasoning to banish him.

But to leave this Digression, and return to the use of these water-poises.

A fifth effect may be for washing and refining of Earth, Clays, Powders, and the like; the clear water by these contrivances being made to run over gently at the top, and so leaving all the settlement from the water at the bottom.

By any one of these, with a receptacle Cistern added to it, the stream of water from that Cistern may be accelerated or retarded by any degrees desirable. This doth depend partly from the proportion of the Tap of the Receptacle Cistern to the Tap of the counterpoised Cistern, and partly from the shape and make of the Receptacle Cistern, by the proportion and shape of which the stream of Liquor through the Tap of the Receptacle Cistern may be modulated at pleasure, as any one, a little versed in Hydrostaticks, will easily perceive and demonstrate.

A sixth effect may be for governing the heat of Lamps for Distillations, Digestions, Fermentations, Putrefactions, Dissolutions, hatching the Eggs of Birds or Insects; accelerating, and seasoning, or timing the growth of Plants; nealing of Glasses and Metals by the gradual access of the heat, so as to make them fit for stronger

G degrees,

degrees, or by the gradual receſs to bring them out of the greater degrees to make them tough and capable to receive the cold of the Air.

It would be too long to give inſtances of contrivances for every of theſe operations but the skilful Mechaniſt, Philoſopher or Chymiſt will eaſily ſupply his own deſires by ſome one of theſe I have inſtanced in, or at leaſt by a compoſition of them. I ſhall therefore only add a deſcription of a Clepſydra or time-keeper or two, and ſo leave this ſubject for the preſent.

A deſcription of a new ſort of Clepſydra.

THis contrivance is nothing elſe than that Two of the ſecond ſort of Veſſels are ſo contrived as to run into each other and to empty themſelves and be filled alternately, and their bigneſs or capacity and the hole through which the Liquor is vented are ſo proportioned as to be emptying the ſpace of an hour, which is eaſie enough, and may be adjuſted to what accurateneſs is deſired. Then the convex Superficies of the Cylindrical poiſe is divided into ſixty equal parts by ſtraight Lines drawn upon its Surface Parallel to the Axis, and to each other; theſe lines by the ſinking or turning of the ſaid poiſe denote the minutes, and if ſmaller Diviſions of time be deſired, the ſpaces between them may be divided by other ſmaller Parallel Lines denoting the parts of each minute to what niceneſs is deſired. One of theſe Cylindrical Receptacles may be fixt, and the other by an eaſie *apparatus* may be made to riſe a little when it is top-full, and fall a little when quite empty below the Level of the other that is fixt: The Chanel between them, through which the water is to run out of the one into the other, may be a ſmall pipe with a hole in it of a bigneſs proportioned, as I ſaid above, to let the Liquor run out of of one into the other in the time desired, and its ends may be fastned to the two Receptacles by a part of the neck of a bladder or gut, so that it may be limber, and may always have a Declivity into the Vessel that is to be filled; the Declivity need not be above half an Inch. The Liquor used in it may be Water, Oyl, or any other Liquor that doth not easily evaporate: But the best of all is Quicksilver, because it doth not with keeping evaporate at all sensibly, which I have carefully observed for these fifteen years last past. Nor doth it grow thick or foul by the alteration of the Air, nor do I find it sensibly alter by the heat and cold, at lest not comparable to the great changes which other Liquors suffer by the alterations of those qualities. It is an excellent material for measuring time in a standing Machine; and there may be hundred of ways contrived to make it measure the space thereof as accurately as a Pendulum; and I have many times admired that *Tycho Brahe*, who was otherwise so curious and exact in the contrivance and make of his Engines and Instruments, was yet so defective in his contrivances of measuring time by Quicksilver, when there were so many obvious and easie ways of doing it, as he seems to complain in his works. I have made trial of several with very good success, and found some of them even beyond expectation certain, of which I may hereafter upon an other occasion add the descriptions, when I publish the various ways of making exact Time-keepers or Watches. In the mean time, being now speaking of Time-keepers, for variety sake I shall mention.

A New Principle for Watches.

THis is a way of regulating both standing Watches, and movable Watches, either for the Sea, or the Pocket, which some ten or twelve years since I shewed the *Royal Society*, when I shewed them my contrivances of the Circu-

G 2 lar

lar Pendulum, which is ſince publiſhed by Monſieur *Hugenius*, which is alſo mentioned in the Hiſtory of the ſaid *Society*, p. 247. lin. 20. This was by a fly moving Circularly inſtead of a ballance, whoſe motion was regulated by weights, flying further and further from the Center according as the ſtrength of the Spring of the Watch had more and more force upon its Arbor. The Weights were regulated from flying out further than they ought to do by the contrivance of a Spiral Spring, drawing both the ſaid Weights to the Center of the motion or fly, in the ſame proportion as I then demonſtrated Gravity to attract the weight of a Circular Pendulum, moved in a Parabolical Superficies, towards the Center or Axis of its motion. The Weights were ſo contrived as always to counterpoiſe each other. The Skeleton of this fly you have repreſented in the Figure. The particular explanation of the parts, and the Geometrical Demonſtration of the Principle both of the Springs, and of the flying from the Center, I ſhall explain in the Theory of Springs, and in the deſcription of Time-keepers and Watches.

> *Οἱ δημιουργοὶ κατασκευάζουσιν ὄργανον κρύπτοντες τὴν ἀρχὴν, ὅπως ᾖ τοῦ μηχανήματος φανερὸν μόνον τὸ θαυμαστόν, τὸ δ᾽ αἴτιον, ἄδηλον. Ariſt. Quæſt. Mechan.*

An Obſervation about the Seed of Moſs.

SInce the publiſhing of my Micrography, I have met with an Obſervation, which though it be of one of the ſmalleſt compound bodies I have hitherto taken notice of, yet does it afford a hint of very great concern in Natural Philoſophy; And it does ſeem to make clear the cauſe of a Phænomenon, that hath appeared dubious, not only to me, but to many other more knowing Naturaliſts. I have often doubted, I confeſs, whether Moſs, Muſhroms, and ſeveral other ſmall Plants (which the Earth ſeems to produce

duce αὐτομάτως) were the off-ſpring of a Seed or Grain; and I have been apt to believe, that they were rather a ſecondary production of Nature; being ſomewhat the more inclined to that opinion, becauſe having formerly examined the ſmall knots or Seed-cods of Moſs with a ſingle Microſcope, I could not perceive any thing in them that I could imagine to be Seed, at leaſt not ſo great a quantity as ſeemed neceſſary to maintain ſo numerous a Progeny, as was every where to be found of it; that, which then came out of them, ſeeming to be rather a pulp or pith, than any thing like the Seeds in other ſimilar Cods. But being ſince ſomewhat more inquiſitive, I did examine ſeveral of the above-mentioned Knobs or Seed-veſſels, and found that there were ſeeds in them, no leſs wonderful for the greatneſs of number, than the ſmalneſs of bulk. Taking then ſome of the ripe and brown or reddiſh ones of them, and preſſing them pretty hard, I found, that there was a ſmall duſt went out of them, which ſeemed to vaniſh into the Air. Preſſing and ſqueezing others of theſe upon a black plate, and examining the powder with a Microſcope, I found it to be a great heap of exceeding ſmall Seeds, Globular, and pretty tranſparent. It is the ſmalleſt, I confeſs, I have yet ſeen, and, it may be, that has hitherto been diſcovered. And, unleſs that be a plant, which I diſcovered growing on the blighted leaves of Roſes, and that thoſe ſmall bodies be ſeed veſſels; or, unleſs thoſe Knobs, I have diſcovered on the top of mould, be the like; I cannot preſently imagine where there ſhould be found a ſmaller. For, I find, that there will need no leſs than thirty ſix hundred of them to be laid one by another in a line, to make the length of an Inch; and, to cover the Superficies of an Inch-ſquare, there will need no leſs than nine hundred and threeſcore thouſands, beſides twelve millions, of ſingle Seeds if laid quadrangularly, but if laid triangularly, there will need no leſs than two hundred and fourſcore thouſand, beſides ſeventeen Millions of ſingle grains. And the number in a grain weight of them cannot be

leſs:

leſs than one thouſand three hundred eighty two Millions and four hundred thouſand ſingle grains, about eighty of theſe ſquare Superficies of Seeds being laid one upon another in the Trigonal order, making, as near as I can gueſs, the thickneſs of a piece of fine Paper, a ſquare Inch of which weigheth a grain. And though this may ſeem a moſt incredible narration; yet I would deſire ſuch as are apt to be too cenſorious, to take the pains to gather a few of theſe Seed-veſſels, and examine them as I have done, and then ſpeak what they find, and believe no more than their own ſenſe and reaſon will inform them, and they may eaſily ſee, that what I have aſſerted, will be rather ſhort of than exceed the real numbers. Now if this Shell of the Seed be thus ſmall, how much ſmaller muſt needs be the rudiment of the Plant that lies encloſed within it? And how eaſily may ſuch Seeds be drawn up into the Air, and carried from place and place, even to the tops of the higheſt Towers, or to places moſt remote, and be ſowed by the paſſing Air, or falling drops of Rain, on the boughs or branches of Trees, ſides and tops of Walls, Houſes, or Steeples? And it is not in the Art of man to leave Earth expoſed to the common Air, and to exclude the entrance, or prevent the ſowing of theſe imperceptible Seeds; and therefore it is not to be wondred at, that, if any earth, though never ſo pure, be expoſed to the Air and Rain, though at the top of a Steeple, it will produce Moſs.

Further inquiry may poſſibly inſtruct us, that there may be Seeds of Muſhroms, Mould and other Vegetables of as ſmall, if not ſmaller, bulk, which may be diſperſed and mingled with the Air, and carried to and fro with it, till waſhed down by the falling drops of Dews or Rains; which, if they chance to light on a convenient ſoyl, do there Vegetate and ſpring up; but dye and periſh, if the ground, they light on, be not natural and agreeable. But whether this conjecture hit right, further obſervation muſt determine.

This

This diſcovery I made the year after the late Fire of *London*, to wit, in the year 1667. there being then vaſt quantities of it to be found every where diſperſed among the Ruines left by that Fire, which made me, I confeſs, very much wonder at firſt how ſuch vaſt quantities ſhould come to be then ſo ſuddenly rooted, and was the occaſion of my more ſtrict examination of it. This I preſently ſhewed to many of my Acquaintants, and the next year 1668. upon the eleventh of *June* I brought an account of it into the *Royal Society*, where I ſuppoſe it may yet remain upon their Regiſter; and it was not a little ſurpriſing to all that ſaw it, when they conſidered how exceedingly ſmall each particular Seed was, and yet how infinitely vaſt the number of them was produced by each Plant. How prodigiouſly ſmall the firſt beginning and rudiment of that Plant muſt be that was produced by it; now, though indeed the Plant it ſelf be one of the ſmalleſt, yet this Seed of it was much ſmaller in compariſon to the Plant than the Seeds of moſt other Plants compared with theirs. But about two years after this I received from a very good friend of mine at *Briſtol*, the Ingenious and Inquiſitive Mr. *W. C.* a Relation of ſome later diſcoveries of his, which ſeemed much to outſtrip even this, whether the comparative magnitude of the Plant, and of the Seeds, or the number of the Seeds, or the curioſity of the Seed-boxes, or the ſtrange way of ſowing and diſperſing, or the place and manner of the Seeds production be conſidered. As they were ſent to me by him in a Letter from *Briſtol*, dated *September* 30, 1669. take them in his own words and deſcription.

NOtwithſtanding my many other Avocations, variety of diſcoveries do almoſt every day inlarge my experience; but more eſpecially this laſt Week I was very happy in the detecting of that which all the Philoſophers and Phyſicians of former Ages, have been ignorant of, as we may well imagine from what remains we have of them. My Diſcovery in ſhort was this, that all the kinds or ſpecies of Ferns together with

with all the like Capillary Plants their Congeners are (though generally denied to have any at all) more abundantly prolifical in Seeds than any other Plant beside, especially the common Female Ferns or Brakes, and those more elegantly formed (I mean chiefly in the little Vesicles conteining the Seed) than many others, among the hundreds I have observed. To make it demonstrable to you, I have now sent you both the Plants with the Seeds on them, and the Seeds of the same Plants apart in Papers by themselves, which I took off from other Plants of the same kind, having plentiful parcels of each (excepting of what I have not sent you) this being the season of perfecting their Seeds. I thought to have sent you draughts of the Seed Vessels, as they appeared presently after gathering, but could not. I presume some of the Vesicles or little boxes may remain whole, so that by your Microscope you may see their true figures and distinctions, some of them being more flatted on each side the little ring or embossed girdle encompassing them, others more swelling.

Also those little rings or bands encompassing the boxes are different, in some of the kinds broader and flatter, in others rounder, and standing up higher, yet all agreeing in the principal parts of their form. I purpose to draw the figures of them all as they appear by the Microscope, together with their Seeds, and to add descriptions of all circumstances considerable, and joyn them to the rest of my draughts of that kind. Some particulars most considerable I now give you in the fol-following account.

1. The little boxes containing the Seeds are in most of these Plants not half, and in some not above one third, or one quarter as big as a very small grain of common white sand; appearing like little bladders infolded with rings or bands, shaped like certain little worms I have met with, which may be referred to the Teredo's and Eruca's.

2. As near as I could compute, some of these bladders contained about 100 Seeds, which were so exceeding small, as to be wholly invisible to the naked eye, and indiscoverable without a Microscope.

3. The

3. The Leaves of both the Ferns, especially the common Female Fern, (which is more abundantly stored with Seed than any of the rest) and the other I now send you, being kept close without bruising, and soon after gathering exposed to the Sun, or dry Air, the bands of as many of them as are ripe, will contract themselves and break, and fling their Seeds all about, after the same manner as some other small Plants, such as the Persicaria Siliquata, *and some of the* Cardaminas *are observed to do. This I have observed with a single convex glass as well as with the Microscope, but with the latter only I could discover the falling of the Seed. And a pretty quantity of the Seed being rubbed or brushed off from the Leaves upon a fine piece of Paper or Parchment, and sweeped together into a heap, many of those boxes breaking together, and justling one another would make the heap seem, as it were, full of Mites or living Creatures, even to the bare eye; and if the place be free from noise, and the Ear be close applied, the crackling of them upon breaking may easily enough be heard, and upon running over the Paper with a Microscope the Seeds will be found dispersed, and thrown at a great distance.*

4. The figures of the Seed-vessels, as also of the Seeds of all the Ferns and those their Congeners, called Capillary Plants, are very near of the same shape and size, notwithstanding the vast disproportion between them, as particular common Fern, Wall Rue, Harts Tongue, and Osmond Royal, the first three of which being very remarkable for their unlikeness to each other, and the last chiefly for its excelling so many thousand times in magnitude that of Wall Rue. Which observations may seem to confirm the opinions of some learned Botanists that the affinity of Plants are to be judged by the figures of their Seeds.

5. That Osmund Royal, which excelleth all the other Ferns both in greatness, comliness, and vertues, and which hath been accounted barren, with the rest hath Vessels and Seeds of the same figure with the other, and very near of the same size, the extreme smalness of which, even to invisibility, and the greatness of the Plant, one root whereof, with all the growth out of it, I have found weighing ten pounds and bet-

H ter,

ter, is ſurpaſſingly more wonderful than that of Moſs Seeds; of which I have ſome kinds of them bearing Seeds, that a great number of them, with their Roots, Stalks, Leaves, and Seeds, do not weigh a Grain. Beſides, I have found of the common Female Fern ſome which have been from the Roots to the utmoſt top of the Leaf nine foot high, and within theſe three days meaſured the common broad-leaved Male Fern ſix foot and an half long; ſome of the Leaves of which are among thoſe I now ſend you.

6. But that which appeared moſt admirable, both to me and ſome other Gentlemen that were witneſſes of it with me, was the many differing kinds of ſmall living Creatures, wholly inviſible to the naked eye, and even through largely magnifying ſpectacles, though ſome of them were to be ſeen through a deep Convex glaſs; but with a Microſcope, when the Plant was newly gathered, they might be ſeen nimbly running up and down among the Seed-veſſels, and ſome of them were ſo ſmall as not to be above twice as big as the ſmall Seeds in the bladders; a deſcription of ſome of which I may hereafter ſend you.

I have incloſed in the box ſent you twelve ſorts of Plants of this tribe, being the greateſt part of the number, and only ſeven ſorts of the Seeds; thoſe wanting are the Cetrach, Wall Rue, Maiden-hair, and Polypody, of which notwithſtanding you may ſatisfie your ſelf in the mean time till I can ſend them green by thoſe ſmall parcels of the Plants which you will find amongſt the reſt, though by keeping they are withered.

The Seeds of the Ferns through a very excellent Microſcope appeared of the bigneſs of a ſmall Vetch or Seed of Lentiles to the naked eye, and ſome of them ſhrink like the ſides of white Peaſe, with ſmall regular knobs and hollows. Thoſe of Polypody are differing in colour and ſhape being yellowiſh, as the others are brown, red, and formed like the Seeds of the ſmaller Medicas that is of a Kidney ſhape. All the reſt I found very near of the ſame form. I cannot omit what I obſerved in Cetrach, which Plant I have heretofore often conſidered, and wondred at the ill-favoured roughneſs on the under

der ſide of the Leaf, appearing like the fleſhy ſide of tann'd Leather, being wholly ignorant what Nature meant in it, but now by my Microſcope I find it a very pleaſant object differing from all the reſt, wherein the curioſity of Nature (in a Plant ſo abject as that appears) is ſhewn beyond imagination. This, when freſh gathered, and not bruiſed, appears through the Microſcope like fine thin Membranes, ſuch as the Wings of Flies, chequered with figures after the manner of Honeycombs when the cells are full of honey, and cloſed with Membranes, amongſt which, as in ſo many Cells, lie the Seed-veſſels, ſhaped as before is mentioned. I doubt not but you have read the ſtrange ſtories and fabulous conceits of Authors about Fern Seeds. But Parkinſon *is more Orthodox in ſome things than any of them: For he poſitively concludes from* Gen.1.11, 12. *that all Plants have their Seeds, and conſequently Fern; where if he had ſtaid, he had aſſerted a general truth: But in coming to particulars, he affirms as great an untruth, in ſaying,* fol. 1036, *and* 1037. *that the Seed is ripe at Midſummer, according to the old traditional Fable, and tells how it may be gathered; whereas now is the very ſeaſon of their ſeeding, and at Midſummer this and the reſt are not come to their full growth, before which no Plant ſeeds. That duſtineſs which he ſpeaks of, and calls the Seed, is no other than what is found on divers other Plants, being an irregular Duſt, and is not found on the borders of the dents of the Leaves on the under ſide, on which the Seed grows, but all over ſprinkled on both ſides, and not found when it is fully grown. This he affirms of the Male Ferns, which are all differing very notably from the common Female Fern, concerning which the fabulous tradition is held. But after in the following Chapter of the Ferns and their Relatives now ſent you, he ſeems to give over his Scripture Propoſition, and, ſpeaking of the Seeds, ſays no more but that they have ſpots, daſhes, ſcales, or marks on their back-ſides. And of the Oſmund Royal (ſpeaking of the buſh at the top of the Plant) ſays it is accounted as the Flower and Seeds. And of the* Lochitis aſpera *ſays plainly they have none at all. Of this laſt I am yet to enquire, but doubt not I ſhall find that it hath*

H 2 *Seed*

Seed like the reſt. Of all which Gerrard *and* Johnſon *his Corrigitor gravely concludes (having indeed no demonſtrable ground to the contrary) that ſome have been too raſh in affirming Ferns to have Seed. I intend next Summer to obſerve whether theſe hitherto unknown Seeding Plants have Flowers. In the mean time* I am, *&c.*

Briſtol. *September 30. 1669.*

W. C.

Maculæ in Sole.

DUring this laſt great heat of weather in *June* I obſerved a very conſpicuous Macula with its immediatly incompaſſing Nubecula, and ſome other leſs conſpicuous Spots at a further diſtance paſs over the Disk of the Sun, and found that it was neareſt the middle when the heat was greateſt, that the heat increaſed as it came nearer the middle, and decreaſed as it departed from it. It may be therefore worth obſerving for the future whether the like weather do not happen upon the next appearance of the like Macula, ſince it ſeems not very improbable to ſuppoſe that the body of the Sun it ſelf may be much hotter when ſuch eruptions appear, thoſe Maculæ often times ending in Fæculæ. And the rather becauſe I am informed that this extraordinary heat hath not been peculiar only to *England*, but very general to *Europe*; what it hath been to other parts of the world further intelligence will inform us.

Upon a ſecond appearance of Spots in the Disk of the Sun at the latter end of *July* and the beginning of *Auguſt*, when at one time, to wit, *July* 29. there appeared about ſix greater and ſmaller in one knot with their proper Nubecules or Umbra's, the heat of the weather again increaſed to a very great degree, and abated as they drew toward the Limb, and grew fainter. But it hath now ſince the diſappearing, *viz.* on the fourth of *Auguſt*, been exceeding hot alſo, though I do not find any Spots this ſeventh of *Auguſt*, it may therefore poſſibly be that other parts of the body of the Sun may have an extraordinary inflammation

inflammation which may cauſe ſo fervent and laſting heats as have hapned this Summer. At leaſt this Hint may deſerve ſome farther Inquiry, for though probably it may not be attained to predict the appearances of thoſe Spots, yet poſſibly the appearances of the Spots may ſerve to predict the future conſtitution of the weather. At leaſt it ſeems worthy remarking that the greateſt heat that hath been in the Air this year was on that day of *June* when the firſt Spot was near the middle of the Sun.

POSTSCRIPT.

THe Publiſher of Tranſactions in that of *October* 1675. indeavours to cover former injuries done me by accumulating new ones, and this with ſo much paſſion as with integrity to lay by diſcretion; otherwiſe he would not have affirmed, that it was as certain that none of my Watches ſucceeded, as it was that I had made them ſeveral years ago: For how could he be ſure of a Negative? Whom I have not acquainted with my Inventions, ſince I looked on him as one that made a trade of Intelligence.

Next whereas he ſays I made them without publiſhing them to the world *in Print*, he prevaricates, and would have it believed that they were not publiſhed to the world, though they were publickly read of in Sir *John Cutlers* Lectures before great numbers at ſeveral times, and though they were made and ſhewn to thouſands both Engliſh and Foreiners, and writ of to ſeveral perſons abſent, and though they were in the year 1665, in the Hiſtory of the *Royal Society* publiſhed to the world in Print, becauſe, forſooth, they were not printed in his Tranſactions.

Thirdly, whereas the Publiſher of Tranſactions makes a long ſtory of my ſeeing his Journal *De ſcavans*, and my deſiring to tranſcribe that part of it which concerned this matter, as if I had requeſted ſome ſingnlar favour thereby. I anſwer,

Firſt,

First, that he knew I deſigned preſently to have printed it with Animadverſions, but he endeavoured to prevent me, deſigning firſt clancularly to get a Patent of it for himſelf, and thereby to defraud me.

Next, I ſay, I had a right without his favour to have ſeen, peruſed, and copied it, as I was one of the *Royal Society*, the intelligence he there brings in being the *Societies*.

Then it is denied that the Deſcriber of Helioſcopes well knew that the Tranſcriber of Intelligence would publiſh it in his Tranſactions, though it was believed if the publiſhing it would injure me it would not be long concealed; which was the ſole reaſon of Printing in the ſame Tranſactions, *viz.* 112. a Letter which he had ſeveral years before.

Thirdly, Whereas he aſſerts that ſeveral diſcoveries of the Accuſer had been vindicated from the uſurpation of others. It is anſwered, the clean contrary is upon good grounds ſuſpected from the Publication of a Book about Earthquakes, Petrifactions, *&c.* Tranſlated and Printed by H. O. the manner of doing which is too long for this place. Such ways this miſ-informer hath of vindicating diſcoveries from the uſurpation of others.

To his upbraiding me with his having publiſhed ſome things of Mine; I anſwer, he hath ſo, but not ſo much with mine as with his own deſire, and if he ſend me what I think worth publiſhing I will do as much for him, and repay him in his own coyn.

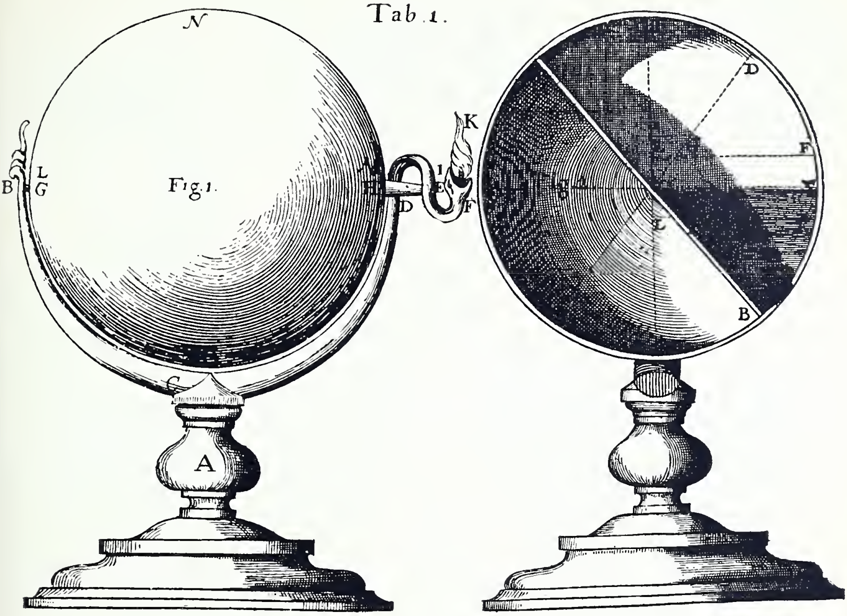
Laſtly, Whereas he makes uſe of We and Us ambiguouſly, it is deſired he would explain whether he means the *Royal Society*, or the Pluralities of himſelf. If the former, it is not ſo, as I can prove by many Witneſſes; if the later, I neither know what he is acquainted with, or what has been imparted or explained to him.

So not deſigning to trouble my ſelf any further with him, unleſs he gives me occaſion, I diſmiſs him with his

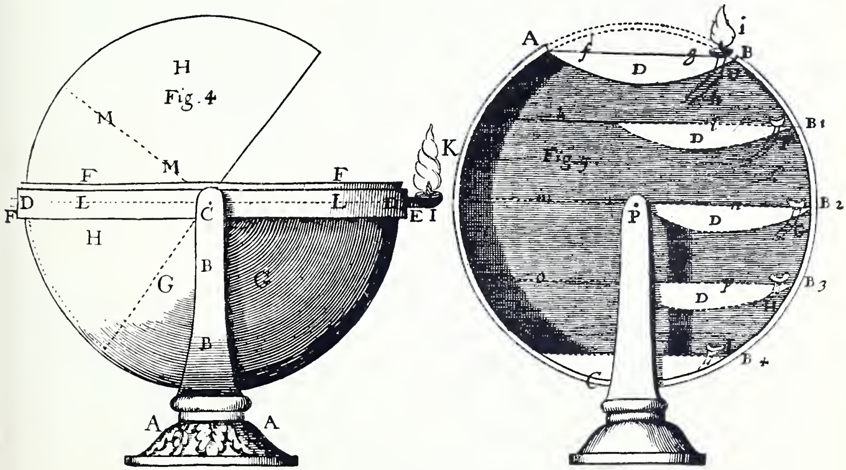
——— *Speque metuque*
Procul hinc procul ito. Ho.

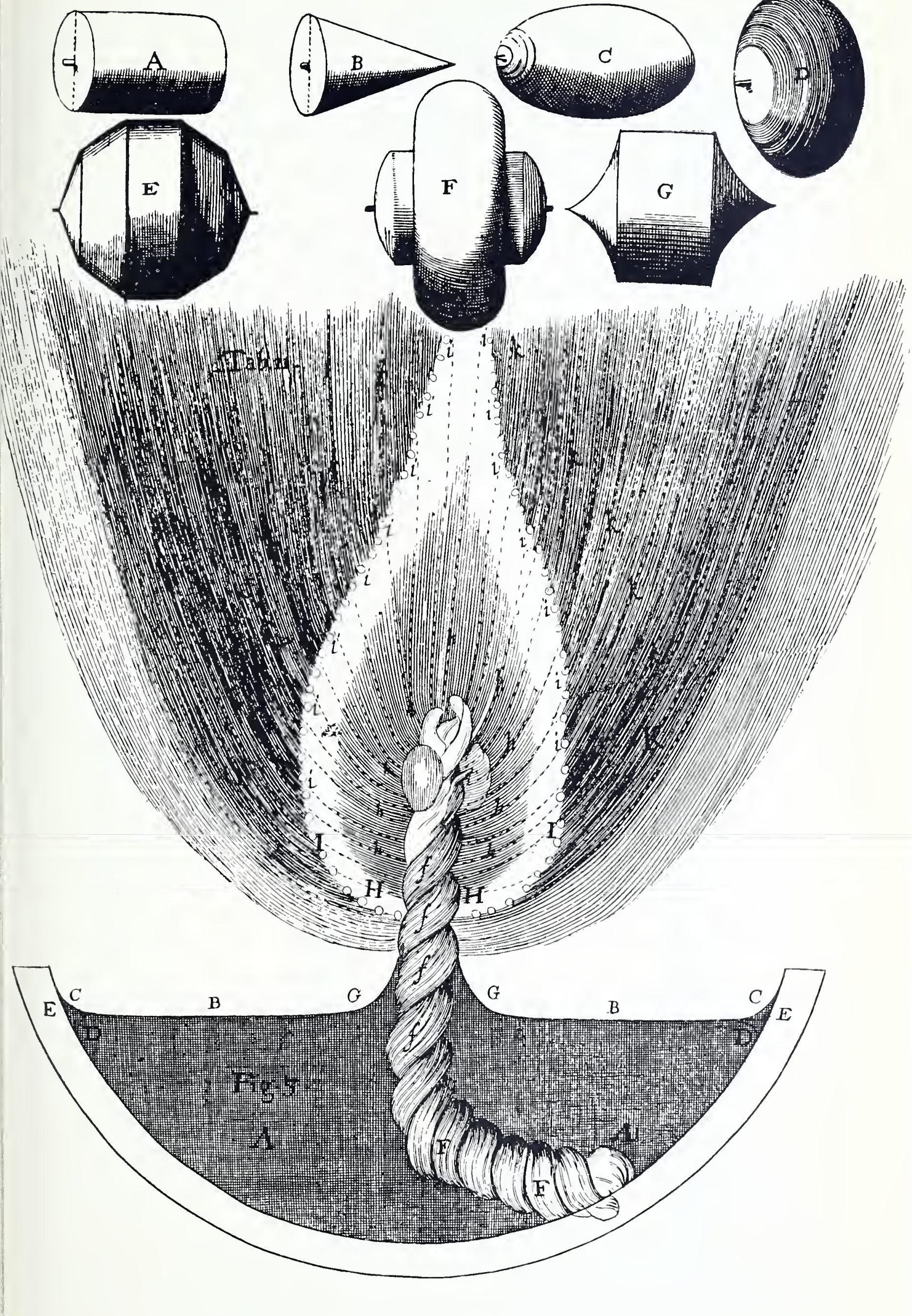
FINIS.

Tab. 1.



Tab. III

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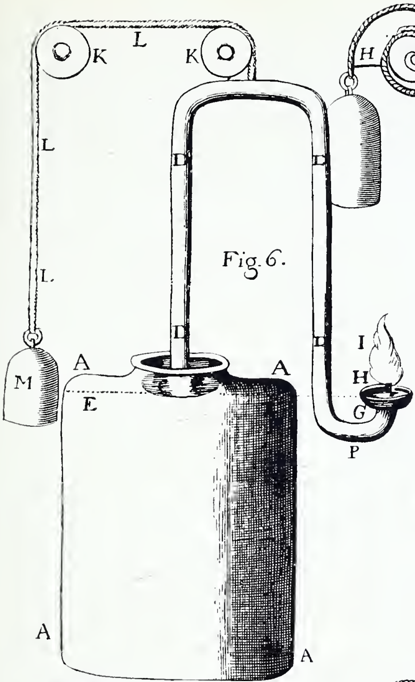


Fig. 6.

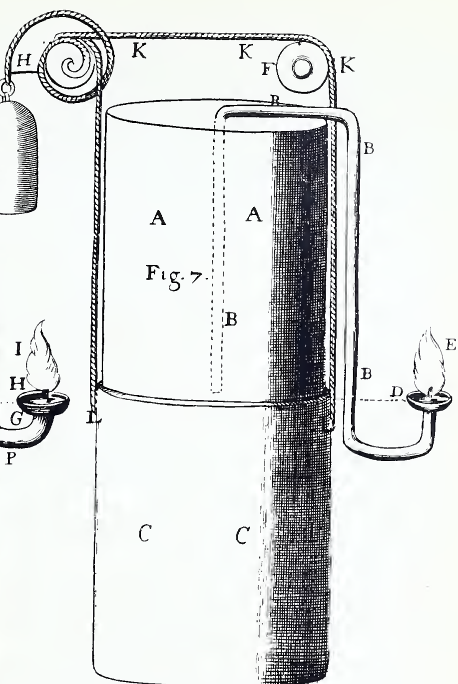


Fig. 7.

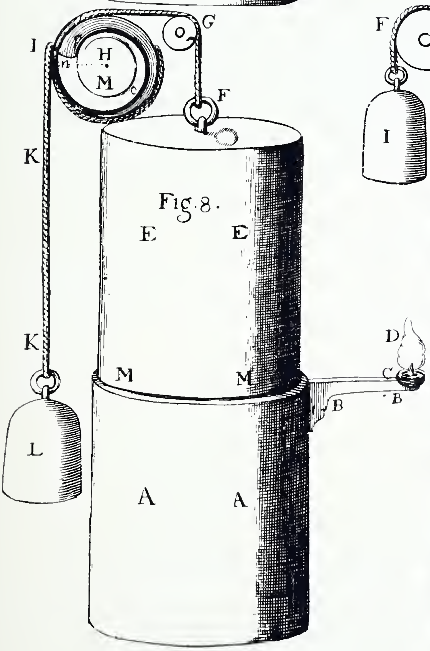


Fig. 8.

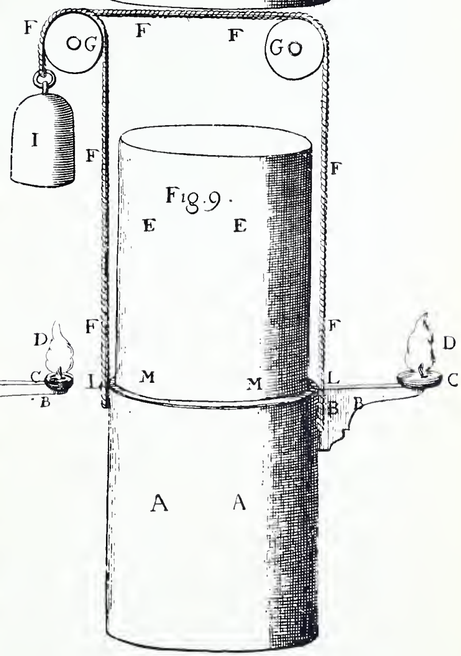
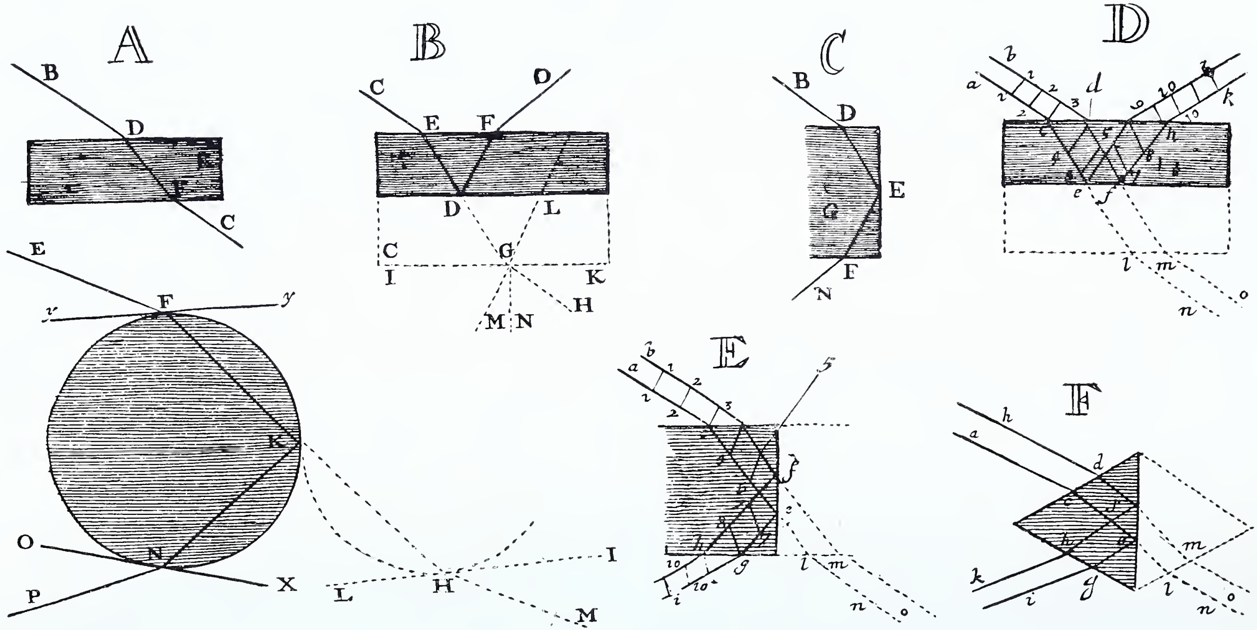


Fig. 9.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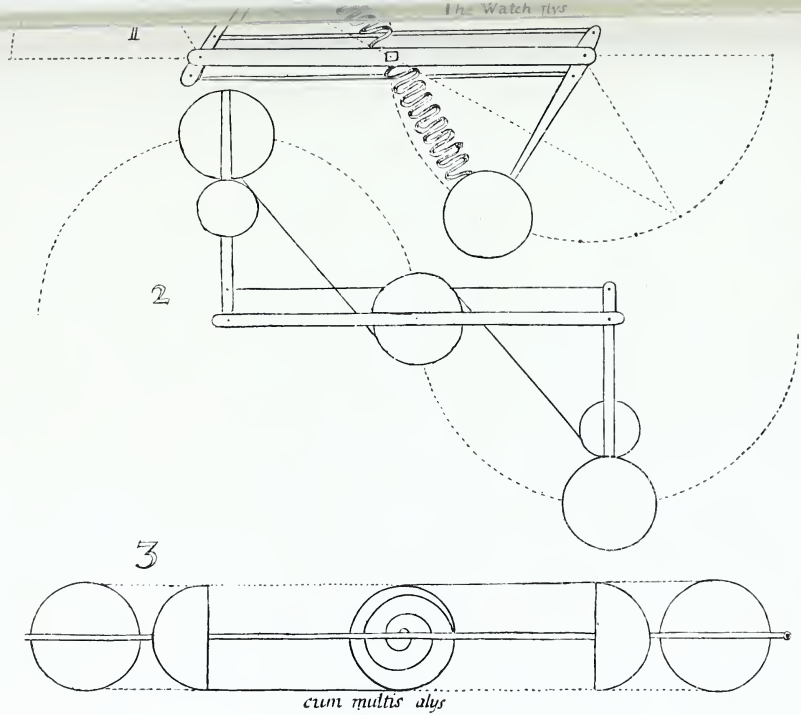
The Watch flys
1
2
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cum multis alys

LECTURES
AND
COLLECTIONS

Made by

ROBERT HOOKE, Secretary of the *Royal Society*.

COMETA.

CONTAINING

- Obſervations of the Comet in *April*, 1677.
- Fragments of ſeveral Lectures about thoſe of 1664. and 1665.
- Sir *Chr. Wren*'s Hypotheſis and Geometrical Problem about thoſe Comets,
- A Diſcourſe concerning the Comet of 1677.
- Mr. *Boyle*'s Obſervation made on two new Phoſphori of Mr. *Baldwin*, and Mr. *Craft*.
- Mr. *Gallet*'s Letter to Mr. *Caſſini*, together with his Obſervation of ☿ *ſub* ☉.
- Mr. *Caſſini*' Reflections upon thoſe of *Gaſſendus*, and *Hevelius*, and upon this.
- Mr. *Hally*'s Letter and Obſervation of the ſame made at St. *Hellena*.
- Mr. *Caſſini*'s Obſervation of the Diurnal motion of ♃, and other changes happening in it.

MICROSCOPIUM.

CONTAINING

- Mr. *Leeuwenhoeck*'s two Letters concerning ſome late Microſcopical Diſcoveries.
- The Author's Diſcourſe and Deſcription of Microſcopes, improved for diſcerning the nature and texture of Bodies.
- *P. Cherubine*'s Accuſations anſwered.
- Mr. *Young*'s Letter containing ſeveral Anatomical Obſervations.

LONDON:

Printed for *J. Martyn*, Printer to the *Royal Society*, at the Bell in St. *Paul*'s Church-yard, 1678.

VIRO PERILLUSTRI

D^no *JOSEPHO WILLIAMSON*

EQUITI AURATO,

Serenissimo *CAROLO* II°.

Mag. Britan. Fran. & Hibern.

R E G I,

A Consiliis Secretioribus, et a Secretis Status,

Nec non

SOCIETATIS REGALIS

LONDINENSIS,

Ad Scientiam Naturalem promovendam

P R Æ S I D I

D I G N I S S I M O.

NEC *potui, nec debui,* Nobilissime Vir, *cujusquam aliûs nomen his Chartis inscribere, præter* Tuum.*Sub* Te *natæ,* Tibi *vitam debent*; Tibi

A 2

bi *quoque debebunt quod lucem aſpiciant. Egregius ille* Tuus *animus ad inſtaurandam Philoſophiam arteſque adeo omnes utiles, mihi homini, alioquin ſubtimido, audaciam hujus dedicationis fecit. Ego quæ nunc potui, profero, magis ad Gratulationem oſtendendam, quam Eruditionem. Spero autem, quemadmodum ſub* Tuo *PRÆSIDIO majora indies Augmenta* Scienti*arum in hâc gente fiunt, ita exorituros viros doctos, qui* Tibi *juſta præconia laudum perſolvant; quod ego præ tenuitate ne conari quidem audeo, quanquam cum primis ſim*

Dignitatis & Honoris Tui

Studioſiſſimus,

ROBERTUS HOOKE.

SYNO-

SYNOPSIS.

THE Comet seen *April* 21. 1677. between the Triangle and the Cloud of ♈, its tail not directly opposite to the ☉, its Magnitude, Brightness, Head, Nucleus, Blaze, (1.) Why sometimes shorter, sometimes longer; without sensible motion of parts, Explanation of the first figure, as seen by the eye. (2.) Of the second Figure, as seen through a glass, of a parabolick termination, differing from the representations of Mr. *Hevelius*. (3.) The Medulla, and blaze with the manner of shortning and lengthening, explained by the third figure; not seen the 22d. but the 23d. The bigness of the Nucleus and Head through a Telescope, compared with the top of a Tower. (4.) The place it then appeared in. Why the motion was not more exactly observed. Its blaze still not opposite to the Sun. The 24th. not seen, nor 25th. (5.) though the Sky clear by reason of the height of Vapors. How they do lengthen the *Crepusculum*. Why Physical Remarks only were made. (6.) Published in order to understand Objections, and propound pertinent Queries. Some Observations, Notes, Queries, *&c.* concerning the Comets in 1664. and 1665. here. Collected out of several scattered Papers and Lectures of them formerly read here imperfect. Queries of its substance, magnitude, density, mutability, dissolution, fluidity, gravity, light, figure, motion bended or straight, (7.) with equal or unequal velocity, in the Atmosphere or Æther, above or below the Moon. Whether it wasts, or lasts to return. The Star of a compacted light (8.) varied possibly from position, partly from real change, Tail transparent, Body supposed more dense, side toward the Sun evenly defin'd, Encompassed with a fluid yielding to motion, but dissolving its parts. Its light from its self. (9.) Its Nucleus supposed dense possibly as the middle part of the Earth, of which some conjectures. Dissolved by the Æther as in our Atmosphere. (10.) Argument for the looseness of the central parts of the Earth from the variation of magnetical direction. (11.) The Nucleus of Comets possibly the same. Internal motion may weaken gravitation. Parts separated may be agitated by the gravitation of the ☉. Tail made not so much by the particles receding as the Stars approaching the Sun. (12.) How the Comet may first lose its Orb in the Universe, and passing through the ſpheres of Activity of ſeveral central bodies is deflected and attracted by them, and the Blaze raiſed to a prodigious length. (13.) The bodies being attracted by ſome gravity, Blaze expelled by levity, explained by ſmoke, and ſteams. Somewhat for poſitive levity. (14.) A digreſſion concerning the method of ſpeculating the great and firſt principles of the Univerſe. The *Coma* and Blaze like ſmoke or flames. (15.) Shining particles a ſhining point, not a line of light. Conſiderations and Experiments about the ways light is augmented by, as by ſwift motion, adjacent dark *medium*, Flame explained. Why the Particles coaleſce into a ſtream. (16.) Enquiry about the magnitude and place of Comets. Many ſuppoſed them ſublunary. *Tycho* and *Kepler* proved them cœleſtial. How far we may rely upon Obſervations for Parallax. Parallax and its effects deſcribed. (18.) *Tycho* ſuppoſed the Comet of 1577. to move about the Sun. *Kepler* that of 1607. to move in a ſtraight line; that of 1664. had no ſenſible Parallax by what means it was found. (19.) Refraction in this way varies little. Theory of Comets defective as to Parallax hitherto. Parallax not to be enquired from the Obſervations of ſeveral men. Errors creep in from the Preſs and the Graver, as in *P. Gottignies* Plates. (20.) Nothing to be concluded from Obſervations made by perſons in differing places for want of accurate Inſtruments, and Obſervations. (21.) Even the beſt as *Hevelius*, *Gottignies*, *Petit*, or *Auzout* err. Some reaſon for this aſſertion. Moſt of the reſt altogether inſignificant. (22.) Want of Obſervers, Inſtruments, and Tables the cauſe. How theſe wants are to be ſupplied. What the world expects from Mr. *Hevelius*. (23.) And of how great uſe his Tables and Projections made by them will be. Parallax from diurnal motion failing. (24.) Other Parallaxes ariſing from other hypotheſes of the proper motions either of the Earth, or Comet, or both together conſidered ariſe to a certainty. (25.) Others depending upon other ſuppoſitions define nothing of the magnitude or diſtance of Comets. The inconvenience of *Tycho*'s, and alſo of *Kepler*'s Hypotheſes explained. A third way I have taken. What conſequences follow from it, (26.) As that it moves in a Circle that comes within the Earth Orb in ♌, and without ♃ Orb in ♒, a ſextant in 130 days, &c. This not relied on, becauſe there may be other hypotheſes to ſolve the phænomena; as that the Earth is unmoved, and the Comet moved in a Circle, whoſe convex ſide is toward the Earth. (27.) This hypotheſis explained by the ſixth figure. (28.) The diſtance and bigneſs of the Circle of the Comet undeterminable this way without a diurnal parallax, ſince the appearances may be ſolved by Circles of any bigneſs, proved by the eighth figure, (29.) Allowing inequality of motion, or more

more compound curve lines, nothing can be determined. The circular Orb it seemed the most probable solves *Kepler*'s acceleration, according to the increase of a line of Tangents. (30.)A gravitation towards the Sun makes out the motion of the Comet, and Planets, and of the Blaze. The Blaze explained by experiment of ♂ dissolved in oyl of Virt. (31.)This experiment and hypothesis farther explained, and applied to explain the Blaze which is from thence bent, brighter on one side than the other, not direct from the Sun. (32.) Cometical body and motion as old as the world, yet wasting in the Æther; explained by fire. Dissolution by *menstruums*. (33.)Thence the proprieties of Comets conjectured, and the sum of the foregoing discourse repeated, being the end of a Lecture. Recourse to *Tycho Brahe*'s Observation(34.)for making out the Comets Orb. His supposing its motion unequal without reason a shift. Mr. *Horrox* his hypotheses in the ninth figure a product of chance. (35.) A discourse on it, and some objections against *Tycho*'s. (36.) *Kepler*'s hypothesis examined by these Observations of *Tycho*'s, found the most likely, but with some alteration. Line of Trajection bent a little. Motion accelerated towards the Sun, retarded from it. (37.) The swifter and further off the Comet from the Sun, the less the bend, explained by the tenth figure. (38.)The way of enquiring parallax by Telescopes, (39.) further explained. A second way by two Observers in distant places propounded. The third way of Sir *Chr. Wren* his Majesties Surveyor-General, (40.)Set down and demonstrated by a Geometrical Problem. (41.) How exactly all those Observations he had were made out by it, together with his own Schemes; both which I had in the beginning of *Feb.* $166\frac{4}{5}$. (42.) Some other Papers about Comets added, being reflections on Mr. *Descartes* and *Kepler*'s hypotheses, from particular tracings of the Comets of 1664. and 1665. A Scheme of the later Observations of that of 1664. added, and some reflections, being all the papers could be found about those Comets. (43, 44.) Animadversions on this of *April* last. Why the former conjectures were adhered to concerning the light of Comets. (45.) Several sorts of shining bodies enumerated. (46) To which the light of the Comet seems to have most affinity, and how produced. (47.) Further described and explained. (48.) The reason of its parabolick figure demonstrated from the proprieties of motion from or toward a gravitating body, as the Sun. (49.) Concerning the wasting and lasting of the Cometical body. The bigness and nature of the Particles that compose the Blaze. (50.) Some difficulties in this supposition concerning the action of the Æther in levitation and ascent, dissolution, shining, &c. cleared and explained by Experiments. (51, 52, 53.) But would have been further examined by

Ob-

Observation if there had been opportunity. (54.) That these assertions about the light of Comets may not seem too paradoxical, some further Considerations and Observations about light are added, and some new ways propounded. (55, 56.) Mr. *Boyle*'s Memorial concerning a Phosphoros, written for his own use, inserted ; in which he first names the Author of it, and describes his *Apparatus*. (57, 58.) Then the observables. 1. Two spoonfuls of matter enlighten a large glass sphere. 2. A little enlightens a large Cylinder. 3. Liquor shaken had a smoke and flasht. 4. A dry substance affirmed to have continued shining 2 years, flashed. (59.) 5. Some dust of this on a Carpet twinckled like Stars. Writing on paper with it shin'd, and smelt of Sulphur and Onions. (60.) 7. The hand on which it was rubbed, shin'd, but felt no heat. (61.) It fired Gun-powder first warm'd. (62.) And white paper held over coals. Other tryals propounded, but refused. (63.) Some Experiments made on the Phosphoros *Baldwini in vacuo*, and in the open air. (64.) Preserved in *Vacuo*, but destroyed in Air. (65, 66.) Monsieur *Gallet*'s Letter to Monsieur *Cassini*, acquainting him with his *Apparatus* for observing ☿ in ☉. (67, 68.) His Observation of four spots in ☉. (69.) The particulars observed. (70. 71, 72.) Monsieur *Cassini*'s Reflections on these Observations. (73, 74.) Mr. *Hally*'s Letter to Sir *Jonas Moore*, containing an account of his Observations of ☿ *sub sole*, three Southern Stars. The two *Nubeculæ*, *&c.* (75, 76, 77.) Mr. *Cassini*'s farther discoveries about the diurnal motion, and several new appearances in ♃. (78, 79, 80.)

A second Discourse called Microscopium, or some new discoveries with Microscopes, in a Letter of Mr. *Leeuwenhoeck*. (81. 82.) A confirmation of some of them by Observations here. (83.) Mr. *Leeuwenhoeck*'s second Letter, containing Observations of the Globules of Blood, Milk, Flegm, Gums first dissolved, then precipitated out of the Spirit of Wine ; Eels a thousand times thinner than a hair. (84, 85, 86, 87, 88, 89.) The ways how these discoveries were made here. 1. By holding the liquor in small pipes, how fill'd, how made. The Lamp, Pipe, Oyl, Manner, Materials for making them described. (89, 90.) Muscovy-glass used instead of these Pipes, and how the Microscope was fitted for this purpose. (91.) What light convenient. Surfaces of bodies not perfectly fluid apt to delude an Observer. (92.) Plates removing that deluding cause, and what farther use of them. (93.) How to find the figure and texture of Animal and Vegetable parts. Instance in a ligament of Beef. (94.) The figure of Muscles hinted, and an instrument stretching them before the Glass described. (95.) A description of the Microscopes used, 1. Of the single Microscope, and its advantages and difficulties. (96.) another sort more easie described, and the ways how to make and use it explained. (97.) Causes that vary the distance of objects from the Globule. The use of Selenites and Looking-glass-plates, for holding the liquor. A Microscope of one single refraction. (98.) The only inconvenience of them hinted, how prevented by double Microscopes. Where these are made. (99.) The double Microscope, and its parts, uses, and advantages described. (100.) The benefit of a dark Room, and appropriated lights. And a digression in answer to P. *Cherubines* Accusation. (101.) Some Observations made with this Microscope hinted. Animalcules in the steeping of other Grains besides Pepper. Their smallness estimated, and compared to a Whale. Muscular fabrick hinted. Milk, Blood, Fat, Sugar, Allum, *&c.* viewed. (102, 103.) Mr. *Young*'s Letter of one who trying to cure a Colick by leaden Pills, slipt one into his Lungs ; grievous symptoms ensue. (105.) Helps of skilful Physicians in vain attempted, and particularly of Dr. *Mayow*, of suspending with the head downward; though in the interim he married and had Children, yet it kill'd him. (106, 107.) His body dissected, and remarkables taken notice of, and their causes explained by Mr. *Young*, (from 107. to 112.)

COME-

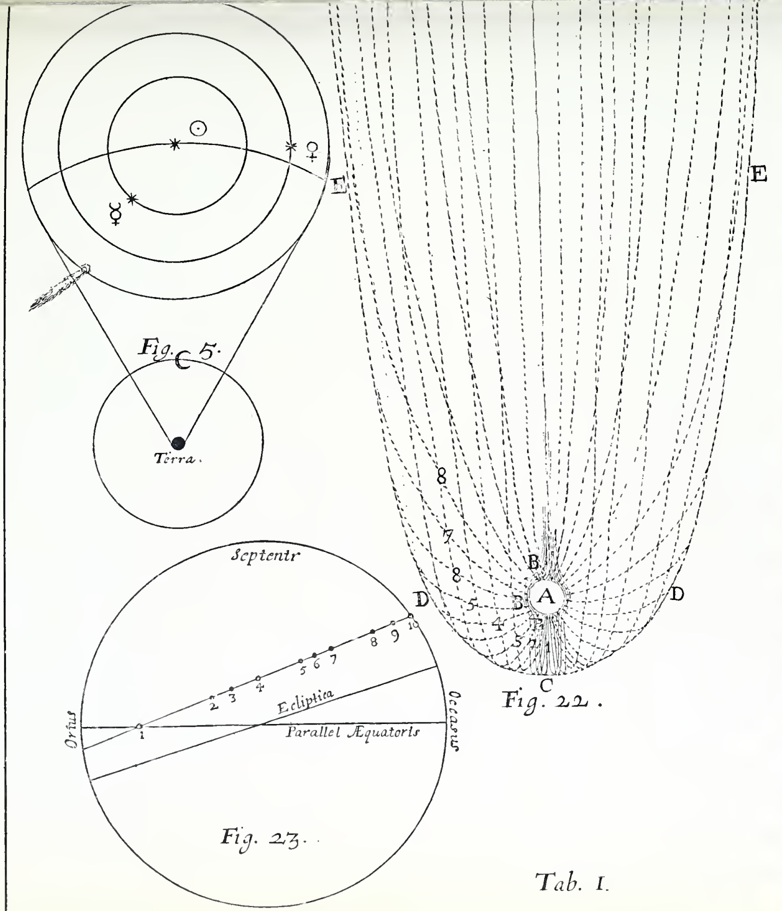

Fig. 5.
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Fig. 22.
Septentr
Orius
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Parallel Æquatoris
Fig. 23.

Tab. I.

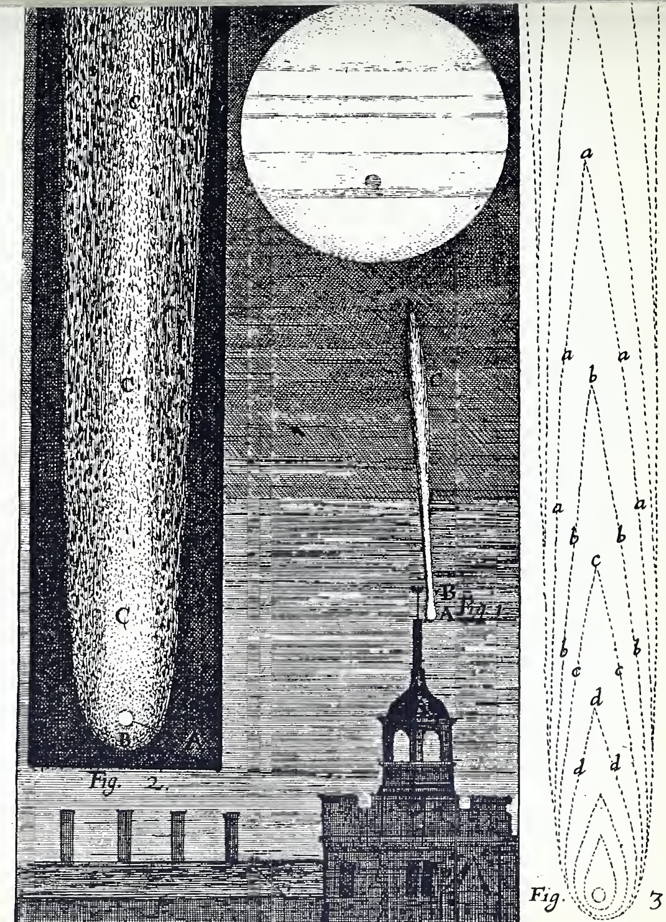

Fig. 1
Fig. 2.
Fig. 3.

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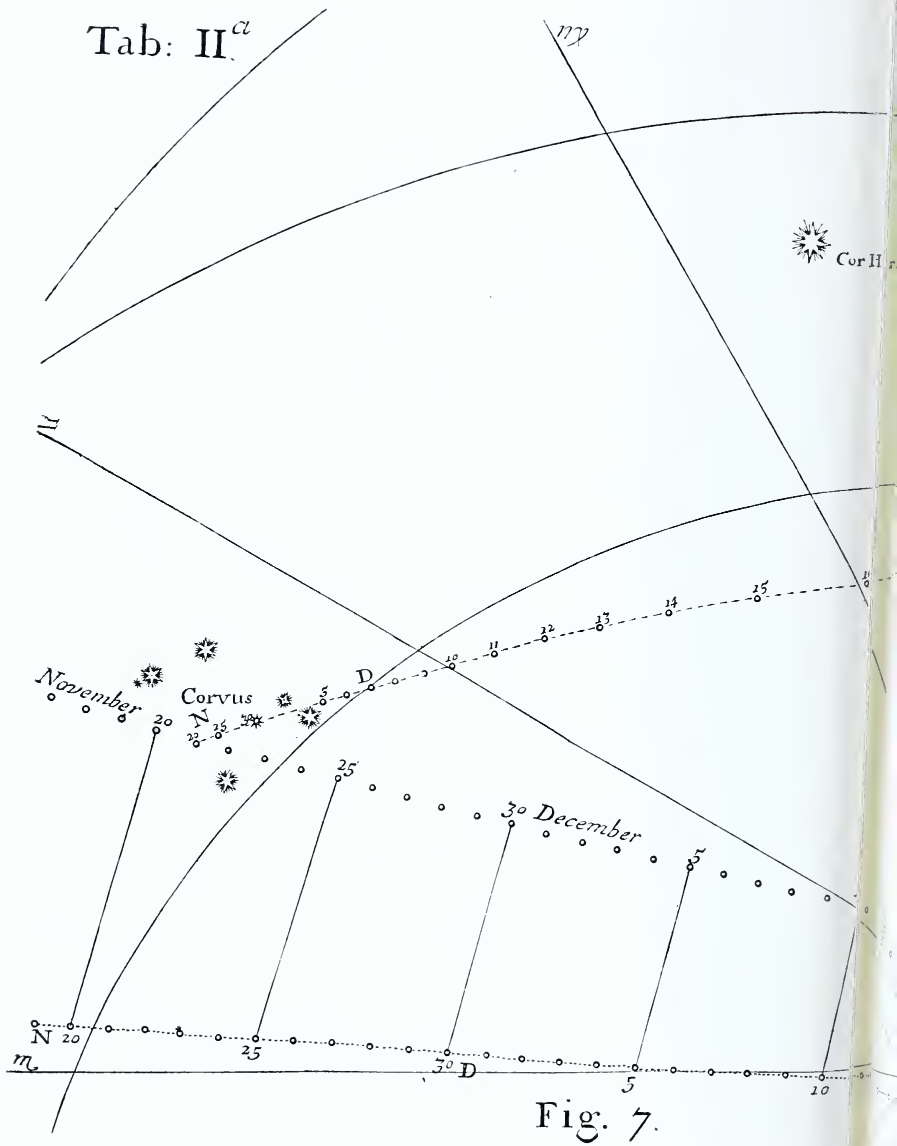

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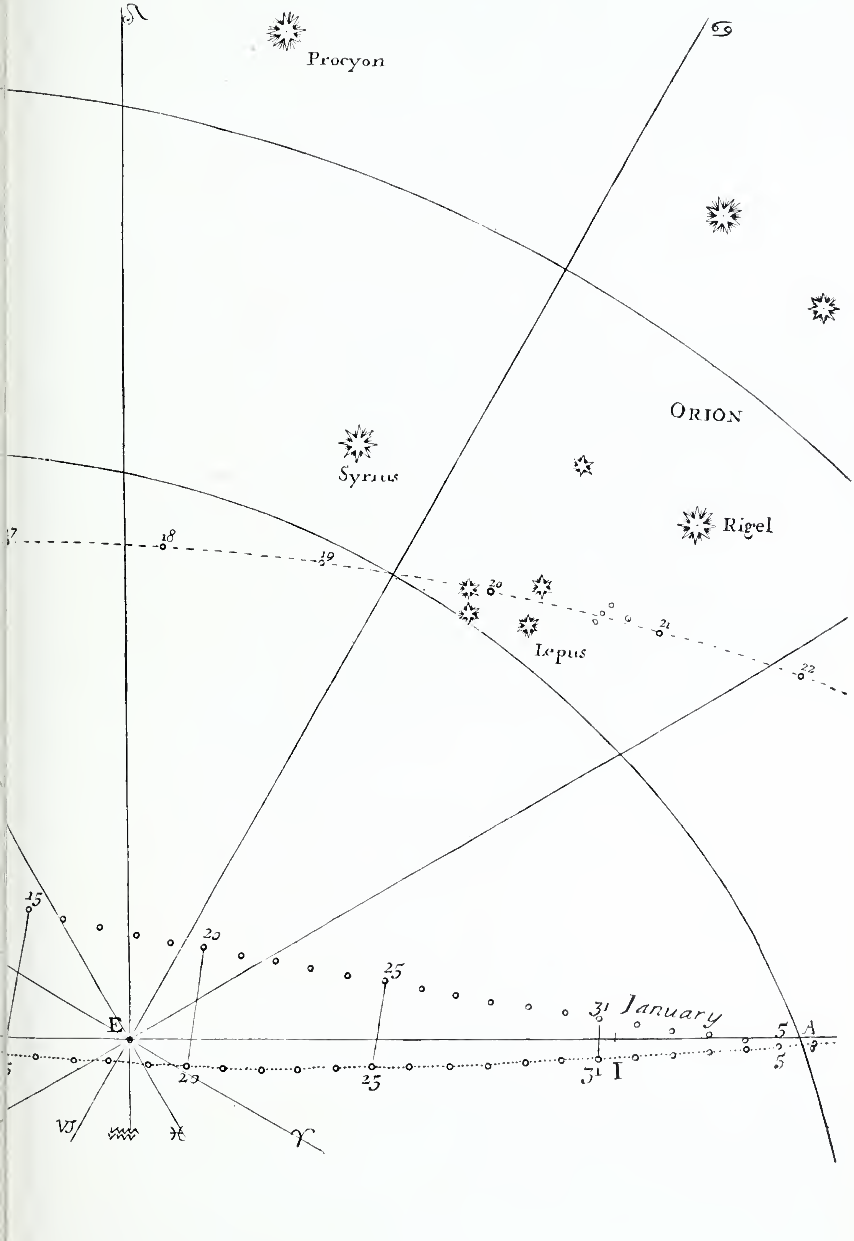
Fig. 7.



Procyon
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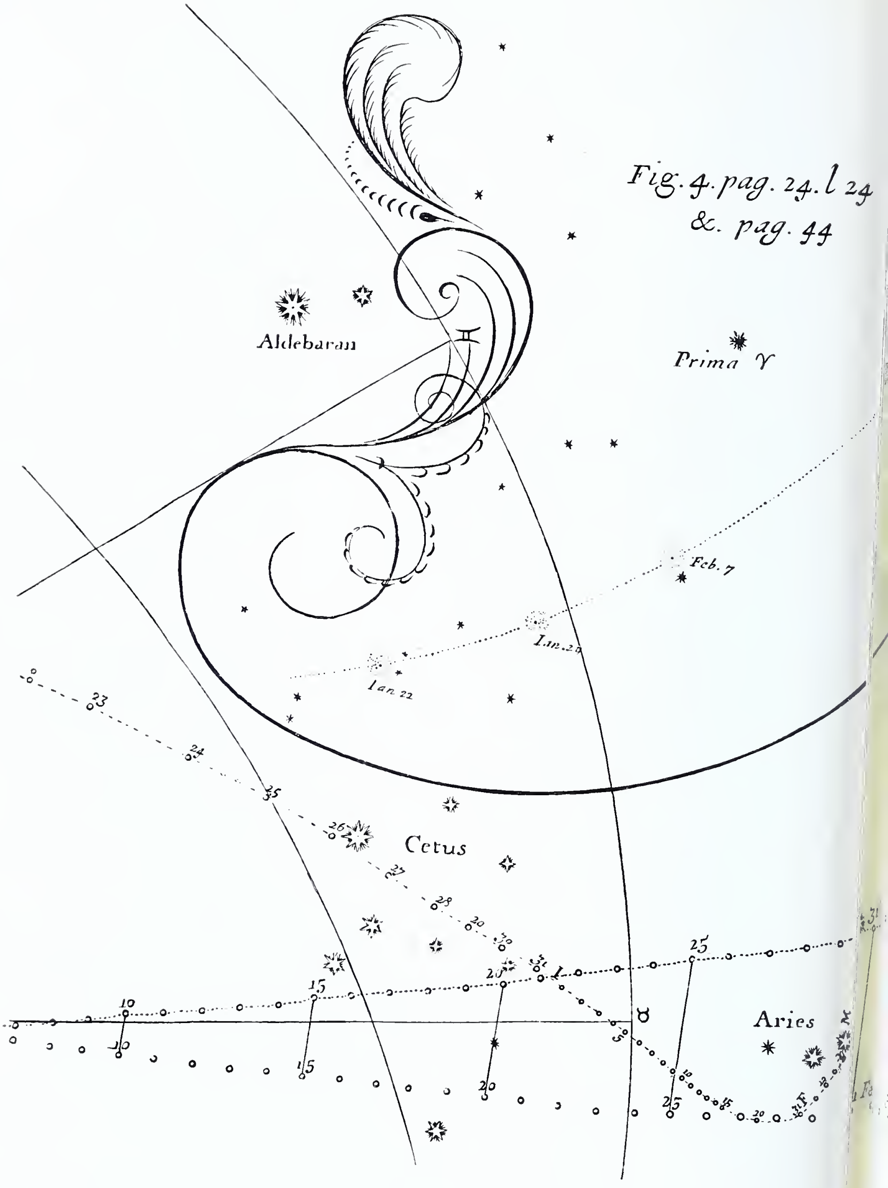


Fig. 4. pag. 24. l 24
&. pag. 44
Aldebaran
Prima ♈
Feb. 7
Ian. 22
Cetus
Aries

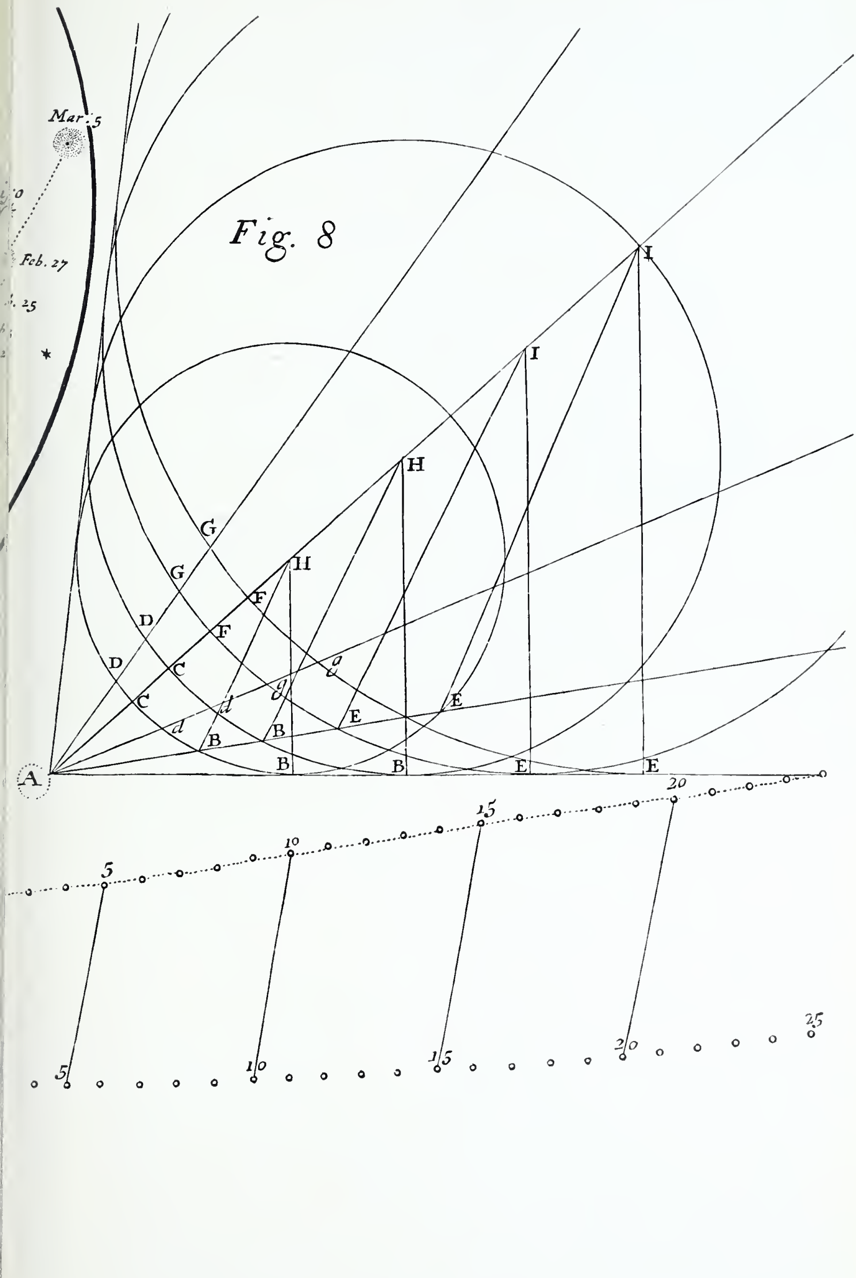


Fig. 8

Tab: III.a

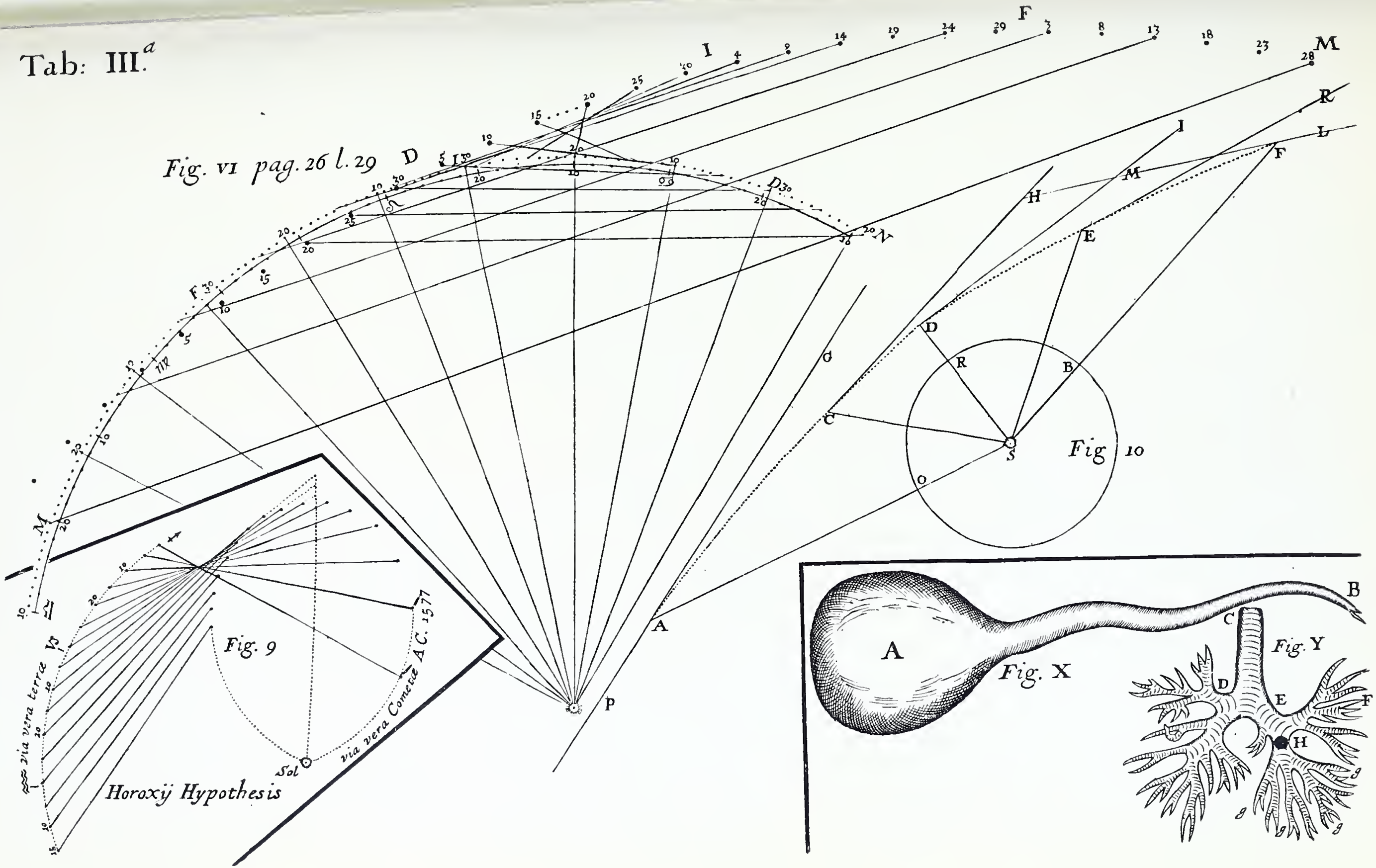
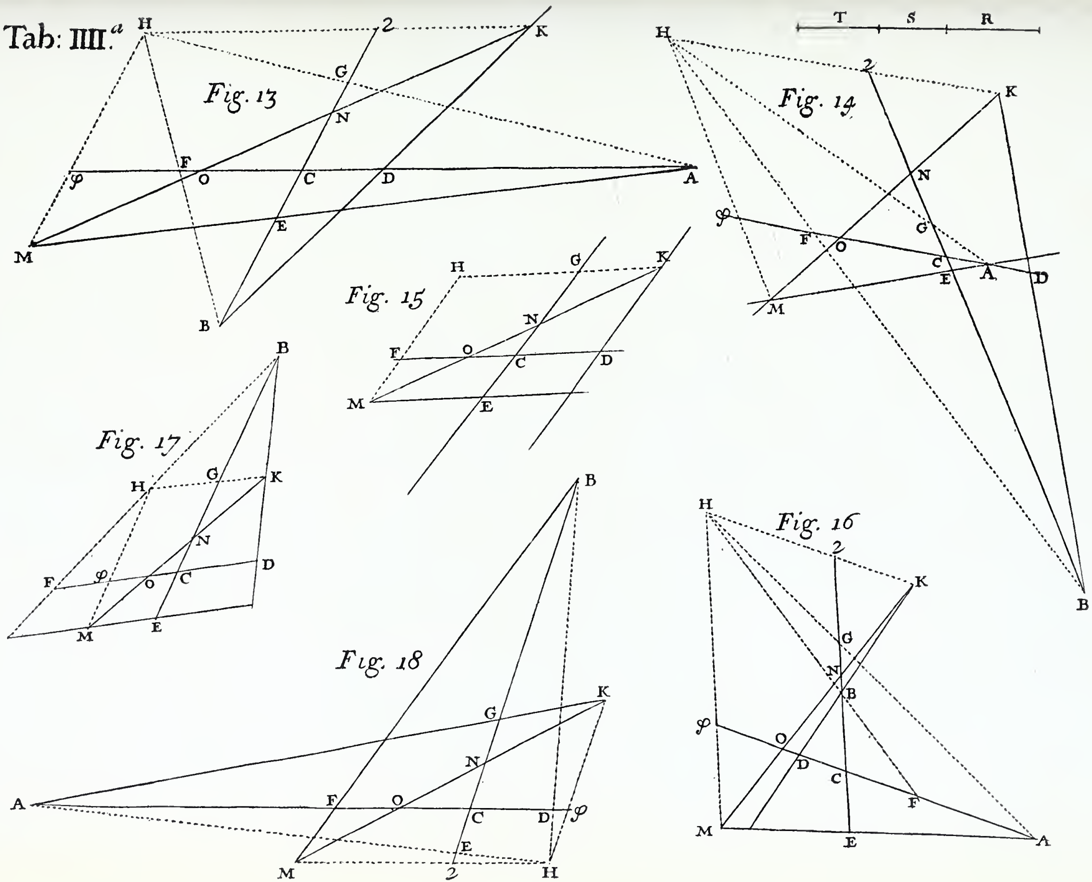


Fig. VI pag. 26 l. 29
Fig 10
Fig. 9
Horoxij Hypothesis
via vera terrae
via vera Cometae A.C. 1577
Sol
Fig. X
Fig. Y

Tab: IIII.ª



Tab. V.^{a} Hypothes^{is} D^r Chr^r Wren. Equ.

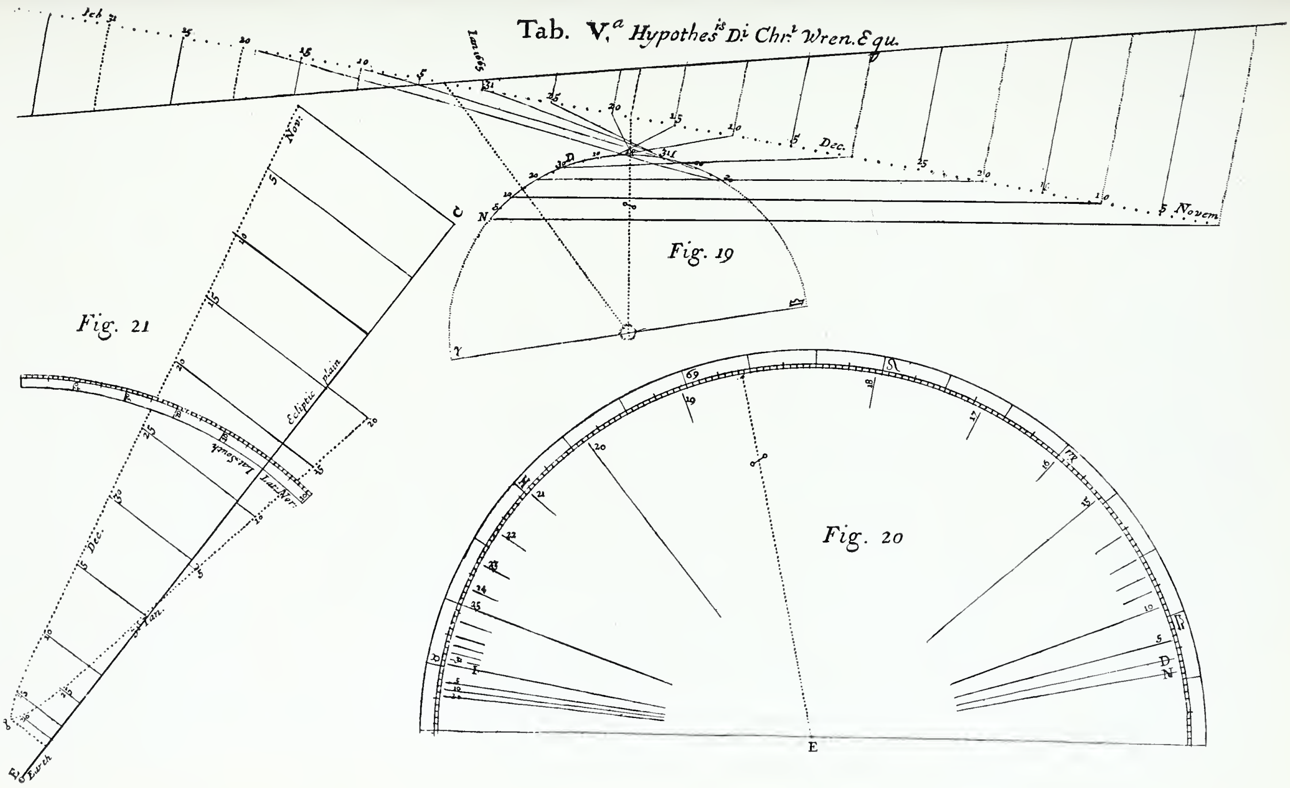


Fig. 19

Fig. 20

Fig. 21

COMETA,

OR,

Remarks about Comets.

ON *Saturday* morning, *April* 21. 1677. I firſt ſaw the Comet, of which I had been advertiſed the day before. It appeared in the Sign *Taurus*, between the baſe of the Triangle, and the unformed Stars in the Cloud of *Aries*; dignified by *P. Pardies*, with the figure of the Flower-de-luce. The head of it was in a right line, with the heart of *Caſſiopea*, and *Alamak*, or the South foot of *Andromeda*. and as near as I could judge by my naked eye (having no Inſtrument or help by me) it was $\frac{1}{6}$ of the diſtance between the feet and the Girdle of *Andromeda*, diſtant from the ſaid *Alamak* towards the South.

Its tail ſometimes as the Air was clearer and darker, extended about three quarters of its diſtance from the aforeſaid *Alamak*, and pointed directly at the Star in the noſe of *Caſſiopea* of the fourth Magnitude, and conſequently the head of the Comet pointed not directly at the Sun (the Sun then being about the eleventh degree of *Taurus*) but rather towards the fourteenth degree of the ſame Sign. Its appearance was very ſmall and ſlender, and as people commonly gheſſed, about two yards long; and the head about the bigneſs of a Star of the firſt magnitude, but of a much fainter and duller light. Its blaze about three o'the clock ſeemed to riſe ſtraight up-

B ward

ward, before that about half an hour after two it lean-ed a little Eaſtwards, or towards the right hand, and after three, as it roſe higher, inclined towards the left ſide or Weſtwards. The head to the naked eye was brighter than the blaze, and ſeemed to be ſomewhat bigger than that part of it which immediately joyn'd to the head; but thoſe parts of it which were farther di-ſtant, were of a much greater breadth; ſpreading wi-der and wider, as they were more remote from the head, and in the ſame proportion alſo growing fainter and fainter in their light, eſpecially towards the out-ſides: but the middle parts or *medulla* appear'd much longer, and the brightneſs much greater, which made the whole blaze to ſeem to taper, or be pointed towards the top.

The length of the Blaze appeared ſometimes ſhor-ter, and ſometimes longer, by ſeveral viciſſitudes; and as the day-break, or dawning increaſed, ſo the Blaze ſhortened, and eſpecially towards the ſides near the top, and ſhortly after before the Sun roſe, diſappeared.

But notwithſtanding this ſhortning and lengthening of the Blaze, I could not perceive any kind of motion in the parts of it, ſuch as is obſervable in flame, ſmoke, or other ſteams riſing from a burning or hot body: but the ſame parts of the Blaze ſeemed to appear and diſap-pear in their proper places as if they had been fixed and a ſolid body.

The firſt Figure I have here annexed will with ſome ſhort explications, repreſent the appearance of it to the eye, more plainly than by a multitude of words, with-out it 'tis poſſible to expreſs.

A, repreſents the head of the Comet, the middle of which appeared brighter than any other part; about which was a hazy light ſomewhat like the ſhining of a Star through a thin cloud; the lower part of which was pretty round and defined. B, the neck of it, which ſeemed to the naked eye of leſs Diameter, and leſs bright than the head, but through a ſix-foot glaſs, as I ſhall

ſhall mention by and by, it appeared bigger, though not ſo bright. The middle of this was very bright, and ſeemed to iſſue from the *Nucleus* or Star in the middle of the head. C, the bruſhy parts which were fainter and paler towards the ſides, eſpecially nearer the top, which made the whole ſeem to taper and reſemble the Figure here expreſt: Obſerving it with Teleſcopes (one of which was fifteen foot, and the other ſix foot long) I found the ſhape of it much like this, which I have repreſented in the ſecond Figure.

It had a pretty bright Star (if I may ſo call it) near the middle of the head, ſeeming much about the brightneſs of ♄ when near the Horizon, and was about 25 ſeconds in Diameter; as is repreſented by A, not perfectly defined, but hazy; the cloudy part or beard of the body encompaſſing it on all ſides: but that part of the *Coma* B, which was next towards the Sun, was the narroweſt: nor was this *Coma* well defined, but the outward parts of it were fainter and fainter. However they were regularly enough terminated to make the outwardmoſt bounds of it of a kind of Parabolical figure; the moſt bent part of which was towards the Sun, and moſt defined: And the bright Star of it was, as I have expreſſed it about four of its Diameters diſtant from the ſaid parabolical limb. The light parts of the ambient Cloud ſeemed to ſpread gradually towards that ſide of it, which was oppoſite to the Sun; but thoſe which were next the middle were the brighteſt: and always as they were farther and farther from the Star in the head, the fainter and paler they were.

I could not obſerve any repreſentations like thoſe which are given us by Mr. *Hevelius*, in his Cometography, neither in the Head, nor the Blaze, no more than I could in thoſe which appeared in the years 1664. and 1665. as may be eaſily taken notice of by comparing theſe which I have here delineated with thoſe.

The middle part of the Blaze CC, which aſcended from the Star in the middle, ſeemed the brighteſt, and

B 2 of

of this *medulla* or ſtem, thoſe parts were brighteſt which were neareſt ſituated to the ſaid Star. The ſides of it grew fainter and fainter, as they were farther from the head; and though they had brightneſs enough to make them appear in a dark and clear sky, yet the dawning quickly made them vaniſh, and diſappear, as did any hazineſs of the Sky: and according as the light increaſed, ſo was the Blaze diminiſhed, after the order of the tapering prickt lines expreſt in the third Figure by a a a, b b b, c c c, d d d, *&c.* and even in a clear and dark Sky, towards the farther end of the Blaze they often diſappeared for ſome ſhort ſpace of time, though the middle or ſtem continued; and ſo it cauſed the remaining appearance to reſemble the figure of a very ſlender birchen whisk or bruſh, much like that repreſented in the firſt figure.

The 22. from half an hour after two, till half an hour after three, the North-eaſt part of the Heavens to me was cloudy, and the Sky between the Clouds was hazy, and the dawning ſtruck much higher than the day before, ſo that I could not find it.

The 23. with ſeveral friends I obſerved it again, the Sky being clear, and confirmed my ſelf in all my former obſervations, taking again diligent notice of all circumſtances remarkable, both with my naked eye, and with Perſpective-glaſſes. And I had this morning a very notable obſervation in order to meaſure the bigneſs of the Star and its *Coma* which encompaſſed it, by comparing it with ſomewhat fixt: for ſome few minutes before three of the Clock the head of it paſt juſt behind the type or top-poſt of a tower not far diſtant, and was quite eclipſed by it; and as ſoon as it appeared to have paſt it, ſeeming yet contiguous, I obſerved it with my ſix foot Teleſcope, and found the Coma or whole head to appear full as big as the ſaid type or timber poſt, and the *Nucleus* or Star in the middle of it, to be very near of the ſame bigneſs of the iron ſpindle, upon which the weather-cock was fixt. Whence upon examining the bigneſs

bigneſs of the ſaid parts, ſince by an accurate Inſtrument I judge the head or Coma was about $4\frac{1}{6}$ minutes in Diameter, and the *Nucleus* or Star about 25 ſeconds. I took notice this morning that it had much altered the poſition in the Heavens, which it had upon Saturday morning, and that the Blaze of it was very much deflected out of the line it appeared in the laſt time. And with a ſmall croſsſtaff, taking the diſtance of it from *Alamak*, and from *Genib*, in the left ſide of *Perſeus*. I judged it to be in the mid-way between the Flower-de-luce aforeſaid, and *Algol*, or the head of *Meduſa*, that is, about 14 degrees of ♉, and 17 degrees of Northern Latitude: ſo that I judged its motion almoſt Eaſt, but a little deflecting South. I was not much ſolicitous of making obſervations of its true place, as not deſigning my preſent enquiry to be for what kind of motion it had, conceiving its motion to be towards the Sun, and ſo of very little duration: and expecting to hear an account of that from other places, and perſons that were better furniſhed with Inſtruments and conveniences for obſervations of that kind than I was then.

The Blaze extended it ſelf in a right line towards the Star in the right thigh of *Caſſiopea*, being a Star of the third magnitude. Its length at firſt was about 7 or 8 degrees, and did ſometimes ſeem longer, ſometimes ſhorter, as I noted before, without ſeeming to have any other motion in it but the Diurnal motion, the ſame with the fixt Stars on Earth. Whence I collected, that the head of it pointed towards the ſeventeenth degree of *Taurus* in the Ecliptick, though the Sun at that time was about the thirteenth degree of the ſame Sign.

The 24. with ſeveral others, I attended the appearance of it, but the Sky in that part of the Heavens was over-caſt with Clouds.

The 25. I expected to have a farther Obſervation of it from half an hour after two, till a quarter after four; but notwithſtanding the South-eaſterly wind, and the clarifying quality of the air, which before half an hour

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after three had partly carried off, and partly diſſolved the black thick Clouds (with which the North-eaſt parts of this Horizon was over-caſt about three of the Clock) and left that part of the Heavens where the Comet ſhould have appeared clear, and without Clouds. Yet the air being very high and heavy, as the Barometer ſhewed, the upper parts of it were ſo filled with the dawning light of the morning, that neither the Blaze head or Star of the Comet appeared to me in the leaſt: nor had I any ſight of it ſince.

The like appearance of the great height of vapors in the air, when it is very heavy, I have often taken notice of, and have obſerved, that the twy-light and dawning between the night, and appearing of the Sun is very much altered thereby. And that a heavy air, when the vapors are raiſed high, will make the length of them much greater, and conſequently the night ſhorter. And a light air, on the contrary, ſhortning them, doth lengthen the night.

Theſe were the moſt remarkable circumſtances I took notice of in this Comet, being altogether Phyſical, and deſigned only for enquirng into the conſtitution of theſe wonderful bodies: the accounts and opinions we have hitherto had of them of that kind, being very unſatiſfactory. Though other Obſervations, to wit, Mathematical, of the way, celerity, and magnitude of Comets have been proſecuted with very much care, and great skill; ſuch as thoſe of the noble *Tycho*, and the learned and diligent *Hevelius*, inſomuch that I could not expect to have better; yet as to Phyſical remarks, I wanted much information to be able to ſatisfie many difficulties that occurr'd to my thoughts, upon enquiry into the particular natures of them. I did therefore, as I deſigned, employ all the time I could get of obſerving this Comet, in taking notice of ſuch circumſtances as I judged would be pertinent to reſolve any of thoſe Queries I had formerly made, in order to find out the nature of Comets in general. And though the little opportunitunity I now had, and the diſadvantageous appearance of this laſt were very ſhort of giving me that ſatisfaction in many particulars which I wiſh'd for, and expected at firſt, yet ſince they may poſſibly ſerve for hints to others that may hereafter have better oportunity than I, and that I might underſtand what material objections could be made by obſervers from preceding Comets, and that they might for the future more diligently take notice of what from theſe queries and hints may be judged ſignificant to this deſign, ſuch as they are I have here publiſhed as I had done formerly by my Lectures in *Graſham*-Colledge, thoſe which I had made of thoſe in 1664. and 1665.

Now before I come to make reflexions upon theſe remarks, I thought it might not be improper to add ſome few of thoſe things concerning thoſe two former Comets obſerved by me in the ſaid years. I ſay, ſome few, becauſe it would be needleſs to ſet down all, eſpecially ſuch of mine as do agree with others ſince publiſhed. I did therefore ſoon after I had ſeen the firſt Comet, to wit, *December* 23. 1664. propound to my ſelf certain Queries neceſſary to be anſwered, in order to find out a true theory of them, and directed my Obſervations accordingly; and they were theſe.

Of what ſubſtance its body, beard, and blaze is? and next, of what magnitude each of thoſe parts appear, and of what real magnitude they are?

Other Queries were concerning its denſity and rarity, its mutability or immutability; that is, whether it diſſolved and waſted or not? whether it were fluid or ſolid? whether it participated of gravity or levity?

Whence it had its light, colour, *&c.*

What was the figure of the Star, Radiation, Blaze, *&c.*

Whether the Blaze were always oppoſite to the Sun, or deflected? whether ſtraight or bended, *&c.*

What kind of motion it was carried with? whether in a ſtraight or bended line? and if bended, whether in a circular or other curve, as elliptical or other compounpounded line, whether the convex or concave ſide of that curve were turned towards the earth? Whether in any of thoſe lines it moved equal or unequal ſpaces in equal times?

Through what parts of the univerſe it moved, and how far diſtant it was at ſeveral times? Whether in the lower Regions near the Earth in the Atmoſphere, or near it, or in the Heavens, or fluid Æther, with which the ſpace of the Heavens is filled? Whether above or below the Moon, *&c.*

Whether it waſts, and is diſperſed and conſumed? or whether it laſts and endures for a longer time? If it laſts, Whether it ever appears again, being moved in a circle; or be carried clear away, and never appear again, being moved in a ſtraight or paraboloeidical line? Whether it be collected or generated when it firſt appears? and diſſipated or deſtroyed when it diſappears; or whether the ſeveral diſtances of it do not make that appearance?

Whether it may not have ſome ſuch propriety, as the Star in *Cete*, whereby it may ſhine and appear for a certain period, and again loſe its light, and diſappear by ſeveral viciſſitudes? and whether that may not give ſome account of the appearance of ſo many Comets about *Aries*?

Firſt, As concerning the matter or ſubſtance of the *Nucleus* Star or body, of the hazy ſhining part encompaſſing it, and of the Tail or Blaze: I ſay, that by comparing all the circumſtances that I was able to take notice of from the beginning to the end, I found that the Star in the head was of a very compacted and denſe light, and almoſt equalled that of *Saturn*; though it were not like that confined by an equal limb: that there were ſome parts diſtinguiſhable in this body, ſome having a brighter, others a fainter light. That theſe parts did not continue the ſame, but conſiderably varied, which might in part be cauſed by the differing poſition of thoſe parts which were ſeen before, from the ſame ſeen afterwards, in in reſpect of the eye, ſituate on the ſurface of the Earth, moved one way, and the Comet moved another; though I do not conceive it wholly aſcribable to that, but partly alſo to a real alteration of the parts of the Comet. That I did very diligently watch to obſerve if it were poſſible, when it paſs'd over any fix'd Star to find whether it were tranſparent; as I had ſeveral times obſerved the tail of it to be even in its brighteſt parts, but I had not the opportunity; but that I did ſeveral times obſerve the tail of it tranſparent, not only with the naked eye, but through a Teleſcope: if at leaſt the fixed Stars be above it, which I think few doubt, that the light diminiſh'd by degrees towards the extremes of the hazy part encompaſſing it; and yet the extremes of it as to that part of it which reſpected the Sun, ſeemed pretty evenly and ſmoothly defined, eſpecially through a Teleſcope: From all which remarks, and from the velocity of its motion, I conjecture it to be made up of ſolid matter, not fluid; that the body of it eſpecially, is conſiderably denſe, but that the hazineſs or *Coma* about it is much more rarified, and the tail thereof is moſt of all. That this body is encompaſſed with a body moſt fluid, and eaſily permeable, and which doth with very little reſiſtance give way to the motion of it, or any other body through it, that it doth eaſily admit at leaſt (if not actually take into it ſelf) the parts of this body, *Coma*, and Blaze. I ſay, admit at leaſt, (though there may be many reaſons alledged that it doth actually prey upon, and diſſolve thoſe parts into it ſelf, as I ſhall ſhew by and by) becauſe that we find that the extreme parts do extend but to ſuch a diſtance, and beyond that there is no appearance of light, and that the light is from it ſelf, and not produced by refraction or reflexion of the beams of the Sun, I ſhall ſhew reaſons by and by. And conſequently, where there is moſt light appears, there are the greateſt number, and there is the greateſt denſity of the Cometical parts. The middle of the body may be as

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denſe as the body of the earth; and I have not obſerved my ſelf, nor met with any body elſe that hath taken notice of any thing to the contrary: If I could have ſeen any Comet to have covered any Star in its way, it would have afforded a very circumſtantial information, eſpecially if for this purpoſe it had been taken notice of with a good Teleſcope. What the denſity of the innermoſt parts of this Earth we live on is, none knows; for though we find the parts on which we tread to be very compact, and though by the induſtry of Miners it hath been proved ſo alſo to the depth of many hundred foot, as *Georgius Agricola* relates: and though it hath been found ſo even to a greater depth by the ſoundings of the bottom of the Sea, yet none can bring an undeniable proof that the ſame is ſo ſolid to 25 miles deep; much leſs that it is ſo to the center: if therefore the external ſhell of this Globe were broken, and removed, 'tis not impoſſible but that the middle parts thereof may be of the ſame nature with the middle parts of the Comets body; and that thoſe parts (were the ſuperficial parts or ſhell removed) might, like theſe of Comets expand themſelves into the encompaſſing Æther. Nay we find, that notwithſtanding the compactedneſs of the ſuperficial parts of this Earth, yet the Æther is able to take up into it ſelf vaſt quantities of them, and to keep them ſuſpended, ſome of them, even to the height of many miles, if any argument may be drawn from the height or length of the dawning or *Crepuſculum*; and this, notwithſtanding the attraction of the Earth in its perfect vigor, or the gravitation of theſe parts thus taken up, or their endeavour towards the center of the Earth. How much more freely then might we imagine the encompaſſing Æther to prey upon, and take up into it ſelf the internal parts, if they were of a looſe and pervious texture, and almoſt in a ſtate of fluidity, like a heap of Sand, or a veſſel of Alabaſter-duſt in boyling, and were not ſo firmly united by the bonds of gravity, and the *vinculum* of petrifaction,

ction, as we find the ſuperficial parts of the earth now are. There is one argument to prove to us, that there may be ſuch a looſeneſs of the internal parts of the earth, and that is that the magnetical virtue varies, which virtue without controverſie diffuſed through the whole body of the Earth, and which hath a relation to the whole Globe, and to every magnetical part thereof. For by obſervation 'tis found, that the magnetical virtue acts upon a needle without it, as the magnetical virtue of a round Loadſtone doth on a Needle applied without that, which, as I may elſewhere ſhew, hath a reſpect to the center of the ſtone differing from all the reſpects that Authors have hitherto aſcribed to it, even of *Gilbert*, *Kepler*, *Kircher*, *Deſcartes*, and our Countryman Mr. *Bond*, who I think was the firſt man that endeavoured to reduce the variations obſerved by *Wright*, *Gellibrand*, *Coſter*, &c. into a Theory and calculation. Now this magnetical virtue, (which may be called one emanation of the *Anima mundi*, as gravity may be called another) being diffuſed through every part of it, and ſeeming to be, as it were *Tota in toto & tota in qualibet parte*, and to be more ſpiritual, and to act more according to Magical and Myſtical Laws than Light, Sound, or the like, it giving to every magnetical body, and every piece of it, though infinitely divided, the ſame proprieties it hath it ſelf; This magnetical virtue, I ſay, having ſuch a relation, and being forced thus to vary, 'tis very probable that the internal parts to which it hath a reſpect, have a variation likewiſe; and conſequently, that theſe internal parts which are ſuppoſed generally very denſe, compact, and very cloſely and ſolidly united, may be notwithſtanding more looſe, and ununited, and movable from certain cauſes.

To proceed therefore, I ſay, that it ſeems very probable to me, that the body of Comets may be of the ſame nature and conſtitution with that of the internal parts of the Earth, that theſe parts may, by the help of the

C 2 Æther,

Æther, be ſo agitated and blended together, as to make them work upon, and diſſolve each other in the ſame manner, as we have often had examples of ſome of the parts of the Earth; a late inſtance of which was at *Mongibel* or *Ætna* in *Sicily*, where the Fire continued for a long time, and produced very conſiderable effects. That this internal agitation may confound the gravitating principle, and ſo leave the parts in a greater freedom to be diſſolved by the encompaſſing Æther, which is the agent that ſets the other two at work to deſtroy each other, that it may at length prey upon both, and diſſolve them both into it ſelf; and conſequently, not only the parts thus diſſolved are elevated to a greater diſtance from the center of the Star or *Nucleus*, or the ſuperficies of it, whoſe gravitating or attractive principle is much deſtroyed, the *Coma* being in this Comet four or five Diameters of the Star or *Nucleus*: but having given thoſe parts leave thus far to ramble, the gravitating principle of another body more potent acts upon it, and makes thoſe parts ſeem to recede from the center thereof, though really they are but as it were, left behind the body of the Star, which is more powerfully attracted than the minuter ſteaming parts: for, I ſuppoſe the gravitating power of the Sun in the center of this part of the Heaven in which we are,hath an attractive power upon all the bodies of the Planets, and of the Earth that move about it, and that each of thoſe again have a reſpect anſwerable, whereby they may be ſaid to attract the Sun in the ſame manner as the Load-ſtone hath to Iron,and the Iron hath to the Load-ſtone. I conceive alſo that this attractive virtue may act likewiſe upon ſeveral other bodies that come within the center of its ſphere of activity, though 'tis not improbable alſo but that as on ſome bodies it may have no effect at all, no more than the Load-ſtone which acts on Iron, hath upon a bar of Tin, Lead, Glaſs, Wood, *&c.* ſo on other bodies, it may have a clean contrary effect, that is, of protruſion, thruſting off, or driving away, as we we find one Pole of the Magnet doth the end of a Needle touched on the oppofite part; whence it is, I conceive, that the parts of the body of this Comet (being confounded or jumbled, as 'twere together, and fo the gravitating principle deftroyed) become of other natures than they were before, and fo the body may ceafe to maintain its place in the Univerfe, where firft it was placed. Whence inftead of continuing to move round fome central body, whether Sun or Planet, as it did whilft it maintained it felf entire, and fo had its magnetical quality (as I may fo call it) unconfounded, it now leaves that circular way and by its motion (which always tends to a ftraight line, and would be fo were it not bended into a curve by the attractive virtue of the central body) it flies away from its former center by the Tangent line to the laft place, where it was before this confufion was caufed in the body of it. In this line ('tis probable) it paffes from one part of the Heavens to another, and fo paffes through the fpheres of the activity of multitudes of central bodies; in the paffing through which fpheres, 'tis not improbable that thofe parts which by their diffolution are made of a nature differing from the body in the center, are rather expelled from, than attracted towards it; and fo being by this diffolution rarified, and loofened from the middle, and by their acting upon one another, and diffolution of the Æther made of another nature, after they have every way difperfed themfelves to a confiderable diftance from their proper body, are converted and driven in a way almoft oppofite to that expelling body, and fo continue to be driven away to fuch a vaft diftance, as to make out that prodigious length of the tail or Blaze of fome Comets (fuch as was that of 1618. which,as *Kepler* reports, was extended to 70 degrees from the body or head of it) till at laft they are diffolved alfo, and commixed with the Æther within them. So that though I fuppofe the attractive power of the Sun, or other central body may

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draw the body towards it, and so bend the motion of the Comet from the streight line, in which it tends, into a kind of curve, whose concave part is towards the Sun, by reason that there are some central parts of it, which are not yet destroyed, and so retain somewhat of its gravitating principle: yet I conceive that all those parts of the Comet which are thus wrought upon by the other, and changed into another state, and are very much rarified, and produce light, are of a clean contrary nature, and recede from the center of the Sun: much after the same manner as we find any combustible body with us; as Coal, *&c.* where we find that the body of the Coal, before it be resolv'd into smoke, is a very dense, and very heavy body, and tends to the center of the earth; but the parts thereof agitated by the Air and Æther into steams and smoke, and those yet farther dissolved into flame, do tend upwards, and from the center of the earth. Now though one cause of the recess of flame from the center of the Earth be the gravity of the ambient Air. Yet 'tis not impossible, but that there may be somewhat also of positive levity conjoyned therewith. Most certain it is, that there must be a tendency of receding, as well as a tendency of approaching the center of the Earth, and other attracting body. And there may be much said for the supposition, that the recess of the purest Æther, from the center, is the cause of the motion of the grosser Æther, and of all other bodies towards it, though there are also very considerable arguments against it. But this discourse is not my present business, though it may hereafter be the subject of a Lecture in this place; for upon it do depend some of the greatest operations in the universe. And as in the History of the Creation, we have an account of the production of light, immediately after the making of matter, which is a motion of recess from the center of the shining body. Next that, a Firmament which divided between the waters or the fluids of the one, and the fluids of another part of the

the world. And in the third place, the collections of particular fluids to one center, as the center of the Earth: and laſtly, out of that collection of fluids appeared the dry and ſolid land. So I conceive the moſt proper way of ſpeculating on theſe great productions of the omnipotent Creator, may be to begin with the conſideration of light, or the motion of receſs from the center of a body. Next, with the conſideration of the cauſe of the ſeparating of fluid from fluid, as Æther from Æther, as I may ſo call differing Æthers; becauſe we have not yet diſtinct names in uſe, and the reaſon of their conglobation, the Æther from the Air, the Air from the Water, the Water from Quickſilver, Oyl, or other fluid. Thirdly, the cauſe of the conglobating property of each of theſe fluids when ſeparated, how they accept and embrace *Homogenea*, and reject or expel *Heterogenea*. And fourthly, how they condenſe and ſettle together, and produce a ſolid body: whence proceeds the confirmation of attraction or gravitation, *&c.* But to digreſs no further, but conclude this part of enquiry in ſhort, I ſuppoſe the *Nucleus* or Star of the Comet may be much of the like nature with the central parts of the Earth, Moon, *Mars*, *Jupiter*, *Saturn*, or other Planets, but much impaired in its attractive or gravitating power.

Next, that the *Coma* or Hazy Cloud about it, may be of the nature of the Atmoſphere or Air about the Earth, or the Smoke or ſteams about a heated or burning body, before they are quite kindled, converted into Flame, or diſſolved into the ambient Air.

Thirdly, that the Tail or Blaze is much of the nature of the parts of Flame, though with thoſe differences I conceive, that the parts of theſe ſteams are not ſo cloſe together, as are thoſe of Smoke: nor doth the motion of them, though much ſwifter upwards than that of our Flame, ſerve to make them appear a ſhining line; but being at that diſtance, they appear much ſlower to the eye, and ſo diſcontinue the appearance; whence every ſhining

ſhining particle appears only a ſhining point, though in the parts of flame (where notwithſtanding the motion be much ſlower, yet being nearer, and ſo varying the poſition to the eye much quicker) each of the ſhining parts makes an appearance of a line of light, and all of them paſſing pretty near together, make the appearance of a continued fluid flame; though that indeed be nothing but a great number of ſingle parcels of the burning body, raiſed up in the particles of Smoke. This will appear evident if we conſider the appearances eaſily to be taken notice of in light: for 'tis obvious from multitudes of experiments, that any ſhining body, as a candle or brands end, being moved very quick, makes the ſame impreſſion on the eye, that a line of light doth ſtanding ſtill: And as obvious alſo that any very light body incompaſſed with a dark *medium* appears to the eye under an angle bigger than really it is, and a dark body encompaſſed with a light *medium* much leſs. This any one may preſently find, if he make a ſmall hole through a thin plate of metal, and holding it firſt between the light and the eye, and ſo ſeeing the light appear through it, and then placing it ſo as there is nothing but darkneſs appears through the ſaid hole, for he will plainly perceive that the ſame hole will appear much bigger in the former poſition than in the latter. Upon this account indeed each of the ſhining parts of the Comet ſeems to fill and occupy a much greater ſpace than really it doth: and ſo, as 'tis obſervable in the milky way, a great number of theſe ſmall ſhining bodies though diſperſed at a pretty diſtance one from another, yet by reaſon of the imperceptibleneſs of each of them they all ſeem to coaleſce into a ſtream or Blaze of light, the brightneſs of which is yet farther augmented by a clear and unenlightened air, and by ſuch a part of the Heaven wherein there appears feweſt of the Stars, whether they be greater or leſſer.

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To the Query, *Of what magnitude the Body*, Coma, *and Blaze of Comets may be?* No anſwer can be given until another queſtion be firſt anſwered; and that is, *What is the place of Comets, and what is their diſtance from the Earth?* It was the opinion of moſt Modern Writers before *Tycho Brahe* and *Kepler* (I know divers of the Antients thought otherwiſe) that Comets were ſublunary Meteors, drawn up into the higher Regions of the Air, and there ſet on fire, and ſo continued burning till the Meteor were conſumed; and as the matter increaſed, or waſted, ſo did the appearance of the Comet. But this noble *Dane*, and ſeveral others about that time found by accurate obſervations made, that its Parallax was leſs than that of the Moon; and conſequently, that it was farther diſtant from the earth: that it muſt be a body of another magnitude, and nature, than moſt before that time had imagined; and therefore that it ought to be otherwiſe thought of than the generality of mankind believed concerning it. Many had been the attempts of former Writers concerning them, to find out their parallax; and whether from their unaccurate inſtruments, or from their leſs skill and diligence in uſing them, or from an imagination of the ſolidity, and impenetrability of the Cœleſtial Orbs, or from error in their calculations, or from comparing Obſervations made at diſtant places, one or both whereof were unaccurate, or from a prepoſſeſſion of Tradition or common Fame, or from what other cauſe ſoever it were is uncertain; but 'twas generally concluded by them, that all Comets were ſublunary Meteors: and there are not even at this day wanting ſome of the ſame opinion, though for what reaſon I know not. 'Twill be hard to convince ſome of theſe, that the opinion they have hitherto received for good, is not ſo, becauſe they will hardly give themſelves the trouble of examining ſtrictly into the matter: And to underſtand the nature of Parallaxes, and how ſignificant they are in determining the diſtances of bodies from the ſurface of the

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Earth, to certain degrees thereof; beyond which, by reaſon of the imperfections in Inſtruments, and Obſervations, and the exceeding niceneſs and curioſity neceſſary, they ſignifie very little. It is not my preſent deſign to explain what Parallax is, that I would ſuppoſe my Reader to underſtand; otherwiſe there can be no reaſon ſhewn him to convince him that 'tis poſſible to prove that this or that Comet was not nearer than ſo many ſemidiameters of the Earth, nor farther off than ſo many. There are then two ways, by which we may come to ſome certainty of what diſtance a Comet is; and thoſe are, firſt the Parallax of its Diurnal motion, or its Parallax cauſed by the Diurnal motion of the Earth. And ſecondly, the Parallax of its proper motion compared with the Periodick or Annual motion of the Earth. The firſt of theſe may be obſerved two ways; either by two Obſervers at parts of the Earth very far diſtant from each other, but as near as may be under the ſame Meridian: as ſuppoſe the one in *London*, the other in St. *Helens*; both conſpiring in their obſerving of the place of the Comet amongſt the fix'd Stars at the ſame time. Or ſecondly, by one Obſerver in the ſame place, by obſerving the place of it amongſt the fix'd Stars, in its riſing or ſetting, and in a greater, or if it may be, its greateſt height: The noble *Tycho* by very accurate Obſervations of the Parallax, proves the Comet of 1577. to be above the Moon. *Kepler* by his own Obſervations proves that of 1607. at its beginning to be four times farther diſtant; and I doubt not but ſome may have been above forty times farther. But I do not yet find that any Obſervations have accurately determined that which is indeed the great help by which we are inabled to judge of the nature, and all the other accidents and proprieties of Comets. The *Ariſtotelian* Philoſophy for a long time prevailing, made the world believe them to be nothing but Exhalations from the Earth, drawn up into the higher Regions of the Air. But *Tycho* by his Obſervations of their Parallax, raiſes them

them out of that confinement, but yet he ſeems to place them in an Orb about the Sun. But *Kepler* frees them from that confinement, and aſſigns them the Univerſe to expatiate in. But none of all theſe do accurately prove the true diſtance of them, their Parallax being for the moſt part ſo very ſmall, that I fear Inſtruments with common lights will hardly reach them. But we muſt expect from future obſervations made with Teleſcopical Inſtruments to receive a certain Anſwer to this Query. Certain I am, that the Comet which began to appear in *November* 1664. and diſappear'd in *March* following, was far removed beyond the diſtance aſſigned by *Kepler*. For by my own Obſervations divers times repeated, I could not find any ſenſible Parallax, though I endeavoured by a new method to make my Obſervations more accurate. Now though I had not the convenience of making uſe of a Quadrant, or any ſuch Inſtrument, to obſerve its place when near the Horizon, yet the way I took, would, I think, be as good; which was this: With a very good ſix foot Perſpective-glaſs or Teleſcope, I obſerved the place of the Comet, in reſpect of the adjacent ſmall Stars, as ſoon as it appeared, and ſo traced its way till it diſappeared in the vapors of the Horizon: the like I did ſeveral other days ſucceſſively, taking notice by what degrees, in what times it made its progreſs, to ſee whether by its Parallax, when near the Horizon, it would have been depreſt below that line of its motion, which it kept, when at a greater height above it. But though I tried this ſeveral times, yet I was not able to diſcern that the Parallax of it cauſed either any ſenſible bending of the line, or any ſenſible inequality in its progreſs, by which I ſhould have ſooner found it, than by taking its altitudes with common Inſtruments: though I confeſs theſe Obſervations were made when the motion of the Comet was ſlow, and conſequently, when in probability it was far diſtant from the earth. To me there ſeems no doubt but that it was a long way removed above the Moon

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when I made theſe Obſervations: for had it been of an equal diſtance with that they allow the Moon, it muſt this way have manifeſted a very ſenſible Parallax of divers minutes: but whereas I could not certainly diſtinguiſh any ſenſible at all, it muſt be many times higher than the Moon. Now that this way is abundantly to be preferred before an Obſervation made with a Quadrant for the taking of its altitude, is pretty evident; becauſe, by this means the greateſt part of the irregularity, cauſed by the refraction or inflection of the Air is removed; for by this means, though the Parallax be very large, yet the refraction or inflection of the Air will not amount to many ſeconds, both the objects being almoſt equally raiſed by refraction, eſpecially when 5 or 10 degrees high; nearer than which the ſmall Stars vaniſhed out of ſight by the thickneſs of our air. It follows therefore that a Semidiameter of the Earth muſt be a very inconſiderable meaſure in its diſtance.

This part therefore of the Theory of Comets hath been much defective hitherto. If we enquire the Parallax of them from the Obſervation of divers men made in differing places. we ſhall find them ſo differing one from another, that there is great reaſon to ſuſpect them all: Nay, not only ſo, but in this Comet of 1664: by comparing two Tables or Charts of the Stars, and Conſtellations of that part of the Heavens, through which the Comet paſt, on which was alſo markt out its way and place from day to day, both of them Printed from Copper Plates, I find that ſtrange errors and miſtakes may be created, notwithſtanding all the Authors care and accurateneſs poſſible, from the careleſneſs or neglect of the Graver: This I noted in the two Tables of the learned and accurate Mathematician, *P. Ægidius Franciſcus de Gotignies*, (whoſe skill and care from other works of his and other Obſervations of this Comet I am ſufficiently aſſured of) and found that by the firſt table upon the $\frac{21}{31}$ of *December*, 1664. it was

was in $4\frac{1}{2}$ of ♊ in Longitude, and in $33\frac{2}{3}$ of Southern Latitude; but by the ſecond it is placed at the ſame time in 4° ♊ for its Longitude, and in $34\frac{1}{2}$ of South Latitude. And this error is not only committed in the place of the Comet, but alſo in the place of the fix'd Stars: for *Riget* in the firſt Table is placed in $30\frac{3}{4}$ South Latitude, and in $12\frac{1}{4}$ ♊ for Longitude, but in the ſecond in $31\frac{1}{2}$ South Latitude, and in $11\frac{1}{2}$ ♊ for Longitude: both which differ conſiderably from the place of it aſſigned by *Riccioli* and *Grimaldi*; according to whoſe Obſervations it ſhould be in 31. 11 South Latitude, and $12^{\circ}.\,11'.\,40''.$ ♊ in Longitude.

Now if there be theſe differences to be remarked in the Obſervations of one, we cannot but expect that much more diſagreement ſhould be found between thoſe which have been made by differing perſons in differing places, and with differing ways, and differing Inſtruments. And upon examination I have found it no better: for from comparing ſuch Obſervations as I have received from ſeveral parts of the world, even of thoſe which have ſeemed more than ordinarily exact, I find them for the moſt part ſo unaccurate, that though they ſufficiently manifeſt that the Comet of 1664, which laſted above four months, was viſible in moſt parts of the world, and ſeen to paſs in all thoſe places pretty near in the ſame way amongſt the fixed Stars. Yet they are ſo far from manifeſting the Parallax, that ſome of them make the place of the Comet to be quite contrary to what Parallax would make it; ſome of the Southern Obſervators placing it much more Southwardly than thoſe of the North. Others indeed of them make the Parallax ſo great, that one might gheſs it to be not ſo far removed from the Earth. Something indeed in the general might be gheſt of the way of that Comet amongſt the fix'd Stars, eſpecially when it approaches them pretty near: but for exactneſs of Calculation for Parallax, they were no way uſeful. And even

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in the former uſe too it ſeems very doubtful for comparing the Charts of the Comets way amongſt the fix'd Stars publiſhed by that diligent and unwearied Obſerver Mr. *Hevelius* of *Dantzick*, the above-mentioned *P. Gottignies*, Profeſſor at *Rome*, and Monſieur *Petit* of *Paris*, I find, that the two former make the way of the Comet to lie below the Star in the Bill of *Corvus*; whereas the later, though in a Latitude interpoſed between the parallels of the former, makes it to lie above, or to the North of it: and with him agree ſome Obſervations which I have ſeen of Monſieur *Hugenius*. Other differences I found between thoſe Tables in the way of the Comet of 64. near the middle of its arch; wherein Monſieur *Hevelius* all the way places it more Southward than either Monſieur *Petit*, or *P. Gottignies*: for whereas both *P. Gottignies*, and Mounſieur *Petit* make it paſs above the Star of the third magnitude in the right ſhoulder of *Lepus*, Monſieur *Hevelius* makes it move below it, which ſeem to be aſcribable to Parallax. But I fear much cannot be concluded of certainty from them.

I ſhall not trouble the Reader with a multitude of other Hiſtories, which I have received concerning that Comet of 64. nor with the diſagreements of them one with another, and perhaps of moſt with the truth. They have given me ſufficient trouble in the examination of them, having little other benefit from them, ſave only this, that I was thereby informed what a man might think of a great number of Aſtronomical Obſervations that have been made: for, ſaving the exact Obſervations of ſome few ſuch, as Mr. *Hevelius*, Mr. *Aurout*, *P. Gottignies*, *&c.* truly diligent and accurate men, the greater the Collections of Obſervations are, the more trouble and difficulty is created to the Examiner; they not only confounding one another, but perplexing thoſe alſo which are real and perfect.

Now the reaſons or cauſes of theſe inconveniences ſeem to be theſe.

Firſt,

First, the want of accurate and knowing Obſervators.

Secondly, The ſcarcity of convenient Inſtruments.

Thirdly, The Imperfection of the Tables of the fix'd Stars.

For the Obſervators, 'tis not enough to know how to manage an inſtrument, or to have a good eye, or a dextrous and ſteady hand; but with theſe there muſt be joyned a skilfulneſs in the theorical and ſpeculative part, and add to all a love and delight in the thing it ſelf; and even all theſe will ſignifie but little, without convenient and accurate Inſtruments, ſuch as may be eaſily manageable and ſufficiently exact.

The firſt of theſe the love of the ſtudy being in it ſelf the moſt excellent, or the encouragement of Princes, Noblemen, and other Patrons of this Learning muſt procure: and where both of theſe concur, thence moſt is to be expected, and moſt fruit hath hitherto been proceeded; though there are not wanting divers eminent inſtances where the firſt reaſon hath been the only inducement.

As to the ſecond, I have already in ſome of my former Lectures deſcribed ſeveral convenient ones for theſe purpoſes; and therefore I ſhall not here add any more concerning it.

But as to the third, I hope the indefatigable labour and skill of Monſieur *Hevelius* will ſhortly ſupply the preſent defect, though it had been much to be wiſh'd, that the Inſtruments he had made uſe of had been fitted with Teleſcopical ſights. Theſe Tables, if well done, will alone (as to the buſineſs of Comets at leaſt) ſupply the place of all other Inſtruments almoſt, ſave only a thread, eſpecially if they be ſo delineated in Tables after the Tangent projection, as that the minutes of every degree may be very diſtinguiſhable, which will not ſwell the Maps of the Heavens into an extraordinary large volume, and may poſſibly be the cheapeſt Inſtrument for this purpoſe an Aſtronomer can be furniſhed withal:

withal; for having ſuch a volume of Tables, it will be very eaſie with a thread and one's eye, ſcreen'd only with a ſpectacle made of a thin plate of Braſs, with a ſmall hole through it, inſtead of a glaſs, to obſerve what place the Comet poſſeſſeth amongſt the fixt Stars: for having by the help of the ſaid thread obſerved what two Stars lie in the ſame line with the Comet on one ſide of it, and what other two Stars lie in a line with it, which is at right angles (as near as may be) with the former line, by finding out thoſe four Stars in the Tables, ordered according to the Tangent projection, and with a Ruler, drawing lines over them reſpectively, where thoſe lines do interſect, there will be the true place of the Comet, from which it will not be difficult to find out the true Longitude and Latitude of it by a Sector with Tangents. Now as theſe Tables of all the fixt Stars viſible to the naked eye, would ſerve for finding its place whilſt very big and ſwift of motion; ſo the like Tables of the ſmall Teleſcopical Stars that lie near its way, when almoſt diſappearing, and moving very ſlow, will by the help of a pair of meaſuring Compaſſes placed within the eye-glaſs of the Teleſcope, and a ſtraight line or hair drawn croſs it, ſerve to find the true motion and way of it, when only viſible with a Teleſcope: according to which method I made the annexed Schemes, and Obſervations of the laſt appearances of the Comet.

Now ſince neither from my own, nor from any other Obſervations that I have hitherto met with, there can be any certain concluſion drawn of the diſtance of theſe Comets, ſave only this, that their diſtance was very great, and much higher than the body of the Moon, becauſe elſe there muſt have been a conſiderable Parallax cauſed by the Diurnal motion. The next enquiry will be, what other ways there are of knowing its diſtance. Now though none could be more demonſtrative than the Parallax found this way by the Diurnal motion, yet there are ſome other which ſeem more eaſie

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arifing from the confideration of the motions that may be thought to be concern'd in the producing the appearances. And though they be wholly hypothetical, and fo need fome other arguments to prove the ground and principles on which they are founded, yet fince there are not very many confiderable ones wanting to make them probable and rational, I fhall here add fomewhat of my inquiries after the diftance, pofition, motion, magnitude, *&c.* of thefe Comets by thefe means.

Of thefe ways there are feveral depending upon feveral fuppofitions which produce very differing effects, as to the magnitude, diftance, motion, and way of the fame Comet.

The fuppofitions are thefe:

Either that the Earth moves in an annual orb about the Sun, as the Sun is fuppofed by others to move about the Earth: Or that the Earth is perfectly fix'd, and hath no fuch motion.

Next, that the Comet moves either in a ftraight line, or in a curve line; and the curve is either a circle, or fome other regular or irregular curve.

Further that the motion of the Comets in thefe lines is either by equal or unequal fpaces in equal times.

Now according as we take this, or thofe of thefe differing fuppofitions, and compound them together, fo will the product of them be ftrangely differing. Amongft the great variety of compofitions of thefe principles or fuppofitions, thefe feem the moft fimple, and confequently being any otherwife proved, will beft determine the true diftance and way of the Comet.

Firft, To fuppofe the Earth to ftand ftill, and the Comet to move equal fpaces in equal times in a circle.

Secondly, To fuppofe the Earth to move in an annual Orb about the Sun, and the Comet to move through the Æther or *Expanfum*, equal fpaces in equal times in a ftraight line.

Thirdly, To fuppofe the Earth to move (as above)

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in its annual Orb, and the Comet alſo to move equal ſpaces in equal lines in a circle.

The other are indeterminate and infinite, and nothing can be concluded from them as to the diſtance, magnitude, motion, *&c.* of Comets; for the line or way of the Comet may be placed at any diſtance, if we will ſuppoſe it moved in an uncertain curve, with unequal degrees of velocity: And indeed, upon a ſuppoſal of an inequality of motion, nothing of its way or diſtance can by any of theſe ſuppoſitions be found out. This fault had that of *Tycho Brahe*, where he ſuppoſed an unequal motion of it in its Orb about the Orb of *Venus*, which was founded upon the firſt Hypotheſis, but had introduced into it ſome inequality of motion; beſides his own ſuppoſition, that it was moved about the Sun, and the Sun about the Earth. See the fifth Figure. *Keplers* way, which was after the ſecond Hypotheſis, had the ſame fault; for he ſuppoſed the annual motion of the Earth, and the motion of the Comet in a ſtraight line, but introduces an acceleration of motion in the Tangent towards the latter end.

The third way I have here taken, and from the beſt obſervation I could meet with, I have delineated its reſpects or angles to the Sun: and accordingly ſuppoſing it to move equal ſpaces in equal times, in a curve which for ſo much of it as the Comet was obſerved to paſs was very near a Circle, I found this Circle would fall as it is expreſs'd in the ſeventh Figure, where 'tis obvious to take notice, that when the Comet was neareſt to the Earth, namely, about the 19. or 20. of *December*, that it was not nearer than an eleventh part of the diſtance of the Sun; that on the 23, it was twice as far, that on the 29. it was four times as far; that on the 15. of *January* it was as far as the Sun, and on the 14. of *February* it was above twice as far diſtant as the Sun. That this way or Orb of the Comet is here bended ſo as (if it were an entire Circle;) one part of it would go go without the Orb of *Jupiter*, as the other which is here delineated comes within the Orb of the Earth; that the plain of this Orb is inclined to the plain of the Ecliptick about 18 degrees, that if from ſeveral parts this Orb perpendiculars be let fall upon the Plain of the Ecliptick, thoſe perpendiculars ſhall fall in an Ellipſis, part whereof ſhall fall within the Orb of the Earth in ♌, and the oppoſite without the Orb of ♃ in ♒. That the Comet moves a Sextant of this Orb in about 130 days, and conſequently if its motion ſhould continue the ſame in ſuch a Circle, it would appear about *February*, *March*, or *April*, 1667. but being ſo far removed towards the South Pole, will here hardly be ſeen: but by thoſe that live towards the South, it may appear to have ſome ſuch motion by the South Pole, as that of 1618. had by the North. And 'tis not impoſſible, but that the Comet of 1618. might be the ſame with this, if we ſuppoſe the Nodes of it to have a motion contrary to the order of Signs: and that the ſame Node which in this Comet, according to this ſuppoſition was in ♊, was then about ♍ or ♐: but theſe as conjectures I ſhall not inſiſt on, becauſe neither in this, nor in that have we Obſervations ſufficiently accurate to build any Theory upon. Now though upon theſe ſuppoſitions the motion and appearances of the Comet ſeem to be very regularly, and very naturally made out, yet 'tis not the only Hypotheſis for that deſign: nor do I believe it ſo evident a demonſtration for that end, as ſome would ſuppoſe; though for other reaſons I am apt enough to think that opinion of the Earths motion very probable: but the motion of this Comet is ſo well made out, by the contrary ſuppoſition, that I think it may be alledged for a greater argument againſt the motion of the Earth, than for it: for if we only grant one of the former *poſtulata*, namely, that the body of the Comet is moved equal ſpaces in equal times, and a quite contrary *poſtulatum* to the former; namely, that the Earth remains fix'd as to an annual motion, we may

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find all the obſervations of this Comet, eſpecially the moſt accurate of them, to happen ſo, that the Comet being ſuppoſed to be moved in a great Circle, whoſe convex ſide is turned towards the Earth, whoſe center is extended towards the fix'd ✳ in ♋; and whoſe Semidiameter is about ſixſcore times the neareſt diſtance of the Comet from the Earth, and the Comet be ſuppoſed to be moved very near equal ſpaces in equal times, we ſhall find, I ſay, all the appearances moſt exactly ſolved, and indeed much more exactly than by the other ſuppoſition I was able to find any; for by this ſuppoſition both the magnitude, longitude, latitude, retrogradation, ſtation, and direction of the Comet is moſt exactly made out as any one might have found that ſhould have by this means examined with me the obſervations I have hitherto either made or met with: and indeed all the Obſervations hitherto have ſo well anſwered this Hypotheſis, that I do almoſt promiſe my ſelf to be able to ſee this Comet a month or ſix weeks hence, after the Sun has paſt by it; if by its exceeding elongation it be not quite grown out of ſight, as it is now indeed already ſo exceeding dim, and faint, that it cannot be ſeen without a very good glaſs, which will endure an exceeding big aperture: nor could I theſe two laſt nights perceive it, though the Air were clear; but the reaſon I attribute to its nearneſs to a fixed ✳ of ♈: This Hypotheſis is explained in the ſeventh Figure. By this ſuppoſition the return of the Comet will be much longer, and the time of ſeeing of it much more uncertain; becauſe the curvature is ſo little that the making the circle a twentieth, or a ſixteenth part bigger or leſs, does not much alter the regularity; whence 'tis exceeding difficult, unleſs we had much more accurate Obſervations than I have hitherto met with, to determine exactly the bigneſs of the circle, and conſequently the time of the return. And by this ſuppoſition the Comet may be ſuppoſed either nearer or farther from the Earth at any diſtance, which is not con-

contradicted by a Diurnal Parallax; that is, it may be ſuppoſed either above *Saturn*, or below the Moon, or in any place between; by ſuppoſing only, that the farther the neareſt part of the Circle is diſtant from the Earth, the greater muſt that Circle be, and the ſwifter the motion of the Comet in it: to prove which affirmation, let in the Eighth figure A be the Earth, BCD the Orb of the Comet ſuppoſed very near the Earth, and E F G the Orb of it ſuppoſed at a greater diſtance: let H be the center of B C D, and I of E F G, and let A C, be to C H, as A F, to F I, all the lines drawn from the point A, ſo as to cut the Circles B C D and E F G, ſhall divide thoſe Circles E F G, and B C D, into ſimilar ſegments: as let A B E be a line drawn cutting thoſe Circles in B and E: I ſay, the Arch B C ſhall be ſimilar to E F. In which Hypotheſis if we have together with the place of the Comet when ſtationary, the place of it when in its greateſt celerity, perige, or the places of it when of the ſame celerity on each ſide of its perige, we have from thence the proportion of the Radius of its Orb to the perigean diſtance, and conſequently all the other diſtances, the line in which it appears when ſtationary, being the Tangent to the Circle in which it moves, as A B E, to which a Perpendicular raiſed at B B E, and produced till it cut the line A C, (produced) at H H I, it gives the Center of its Orb H H I, and the proportions of the lines A B, AC, B H = H C, or of A E, A F, E I = F I, the Angle B A C, being given by obſervation. So that by this Hypotheſis the Phænomena of the motion and bigneſs of the Comet will be ſolved, though ſuppoſed of any diſtance. Nor are theſe the only Hypotheſes by which the hitherto obſerv'd Phænomena may be ſolv'd: for if we will admit an unequal motion, ſuch as is now granted to all the Planets: and if further we will admit it to be moved in an Elleipſis, or other ſuch like curve, there may be divers other Hypotheſes that will ſolve the Phænomena; ſo that the Comet may be ſuppoſed to

E 3 have

have no motion at all as to Longitude, but only as to Latitude: that is, it may be ſuppoſed to be moved in an Elleipſis, deſcribed in a plain which ſhall be at right Angles with the plain of the Ecliptick, and the ways of the Earth in it: it may be ſuppoſed alſo to have been mov'd direct, according to the order of the ſigns, that is, to have been firſt about *Gemini*, in reſpect of the Sun, and to be now in ſome part of *Leo:* And it is not impoſſible to ſolve the phænomena of its periodick or proper motion, though it be ſuppoſed not ſo high as the Moon, and that the motion of the Earth paſſing by it did really alter its motions, had there not been made ſome Obſervations about the Parallax of it, which prove it higher: ſo that according to this or that Hypotheſis which we take, the time of its return, if permanent, will be longer or ſooner.

And theſe Hypotheſes may be ſo various, that till regulated by very exact Obſervation of the Parallax, 'tis not to be hoped that the appearance of a Comet can be certainly predicted: So that I fear the prophetick ſaying of *Seneca*, *Erit qui demonſtret aliquando in quibus Cometæ partibus errent, cur tam ſeducti a cæteris eant, quanti qualeſque ſint*, will hardly be verified at this time by the help of this preſent Comet. Though in truth I cannot find by the examination of ſeveral of them, but that they all ſeem to promiſe very fairly a return of it: for all the Calculations I have hitherto made of its motion, ſeem to caſt it into a circular, and not a into ſtraight line, as *Kepler* ſuppoſed; and indeed upon examining even *Keplers* own Calculations of thoſe Comets which he obſerved, and has endeavoured to make to move in a ſtraight line, I cannot find that any of them will be found to move equally in ſuch a line: but to ſolve the appearances, he is fain to make them move in ſuch ſuppoſed ſtraight lines, by a line of Tangents, that is, to make the motion of Comets accelerated the further they are moved; all which Phænomena may be very eaſily ſolved by ſuppoſing them to have moved equal ſpaces in in a curve or circle. The phyſical reaſon indeed ſeems pretty difficult, by what means it ſhould be confin'd or bound ſo as to move in a Circle: but this is no more than is uſually ſuppoſed in all the Planets, and without ſuppoſing a kind of gravitation throughout the whole Vortice or *Cœlum* of the Sun, by which the Planets are attracted, or have a tendency towards the Sun, as terreſtrial bodies have towards the center of the Earth. I cannot imagin how their various motions can with any ſatisfaction be imagined, but that being granted (for which had I now time, I could alledg many reaſons, and may do it hereafter on another occaſion) not only the reaſon of all the irregular motion of the Planets may be eaſily found, but the reaſon alſo of the ſtrange and various motions of the Comets. The reaſon why its Beard is for the moſt part oppoſite to the Sun, which was another Query, of which I have already ſaid ſomewhat of my ſuppoſitions, and ſhall now add, that the brighter ſpot or kernel in the middle did ſeem to be ſome kind of body, which though it be not actually burnt, may yet by the encompaſſing fluid Æther be diſſolved and waſted, and thoſe diſſolved parts may aſcend upwards, or from the center of the Sun, (which ſeems indeed to be the center of gravitation throughout the whole ſyſteme of it.) To illuſtrate which explication, I could produce ſeveral experiments which would make a perfect repreſentation of the phænomena of the body, and beard of the Comet: I ſhall only inſtance in one. Take a very clear long Cylindrical Glaſs, which may hold about a quart of water; fill it three quarters full with water, and put into it a quarter of a pound of Oyl of Vitriol, and in the midſt of this ſuſpend by a ſmall ſilver wire, a ſmall wax-ball, rould in filings of iron or ſteel, and you may plainly obſerve a perfect repreſentation of the Head, Halo, and Beard of the Comet; for the *menſtruum* falling on, or diſſolving the iron, there is a continual eruption of ſmall bubbles, and diſſolv'd particles from all the ſides

ſides of this body; and after the eruption they all aſcend upwards from the center of the earth; for being of a much lighter conſiſtence than the anbient liquor, they are by the greater gravity of that, continually protruded upwards. The ſame appearance may be made with any kind of *menſtruum*, and a convenient diſſoluble body ſuſpended in it; ſo that if we ſuppoſe the Æther to be ſomewhat analogous to a *menſtruum*, and that there is a gravitation towards the center of the Sun, if the *Nucleus* or head of the Comet be ſuppoſed ſuch a diſſoluble ſubſtance, the phænomena of the ſhape of the Comet may, I think, be rationally explained. Now that the Æther may have ſuch a kind of propriety, ſeems to me to be argued from this, that the Air about the Earth ſeems to owe its original to it, it being only a diſſolution of terreſtrial bodies into the Æther, the Æther being the principal fluid body, and greateſt part of this diſſolution; and the ſubſtance of the Air, ſome very few and ſmall ſaline and earthy particles: of which elſewhere. By this Hypotheſis the phænomena of the Comet may be ſolved; for hence 'tis eaſie to deduce the reaſon why the Beard grows broader and broader, and fainter and fainter towards the top: why there is a Halo about the body; for this will appear clearly in the experiment: why the Beard becomes a little deflected from the body of the Sun; for if the diſſolving Ball be by the wire mov'd either this way or that way, the ariſing ſtream or bubbles will bend the contrary: and to countenance this ſuppoſition, both in thoſe Comets obſerved by *Tycho*, *Kepler*, and alſo in this laſt the beard was contrary to the motion; ſo that the head or body going faſter, ſeemed to leave the beard or tail ſomewhat behind: by this ſuppoſition alſo 'twill be eaſie to explicate why the beard is ſometime bended, and not ſtraight, and why it is ſometimes brighter upon one ſide than upon another? why the bottom of it is more round, and the other ſides more undefin'd; and divers of the like phænomena. Againſt this

this ſuppoſition it ſeems difficult to conceive whence ſo vaſt a body ſhould be generated; next, how it ſhould be able to ſupply ſuch a conſtant ſtream of aſcending parts, and yet laſt ſo long as this has done, almoſt a quarter of a year. Thirdly, Whence ſuch a newly generated body ſhould receive ſo great a degree of motion. In anſwer to which, I ſay, 'tis not impoſſible but that the body of it may be as old as the world, and that it may have then received its firſt determination, or laws of motion, and may have ever ſince preſerved them, that it may have been all this time alſo in diſſolution,and yet not be quite waſted;and that it may continue yet for many ages before it be quite diſſolved into the Æther. And to make this probable, divers experiments and reaſons might be alledged, as that of the ſlowneſs of the waſting of many bodies, by the diſſolution made on them by the fire: the ſlowneſs alſo of the diſſolution of multitudes of bodies in *menſtruums*. And I have already ſhewn how ſmall a quantity of diſſolved particles will be able to make as great a ſhew of light: beſides that, the motion of the aſcending ſtream or beard being but ſlow, there needs no very quick ſupply of other parts. We ſee alſo into what a vaſt quantity of ſmoke a ſmall parcel of a combuſtible body may be turn'd. From all which particulars, 'tis not unlikely but that the Comet may be a body moved with a regular circular or elliptical motion as the Planets are, that it may be a body of ſuch a conſtitution, as that the fluid Æther through which it paſſes, may diſſolve it much after the manner as a *menſtruum*; (ſuch as *Aquafortis*,Spirit of Niter,*&c.*) does a diſſoluble body; that by this means there may be a flow, but continual eruption of ſomewhat opacous parts, which may by their diſſolution afford a ſufficient quantity of light to make as great an appearance as any of the Comets, that this ſtream or beard may by the reſiſtance of the Æther be a little deflected backwards in the ſame manner as an aſcending ſtream of ſmoke will be by the reſiſtance of

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the Air, if the burning body be mov'd this or that way through it, that the body of the Comet may be both as ancient and as lasting as the world; and that this which has lately appeared may have appeared heretofore, and may likewise hereafter appear again; that 'tis probable the nearest distance of it was much greater than that of the Moon, that the length of its Beard was longer than its distance from the Earth, and consequently several times longer than the distance between the Earth and the Moon; that its visible way among the Stars was very differing from a great circle, especially towards the latter end, when it became retrograde; that its way through the Æther could not be supposed equal in a straight line, though it might be supposed equal in a curve or circle, that the exact way of it could not be certainly determined by the best Observations I have yet met with: and that therefore the best help we have to ghess of its way and distance, is by its manner of moving, as to appearance among the fixed Stars, which I have already shewn to be explicable by various Hypotheses: for both the Earth and Comet may be supposed to be moved, either both one way, or contrary ways, or cross ways, the Earth may be supposed to stand still, and the Comet only to be moved, and the like.

These Requisites therefore being hitherto wanting in the Observations I have met with of this Comet, all that can be said of it will at best be but conjectural and hypothetical; since nothing can be reasonably built upon those Observations where the truth of them is dubious; wanting therefore sound materials to work upon in this Comet, I had recourse to the Observations of the noble Dane *Tycho Brahe*, being sufficiently satisfied both of the ability, industry, and veracity of that excellent Author, who left nothing unattempted for the perfecting of such Observations as seem'd to him requisite for the compleating a History of that Comet which appeared in 1577. And from those Observations of his

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I endeavoured to trace the way of it according to ſeveral hypotheſes; and found, that ſuppoſing the Earth not to be moved with an annual motion, but only a diurnal about its own Axis, the way of Comets will fall in a line very near approaching the nature of a circle, though neither into an exact circle, nor an exact ellipſe; and therefore ſeems irregular, and not at all probable. Again, ſuppoſing it moved about the Sun, as *Tycho* has done, we find from his Calculation of it, he was fain to allow it a quicker and ſlower motion in its Orbit, to ſolve the Phænomena, which ſeems to me but a ſhift, that will ſerve to help out any lame Hypotheſis whatſoever: And that granted, and the Parallax of the Comet unknown, I will undertake very eaſily to make out almoſt any Hypotheſis, which is the fault alſo of Mr. *Horox* his Hypotheſis, wherein he ſuppoſes the Earth to be moved about the Sun, and the Comet like a Rocket to be ſhot out of the Sun, and by degrees to return to it again; in which Hypotheſis indeed there ſeems to be much more reaſon for an inequality of motion, though not in the manner as he has placed it; 'twas very rational that the motion of it at firſt, if caſt out of the Sun, ſhould be very ſwift; but then it ought likewiſe to have accelerated its motion in the ſame manner in its return back to it again, which it does not in his Hypotheſis; for a ſtone or any other heavy body being ſhot up into the Air, does make its return back again to the Earth, almoſt by the ſame degrees of velocity, by which it aſcended from it: almoſt, I ſay, becauſe the reſiſtance of the Air does ſo far impede the motion of the body through it, that it never ſuffers it to acquire the ſame degree of velocity with which it was firſt ſhot upward. This is ſufficiently evident from a *Pendulum*, which if it be thrown upwards, and be ſuffered to return back, it will never riſe again on the oppoſite ſide to an equal height, with that it deſcended from, on that ſide towards which it was throʌn: but beſides, in his Hypotheſis he ſeems to take no notice at

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all of the Latitude of the Comet, which ſeemed to carry it much farther off from the Sun, when he ſuppoſes it to be returning nearer. And indeed upon the whole his Hypotheſis ſeems rather a product of chance than of any contrivance. For he in endeavouring to ſet off the Longitude of the Comet according to *Tycho*'s Tables, and to trace its way by ſuppoſing the Earths annual motion, making uſe always of the ſame Radius to ſet off the aſpect, or apparent angle of it with the Sun, his line of Chords he made uſe of did always direct the point of his Compaſſes to the place where he ſituates the Comet, as may be eaſily found by examining the ninth figure; where you may find that he places the Comet always equally diſtant from the Earth, and that diſtance is always equal to the diſtance of the Sun, which has ſo many inconveniencies and improbabiſities, that I ſhall not inſiſt farther on it; eſpecially ſince I do not find that he beſtowed any farther pains in explicating or cultivating this his Hypotheſis, than only the bare delineation of this ninth figure. But to return to *Tycho*'s Hypotheſis, if that be true, why did not the Comet again appear after a certain ſpace of time? and why could not he have foretold when it ſhould again appear, as well as he could predict the appearance of *Venus*, about whoſe Orb he ſuppoſes it to circulate? I ſhall paſs by ſeveral other very material objections that might be made againſt that his ſuppoſition, becauſe many of them might be made alſo againſt his Hypotheſis of the Heavens in general, which I ſhall the rather omit, becauſe I do not find he has many followers in that ſuppoſition; the generality of Aſtronomers embracing rather the *Copernican* Syſtem, eſpecially as it is refined and rectified by the ingenious *Kepler*.

Laſtly, I endeavoured to trace the way of the Comet from *Tycho*'s Tables, according to *Keplers* Hypotheſis; which was, that the appearances of the motion of the Comet were aſcribable to two cauſes; namely,

namely, the motion of the Earth about the Sun in its annual Orbit, and the motion of the Comet in a ſtraight line, not accelerated according to the proportion of the increaſe of Tangents; but upon ſuppoſiſion that it mov'd equal ſpaces in equal times: (for I cannot imagine what reaſon he had to ſuppoſe its motion to be accelerated, and much leſs why he ſhould aſſert it to be according to the proportion of Tangents, which in a little time muſt neceſſarily come to move infinitely ſwift: than which nothing is more hard to be granted.) And I found it after many trials and eſſays to fall in a ſtraight line, inclining to the plain of the Ecliptick by an angle of 47.40. and cutting it in 9 degrees of *Scorpio*, if computed out of the Sun, and moved faſter by half than the Earth in its Orb; and this to ſo great an exactneſs to anſwer all the Obſervations of *Tycho*, that from a very large Scheme which I drew of it on a plain, I could never find many minutes difference; ſo that I concluded that to be the moſt likely Hypotheſis for that Comet, it ſeeming to ſolve all the ſeveral Phænomena of the motion and magnitude of the Comet, with the leaſt imaginable difficulty, and to be moſt agreeable with my phyſical notions of Comets: For, firſt it only ſuppoſes a ſolid body moved in a fluid, with an almoſt direct motion. I ſay, almoſt direct, becauſe for ſome phyſical reaſons, as I have ſaid before, I imagine it not exactly ſtraight, but inflected a little towards the curvity of a circle, which I ſhall preſently endeavour to explain farther in this Comet. Next, it ſuppoſes that body to move in that line almoſt equal ſpaces in equal times; I ſay, almoſt equal, becauſe ſome of thoſe equal ſpaces may be increaſed by an accelerating cauſe or principle, ſuch as that of a gravitation towards the body of the Sun, placed in the center of its Vortice or Syſtem, when the motion of the Comet carries it towards the Sun, and may be diminiſh'd from other impeding cauſes, such as the impediment of the fluid medium through which it paſſes, and the attraction of the

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Sun operating on it when its motion carries it farther and farther off from it: besides, 'tis not unlikely, but that the attraction of the Earth, or some of the other Planets may have some kind of influence on it, especially, when its line of Direction does somewhat nearer approach those attractive points. But the deflection from a straight line is always so much the less by how much the swifter the body is moved, and by how much the farther off its line of trajection is perpendicularly distant from those attracting bodies. According to this supposition of mine, I have endeavoured to make out all the appearances of this last Comet, taken notice of in the best observations I have yet met with, amongst which I find no one of the Parallax satisfactory, as in the tenth figure, let S represent the Sun, O R B, the Orb of the Earth, A C D E F, a bended or curve line in which the Comet is supposed to move: the Comet then coming into the Sphere of the attractive power of the Sun, by the straight line P A G, at A, the power of the Sun worketh on it, and by degrees attracting it towards its own Center by that time the Comet hath moved to C, the attractive power hath deflected its direct course from P A G, to C H, and so the Comet would continue to move in that straight line C H, but it is still deflected so, that at D, it moves towards I, but the gravitation of the Sun attracting it, deflects it from that line towards E, and so from E to F, when it begins again to Jet out of the attractive beams of the Sun, and so it will continue to proceed, as if it had come to that point by the line M F L, the reason of which is the great velocity of these bodies, which are generally much swifter in their motions than the Earth or other Planets are supposed to be, in theirs. We must seek out some other way therefore of finding of the distance of Comets than the commonly used: I shall therefore somewhat further explain the contrivance I newly invented for this purpose, by which not only the Parallax of the Comet but of the Planets

nets alſo may be found with great facility and exactneſs.

Having a large Teleſcope prepared (as I formerly directed) with Eye-glaſſes capable of taking in an Angle of about two degrees at once, and furniſhed with a dividing Scale, obſerve when the motion of the Comet or Planets is not too faſt, the poſition and diſtances of the ſmall fixed Stars which are next adjoyning to the moved body whoſe Parallax you would find; of theſe ſmall fixed Stars you ſhall ſeldom miſs a ſufficient number, which will be taken into the glaſs at once, if at leaſt the object-glaſs be allowed a very large aperture; and having found ſuch Stars as will be convenient for your purpoſe, be very diligent in taking, by the help of the dividing Scale, the exact diſtance of them one from an other, and when the body is higheſt above the Horizon, *viz.* in or near the Meridian, by the ſame means take the exact diſtance of it from two or three of the neareſt and moſt conſpicuous fixt Stars about it, and by the help of a plumb-line, hung likewiſe within the cell, near the dividing Ruler, find exactly the poſitions of all thoſe bodies you take notice of to the Perpendicular or Horizon, which may be eaſily enough done, if together with a Plumb-line or Perpendicular plac'd within the glaſs you have alſo a ſmall Diagonal thred faſtned to a ring, whoſe circumference is divided into 360 degrees, and moveable ſo as by the finger eaſily to be turn'd any way, by which means this Diagonal thred may be made to croſs over any two of the bodies you obſerve, and by obſerving what diviſion of this divided limb the Perpendicular cuts, it will be eaſie to determine the exact poſition of thoſe Stars to the Horizon; this ſame may be done by the dividing Scale alſo, if that be fixt in a divided Circle which is movable, in the ſame manner as the thred is ſuppoſed to be. This Obſervation, with all other circumſtances of it is likewiſe to be repeated at the ſetting or riſing of the Planet or Comet, and again

gain the next night when it comes to the Meridian, and in each of thoſe obſervations the exact time is to be noted by a time-keeper, and the altitude by ſome of thoſe I have before deſcribed, for by comparing theſe three obſervations together it will be very eaſie to find what irregularity in its motion is aſcribable to its Parallax. And this will be ſo much the eaſier becauſe the examination and reduction of it may be done (with as great exactneſs as the obſervation can be made,) by the help only of Ruler and Compaſſes, for all the diſtances will be ſet off by equal diviſions of ſtraight lines, the line alſo of the periodick motion, whether of the Comet or Planet, eſpecially if the obſervations be made when the body is near an oppoſition with the Sun, which is much the beſt time, will be with ſufficient exactneſs taken for a ſtraight line, and the motion in that line may be ſuppoſed by equal ſpaces in equal times; for the difference between the Tangents of the centeſms of a degree to two degrees is not increaſed much more then $\frac{2}{1745}$ that is not a quarter of a centeſm of the hundredth part of a degree, which is much more exact than I fear our obſervations will ever be.

Another way of finding the Parallax may be by the help of exact obſervations made by ſeveral perſons at the ſame time, in places much differing in Latitude, though as near as may be under the ſame Meridian (becauſe of ſaving the trouble of Calculation, and for being aſſured that the obſervations were both made exactly at the ſame time) each perſon by the help of very long Teleſcopes obſerving the exact diſtance of the body from the ſmall fixt Stars next adjoyning.

A third way of finding the Parallax of Comets is wholly new, and though hypothetical (as ſuppoſing the annual motion of the Earth, and the motion of the Comet in a right line through equal ſpaces in equal times) yet 'tis founded upon a Problem in Geometry (invented by the incomparable Mathematician, Doctor *C. Wren*) which is truly noble and wholly new, and though

though it had been of no ufe in Aftronomy, deferves none of the meaneft places in Geometry, by the help of which (which is much more than either of the other ways is capable of) one may eafily find the true parallax of the Comet, from any four exact obfervations of it, made at differing times in the fame place: Nor does it require fo nice and accurate Inftruments and Obfervators as are altogether neceffary in the other ways. The Problem as I received it, is this.

Problema.

Datis quatuor lineis utcunque ductis (quarum nec tres ſunt parallelæ neque ab eodem puncto ductæ) quintam ducere quæ à quatuor primo datis in tres partes ſecetur ratione & poſitione datas.

Sint in Figuris 13, 14, 15, 16, 17, *&* 18, *quatuor rectæ* ADC, BEC, AE, BD, *productæ verſus* K, γ, φ, M. *oportet quintam ducere ut* KM, *quæ ſecetur à primo datis in ſegmenta* KN, NO, OM, *ſecundum datas rationes* R, S, T. *Fiat ut* R *ad* S, T, *ſimul ſumptas ita* CD, *ad* CF. *Rurſus ut* T *ad* S, R, *ſimul ſumptas, ita* EC, *ad* CG, *ductis autem* AGH, BFH, *à mutua interſectione* H, *ducantur* HγK, HφM, *parallelæ nimirum lineis* AC, BC, *quæ mediæ interjacent inter extremas,* BD, AE. *Denique inter puncta extremarum* KM, *ducatur Recta ſecans medias in* NO. *Dico ſegmenta* KN. NO, OM, *eſſe in Data ratione* RST.

Quoniam FD, *parallela eſt ipſi* HK, *ergo ut* CD, *ad* CF, *ita* Kγ, *ad* γH, *& quoniam* γN, *parallela eſt ipſi* HM, *ergo, ut* Kγ *ad* γH, *ita* KN, *ad* NM, *ergo ut* KN, *ad* NM, *ita* CD, *ad* CF; *ſed* CD, *ad* CF, *eſt ut* R *ad* S, T, *ſimul ſumptas, ergo* KN, *eſt ad* NM, *ut* R, *ad* ST, *ſimul ſumptas. Similiter quoniam* EG, *parallela eſt ipſi* MH, *&* φO, *ipſi* HK, *demonſtratur* MO, *eſſe ad* OK, *ut* T *ad* S, R, *ſimul ſumptas. Quare tres* KN, NO, OM, *erunt ad invicem ut* R, S, T, *ergo ducitur linea* KM. *cujus tria ſegmenta à quatuor lineis datis intercepta ſunt in data Ratione* R, S, T, *& ſervata quidem*

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dem positione sive rationum ordine R, S, T, quod erat faciendum.

From the invention of which Problem twill be very easie by any four observations Graphically to describe, or Geometrically to calculate the true distance of the line of the trajection of the Comet, and consequently to answer all those questions that can be demanded concerning the bigness of the body and head, and concerning the bigness and length of the blaze, and concerning the distance of it from the Earth in every part of its way when it was nearest the Earth, when nearest the Sun, where it cuts the Plain of the Ecliptick, seen from the Sun, and where seen from the Earth, with what Angle it was inclined to the said Plain, how swift the motion was, that is, what length it passed, in what time, when it must appear Stationary, when Retrograde, when disappear, and the like.

According to this method I received at the same time, (whilst it yet appeared very visible to the Eye, and was not Retrograde,) the way of the first Comet delineated by the said person, which did very near solve all the appearances preceding and subsequent, which I have therefore here annexed in the Table expressed in the 19. 20. and 21. figures, where in the 19. is delineated the Place of the Sun in the Center of the Circle ♈, N, D, I, ♎, which represents the annual Orb of the Earth about the Sun, the points between N and D represent the places of the Earth in that Orbit in the days of *November*, and the lines drawn from them to the points in the straight line, represent the lines in which the Comet appeared in respect to the Sun; in like manner the points between D and I, the places of the Earth in *December*, and the lines drawn from them to the straight line, as before the visible places of the Comet at those times, *&c.* The 20. figure represents singly the several Longitudes of the Comet at several times seen from the Earth. And the 21. represents the several Latitudes, at the several times, together with the true

true diſtances of the Comet at thoſe times, both which are made out of the 19. figure, where E at the end of the line repreſents the Center of the Earth, from which to the figures in the prickt curve-line, are the true diſtances of the Comet, the Perpendiculars from thoſe figures to the line E C are the ſigns of the Latitude of the Comet from the plane of the Eclipticк E C, the aforeſaid diſtances being made the Radii.

Now though according to my former Delineation the Comet ſeemed to take a circuit, as if it would within three years return to its former poſition, yet I am not wholly convinced that it moves in a circle or Ellipſe, but I rather incline to the incomparable *Keplers* opinion, that its natural motion tends towards a ſtraight line, though in ſome other ſuppoſitions I differ from him.

As firſt that the Comet perſeveres exactly in a ſtraight line. Secondly, that after it has paſt its Perige it accelerates its motion in proportion to Tangents of equal Angles. Thirdly, that it either is extinguiſht diſſipated, broken in pieces, or burnt out into aſhes. Fourthly, that it receives all its light from the Sun. Fifthly, that if the blaze were not made by the beams of the Sun paſſing through the head of the Comet, and ſo carrying the parts along with them, the blaze would not be oppoſite to the Sun. Sixthly, that the cauſe of the bending of the blaze is the refraction of the Suns raies in the body, and their being bent by the Æther as with a wind (which is the opinion that the Ingenious *Deſcartes* follows alſo.) To theſe I cannot conſent, and I have many objections to ſeveral other of his opinions concerning this matter, which would be too tedious to inſert; only I ſhall add, that having traced ſeveral of the Comets according to the beſt obſervations I could get, I found it very difficult to make their motion fall in a ſtraight line, unleſs it be granted that their motions are really accelerated and retarded in that line, which ſeems not ſo probable, at

G 2 leaſt

leaſt not in thoſe parts of their tranſit where he places them. And particularly by tracing the way of this Comet of 1664. it is very evident that either the obſervations are falſe, or its appearances cannot be ſolved by that ſuppoſition, without ſuppoſing the way of it a little incurvated by the attractive power of the Sun, through whoſe ſyſtem it was paſſing, though it were not wholly ſtayed and circumflected into a Circle, as I have already mentioned.

That it is not extinguiſht or quite burnt out, when it ceaſes to appear, I argue from this, that I was able to ſee it with a Teleſcope above a month after it diſappeared to the naked Eye, as may be ſeen by the obſervations I have annext in *Fig.*4. and had not the cloudy weather and the light of the Moon, and nearneſs of the *Crepuſculum* hindred, I ſuppoſe I might have ſeen it much longer, as I am apt to believe the great one in 1618. might have been ſeen ſeveral months longer, if it had been diligently followed with Teleſcopes, it diſappearing in ſuch a part of the Heavens as might have been ſeen every clear night between the *Crepuſculum* and Dawning.

Nor can I ſuppoſe it to receive all its light from the Sun, ſince if ſo it would follow, that the *Nucleus* in the head, would have a dark ſhadow oppoſite to the Sun, the contrary of which has always been obſerved. Nor can I well underſtand that the Sun beams are like a ſtream of water, carrying the parts of the Comet along with them ſo as to make its blaze, ſince no ſuch effect is found of them here with us upon the Earth Nor how they ſhould come to be bended like ſmoke, ſince we obſerve no ſuch property of light in a uniform medium, ſuch as in probability the Æther is.

Theſe were my thoughts about thoſe Comets which appeared in 1664. and 1665. which I have found in ſeveral looſe papers of Lectures, read in the beginning of 1665. And I have not had the opportunity of making many obſervations ſince, concerning Comets, ſave theſe two laſt, in which I had not

not the convenience of obſerving any thing certain concerning its motion or Parallax. And therefore I applyed my ſelf to mark as near as I could the true figure of it, through a ſix foot Teleſcope, and to take notice of as many circumſtances as the ſhort time I had would permit, which though they were very ſhort and tranſitory obſervations, and I wanted time to repeat them ſo often as I could have deſired, yet even from them I was ſufficiently ſatisfied, that I had reaſon to adhere to my former conjecture, that the light of the Comet did not depend wholly from the reflection of the Sun beams, from the parts thereof, but rather from its own light, for upon well conſidering of the form of this Comet, I manifeſtly ſaw that the middle of the blaze was brighter than the ſide parts thereof, and eſpecially that part which was immediatly oppoſite to the Sun, was the brighteſt of all, which would have been otherwiſe if the light had depended wholly from the deflection of the rays of the Sun, for one might rationally conclude that the *Nucleus* or Star in the middle, which reflected ſo great a quantity of light ſhould have cauſed a darkneſs in the parts behind it, as we ſee all ſtrong reflecting bodies do, and conſequently that the middle part of the ſtream or blaze, eſpecially that which was next the body ſhould not have been ſo bright as thoſe other parts to which the light of the Sun had a more free acceſs, unleſs it may be ſaid that even the Star it ſelf, though it ſeem ſo bright, is notwithſtanding not ſo Denſe, but that it admits rays enough to paſs through it unreflected, to inlighten the parts behind it. But this ſeems not ſo likely, ſince be the body of the Star ſuppoſed a thouſand times thinner than a Cloud (which yet tis hard to ſuppoſe, ſince it gives ſo conſiderable a reflection,) yet it being in all probability ten thouſand times bigger in bulk, the rays in paſſing through ſo great a bulk, muſt needs meet with more obſtruction than in the thinneſt Cloud, and yet we find that there is no Cloud ſo thin, but caſts

G 3 ſhadow

ſhadow oppoſite to the Sun, and therefore in probability this would do the like, but I diligently obſerved that there was no ſuch appearance here, but the contrary, that is, that where the ſhadow ſhould have been, there was the lighteſt part of all the blaze, and conſequently in probability it did depend upon ſome other cauſe than a reflection of light.

It is a hard matter to aſſign the particular cauſe of its light, but it ſeems from theſe circumſtances to be very probable that it was (in part at leaſt) from its own nature, whether that might be ſomewhat of that of the Sun and Stars, or of that of our fire, or of that of decaying fiſh, rotten wood, glow-worms, *&c.* or of that of the Ignis Fatuus, at Land or Sea, or like that of Sea-water, or a Diamond, or like that of the falling meteors, or Star-ſhoots, it will be very hard to determine, unleſs one had a much greater ſtock of obſervations to build upon. But it may poſſibly be ſomewhat of the nature of them all, though it agree not in all particulars with any one of them. All theſe ways that I have named ſeeming to agree in one particular, and that is an internal motion of the parts which ſhine, whether that motion be cauſed by ſome external menſtruum diſſolving it as in fire, and Ignes fatui, or an external motion, ſtroke, or impulſe as in a Diamond, Sea-water, and poſſibly ſome Ignes fatui, or from the parts of the bodies working and diſſolving one another,as in decaying fiſh, rotten wood, glow-worms, or whether it be ſuſceptible of a much more ſubtil impulſe, even from light it ſelf, as the *Bononian* ſtone, and *Bladwines* Phſophorus,which ſeems to be ſo harmonious (as I may, ſo ſpeak) to the motion of light, that a new motion is thereby raiſed in it, and continues for ſome time to move of it ſelf after the impulſe or influence ceaſes, not much unlike the uniſon ſtring, or other ſounding body, which in Muſick receives a tremulation and ſound from the motion and ſound of the uniſon body, or ſtring that is ſtruck.

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To me It ſeems moſt probable that the body and parts of the Comet are in a ſtate of diſſolution, whether that diſſolution be cauſed by the parts of the Æther through which it paſſes, after the manner as a Torch is diſſolved by the air, or whether by the internal working of the conſtituent parts one upon the other, as in Gun-powder, ſhining Fiſh and rotten Wood, I cannot determine; but I rather gueſs it to be in ſome things analogous to the one, and ſomewhat to the other, though not exactly the ſame with either. And this I conceive from the figure and make of the ſhining parts, for if it had been of the ſame nature with a Torch, the blaze would have reſembled that of the flame of a Torch or Candle, that is, the ſides would have been brighter, and the middle darker, as I have ſhewn in my *Lampas*; whereas it was very manifeſt that the middle of the blaze was brighteſt, and of that blaze that which was next the Star or *Nucleus* was brighter than that which was further off: whereas in flame the contrary is very obſervable, as I have in the ſaid Treatiſe ſhewn.

From the ſhape of the figure, the manner of its diſſolution ſeems to be thus. The Star or *Nucleus* in the middle, ſeems to be the *fomes* or ſource from whence all the light proceeds: this we ſuppoſe to be a denſe body encompaſt with a very fluid body (ſuch as the Æther ſeems to be) but of ſuch a looſe and ſpongy nature, as that the Æther doth cauſe thoſe parts which are contiguous to it, to be diſſolved and expanded into it ſelf. This diſſolution and expanſion I conceive doth generate or cauſe the light that ſeems to proceed from it, that diſſolution cauſing ſuch a motion of the Æther, as is neceſſary to produce the appearance of light; now ſo long as any part thereof remains in diſſolution, ſo long doth it continue to ſhine, as is alſo obſervable in the flame of any body burning in the air, but when the part ſeparated from the body is quite diſſolved into the Æther, the effect of ſhining ceaſes, as it doth alſo

alſo in the parts of flame. Now I have obſerved that the blaze is ſo very much rarified, that firſt the Æther I conceive comes very freely to every particle of the body after it is ſeparated from it, but eſpecially to the outermoſt, and continues to be incompaſſed with it ſo long as till it be quite diſſolved into it, which I conceive to be at a little farther diſtance from the head than the greateſt length of the blaze ſeems to be to our ſight. And further I conceive that the outward parts being thus incompaſſed more perfectly with the free and undiſturbed Æther, are ſooner diſſolved into it than thoſe of the middle, and conſequently the ſides ſeem firſt to diſappear, and the middle parts continue their ſhining to a much greater diſtance from the Star in the head, though ſomewhat alſo of that appearance may be aſcribed to the diſperſing and rarity of the parts near the ſides.

The *Nucleus* or Ball in the middle of the head, which I have called the Star, I conceive to be diſſolved equally on all ſides, and the parts which are diſſolved or ſeparated from it, I conceive to fly every way from the center of it, with pretty near equal celerity or power, like ſo many blazing Granadoes or Fire-balls, theſe continue their motion ſo far toward the way they are ſhot, till the Levitation from the body of the Sun deflect them upwards, or in oppoſition to the Sun into a Parabolick curve,in which Parabolick curve,every ſingle particle continues its motion till it be wholly burnt out, or diſſolved into the Æther. Theſe are continually ſucceeded by new ſeparations from the aforeſaid body in the ſame manner as tis obſervable in a burning, ſteaming, or ſmoaking body in our air, or a diſſolving body incompaſſed with its proper menſtruum, as I before mentioned, and will ſo continue until the whole be at length diſſolved into the Æther, through which it paſſes.

It hath been demonſtrated by *Torricellius*, of bullets or other bodies caſt or ſhot upwards, that the ſame or or equal bullets discharged or shot out from the same point, with the same degree of strength, but with differing degrees of inclination to the Horizon, each of them shall be moved in a parabolical line, and every one of those parabolical lines shall touch a parabolical line, whose axis is the perpendicular, and whose apex is distant from the said point, the full altitude of the perpendicular shot: So that supposing in the twenty second figure, A to be the point from whence all the shots are made with equal velocity, A C the greatest height of the perpendicular shot, and A D the greatest Horizontal random at 45 degrees of inclination, and suppose E D C D E a parabola passing through those points D C D, all the shots made with equal bullets, with equal velocity from A, but with all variety of inclination between the perpendicular upwards, and the perpendicular downwards that touch the said parabolical line, and consequently if there be an indefinite number of such balls continually flowing out of the point A, with equal degrees of celerity every way dispersing themselves equally *in orbem*, the whole aggregate of such an emanation will make a solid parabolical conoeid E D C D E. Now about the point A, if we suppose a Sphere as B B B B, and from this Sphere an indefinite number of such equal Balls be thrown off perpendicularly to the superficies of it, from every point thereof, with equal celerity at their leaving it, those emanations will form also a conoeid, which will be very near the same with the former: And if this Ball in the middle be supposed a burning and shining body, and that all these emanations have every one of them equal light in proportion to the Globe B B B B A, the effect produced hereby will perfectly resemble the appearance and figure of Comets, if at least the Parabolical conoeid be inverted; which will somewhat explain the manner how I conceive the figure of the Cometical body is naturally, and most proportionably formed; for if the effect of

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ſuch an emanation of ſhining bodies be examined, it will very plainly exhibit the exact and true apparent figure of Comets, as they may be ſeen through a good Teleſcope, which is to me a very great argument, that 'tis the genuine cauſe of its ſhape and figure: Now though the Comets appearance be this way cauſed, and ſo a man might conceive the Globous body would in a little time (by ſo copious an emanation) be conſumed, yet I do not believe that it doth in a ſhort time waſt and diſperſe the whole Ball, nor can I conceive that the diſappearing of thoſe blazing bodies toward the latter end, does depend upon their diſſolution (though poſſibly that may ſomewhat diminiſh them) but that rather is to be aſcribed to their diſtance and poſition in reſpect of us: Though this I remember I obſerved very manifeſtly in that of 1664. that the body toward the latter end of its appearing was very much leſs in proportion to the radiations about it, than it ſeemed to be at the beginning, but whether that might not be partly aſcribed to the great diſtance it then was from us, and the turning of the head pretty near towards us, and thence the ſpreading of the Tail (appearing beyond it,) might add to the breadth of the radiation about the *Nucleus*, I will not poſitively determine. Now though for explication ſake, I have compared the parts ſeparated from the body of the Comet to blazing Granadoes or Fire-balls, yet I would not be underſtood to ſuppoſe theſe parts ſo ſeparated to be of any very large bulk, for I ſee no neceſſity to ſuppoſe them bigger than the Atoms of ſmoke, or the particles of any other ſteaming body, or than the parts of the Air, which make the body of it appear thick and hazy; nor do I believe that all the light of the Star, head, and blaze, does depend only upon the ſhining of the diſſolving body and particles thereof: but I do ſuppoſe that it doth proceed both from the reflection of the Sun-beams from thoſe parts, and alſo from an innate and momentaneous light produced by the

the action of diſſolution wrought on the parts by the incompaſſing Æther.

It may poſſibly ſeem very difficult to ſuppoſe that the diſſolution of the parts of the *Nucleus*, by the incompaſſing Æther, ſhould cauſe or impreſs ſo violent a motion into the ſeparated parts, as to make them depart from it to the ſpace of four or five Diameters, before it be over-powered by the power of Levitation from the body of the Sun, and ſo deflected into a parabolical line upwards. It may likewiſe ſeem ſtrange to ſuppoſe that the Æther ſhould have ſuch power in it, as firſt to diſſolve a body into it ſelf, and ſecondly to cauſe a ſhining, and thirdly to cauſe a Levitation of the diſſolved parts upwards; whereas I ſuppoſed before (and I think 'tis very manifeſt) that they cauſe a gravitation downwards, towards the Center of the Sun: But to theſe for explication, I anſwer that we need not go far for inſtances to make theſe things probable, the Atmoſphere about the Earth, as I have formerly mentioned in my *Micrographia*, I take to be nothing elſe but the diſſolution of the parts of the Earth into the incompaſſing Æther; for the proof of which, I could bring many arguments, were it here a proper place, by which I could moſt evidently demonſtrate the thing to be as I have aſſerted. It is here evident that this Æther doth take up the particles of bodies to a very great diſtance from the ſurface from which they were ſeparated, and it doth not only raiſe them but ſuſteins them at thoſe heights,nor is this peculiar only to the Æther when a menſtruum, but to all diſſolving menſtruums in general.

As to give one inſtance, in ſtead of many, we find that Gold (the heavieſt of all Terreſtrial bodies we yet know,) being diſſolved by *Aqua Regis*, is taken up into it, and kept ſuſpended therein, though the parts of the Gold be fifteen times heavier than the parts of the *Aqua Regis*. So Pit-coal though very heavy, is yet taken up into the Air, and kept ſuſpended there-

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in, though it will be found to be ſome thouſands of times more ponderous than the menſtruum of the Air that keeps it ſuſpended.

Many reaſons I could produce to ſhew the great power of the Æther, and the univerſality of its activity almoſt in all ſenſible motions, but reſerving them for another Diſcourſe hereafter, I ſhall at preſent, only mention thoſe ſuppoſitions which ſeem to have the greateſt difficulty, in this Theory, *viz.* how the diſſolution of the parts of the Star by the incompaſſing Æther ſhould cauſe light, and ſecondly how it ſhould cauſe an actual Levitation of the diſſolving particles upwards. For the explication of theſe two difficulties, I muſt at preſent crave favour to explain them by examples taken from operations of Nature in the Atmoſphere wherein we live, very ſimilar and analogous to them. Firſt, for the production of light, we find that the Air incompaſſing the ſteams of bodies prepared by heat or otherwiſe, and made fit for diſſolution, doth ſo operate upon them, as to make them fly and part aſunder with a very impetuous motion, inſomuch that the ſmall particles or Atoms of the diſſolved bodies, do not only leave one another, but depart and dart out with ſo great an impetuoſity, as to drive off all the incompaſſing Air from their Center from whence they flew, and this I take to be the cauſe not only of their Light, but alſo of their Levity upwards, this may be ſeen very plainly by the ſmall parts of crackling Char-coal, which upon the blowing them with Bellows, and ſo crowding a great quantity of the freſh menſtruum on them, fly and dart aſunder with great celerity and noiſe, but is abundantly more evident in the kindling of Gun-powder, where the impetuoſity is ſo very great as to drive away not only all the incompaſſing Air but all other bodies, though never ſo ſolid, that hinder its expanſion, in the performing of which operation the Æther hath a great ſhare, as I may hereafter ſhew, 'tis very probable that the Æther

Æther in the ſame manner diſſolving the particles of the Star, cauſeth the Atoms thereof to fly aſunder with ſo great an impetuoſity as to leave a vacuity even of the parts of the Æther, which flying aſunder doth not only cauſe light by impreſſing on the Æther a ſtroke or pulſe which propagates every way *in Orbem*, but maketh ſuch an agitation of the the Æther, as cauſes a rarefaction in the parts thereof, whilſt the parts that are once actually ſeparated, by continual rebounding one againſt another before they come to be at reſt and quietly to touch each other, prolong that firſt ſeparation or vacuity between them.

This Explication, though it be ſomewhat difficult, yet I hope it is intelligible, and may be, with probability enough, ſuppoſed to be the true cauſe of the appearance, whilſt there is nothing therein ſuppoſed which is not manifeſtly the method of Nature in other operations; and though the ſuppoſition even of the Æther, may ſeem to be a Chimera and groundleſs; yet had I now time, I could by many very ſenſible and undeniable experiments, prove the exiſtence and reality thereof, and that it doth actually produce not only as ſenſible effects as theſe I have named, but very much the ſame, and many others much more coſiderable, which by Philoſophers have hitherto been aſcribed to quite different cauſes.

Had I been able to have made ſome other obſervations (which I deſigned, if I had had the opportunity of ſeeing it, ſome of the ſucceeding Nights,) I ſhould have hoped to have explained ſeveral other difficulties concerning the nature of the body and blaze of Comets, but being therein prevented, I muſt leave them till I can make ſome further obſervations on ſome Comets that may hereafter appear.

In the mean time that what I have diſcourſed concerning the light of Comets, may not ſeem ſo altogether paradoxical and unintelligible as ſome may ima-

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gine, I have here added an account of ſome trials and obſervations made on ſhining ſubſtances of natures exceedingly differing from thoſe that are commonly to be met withal. And this I the rather do, not only becauſe it affords an inſtance of ſhining where there is no Air, but that hereby I may enlarge the limits of their imagination, who ſhall conſider of this ſubject. For nothing is more apt to miſguide our reaſoning than a narrow and limited knowledg of cauſes, we are not to conclude the body of a Comet a ſulphureous vapour exhaled from the Earth and kindled above, becauſe here are ſuch vapours obſerved and ſuch effects produced, nor a collection of Sun beams made by a Lentiformed vapour, after the manner of a Burning-glaſs (as ſome eminent Writers have lately done,) becauſe ſome ſuch appearances may be Artificially produced in a ſmoaky or thickned Air; ſince if we diligently inquire, we may find that light which is the moſt ſenſible quality of Comets that affects our ſenſes, may be, and really is produced by very many, and thoſe very differing ways. In Nitre and Sulphur kindling each other by heat, we have one way; in a body burning in the Air a ſecond, in a heated Iron or Glaſs a third, in a piece of Iron hammered till red hot a fourth, in rotten Wood and decayed Fiſh a fifth, in Glow-worms, *Scolopondras*, and other living Worms, and in the ſweat and excrements of other living creatures a ſixth, in a Diamond rubbed a ſeventh, in Dews Ignes fatui, *&c.* an eighth, in Sea-water a ninth, in the *Bononian* ſtone, and in the Phoſphorus *Baldwini* (which I take to be much of the ſame nature) a tenth, in the Phoſphorus of Mr. *Kraft* an eleventh, and poſſibly wholly differing from all theſe, may be the light of the Sun, a twelfth, and that of the Star may differ from that of Sun, and the Comet may be differing from all the reſt. Whether they be ſo or not, the being acquainted with the ſeveral proprieties of them will the better enable one to judg of what is perti-
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nent to be obſerved in Comets, in order to find out which is concerned.

The Phænomena of moſt of theſe ſhining bodies are very common and obvious, and therefore needleſs to be added; but that of the *Bononian* ſtone prepared, and that of the *Phoſphorus Baldwini* (lately diſcovered by Mr. *Baldwine*) are rare and hard to be got, and the effects of them are wholly differing from all the ways I have yet met with, and will therefore prove *Experimenta Crucis*, highly inſtructive in the Theory of Light, of which more hereafter. As for the *Phoſphoros Fulgurans* of Mr. *Kraft* (more ſcarce and rare than the other) 'tis wholly differing from any of the reſt, and very ſtrange and ſurpriſing, at leaſt it appeared ſo to me, who had the good fortune to be preſent at a good part of the experiments made by the Author in the preſence and at the Chamber of the Honourable *Robert Boyle*, Eſq; that great Judg and Promoter of all curious inquiries into Nature and Art, who at my earneſt intreaty, was not only pleaſed to commit to writing what he obſerved, but (for the information of Curious and Inquiſitive Naturaliſts,) to give me liberty here to publiſh it.

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A ſhort Memorial of ſome Obſervations made upon an Artificial Subſtance, that ſhines without any precedent Illuſtration.

September, 1677.

ON *Saturday* the fifteenth of this month I was after ſupper viſited by Mr. *Kraft*, a famous *German* Chymiſt, who was pleaſed to come and ſhew me a ſtrange rarity he hath newly brought into *England*, to the ſight whereof he allowed me to invite ſeveral members of the Royal Society, he being deſirous, becauſe the matter he imploys is very coſtly and of difficult preparation, to be a good Husband of it, and by ſhewing it to ſeveral curious perſons at once, to exempt himſelf from the need of ſhowing it often. The Company being met, the Artiſt took out of a pretty large box he had brought with him, divers Glaſs Veſſels and laid them in order on the Table. The largeſt of them was a Sphere of Glaſs, which I gueſſed to be four or five Inches in Diameter, being hollow and intire, ſave that in one place there was a little hole, at that time ſtopt with ſealing wax, whereat to pour in the Liquor, which ſeemed to me to be about two Spoonfuls or ſomewhat more, and to look like muddy water made a little reddiſh with brick-duſt or ſome other powder of that colour, he alſo took out of his Box three or four little pipes of Glaſs ſealed, or o-

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therwiſe ſtopt at both ends, being each of them ſomewhat bigger than a Swans quill, and about five or ſix Inches long, and having at one end a ſmall fragment or two of that matter that was to ſhine in the dark.

He likewiſe laid upon the Table three or four Vials of ſeveral ſizes, but none of them judged capable to hold above very few Ounces of water: in each of which Vials there was ſome Liquor or other, that was neither tranſparent nor well coloured, which Liquors I confeſs upon his making no particular mention of what they were to do, I was not curious to compare together, either as to quantity or as to colour. Beſides all theſe ſubſtances which were fluid, he had in a ſmall Cryſtalline button Bottle, a little lump of matter, of which he ſeemed to make much more account than of all the Liquors, and which he took out for a few moments to let us look upon it, whereby I ſaw that it was a conſiſtent body, that appeared of a whitiſh colour, and ſeemed not to exceed a couple of ordinary Peaſe, or the kernel of a Haſel Nut in bigneſs, ſome other things 'tis poſſible Mr. *Kraft* took out of his Box, but neither I or (for ought I know) others of the Company took notice of them, partly becauſe of his haſt, and partly becauſe the confuſed curioſity of many ſpectators in a narrow compaſs, kept me from being able to obſerve things as particularly and deliberately as I would gladly have done, and as the occaſion deſerved. Which Advertiſement may I fear be but too applicable to a great part of the following Narrative.

The forementioned Glaſſes being laid in order upon the Table, the windows were cloſed with woodenſhuts, and the Candles were removed into another Room by that we were in; being left in the dark we were entertained with the enſuing Phænomena.

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I. Though I noted above that the hollow Sphere of Glaſs had in it but about two Spoonfuls (or three at moſt) of matter, yet the whole Sphere was illuminated by it, ſo that it ſeemed to be not unlike a Cannon bullet taken red hot out of the fire, except that the light of our Sphere lookt ſomewhat more pale and faint. But when I took the liberty to hold this Glaſs in my hand and ſhake it a little, the contained Liquor appeared to ſhine more vividly, and ſometimes as it were to flaſh.

II. I took one of the little pipes of Glaſs formerly mentioned, into my hand, and obſerved that though the ſhining matter had been lodged but at one end, yet the whole Glaſs was enlightened, ſo that it appeared a luminous Cylinder, whoſe light yet I did not judg to be always uniform, nor did it laſt like that which was included in the Vials.

III. In the largeſt of the Vials next the Spherical already mentioned, the Liquor that lay in the bottom being ſhaken, I obſerved a kind of ſmoke to aſcend and almoſt to fill the cavity of the Vial, and near the ſame time there manifeſtly appeared as it were a flaſh of lightning that was conſiderably diffuſed, and pleaſingly ſurprized me.

IV. After this I took up that ſmall Cryſtaline Vial that I lately called(by a name familiar in our Glaſs-ſhops) a Button-Bottle, wherein was contained the dry ſubſtance which the Artiſt chiefly valued, as that which had continued luminous about theſe two years, and having held that Vial long in my hand, in the ſame poſition in reference to my eye, and lookt attentively at it, I had the opportunity to obſerve (what I think none of the Company did) that not only this ſtuff did in proportion to its bulk, ſhine more vividly than the fluid ſubſtances, but thaat which was the Phænomenon I chiefly attended) though I could perceive no ſmoke or fumes aſcend from the luminous matter, yet I

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could plainly perceive by a new and brisker light that appeared from time to time in a certain place near the top of the Glaſs, that there muſt be ſome kind of flaſhy motion in the matter that lay at the bottom, which was the cauſe of theſe little coruſcations, if I may ſo call them.

V. The Artiſt having taken a very little of his conſiſtent matter, and broken it into parts ſo minute, that I judged the fragments to be between twenty and thirty, he ſcattered them without any order about the Carpet, where it was very delightful to ſee how vividly they ſhined; and that which made the ſpectacle more taking, eſpecially to me, was this, that not only in the darkneſs that inironed them, they ſeemed like fixt Stars of the ſixth or leaſt magnitude, but twinkled alſo like them, diſcovering ſuch a ſcintillation as that whereby we diſtinguiſh the fixt Stars from moſt of the Planets. And theſe twinkling ſparks without doing any harm (that we took notice of) to the Turky Carpet they lay on, continued to ſhine for a good while, ſome of them remaining yet vivid enough till the Candles being brought in again made them diſappear.

VI. Mr. *Kraft* alſo calling for a ſheet of Paper and taking ſome of his ſtuff upon the tip of his finger, writ in large Characters two or three words, whereof one being *DOMINI*, was made up of Capital Letters, which being large enough to reach from one ſide of the page to the other, and being (at leaſt as I gueſſed) invigorated by the free contact of the external Air, ſhone ſo briskly and lookt ſo oddly, that the ſight was extreamly pleaſing, having in it a mixture of ſtrangeneſs, beauty and frightfulneſs, wherein yet the laſt of thoſe qualities was far from being predominant. And this Phænomenon did in more ſenſes than one afford us the moſt of light, ſince not only the Characters ſhone very vividly upon the white Paper, but approaching it to my Eyes and Noſtrils, I could diſcern

diſcern that there aſcended from them a fume, and could ſmell that fume to be ſtrong enough, and (as it ſeemed to me) to participate of the odour of Sulphur and of that of Onions. And before I paſt from the mention of theſe reſplendent Characters, I muſt not forget that either by their light, or that of the Globe, or both by the one and the other a man might diſcern thoſe of his fingers that were neareſt the ſhining ſtuff, and that this being held to the face though without touching it, ſome of the conſpicuouſeſt parts, eſpecially the Noſe, were diſcoverable.

VII. After we had ſeen with pleaſure, and not without ſome wonder, the fore-going particulars, the Artiſt deſired me to give him my hand, which when I had done, he rub'd partly upon the back of it, and partly on my cuff, ſome of his luminous matter, which as if it had been aſſiſted by the warmth of my hand ſhone very vividly, and though I took not notice of any thing upon my skin, that was either unctuous or rough, yet I often times tried in vain by rubbing it with my other hand to take it off, or manifeſtly diminiſh its ſplendor, and when I divers times blow'd upon ſome of the ſmaller parts of it, though they ſeemed at the inſtant that my breath beat upon it, to be blown out, yet the tenacious parts were not really extinguiſht, but preſently after recovered their former ſplendor. And all this while this light that was ſo permanent, was yet ſo mild and innocent that in that part of my hand where it was largely enough ſpread, I felt no ſenſible heat produced by it.

By that time theſe things were done 'twas grown late, which made Mr. *Kraft*, who had a great way to go home, take leave of the Company after he had received our deſerved thanks for the new and inſtructive Phænomena, wherewith he had ſo delightfully entertained us.

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Becauſe Mr. *Kraft* had twice attempted to fire heated Gun-powder with his Phoſphorus, but without ſucceſs; probably becauſe the powder was not very good (as by ſome circumſtances I conjected) and becauſe it was not ſufficiently heated before the matter that ſhould ſet it on fire was put upon it, he promiſed me he would come another time to repair that unſuccesfulneſs: And accordingly, On the two and twentieth of *September* in the Afternoon I recived a viſit from Mr. *Kraft*, who told me he came to make good his promiſe of letting me ſee that his ſhining matter was able to kindle heated Gun-powder, and becauſe no ſtrangers were preſent, I had the fairer opportunity to view it, which I was able to do better by day light, than I had done by its own light, for when he had taken it with a new Pen out of the liquor with which he kept it covered to preſerve it, I perceived it to be ſomewhat leſs than the nail of one of my fingers, and not much thicker than a ſhilling, and I obſerved that when it had lain a little while upon a piece of clean Paper and diſcharged it ſelf from its ſuperfluous moiſture, it began to emit whitiſh fumes which ſeemed to be very ponderous, ſince for the moſt part they did not aſcend but ſurrounding the matter whence they iſſued, by their ſtagnation made as it were a little Pond or ſmall Atmoſphere about it; ſo that leſt it ſhould waſt too faſt, he was obliged as ſoon as he had cut off a little corner leſs than half a pins head, to put the ſtuff nimbly back into the Vial out of which he he had taken it; where I obſerved it for a very ſhort time to ſend up exhalations into the liquor that covered it, and quickly after, as it were, quencht it. This done the Artiſt divided the little corner he had cut off into two parts, one of which he ſpread as far as it would reach upon a piece of white Paper, which he preſently after held at a diſtance over a chafing-diſh of burning Coals, by whoſe heat being excited it preſently flaſht and burnt away, and I having perceived that there was another part of the Paper

Paper which though not heeded by him, had been lightly befmeared by the fame matter, I held it over the Coals, but at a confiderable diftance from them, and yet this little matter nimbly took fire and burnt a hole in the Paper. And to fatisfie my felf that the heat did but excite the luminous matter, and that twas this its felf that lighted the Paper, I held the reft of the fame piece of Paper far nearer the fire and kept it there a pretty while without finding it at all fcorched or difcoloured. Laftly, the other part of the divided fragment of the hitherto mentioned matter, Mr. *Kraft* put upon the tip of a quil, and having at a diftance from the fire, very well dryed and warmed fome Gun powder upon another piece of Paper, he laid that Paper upon the ground, and then holding his quill upon it, as if it had been a match, within half a minute (by my guefs) that powder took fire and blew up.

'Twill not perhaps be impertinent to add that on occafion of the operation I obferved the Air to have on the fhining fubftance when freely expofed to it. I took a rife to tell Mr. *Kraft* that I prefumed it might be worth while to try whether his Phofphorus did fhine by virtue of a kind of real or (if I may fo call it) living flame, which like almoft all other flames required the prefence and concourfe of the Air to maintain it, or whether it were of fuch a kind of nature as the Phofphorus of the learned *Baldwinus*, which I fufpected to fhine not like a flame or a truly kindled fubftance; but like a red hot Iron, or an ignited piece of Glafs, wherein the fhining parts are not repaired by fewel, as in other burning bodies, but are put by the action of the fire into fo vehement an agitation as whilft it lafts fuffices to make the body appear luminous. This conjecture Mr. *Kraft* feemed much to approve of when I told him that the way I propofed to examine his *noctiluca* by, was to put a little of it into our Pneumatick Engine, and Pump out the Air, whofe abfence abſence, if it were of the nature of other flames, would probably extinguiſh, or very much impair its light, but yet ſince he offered not to have the trial made; probably becauſe he had but very little of his ſhining ſubſtance left, I thought it not civil to preſs him. But to countenance what I ſaid of the nature of *Baldwinus Phoſphorus*, I ſhall recite an Experiment that I purpoſely made, to examin whether the preſence of the Air were neceſſary to the ſhining of this Phoſphorus, as I had long ſince found it to that of ſome pieces of ſhining wood.

We expoſed for a competent time to the beams of a vigorous light, a portion of matter of about the breadth of the palm of ones hand, which we had prepared to be made luminous by them. And then cauſing the Candles to be removed (for we choſe to make tryal by night) we nimbly conveyed the matter into a receiver that was kept in readineſs for it, preſuming (as the event ſhewed we might) that by uſing diligence the light would laſt as long as the experiment would need to do; making haſt therefore to Pump out the Air, we heedfully watched whether the withdrawing of it would, contrary to my conjecture, notably diminiſh the light of the ſhining matter. And after we had thus withdrawn the Air gradually, we tryed whether by letting it return haſtily, it would produce a more ſenſible change in the matter (which had been purpoſely put in without any thing to cover it, that it might be the more expoſed to the Airs Action.) But neither upon the gradual receſs of the Air, nor yet upon its ruſhing in when it was permitted to return, could we certainly obſerve any manifeſt alteration in the luminouſneſs of the Phoſphorus, other than that ſlow decrement that might well be imputed to the time during which the experiment was making. It being well known that this luminous ſubſtance requires no long time to make it decay, and by degrees to loſe all its light; ſo that though once there ſeemed to one

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or two of the by-ſtanders, upon the return of the Air, to be ſome recovery of part of the loſt ſplendor, yet after repeated experiments it was concluded that the preſence of the Air was not at all *neceſſary* to the ſhining of our matter, and it was judged moſt probable that the abſence or preſence of the Air, had *no manifeſt* operation on it. I might add to this that perhaps the preſence of the Air is rather hurtful than advantagious to this ſort of lights, ſince for having had a large Phoſphorus that was much eſteemed, and, whilſt I kept it, exactly protected from the Air did very well; a part of the Glaſs that covered it, having by miſchance been ſomewhat crackt, though none of the ſplinters appeared diſplaced, yet it ſeems ſome of the Corpuſcles of the Air made a ſhift to inſinuate themſelves at theſe chinks (as narrow as they were) and in not many days made the matter ceaſe to be capable of being made luminous as before. I cannot ſtay to inquire whether this unfitneſs or indiſpoſition may be imputed to the bare moiſture of the Air, or to ſome other ſubſtance or quality that alone or in conjunction with the moiſture, may ſpoil that peculiar texture, or conſtitution that fits the matter of the Phoſphorus aſſiſted by the impreſſions of external light to become luminous. This, I ſay, I cannot ſtay to examine, though, That this Phoſphorus is of a nice and tender conſtitution, and eaſily alterable, I was induced to think, by finding that the want of circumſtances, ſeemingly ſlight enough, would keep it from being made; and I gueſs that a convention of circumſtances did more contribute to the production than any peculiar and incommunicable nature of the matter: Becauſe having had the curioſity to make ſome trial upon ſo obvious a material as quick Lime, though the ſucceſs did not anſwer my deſigns, yet, neither was it ſo bad, but that ſome luminous quality was produced in the Lime by the action of the fire, and a ſaline Liquor; and I ſcarce queſtion but other materials will be found

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capable of being made luminous by the ſame or the like operation, that is imploy'd by *Baldwinus*, when that learned man ſhall think fit to communicate his way to the Publick. But to return to what I was ſaying, that the contact of the Air might be rather hurtful than advantagious to the Phoſphorus, I ſhall only add here as matter of fact, (for my conjectures about Light belong to my yet unpubliſht Notes, *of the Origine of Qualities*) that whereas the contact of the Air, though it were not free, did in a few days deſtroy the luminouſneſs of a good Phoſphorus, yet having included another in a Receiver, whence we afterwards pumpt out the Air, this matter though inferior to the other in vividneſs was ſo little ſpoiled by lying open in our Vacuum, that at the end of not only ſome weeks, but ſome months, I found that the beams of a Candle paſſing to it through the Receiver, would notwithſtanding the Vacuum it yet continues in, ſuffice to re-excite in it a manifeſt light.

Thus far was the communication of this excellent perſon, who it's hoped may be further prevailed with to communicate thoſe other accurate obſervations, and curious reſearches he hath made concerning the light of the *Bononian* Stone, and the *Phoſphoros Baldwini*, which are indeed truly admirable, and very much differing from the uſual proceſſes of Nature for the exhibiting of light.

Before I take leave of my Aſtronomical Readers, I ſhall here acquaint them with ſome Collections I have made of other Aſtronomical matters and diſcoveries, which I hope will not be leſs pleaſing to them than they were at firſt to me. The Diſcoveries are new, and not leſs ſignificant. The firſt is,

A Let-

A Letter from Johannes Carolus Gallet, *L.L.D. and Provost of the Church of St.* Symphorean *at* Avignon, *directed thus.*

CLarissimo Eruditissimoque viro D. Johanni Dominico Cassino Matheseos Professori Celeberrimo, Astronomo præstantissimo & Academiæ Regiæ scientiarum alumno meritissimo.

Conteining an account of his observation of Mercury passing under the Sun.

Mr. *Gallet* then acquaints Mr. *Cassini* with his observation of ☿ *sub* ☉ and the whole method and process of his observation. First, he fitted two excellent Telescopes, the Glasses of which were given him by Mr. *Jac. Borrellius*, one of the Academy Royal of *Paris*. The one of twenty three foot, he fitted with a Glass covered with smooke, placed in the outward *focus* of the Eye-Glass: The other of three foot he fixt to the Arm of his Quadrant of the same Radius, this was so exquisite that compared with one of *Divini*, which was chosen by the care of *Honorato Fabri*, and procured by Monsieur *de Beauchamps*, it was found to represent the objects clearer: By this the figure of the Sun was cast on an opposite Table, on which he had drawn a Circle of the bigness proper to the Distance and Magnifying of the Glasses to contain the whole Face of the Sun, and by Parallel Circles had subdivided the same into digits and Sexagesimals, he had also placed three threds in the interior *focus* of the Glasses, that the middlemost went through the Center, and the two outward touched the Limb of the Sun by their shadow on the Table, he had also a Pendulum Clock that vibrated thrice in a second. Thus accoutred he watched the fifth

K 2 and

and ſixth day, from Sun riſing to Sun ſetting, and the ſeventh after the Cloudy Sky had ſeemed to delude his curioſity till Eleven a Clock almoſt, it then began to open and diſcovered to him Mercury got within the Eaſtern Limb of the Sun, about $\frac{1}{6}$ of its Semidiameter; at length the Clouds being diſperſed, the Sun being 27° 45. high, or at 10 h. 54′ ☿ it ſelf marked out its own place in the disk of the ☉ by its own ſhadow caſt on the Table by the ſhorter tube. Then he diſpoſed the ſhadow of the aforeſaid thred ſo Paralel to the Equator, that this figure of the Sun ſhould move between the outward ones, and that the middle ſhould mark out the Paralel deſcribed by the Center of the Sun in motion,at the ſame time he took the declination of ☿ from this middle Parallel and the right Aſcention, by the number of Vibrations of the Pendulum, from the Weſtern Limb of the Sun, taken by the ſhadow of a Perpendicular Croſs-line to the other 3. by the ſame means, alſo he meaſured the Diameter of the Sun and of Mercury.

Then to the end he might give leſs cauſe of doubt,according to his uſual cuſtom, he procured ſeveral friends who were preſent and witneſſes of all the obſervations after the fourth mentioned in the Table. During the obſervation he took notice of theſe remarkable accidents. Firſt, that Mercury through the long Tube was very black, and of an Elliptical figure whoſe longeſt Diameter was Parallel to the Equator, but in the Species through the leſſer Telecope, it appeared round and of a dusky red (like a ſpot obſerved by him in the Sun from the Ninth to the Fifteenth of *April.*) Secondly, that the Diameter of Mercury going out of the Diſc of the Sun, when it toucht the periphery ſeemed to be of four times the Diameter it appeared of through the whole Phaſe, ſo that Mr. *Beauchamp*, who watched the exit with the longer Tube, whilſt he himſelf minded the Quadrant in order to take the Altitude of the Sun, at the time of the exit cried out, O how large do

I ſee

I ſee the Diameter of Mercury now, it does not only leave the Sun, but is confuſed with it, or as it were melts into it, and preſently it vaniſht, the Sun being then 13. 23′. high.

He further adds that before he leaves to ſpeak of the Sun, he will here inſert an obſervation that he had made of four ſpots he had ſeen in the Sun in the firſt of *October* laſt (*St. No.*) with this his longer Teleſcope, one only of which was viſible by the Species caſt with the leſſer Glaſs.

Octob. Die.	hora		*Declinat. maculæ principalis à paral. Centri* ⊙.			*Differentia temp. inter limbum* ⊙ *Occident. & maculam.*			*Tempus tranſitus diſci Solaris.*		
1	10	0	4	44	*auſtr.*	1	4	20	2	10	0
2	10	0	2	43	*auſt.*		49	0	2	10	20
3	10	30	1	21	*auſt.*		35	40	2	10	20
4	10	35	0	40	*auſt.*		25	40	2	10	30
6	10	0	3	0	*boreal.*		3	20	2	10	4

Thus ſubmitting his method to the judgment of the Learned *Caſſini*, and earneſtly deſiring his thoughts thereon, he ends his Letter, and Dates it from *Avignon*, *Nov.* 21. 1677.

To this Letter he ſubjoins the obſervation it ſelf, Intituled,

Mercurius ſub Sole viſus Avenione *die* 7. Novemb. 1677. *Obſervante me* Joanne Carolo Gallet, *J. V. D. Præpoſito Eccleſiæ Sancti Symphoriani* Avenionenſis.

The Contents of which are,

That deſigning to obſerve this paſſage of ☿ under ⊙ he with his Tube watchfully looked for it in the Suns place, from the 5th to the 7th. day, with a Teleſcope of

23 foot (as above) he obſerved a ſpot of an elliptical figure which had already gotten a 16th. part of the ſemidiameter of the Sun within the limb, and declined a little to the South in reſpect of the parallel of the Æquator drawn through the Suns center, at 10 hours 26 min. but the Clouds hindering he could not obſerve its motion till it had aſcended as high as the parallel: when the Suns altitude was 27. 45. or 10 a Clock 54 minutes. From the quickneſs of its motion he ſoon found it to be ☿ and not a ſpot, and therefore he forſook not his Quadrant to which was fitted his three foot Teleſcope and Table to receive the figure of the ☉ but obſerved the times of the Immerſions and the Emerſion of ☿ by the help thereof, being aſſiſted by ſeveral of his friends who were witneſſes of what paſſed, and particularly by the Illuſtrious Monſieur *De Beauchamp*, who with the twenty three foot Glaſs determined the Exit of *Mercury*, whileſt he himſelf took the Altitude of the Sun with his Quadrant, as in the tenth Obſervation.

The

The Order of the Obſervations of Mercury ſeen under the Sun.

The number of the Phaſes obſerved.	The north Declination of ☿ from the Parallel of the equat. through the Center.		The difference of the time between the Tranſit of Weſt Limb of the Sun, and the body of ☿ under the ſame Meridian, collected from the Pendulum Vibrating $\frac{1}{3}$ of a ſecond.				The diſtance of ☿ from the Center of the Sun.			The apparent altitude of the Sun.		The hour Collected by the Altitude.		
	M.	S.	vib.Pend.	M,	S.	T.	M.	S.	T.	G.	M.	H.	M.	S.
1	0	0	345	1	55	0	11	20	37	27	45	10	53	58
2	2	3	276	1	32	0	6	0	55	29	36	12	0	0
3	2	45	258	1	26	0	5	9	20	29	34	0	9	55
4	3	40	224	1	14	40	4	7	30	29	03	0	35	50
5	5	30	164	0	54	40	7	13	7	25	0	1	44	10
6	6	30	148	0	49	20	8	15	0	24	5	1	55	22
7	6	53	132	0	44	00	9	16	45	22	30	2	11	58
8	8	14	100	0	33	20	12	1	45	19	30	2	39	14
9	8	55	80	0	26	40	14	5	30	17	17	2	57	28
10	9	38	39	0	13	00	16	30	0	13	23	3	26	56

	The time of the Tranſitus.				The Diameter in the Parallel.			The Diameter in a great Circle.		
of the Sun.	414	2	18		34	30	0	43	0	0
of Mercury.	3½	0	1	10	0	17	30			

From this Obſervation he had the Declination of Mercury in reſpect of the Parallel through the Center of the Sun, and thence its abſolute Declination from the Equator, ſuppoſing the place of the Sun according to *Hecker*, and the obliquity of the Ecliptick, 23, 30 the right Aſcenſion alſo of ☿ appeared by the difference of time between the Tranſit of ☿ and the Weſt limb of the Sun by the ſame meridian. Then from the Declination and right Aſcenſion of ☿ given by Trigonometrical Calculation, he found out the Longitude and Latitude of it in every Obſervation, and the time of its true Conjunction.

The

	The time of the phases observed.			*Heckers* place of the ☉ in ♏			The Decl. of ☿ South ascen.			The right Ascension of Mercury.			The N. Lat. of ☿ ascen.		The Long. of Mercury in ♏		
	H.	M.	S.	G.	M.	S.	G.	M.	S.	G.	M.	S.	M.	S.	G.	M.	S.
1	10	53	58	15	33	55	16	32	33	223	16	40	3	10	15	44	48
2	12	0	0	15	36	41	16	31	38	223	13	43	3	14	15	40	40
3	0	9	55	15	37	6	16	30	43	223	12	37	3	53	15	40	30
4	0	35	50	15	38	11	16	30	7	223	10	51	3	55	15	38	27
5	1	44	10	15	41	3	16	29	7	223	8	54	4	15	15	36	3
6	1	55	22	15	41	31	16	28	12	223	7	59	4	55	15	35	6
7	2	11	58	15	42	13	16	28	4	223	7	36	4	56	15	34	40
8	2	39	14	15	43	22	16	27	4	223	7	4	5	48	15	34	5
9	2	57	28	15	44	8	16	26	36	223	6	10	5	57	15	33	0
10	3	26	56	15	45	23	16	26	15	223	5	50	6	12	15	32	37

Therefore the time of the true conjunction of the Sun and Mercury at *Avignon*, was *Nov. 7. Hor.* 2. *Min.* 39. *Sec.* 14. Afternoon.

To this he hath adjoyned this ensuing Table, to shew how much the Heavens do differ from the Astronomical Tables.

Tempus datum ex Tabulis Novemb.	D.	H.	M.	D.	H.	M.	*Differentia ab observata conjunctione.*
Rudolphinis Reyneri.	7	8	3	0	7	24	excessus.
Calculis Heckeri.	7	6	9	0	5	9	excessus.
Lansbergianis.	6	1	12	0	23	27	defectus.
Philolaicis Bullialdi	7	4	18	0	3	39	excessus.
Ricciolinis juxta calculum. R. admodum Patris Bonifa. Societatis Jesu.	7	8	17	0	7	38	excessus.

These Obſervations are delineated in the 23. Figure.

Upon this Obſervation I find in the twenty third Journal de Scavans *of the Year* 1677. *Mr.* Caſſini *made theſe Reflections,*

THat having compared this Obſervation of Monſieur *Gallet*, of 1677. with that of Mr. *Gaſſendus*, of 1631. the ſame day of the year, to wit the ſeventh of *November*, he found that the Latitudes of ☿ at its leaving the Diſc of the Sun, determined by theſe two Aſtronomers were equal, even to the ſixth part of a minute. And by conſequence that ☿ was both in the one and the other Obſervation at the ſame diſtance from its North node, and that it traced in the Diſc of the Sun an equal line: And for that ☿ was here at the like diſtance from its Apoge; as the Sun was alſo pretty near, the ſwiftneſs of its apparent motion in the Sun was equal. By the Obſervation of Mr. *Gallet* it is found conſiderably more ſlow than that which Mr. *Gaſſendus* hath ſuppoſed from the Rudolphin Tables of which he made uſe for the determining of it, not having been able to make Obſervation immediately by reaſon of the Clouds. He believes then that ☿ ſpent more then five hours in running through the Diſc of the Sun, ſince by the Obſervation of Mr. *Gallet*, it hath ſpent 5 hours and 35 minutes, which may ſerve for an Advertiſement for determining more exactly the time of the true conjunction of ☿ with the ☉ in the year 1631.

The ſame equality of Latitude at Mercury's leaving the Sun ſhews that the Sun was equally diſtant from the Node of Mercury at the time of theſe two Obſervations. And as the Sun was more advanced in that of this year from 63 to 64 minutes, than in that of the year 1631, So it follows that the ſeptentrional

L Node

Node of ☿ is advanced from 63 to 64 minutes in the ſpace of 46 years, as preciſely as by the Rudolphin Tables, which agree alſo exactly in the Epochas of the Nodes: a matter of no ſmall Importance in Aſtronomy, which hath not a little difficulty to determine with preciſeneſs the Nodes of the Planets and their motions.

But having compared the obſervation of Mr. *Gallet*, with that of Mr. *Hevelius*, in 1661. which hapned the third of *May*, in a place of the Zodiac oppoſite to that of this year, he hath found the ſeptentrional Node of ☿ leſs advanced than the Meridional was in the preceding Obſervation; ſo that if the Nodes of ☿ in regard of the Sun are preciſely oppoſite the one to the other, it appears that they have gone backward ſince the year 1661. as do thoſe of the Moon, and by conſequence their motion is ſometimes direct, ſometimes retrograde: But if their motion is ſuppoſed uniform, it will follow that the Line of the Nodes of ☿ doth not paſs at all through the center of the Sun, but that it is removed from it towards the ſeptentrional limit about a two hundredth part of the Semidiameter of the Orb of *Mercury*.

Thus far this knowing and accurate Aſtronomer Monſieur *Caſſini*, who we hear hath ſince farther diſcourſed concerning this matter, which we hope to procure ſo ſoon as he ſhall make it publick; and to add ſome other curious Obſervations made by other hands, I have as yet been able to procure but one more; but that is one ſo conſiderable, that it will excite the skilful Aſtronomers anew to ply their Calculations, to ſee what the comparing of this with the reſt will produce; which as they come to my hands, I deſign to publiſh, as I ſhall alſo ſomewhat of my own Obſervations thereupon: and therefore I omit to make any reflections at preſent. This Letter is of Mr. *Edmund Hally*, now reſiding at St. *Helena*, directed to Sir *Jonas Moore*, Surveyor of his Majeſties Ordnance; a perſon to whom the

the Learned world is very much obliged for his patronizing and promoting these Cœlestial enquiries; who hath not been sparing of his own pains and purse in providing the best *apparatus* of instruments and other conveniences for such Observations the world ever had; from whom we may with good reason hope a great advancement towards the perfecting thereof.

St. Helena, *Novemb.* 22. 1677.

Honored Sir, You may with reason wonder that I should so long be negligent to write to your Worship, to give you an account of my proceedings since my departure from you, seeing that in the business I am now engaged upon, the Honorable Sir Joseph Williamson, *his Majesties Principal Secretary of State, and your self are my only Patrons: but I have not been unmindful of my Duty in this particular, only I delayed, that what I sent you might not be altogether inconsiderable. I hoped still that we might have some clear weather when the Sun came near our Zenith, that so I might give you an account that I had near hand finished the Catalogue of the Southern Stars, which is my principal concern; but such hath been my ill fortune, that the Horizon of this Island is almost always covered with a Cloud, which sometimes for some weeks together hath hid the Stars from us, and when it is clear, is of so small continuance, that we cannot take any number of Observations at once; so that now, when I expected to be returning, I have not finished above half my intended work; and almost despair to accomplish what you ought to expect from me. I will yet try two or three months more, and if it continue in the same constitution, I shall then, I hope be excusable if in that time I cannot make an end. However it will be a great grief to be so far frustrated in my first undertaking: I have notwithstanding had the opportunity of observing the ingress*

L 2 *and*

and egreſs of ☿ *on the* ⊙, *which compared with the like Obſervations made in* England, *will give a demonſtration of the Suns Parallax, which hitherto was never proved, but by probable arguments. Likewiſe I have ſeen thoſe two Eclipſes, one of the Sun, the other of the Moon in* May *laſt, both which I ſend you, but the* mighty winds, and extraordinary ſwift motion of the Clouds *hindred the exactneſs of the Obſervations. That of the Moon may help for the difference of our Meridians, which is about 7 degrees to the Weſtwards of* London: *but it may more curiouſly be found by* Mercury ſub Sole. *There are three Stars of the firſt Magnitude that never appear in* England, *but none near the South Pole of any brightneſs, except one of the third Magnitude, which is about ten degrees diſtant from it. The two Nubeculæ called by the Saylors the* Magellanick *Clouds, are both of them exactly like the whiteneſs of the milky way lying within the Antartick Circle; they are ſmall, and in the Moon ſhine, ſcarce perceptible; yet in the dark the bigger is very notable. I need not relate unto you the temperature of the Weather for heat and cold here in the Torrid Zone, you your ſelf having long ſince had experience of a Latitude little different: only this I ſhall certifie you, that ever ſince I came to this Iſland, we have had no weather that is hotter than the Summer of* England *is ordinarily. Mr.* Clark *is a perſon wonderfully aſſiſtant to me, in whoſe company all the good fortune I have had this Voyage conſiſteth, to me all other things having been croſs: nevertheleſs I deſpair not of his Honors and your Worſhips favour, which alone is ſufficient to encourage me to bear with patience theſe diſappointments, and expect ſome fitter opportunity.*

I am your Worſhips moſt obliged
Servant, and true Honorer,

Edmund Halley.

Octobris

St. Helenæ, Latitudo Auſtralis, 15. 55.

Anno 1677.

Octobris 28. *die* ☉ *mane* ☿ *apparuit intra* ☉.

h.	m.	s.	
9	26	17	*Pars aliqua corporis* ☿*ii intraſſet Solem decem gradus à nadir ad dextram circiter.*
9	27	30	*Formabat angulum contactus totus* ☿ *ſcilicet. intus*
2	38	39	*Limbus* ☿*ii proximus diſſiti à limbo Solis ſui Diametro.*
2	40	8	*Limbus* ☿*ii tetigit limbum* ☉.
2	41	0	*Centrum* ☿ *exiit è Sole* 30 *grad. circiter à Nadir ad dextram.*
2	41	54	☉ *limbus integer factus.*

Longitudo & Latitudo trium Stellarum illuſtrium prope polum auſtrinum.

	Long.			*Latit.*	
Canopus	11	3	♋	75	49
Centauri pes	25	24	♏	42	22
Alcarnar.	10	31	♓	59	18½.

L 3 *The*

The Period of the Revolution of Jupiter *upon it Axis; verified by new Observations made by Monsieur* Cassini:

Extracted out of the Journal de Scavans.

This Revolution of the body of ♃ upon its *Axis* I first discovered in *May* 1664. and published in the first Transaction, which was a considerable time before it was discovered by Monsieur *Cassini*; but we are obliged to him for the perfecting the Theory, as we are also for many other rare Discoveries and excellent improvements in Astronomy.

THE Globe of *Jupiter*, whose Revolution about its *Axis* was determined by the Observations of Monsieur *Cassini*, in the Year 1665. to be 9 hours, and 56 minutes, is as it were a watch for visibly pointing the hours and minutes to half the Earth at once; so that it shews the same time to all under the same Meridian, and a different time to different Meridians, according as they differ in Longitude.

It hath for an Index of its motion one principal spot, which is very neatly distinguished from the rest of its surface, and seems from its figure and situation to have some resemblance to the Caspian Sea of the Terraqueous Globe. By the help of good Glasses it may be seen passing the under Hemisphere of it, from the East to the West, with a velocity so sensible, that one may determine to one or two minutes, the time that it comes to the middle of the Disc, which is the place the most fit for establishing of the Epochas, and for finding the difference of Longitude There may be a great number of such Revolutions observed, since in one year of 365 days

days there are made 882 Revolutions. But it doth not appear in every year, but as if it were ſome kind of Mariſh which is dried at certain times, and ſo diſappears during two or 3000 Revolutions; and after it hath remained thus imperceptible for ſome years, it returns again to its former ſtate. After it had been obſerved the laſt ſix months of the year 1665. and ſome months of 1666. it became inviſible till the beginning of the year 1672. then being returned to its former appearance, Monſieur *Caſſini* compared the intervals of the ſix years, and limited the revolution to be made in 9 hours, 55 minutes, 51 ſeconds; and continuing his Obſervations to the end of the year 1674. he found by theſe two years that it was too ſlow by two ſeconds and a half: ſo that it appeared to be in 9 hours, 55 minutes, $53\frac{1}{2}$ ſeconds.

This ſpot hath been inviſible in 1675. and 1676. during which ſpace there happened other very conſiderable changes in the body of *Jupiter*; for the clear interſtice which was between the two dark belts of *Jupiter* was ſeparated into many little parts, in the manner like ſo many Iſlands; as if the two obſcure belts had been two great Rivers broken one into the other, and had left theſe parts which appeared like Iſlands, which yet were at laſt all effaced, and the two dark belts, and the interjacent ſpace at length all coaleſced into one large belt. But after the coming of *Jupiter* out of the Rays of the Sun in the year 1677. the belts again took their form, and ſituation which they had heretofore; to wit, the ſame which is deſcribed in the 24 figure. The principal ſpot appeared anew after the beginning of *July* laſt. Monſieur *Caſſini* found this ſpot in the middle of *Jupiter* the night after the eighth of the ſaid month, at 13 minutes after one at night; and hath hitherto ever ſince obſerved it at the hours proper to its revolution. Having compared many Obſervations of this year with as many others made the ſame days of the year 1665. for avoiding the ſcruples which may a-
riſe

rise from the inequality of times, he hath found by the intervals of twelve years that those revolutions compared the one with the other, complete themselves in 9 hours, 55 minutes, 52 seconds, and 5 or 6 thirds. And because that in the years 1672, 1673. they appeared more slow by 2 seconds and a half, during the time that *Jupiter* was in its greatest elevation from the Sun. Monsieur *Cassini* inclines to suppose that these revolutions have some little inequality depending on the variation of the distance of ♃ from the ☉, and that they are a little slower when ♃ is more removed, and somewhat faster when nearer approached that body; the same which several great Astronomers have supposed to happen to the Diurnal Revolutions of the Earth in the Copernican Hypothesis.

In this account he hath separated the inequality which doth result from the variation of the two equations of *Jupiter* (as he hath explained in divers Letters in 1665.) the which may amount to one half hour, besides the inequality of natural days, which according to his Hypothesis may amount to 16 minutes.

For the finding then of the return of the principal spot to the middle of ♃ for many years to half an hour or thereabout, there needs nothing but adding still the time of the period to the Epoche of the 8. of *July*, 1677. and for the finding precisely, even to some minutes, the two inequalities of *Jupiter* must be observed according to the following Rule.

Differentiam inter medium locum Jovis & apparentem converte in tempus dando singulis gradibus min. 1_3. *hoc tempus adde tempori restitutionis maculæ supputato, si locus apparens Jovis excesserit medium: subtrahe vero si defecerit à medio.*

We have then the mean time of the return of the spot, and to get the apparent time the, equation of days according to the method of Monsieur *Cassini* (of which a Table is inserted in the Ephemerides of Monsieur *Flaminio de Mezzavachi*) must be made use of.

MI-

MICROSCOPIUM:

O R,

Some new Diſcoveries made with and concerning *Microſcopes.*

A Letter of the Ingenious and Inquiſitive Mr. Leeuwenhoeck *of* Delft, *ſent to the Secretary of the* Royal Society, *October* 5. 1677.

IN this Letter after the Relation of many curious Obſervations made with his Microſcope, he adds, 'By ſome of my former Letters I have related what an 'innumerable company of little Animalcules, I have 'diſcovered in waters; of the truth of which affirma-'tions, that I might ſatisfie the Illuſtrious Philoſophers 'of your Society, I have here ſent the Teſtimonials of 'eight credible perſons; ſome of which affirm they 'have ſeen 10000, others 30000, others 45000 little 'living Creatures, in a quantity of water as big as a 'grain of Millet (92 of which go to the making up the 'bigneſs of a green Pea, or the quantity of a natural 'drop of water) in the deſiring of which Teſtimonials 'I made it my requeſt that they would only juſtifie '(that they might be within compaſs) half the number 'that they believéd each of them ſaw in the water, and 'even ſo the number of thoſe little creatures thát would 'thereby be proved to be in one drop of water would 'be ſo great, that it would exceed belief. Now where-'as by my Letter of the 9th. of *October*, 1676. I affirmed 'that there were more than 1000000 living Creatures 'contained in one drop of Pepper-water. I ſhould not

M have

'have varied from the truth of it, if I had aſſerted that 'there were 8000000; for if according to ſome of the 'included teſtimonials there might be found in a quantity of water as big as a millet ſeed, no leſs than 45000 'animalcules. It would follow that in an ordinary drop 'of this water there would be no leſs than 4140000 'living creatures, which number if doubled will make '8280000 living Creatures ſeen in the quantity of one 'drop of water, which quantity I can with truth affirm I have diſcerned.

'This exceeds belief. But I do affirm, that if a larger 'grain of ſand were broken into 8000000 of equal 'parts, one of theſe would not exceed the bigneſs of one 'of thoſe little creatures; which being underſtood, it 'will not ſeem ſo incredible to believe that there may 'be ſo great a number in the quantity of one drop of 'water.

Upon the peruſal of this Letter, being extremely deſirous to examine this matter farther, and to be aſcertained by ocular inſpection as well as from teſtimonials. I put in order ſuch remainders as I had of my former Microſcopes (having by reaſon of a weakneſs in my ſight omitted the uſe of them for many years) and ſteeped ſome black pepper in River water, but examining that water about two or three days after, I could not by any means diſcover any of thoſe little creatures mentioned in the aforeſaid Letter: though I had made uſe of ſmall glaſs canes drawn hollow for that purpoſe, and of a Microſcope that I was certain would diſcover things much ſmaller than ſuch as the aforeſaid Mr *Leeuwenhoeck* had affirmed theſe creatures to be; but whether it were that the light was not convenient (the reaſon of which I ſhall ſhew by and by) having looked only againſt the clear sky, or that they were not yet generated, which I rather ſuppoſe, I could not diſcover any. I concluded therefore either that my Microſcope was not ſo good as that he made uſe of, or that the time of the year

year (which was in *November*) was not ſo fit for ſuch generations, or elſe that there might be ſomewhat aſcribed to the difference of places; as that *Holland* might be more proper for the production of ſuch little creatures than *England.* I omitted therefore farther to look after them, for about five or ſix days, when finding it a warm day, I examined again the ſaid water; and then much to wonder I diſcovered vaſt multitudes of thoſe exceeding ſmall creatures, which Mr. *Leeuwenhoeck* had deſcribed; and upon making uſe of other lights and glaſſes, as I ſhall by and by ſhew, I not only magnified thoſe I had thus diſcovered to a very great bigneſs, but I diſcovered many other ſorts very much ſmaller than thoſe I firſt ſaw, and ſome of theſe ſo exceeding ſmall, that millions of millions might be contained in one drop of water. I was very much ſurprized at this ſo wonderful a ſpectacle, having never ſeen any living creature comparable to theſe for ſmallneſs: nor could I indeed imagine that nature had afforded inſtances of ſo exceedingly minute animal productions. But nature is not to be limited by our narrow apprehenſions; future improvements of glaſſes may yet further enlighten our underſtanding, and ocular inſpection may demonſtrate that which as yet we may think too extravagant either to feign or ſuppoſe.

Of this, A later Diſcovery of Mr. *Leeuwenhoeck* does ſeem to give good probabilities; for by a Letter of his ſince ſent (the which is hereunto annexed) it appears he hath diſcovered a certain ſort of Eels in Pepper-water, which are not in breadth above one thouſandth part of the breadth of a hair; and not above a hundredth part of the length of a vinegar Eel.

M 2 *Mr.* Leeuw-

SIR, ‘Yours of the thirtieth of *November* I received ‘not till *January*, whereby underſtanding the kind ‘reception of my former by the *R. S.* I here return my ‘acknowledgment to that illuſtrious Company for their ‘great civility: but I wonder that in your Letter I find ‘no mention made of my Obſervations of the ſecond of ‘*December, St. No.* which makes me doubt whether the ‘ſame came to your hands.

‘Since you aſſure me that what I ſend of this nature ‘will be acceptable to the renowned Society, I have ad-‘ventured again to ſend you ſome of my farther Enqui-‘ries, to be communicated to that learned Philoſophical ‘Company. Since I wrote of the Blood of Eels, and ‘of young Eels, I have not been idle to view Blood, ‘but eſpecially my own, which for ſome time I have in-‘defatigably examined, after that I had put it into all ‘conceivable motions. Among which Obſervations I ‘well ſaw that the *globuli* of my own blood took the ‘ſame figure which I formerly mentioned, that the Glo-‘bules of the blood of Eels appeared of to the eye: ‘upon ſeeing which I doubted again at the cauſe of the ‘ſmart which the blood of the Eels cauſes in the eye.

‘Theſe my many times repeated Obſervations of my ‘own blood I made to no other end, than if it were poſ-‘ſible, to obſerve the parts out of which the Globules ‘of the blood conſiſted: With obſerving this, I found ‘the globulous blood much more pliable than I did ima-‘gine the ſame before. I have at ſeveral times bended ‘theſe Globules before my eyes, that they were three ‘times as long as broad, without breaking the Veſicule ‘of them: and beſides I ſaw that the Globules of blood ‘in paſſing by and through one another, did, by reaſon ‘of their pliableneſs receive many ſorts of figures, and ‘coming thence into a larger place, they recovered their former

'former globulofity which was a very great pleafure
'to obferve: and withal, that the Globules of blood
'coming many together, and growing cold thereby,
'came to unite, and made a matter very fmooth,
'wherein there were no more parts diftinct to be taken
'notice of, much after the fame manner as if we fup-
'pofed a Difh filled with balls of wax fet over a fire,
'by which they would quickly be melted together, and
'united into one mafs; by which uniting of the Glo-
'bules, I concluded this to be the reafon of the acci-
'dent which is called the *cold fire*, and of that alfo
'which caufes the hands or fingers to be loft by cold:
'but I leave this to others. And I did very clearly alfo
'difcover that there were fix other fmaller Globules of
'blood contained within each of the former and lar-
'ger Globulous Vefícles: and withal, I took much
'pains to obferve the number of the fame very fmall
'globules, out of which the greater Globules do con-
'fift: that at laft I ftrongly imagined, that every of the
'greater Globules confifted of fix fmaller Globules, no
'lefs pliable than the aforefaid: for oftentimes I faw
'very clearly how the fmall Globules joyned and ada-
'pted themfelves according to the figure the Vefícle
'or larger Globule ftretched at length had taken, being
'themfelves ftretched after the fame manner: and thus
'made one of the larger Globules ftretcht out, to ap-
'pear by the leffer within it ftretched alfo with it,
'as if it confifted of long threads. Moreover, I
'put the greater Globules into fo violent a motion, that
'their Vefícles burft in pieces, and then the leffer Glo-
'bules appeared plainly to be fcattered. This firft
'Globule I can fee as plainly and great, as with the na-
'ked eye one fhould look upon the eggs or fpawn of a
'Cod-fifh.

'About nine or ten years fince Dr. *Graff* opened in
'my prefence the vein of a Dog, and let out fo much
'blood that the Dog grew faint; then he opened the
'Artery of another Dog, and by a pipe transfufed the

M 3 blood

'blood of this ſecond into the firſt, whereby the firſt 'was recovered, the ſecond was faint. Then the ſaid Do-'ctor injected back into the Artery of the ſecond, a 'quantity of Cows milk, ſuppoſing thereby to preſerve 'the ſecond dog alive, ſaying, milk was blood: but 'no ſooner was the milk put into the artery, but the 'dog died. And whereas 'tis commonly ſaid that milk 'is Blood, therefore I ſhall relate of what parts the 'Milk conſiſts, ſo far as I have hitherto diſcovered. I 'have ſaid heretofore that the Milk doth conſiſt of 'Globules ſwimming in a thin clear watery matter 'which we call Whey: but as the great *Globuli* of 'Blood are all of the ſame bigneſs, ſo in the Milk they 'are quite differing, being of as many ſizes and magni-'tudes as we can imagine, between the ſmalleſt ſand, 'and a barley corn; all of them being as clear as Cry-'ſtal; ſave only that through and between the ſame 'drive ſome irregular particles for the moſt part roun-'ded: theſe had a fatty ſubſtance, which I imagined 'to be the latter: their irregularity I imagined came 'from the impreſſion of the encompaſſing Globules 'made on them, in which poſture they grew cold.

'Viewing the aforeſaid differences of the Milk Glo-'bules, I ſuppoſed that the Milk veſſels have no 'other parts included but the matter out of which they 'are all made; and that the ſame matter, ſo long as in-'cluded in the veſſels, conſiſted of one uniform matter, 'ſo that one could not diſtinguiſh parts; and that the 'ſame veſſels diſcharging this uniform matter into other 'veſſels, containing a ſubſtance of a quite differing na-'ture, which I ſuppoſe to be the Whey, comes to be 'ſeparated into theſe Globules of ſo differing magni-'tudes. This may be repreſented by having two veſ-'ſels filled, the one with Fat, repreſenting Whey; the 'other with Quickſilver, reſembling the uniform mat-'ter of the Milk: theſe blended together, the Quick-'ſilver will be ſeparated into ſmall Globules of differing 'magnitudes, and kept diſtinct by the fat.

Or

‘ Or further, it may be explained by a diſſolution of ‘ſome gums in Spirit of Wine, a drop of which being ‘put into rain water (which I compare to Whey) the ‘Gum becomes ſeparated immediately into an incredi- ‘ble number of ſmall clear Globules, which makes it ‘appear alſo as white as Milk it ſelf: and thence I ſup- ‘poſe that the whiteneſs of Milk hath the ſame cauſe.

‘I have been often minded by ſome, that fleſh was ‘nothing elſe but clodded blood; yet for all my en- ‘deavours I was never able to find the firſt particles of ‘blood in the fibers of the fleſh, but only ſuch as are ‘contained in the firſt Globules.

‘The laſt Summer being ſickly for ſome weeks, I ‘voided much Flegm, which was green, tough, and ‘acid in the throat, which yet continues; but nothing ‘near ſo much as before: and ſome of it which I void- ‘ed in the morning was of ſo heavy a matter, that it ‘ſunk in the water: the ponderoſity of it I found to ‘proceed from its not being filled with airy bubbles, ‘which moſt Flegms are mixed with. By this means I ‘obſerved my Flegm very often, and found it to con- ‘ſiſt of tough ſlimy moiſture, mixt with many Globules; ‘and the tougher the Flegm was, the greater was the ‘quantity of Globules; and from them alſo proceed- ‘ed the green colour of it. All theſe Globules were of ‘one and the ſame bigneſs with the firſt Globules of ‘the blood; and indeed the blood is of the ſame ‘make, but only of a different colour: for as I ob- ‘ſerved the greater blood Globules to conſiſt of ſix ‘leſſer, ſo here I could ſee them more plain; only they ‘ſeemed more ſlender and tender than in the blood: ‘the reaſon whereof I ſuppoſe to be that the veſicules of ‘the Flegm Globules had already received ſome kind ‘of corruption: beſides, there was mixt with the tough ‘part of the Flegm great quantity of very thin cuti- ‘cles: and in the ſame manner as I have heretofore ex- ‘plained how our cuticle is ſupplied underneath, as the ‘upper part is rubbed off in ſcurf, ſo I ſuppoſe the in-
ner

'ner cuticles of the gullet *aspera arteria*, and other vessels are taken off by the Flegm. There drove also, 'through the Flegm some other particles, which from 'their smallness I could not assign them a figure, but I 'conceived them rather cubical than round. I did last 'Summer shut up some Caterpillers to spin Webs, and 'within these few days I broke some of these Webs, 'when from each of them came out a flie, which from 'the cold were very weak, and were unable to stand; 'by which I conceive that those which came not out in 'the latter part of the year, remain the whole Winter 'in their Webs, till the warmth makes them come out. 'I was pleased to understand that your self and the '*Society* had seen in so small a quantiy of water as a 'sand, so great a number of Creatures; as also, that I 'shall be partaker of what you shall observe, which I 'shall with longing desire expect. I cannot but mention that that small sort of Creature which I heretofore could give no description of, I now see their figure. And for the pleasure I take in the various pleasing shapes, with their motions, which do now and 'then appear in the water, I have the fourth of this 'month, when it froze hard, taken a third part of beaten pepper, and $\frac{2}{3}$ of high rain water in a clean glass, 'which I set the first night in my Bed-chamber; the next 'day, the weather being milder, I set it in my Counting-house, and in three times 24 hours discovered so 'great a number, and so unexpressible small Creatures, 'that 'tis hard to be conceived; and according to my 'judgment, the most of them were much less than a 'thousandth part of the thickness of the hair of ones 'head, and three or four times as long as thick; the 'which made, with the hinder part of their body, oft-times so swift a progress, as when we observe a Pike 'shooting through the water, and every shoot was in 'length most times about half a hairs breadth; the 'other sorts or kind of which were yet smaller, whose 'shape for brevity I omit; only I shall say, that oft-
times

‘times in pepper-water which hath ſtood ſomewhat ‘long, among the very ſmall Creatures, I have ſeen a ‘ſort of ſmall Eels which had their ſhapes and moti-‘ons as perfect as great ones: theſe were to my ‘appearance a thouſand times thinner than the hair of ‘ones head, and that if 100 of theſe ſmall Eels were ‘laid in length one behind another, the whole length ‘would not extend to the length of the Eel in vine-‘gar: Whether you have alſo obſerved theſe ſmall ‘Creatures with your Microſcope, I ſhall be glad to un-‘derſtand. I would willingly alſo be informed whether ‘my Letter of the ſecond of *December* mention'd above ‘be come to your hands, and how thoſe Obſervations ‘do pleaſe the Gentlemen of your Society; and alſo ‘to underſtand the receipt of this.

The manner how the ſaid Mr. *Leeuwenhoeck* doth make theſe diſcoveries, he doth as yet not think fit to impart, for reaſons beſt known to himſelf; and therefore I am not able to acquaint you with what it is: but as to the ways I have made uſe of, I here freely diſcover that all ſuch perſons as have a deſire to make any enquiries into Nature this way, may be the better inabled ſo to do.

Firſt, for the manner of holding the liquor, ſo as to examine it by the Microſcope, I find that the way preſcribed by Mr. *Leeuwenhoeck* is to include the ſame in a very fine pipe of glaſs, and then to view it by the help of the Microſcope; for by placing that at a due diſtance, whatever is contained in the ſaid liquor will moſt eaſily be diſcovered: The liquor will moſt eaſily inſinuate it ſelf into the cavity of the ſaid pipe, if the end thereof only be juſt put within the liquor. This as it is exceedingly convenient for many trials, ſo is it not very difficult to prepare; but becauſe every one is not inſtructed how to proceed in this matter, and it may cauſe him more trouble than needs to procure them, I will here deſcribe the way; and ſo much the rather, becauſe the ſame *apparatus* will

N ſerve

ſerve for the preparing of Microſcopes: as I ſhall afterwards ſhew.

Provide then a box made of tin, with a flat bottom, and upright on all ſides; let this have fixed within it to the bottom a ſmall piece of tin, hollowed like a ridg tile, ſo that the wiek of the Lamp may lie and reſt upon it, and let the Tin-man fix on it a cover of tin, ſo that there may be only left one part of the aforeſaid box open, to wit, where the bent tin piece and the wiek do lie and come above the ſides: this cover may be turned back on its hinges when there is occaſion to raiſe the wiek, or put in more oyl, &c. but for the moſt part ought to lie flat and covered; for whilſt it is uſing, it is neceſſary to keep the flame from ſpreading too much, and taking fire all over. This box muſt ſtand within another box of tin, made large enough to contain it; the uſe of which is to keep the former Lamp Box from fowling the board or table on which it ſtands: This ſtands upon a board about one foot ſquare, into which is faſtned a ſtandard or ſtick upright, cleft ſo as to pinch and hold the ſodering pipe between its clefts, which may be faſtned with a ſcrew, or a ſlipping ring; through which pipe, blowing with your breath, the flame will be darted forward with great ſwiftneſs and brightneſs: if then into this flame you hold a ſmall piece of a glaſs pipe, made of white glaſs, (for green glaſs, or coarſer glaſs will not be melted eaſily in this flame) and keep it turning round between your fingers and thumbs, you ſhall find that the flame will in a very ſhort time melt the middle part of the ſaid pipe; ſo that if you remove it out of the flame, and draw your hands one from another, you may eaſily draw the former pipe into a very ſmall ſize, which will yet remain hollow, though drawn never ſo ſmall. The beſt Oyl for this purpoſe is good clean Sallat Oyl, or Oyl Olive; but high rectified Spirit of Wine is yet better, and cleanlier, but much more chargeable; and for moſt uſes the Oyl Olive will ſerve. This I have ſet down, becauſe

cauſe many who are far off in the Country cannot have the convenience of going to a Lamp-blower as oft as they have occaſion for ſuch pipes; which if they provide themſelves with ſmall white glaſs pipes from the Potters, they may accommodate themſelves withal, though they have nothing but a large candle, and a tobacco-pipe, inſtead of the aforeſaid *apparatus*, though not altogether ſo conveniently. But I would rather adviſe them to have a Lamp made, which moſt Tin-men know how to fit and prepare; and ſo it will not need much more deſcription.

But this way of Mr. *Leeuwenhoecks*, of holding the liquors in ſmall glaſs pipes, though it be exceedingly ingenious, and very convenient for many examinations, yet for divers others 'tis not ſo well accommodated as this which I contrived my ſelf for my own trials, at leaſt for thoſe Microſcopes I make uſe of; what it may be for thoſe which Mr. *Leeuwenhoeck* uſes I know not.

I take then inſtead of a glaſs pipe a very thin plate of *Muſcovy* glaſs, this ſerves inſtead of the moveable plate which is uſually put upon the pedeſtal of Microſcopes; but becauſe the common pedeſtal hitherto made uſe of in Microſcopes is generally not ſo convenient for trials of this nature, I lay thoſe by, and inſtead thereof I fix into the bottom of the Tube of the Microſcope, a cylindrical rod of Braſs or Iron. Upon this a little ſocket is made to ſlide to and fro; and by means of a pretty ſtiff ſpring, will ſtand faſt in any place. This hath faſtned to it a joynted arm of three or four joynts, and at the end a plate about the bigneſs of a half crown, with a hole in the middle of it about three quarters of an inch wide; upon this plate I lay the *Muſcovy* glaſs, and upon that I ſpread a very little of the liquor to be examined; then looking againſt the flame of a Candle, or a Lamp, or a ſmall reflection of the Sun from a globular body; all ſuch parts of the liquor as have differing refraction will manifeſtly appear. By this means I examined the water in which

N 2 I had

I had ſteeped the pepper I formerly mentioned; and as if I had been looking upon a Sea, I ſaw infinite of ſmall living Creatures ſwimming and playing up and down in it, a thing indeed very wonderful to behold.

If the flame of the candle were directly before the Microſcope, then all thoſe little Creatures appeared perfectly defin'd by a black line, and the bodies of them ſomewhat darker than the water; but if the candle were removed a little out of the axis of viſion, all thoſe little Creatures appeared like ſo many ſmall pearls, or little bubbles of air, and the liquor in which they ſwimmed appeared dark; but when the water began to dry off, the bending of the ſuperficies of the liquor over their backs, and over the tops of other ſmall motes which were in the water made a confuſed appearance, which ſome not uſed to theſe kind of examinations, took to be quite differing things from what they were really; and the appearances here are ſo very ſtrange, that to one not well accuſtomed to the phænomena of fluids of differing figures and refractions, the examinations of ſubſtances this way will be very apt to miſ-inform, rather than inſtruct him; eſpecially of ſuch ſubſtances as are not perfectly fluid, and will not readily and naturally ſmooth their own ſuperficies, ſuch as Tallow, concreted Oyls, Marrow, Brains, Fat, inſpiſſated juyces, *&c.* for if thoſe ſubſtances be ſo examined by ſpreading them upon this plate, and be looked upon againſt the candle, or other ſmall defined light, all the inequalities left on the ſurface by the ſpreading do by the refractions of the rays of light render ſuch odd appearances, that they will eaſily deceive the examinator, and make him to conceive that to be in the texture of the part which is really no where but in the make of the ſuperficies of it. This therefore as another great inconvenience to be met with in Microſcopical Obſervations, I prevent by theſe enſuing methods: Firſt, all ſuch bodies as Fat, Oyl, Brains, Rhobs, *Pus*, tough concreted Flegm, and the like, whoſe ſurfaces are

are irregular, and ought to be reduced to ſmoothneſs before they can be well examined, I order in this manner: Firſt, I provide a very clear and thin piece of looking-glaſs plate very ſmooth and plain on both ſides, and clean from foulneſs: upon the ſurface of this I lay ſome of thoſe ſubſtances I laſt mentioned, then with ſuch another piece of Looking-glaſs plate laid upon the ſaid ſubſtance I preſs it ſo thin as not only to make the ſurfaces of it very ſmooth, but alſo to make the ſubſtance of it very thin; becauſe otherwiſe, if the ſubſtance be pretty thick, as ſuppoſe as thick as a piece of *Venice* paper, if it be a whitiſh ſubſtance, the multitudes of parts lying one upon another in ſuch a thickneſs, do ſo confound the ſight, that none of them all can be diſtinctly ſeen: but if by ſqueezing the ſaid plates hard, and cloſe together, it be reduced to a twentieth part perhaps of that thickneſs, the ſubſtance may be well looked through, and the conſtituent parts may be very plainly diſcovered. Thus alſo 'tis very viſible in the Globules of milk and blood, diſcovered by the ingenious Mr. *Leeuwenhoeck*, for when either of thoſe ſubſtances are thick, the multitude of thoſe little Globules confound and thicken the liquor ſo as one cannot perceive any thing until it be run very thin; for then all the remaining Globules with their motions may very diſtinctly be apprehended. This therefore is an expedient by which thouſands of ſubſtances may be examined; and therefore the more fit to be communicated, that there may be the greater number of obſervers well accommodated for ſuch trials. Theſe plates therefore may be contrived ſo as to be pinched together by the help of ſcrews, and a frame, that thereby they may be forced the cloſer and the evener together, as there ſhall be occaſion; and may be kept firm and ſteady in that poſture, and then, that it may ſome ways or other be conveniently faſtned to the former plate, ſo as to be moved this way or that way ſteadily, as there ſhall be occaſion.

N 3 But

But there are other ſubſtances which none of theſe ways I have yet mentioned will examine, and thoſe are ſuch parts of animal or vegetable bodies as have a peculiar form, figure, or ſhape, out of which if it be put, the principal thing looked after is deſtroyed: ſuch are the Nerves, Muſcles, Tendons, Ligaments, Membranes, Glandules, Parenchymas, *&c.* of the body of Animals, and the Pulps, Piths, Woods, Barks, Leaves, Flowers, *&c.* of Vegetables. Some of theſe which are not made by diſſection or ſeparation from other parts may be viewed alone; but there are others which cannot be well examined unleſs they be made to ſwim in a liquor proper and convenient for them: as for inſtance, the parts of fleſh, muſcles and tendons: for if you view the fibres of a muſcle encompaſſed only with the air, you cannot diſcover the ſmall parts out of which it is made: but if the ſame be put into a liquor, as water, or very clear oyl, you may clearly ſee ſuch a fabrick as is truly very admirable, and ſuch as none hitherto hath diſcovered that ever I could meet with; of which more hereafter, when I ſhew the true mechanical fabrick thereof, and what cauſes its motion. Thus if you view a thred of a Ligament, you ſhall plainly ſee it to be made up of an infinite company of exceeding ſmall threads ſmooth and round, lying cloſe together; each of which threads is not above a four hundredth part of the bigneſs of a hair: for comparing thoſe of Beef with a hair of my head, which was very fine and ſmall, *viz.* about a 640. part of an inch, I found the Diameter thereof to be more than twenty times the Diameter of theſe threads; ſo that no leſs than 163 millions, beſides 840 thouſands of theſe muſt be in a ligament one inch ſquare. I ſhall not here enlarge upon the admirable contrivance of Nature in this particular, nor ſay any thing farther of the reaſon of the greater ſtrength of the ſame ſubſtance drawn into ſmaller than into greater threads; but only this in general, that the mechanical operations of theſe minute bodies

bodies are quite differing from thoſe of bodies of greater bulk, and the want of conſidering this one thing hath been the cauſe of very great abſurdities in the Hypotheſes of ſome of our more eminent modern Philoſophers: For he that imagines the actions of theſe leſſer bodies the ſame with thoſe of the larger and tractable bodies, will indeed make but *Ariſtotles* wooden hand at beſt. This put me in mind likewiſe of advertiſing the Experimenter that he provide himſelf with inſtruments, by which, to ſtretch and pull in pieces any ſubſtance whilſt the ſame is yet in view of the Microſcope, of which there may be many which any one will eaſily contrive, when he hath this hint given him of the uſefulneſs thereof in the examination of the texture of ſeveral ſubſtances; as of Tendons, Nerves, Muſcles, *&c.* thoſe I have made uſe of were made to open like a pair of Tobacco Tongues, by two angular plates of thin braſs rivetted together, which by pinching the oppoſite end, would either open or ſhut at the other, as I had occaſion. Theſe having a part extended between the two tops, were fixt at a due diſtance from the object-glaſs that the body extended between them might be diſtinctly ſeen; then with my finger ſqueezing together the oppoſite ends, the other ends opened, by which means how the parts ſtretched and ſhrunk might be plainly diſcovered. Now as this is of uſe for ſome kind of ſubſtances ſo the two glaſs plates are for others, and particularly for ſqueezing of ſeveral ſubſtances between them, ſo as to break them in pieces, as thoſe little Creatures in pepper-water, or the Globules in blood, milk, flegm, *&c.* whereby the parts within them may yet farther be enquired into, as Mr. *Leeuwenhoeck* I find hath done by his lateſt Obſervations. Whether he makes uſe of this way, or ſome other, I know not.

Having thus given a deſcription of the appurtenances, it remains that I come to the deſcription of the Microcroſcope it ſelf, which is the principal inſtrument, and without which all the reſt are inſignificant.

The Microſcopes then I deſign here to deſcribe, are only of two kinds, either ſingle or double.

The ſingle Microſcope I call that which conſiſteth only of one glaſs, though it have a double refracting ſuperficies; and the double one I call that which is compounded of two glaſſes, though it hath for the moſt part a quadruple refraction of the Rays.

The ſingle Microſcope then conſiſteth of one ſmall *lens* ſo faſtened into a cell, that the eye may come conveniently to look through the middle part or *Axis* of it; of theſe there are various ſorts, as double Convexes, or plain Convexes, or perfectly ſpherical.

I ſhall not need to deſcribe the common *lenſes* which are every where made uſe of for this purpoſe, being plano-convexes of Spheres about half an inch Diameter, ſave only this, that 'tis beſt to turn the plain ſide towards the object, and the convex to the eye: nor ſhall I ſay much concerning thoſe double Convex Glaſſes, there being no great difficulty in the making or uſing of them; but that the ſmaller the ſphere is in which they are made, the nearer do they bring the object to the eye; and conſequently the more is the object magnified, and the better and truer they are poliſht in the Tool, the more clear and diſtinct doth the object appear, but to make any of a Sphere leſs than $\frac{1}{10}$ of an inch in Diameter is exceeding difficult, by reaſon that the glaſs becomes too ſmall to be tractable; and 'tis very difficult to find a cement that will hold it faſt whilſt it be completed; and when 'tis poliſht, 'tis exceeding difficult to handle and put into its cell: beſides, I have found the uſe of them offenſive to my eye, and to have much ſtrained and weakened the ſight, which was the reaſon why I omitted to make uſe of them, though in truth they do make the object appear much more clear and diſtinct, and magnifie as much as the double Microſcopes: nay, to thoſe whoſe eyes can well

well endure it, 'tis poſſible with a ſingle Microſcope to make diſcoveries much better than with a double one, becauſe the colours which do much diſturb the clear viſion in double Microſcopes is clearly avoided and prevented in the ſingle. The ſingle Microſcope therefore which I ſhall here deſcribe, as it is exceeding eaſie to make, ſo is it much more tractable than the double Convex glaſſes made the common way by working them in a hollow Hemiſphere with water and ſand; for thoſe, ſuppoſing them made with all the accurateneſs imaginable, will be far ſhort from being ſo well poliſht as theſe; and wanting the ſtem or handle which theſe have, they are infinitely troubleſome to remove, or place, or to cleanſe when there ſhall be occaſion.

Take then a ſmall rod of the cleareſt and cleaneſt glaſs you can procure, free if poſſible from blebbs, ſands, or veins; then by melting it in the flame of a Lamp made with Spirit of Wine, or the cleaneſt and pureſt Sallet Oyl, draw it out into exceeding fine and ſmall threads; then take a ſmall piece of theſe threads, and in the ſame flame of the aforeſaid Lamp melt the end of it, till you perceive it to run into a little ball or globule of the bigneſs deſired; then ſuffer it to cool, and handling it by the aforeſaid thread of glaſs, which is as it were a handle to it, fix it with a little wax upon the ſide of a thin plate of Braſs, Silver, or the like, that the middle of it may lie directly over the middle of a ſmall hole pricked through the ſaid thin plate with a needle: then holding this plate cloſe to the eye, look through the ſaid little hole, and thereby you may alſo ſee very clearly through the aforeſaid Globule, fixed with wax on the ſide that is from the eye: if then either by a little joynted arm, or by a little ſoft wax, and a needle, or a thin plate of Muſcovy glaſs, you fix the object you would examine; ſo that it may be at a due diſtance from the ſaid little Globule, you will perceive the minute parts thereof very diſtinct. The *focus* of a ſphere looked on by the naked eye, is about half the *radius* of the

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ſphere, without the ſuperficies of it; but this is varied much by the age of the eye that looks through it, by the imagination alſo of the perſon, and by the differing ſpecifique refraction of the glaſs made uſe of.

By this means I have prodigiouſly magnified ſome ſmall bodies, inſomuch that I have been able to ſee and diſtinguiſh the particles of bodies, not only a million of times ſmaller than a viſible point, but even to make thoſe viſible, whereof a million of millions of them would hardly make the bulk of the ſmalleſt viſible ſand; ſo prodigiouſly do theſe exceeding little Globules of glaſs inlarge the proſpect of humane ſight into the more private receſſes of nature.

If the things to be viewed be liquors, they may be included either in thoſe little pipes of Mr *Leeuwenhoeck* I newly mentioned, or elſe they may be put upon exceeding thin plates of *Muſcovy* glaſs or *Selenites*, and the other ſide of the plate may be made to touch the Globule, or at leaſt be fixed at ſuch diſtance, as may make the parts of the liquor diſtinct: If you make uſe of a Looking-glaſs plate to ſpread the liquor upon you would examine, you may turn the liquor towards the Globule, and you may therein eaſily ſee all the parts very diſtinctly, without at all hurting the proſpect by the interpoſition of the *Muſcovy* glaſs; which though it be exceeding clear, eſpecially if the plates be very thin, yet hath it ſome flaws, and ſome opacouſneſſes in it, which do ſomewhat cloud the proſpect.

If further, you would have a Microſcope with one ſingle refraction, and conſequently capable of the greateſt clearneſs and brightneſs that any one kind of Microſcopes can poſſibly be imagined ſuſceptible of, when you have fixt one of theſe little Globules as I have directed, and ſpread a little of the liquor upon a piece of Looking-glaſs plate, then apply the ſaid plate with the liquor, next to the Globule, and gently move it cloſe to the Globule, till the liquor touch; which done, you will find the liquor preſently to adhere to the Glo-

Globule, and ſtill to adhere to it though you move it back again a little; by which means, this liquor being of a ſpecifique refraction, not much differing from glaſs, the ſecond refraction is quite taken off, and little or none left but that of the convex ſide of the Globule next the eye; by which means as much of the inconvenience of refraction as is poſſible is removed, and that by the eaſieſt and moſt practicable expedient that can be deſired. I could add various other ways of making theſe Globular bodies both of glaſs and other ſubſtances which will yet farther advance our proſpect into nature, and are pleaſant to admiration; but thoſe I ſhall yet reſerve till I ſee what effects the publiſhing of theſe may produce, and to the end to excite other perſons to be inquiſitive into this matter: for let me aſſure them, very much more may yet be done by a way I know, than by this I have here publiſhed. And I confeſs I have very often wondered that no farther improvement had been made of this Principle, ſince I publiſht it in the year 1664. in the 20. page of my Preface to *Micrographia*: for though ſome other reaſons diſcouraged me from proſecuting thoſe enquiries, yet I hoped that others might long before this have carried it much farther.

The only inconvenience in theſe kinds of Microſcopes, is, that the object is neceſſarily brought ſo near the glaſs, that none but ſuch as are tranſparent, and to be viewed by a through light are capable of examination by them: ſuch therefore are to be examined by the double Microſcope; which, as it is abundantly more tractable, ſo doth it much leſs ſtrain the eye; and from the eaſineſs of its uſe, when well fitted, is much more pleaſant: and if ordered as it ought, will magnifie as much more than the common ones hitherto made, as thoſe did more than the naked eye.

Both theſe Microſcopes I have directed Mr. *Chriſtopher Cock*, in *Long-Acre*, how to prepare, that ſuch as will not trouble themſelves in the making of them, may know

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where to be accommodated with ſuch as are good.

And of the improvement of this kind of Microſcope, I ſee no limits, eſpecially as to the augmenting the viſible appearance of ſuch objects as are capable of enduring the increaſe of light; for ſince 'tis demonſtrable that light may be augmented upon any one object ſuſceptible to any given degree, and that by the double Microſcope the image can be augmented to any aſſigned magnitude, what but the difficulty of making all things correſpondent ſhould limit the power of ſuch an inſtrument. Now the making of this double Microſcope, though it be ſomewhat more difficult than of the ſingle one, yet the tractableneſs thereof when well fitted, and its eaſineſs to be cleanſed, and applied to uſe, makes amends for the extraordinary charge, eſpecially the ſituation of the object; which being capable of any reaſonable diſtance from the object glaſs, ſo as to be fit for examination, makes it very deſirable. Now as in all other mechanical contrivances, that is beſt which is plaineſt, and moſt ſimple: ſo is it in this, wherein nothing more is required, but two plano Convex glaſſes, the one for the object-glaſs, and the other for the eye-glaſs: the leſs the ſpheres of the glaſſes be, the more do they magnifie the object; and the thinner and clearer the ſubſtance of them be, and the more exactly ſhaped, and the brighter they are poliſht, the clearer do they repreſent it; and the longer the glaſſes are diſtant from each other, the more is the image magnified, *cæteris paribus*, though indeed the ſame thing is performed by glaſſes of very differing magnitudes, due proportions of all things about it being kept and obſerved. For if as the diſtance of one object from the object-glaſs is to the diſtance of another object from another object-glaſs, ſo the diſtance of the firſt image be to the diſtance of the ſecond image, the image in both muſt be equal: if therefore this image be viewed with equal glaſſes the image muſt be equally magnified at the bottom of the eye; ſo that in this way the object is capable

capable of a double way of augmenting, *viz.* firſt, the augmenting the figure in the Tube, by the ſmallneſs of the object-Glaſs, and lengthof the Tube: and ſecondly, by the augmenting that image in the bottom of the eye, and that is by the Eye-glaſs; give therefore light enough to the object, and you may increaſe the image at the bottom of the eye to what proportion you ſhall deſire. And by a way I ſhall ſhortly ſhew, the objects may be perceived diſtinct, defined, and colourleſs, as if ſeen by the naked eye. In all theſe ways the manner of applying the light is very ſignificant, and provided it be very ſtrong, the ſmaller the point be it proceedeth from, the more diſtinctly doth it exhibit the difference of refraction in the tranſparent bodies viewed by it, and the plainer will their parts be diſcovered: The light therefore of the Sun either reflected from a Spherical Convex body, or Spherical Concave body, the object being placed beyond the *focus*, or Refracted through a Concave or through a Convex, if the object be placed beyond the *focus*, do exceedingly well. But theſe with the help of a dark Room do yet better, the object being placed in a Table againſt the Light, and all other Light ſcreen'd from the Eye by the Dark Room. Much the ſame thing is done by the Light of a Lamp or Candle in the Night, which is indeed the moſt convenient Light, where Colour is not ſo much looked after.

Whileſt this Diſcourſe was Printing I caſually met with a Treatiſe of *P. Cherubine*, Printed at *Paris*, 1677. Entituled, *LA VISION PERFAITE, ou les concours des deux axes de la Viſion en un ſceul point de l' object*; Wherein the Author pretends amongſt other things to have promoted Microſcopes extreamly by ſo joyning two together, as through them to ſee the ſame object diſtinct with both the Eyes at once, and to ſee a large object all at one view, by which he affirms to have diſcovered ſome miſtakes and untruths in ſome of thoſe figures I have formerly publiſhed in my Micrography. But if he had pleaſed to have read

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the Deſcription as well as looked on the Figure, he might have been better informed than by his Preface he would ſeem to be. I deny not but that there are many failures in ſome of thoſe draughts, ſome of my own and ſome of the gravers committing. *Humanum eſt.* But thoſe which he charges for ſuch are not, as he might have ſeen if he had made uſe of better glaſſes than thoſe which he deſcribes, for they are ſo far ſhort of equalling thoſe I uſe, that I can demonſtrate from his own Deſcription of them, that thoſe I made uſe of did magnifie 10000 times more than that with which he pretends to have made theſe great Diſcoveries. Nor is it any thing more than common to ſee as large an *Area* as he mentions, with a glaſs that magnifies no more than his doth. But I could have ſhewed him how he might ſee the whole Creature at once, and yet much more magnifie than that which I have deſcribed, nay though the Creature were twice as big, and that with one Eye only, which is much to be preferred before that with two. However I ſhould be very glad to hear what Diſcoveries he doth make with his binocular Microſcope more than was ſeen before. As alſo that he would pleaſe to demonſtrate the truth of Parallelogram preſcribed for certain uſes, pag. of Dioptrique Oculaire, and in the Fourth Chapter of the Fourth Part of this Book. But to digreſs no farther from what I was deſcribing. I muſt add that with both theſe kinds of Microſcopes have I examined ſeveral ſubſtances; as particularly the ſteepings of ſeveral grains and ſeeds in rain-water. And though I have not yet found any one tincture more prolific than this of Pepper; yet 'tis not the only tincture in which they do both breed and increaſe. I have ſeen ſeveral ſorts in the ſteeping of Wheat, Barly, Oats, Coffee, Anniſeeds, Peaſe, *&c.* ſome not above a third part of a hair in thickneſs; others not above the twentieth part of the breadth of a hair, and ſome not more than a thirtieth part of that breadth; ſo that no leſs than 900 of theſe leaſt muſt go to make an

area

area as big as that of an hair cut tranſverſly, and 27000 to make a Cylinder as big as the hair of ones head, and of equal height with the Diameter of that hair, which one may juſt call a viſible point, and no more; few eyes ſeeing things diſtinctly much ſmaller: Now the Diameter of a hair of my head being by examination found but the 640 part of an inch, it follows that no leſs than 19200 or to uſe a round ſumm about 20000 of them may lie in the length of an inch, and conſequently, that a circle an inch Diameter will be to the *area* of one of theſe cut tranſverſly as 400000000 to 1. four hundred millions to 1 and a Cylinder one inch Diameter, and one inch high, will be to one of theſe mites, as 8000000000000 to one, eight millions of millions to one. If therefore we compare the magnitude of one of theſe animals to the magnitude of other creatures living in the water, we ſhall find that theſe will be found much ſmaller in compariſon to the body of an ordinary Whale, than the body of the ſame Whale will be to the body of the whole Earth; which may prove an argument for an *anima mundi* perhaps to ſome. But let every one make his own inferences, and believe his own eyes, for they will make the beſt impreſſion on his reaſon and belief. Now if the Creature be ſo exceeding ſmall, what muſt we think of the Muſcles, Joynts, Bones, Shells, *&c.* certain it is, that the Mechaniſm by which Nature performs the muſcular motion is exceedingly ſmall and curious, and to the performance of every muſcular motion in greater Animals at leaſt, there are not fewer diſtinct parts concerned than many millions of millions, and theſe viſible, as I ſhall hereafter ſhew through a Microſcope; and thoſe that conceive in the body of a muſcle, little more curioſity of mechaniſm than in a rope of the ſame bigneſs, have a very rude and falſe notion of it; and no wonder if they have recourſe to Spirits to make out the Phænomena: but of this hereafter more.

Further, I have examined the conſtitution of Blood,

Milk,

Milk, Flegm, &c. and found them much the ſame with what Mr. *Leeuwenhoek* has declared. A little fat laid upon the glaſs plate whilſt warm, melts, and becomes tranſparent, but obſerved in a convenient poſture againſt the light of a candle, &c. till it congeals, and ſhrinks, make a pleaſant fluid, and ſhews how conſiderably a fluid and ſolid body do vary, and may give us a good hint to conjecture at the reaſon of the ſwelling and greater lightneſs of Ice than of Water. The firſt beginnings alſo of the ſhooting or cryſtalliſing of Sugar into rectangular parallelipipeds, Alum, Salt, Vitriol, &c. are ſtrangely ſurprizing and inſtructive, I could enumerate multitudes of theſe.

But (that I may not detain the Reader too long in the peruſal of theſe anatomical deſcriptions of the minute and inviſible parts of animal ſubſtances) to eaſe both his eyes and imagination I ſhall proceed to acquaint him with ſome Anatomical Obſervations more ſenſible, and which do ſeem more nearly to concern us. And thoſe are contained in the enſuing Diſcourſe, being

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A Relation communicated to me in a Letter by that ingenious and experienced Chirurgion Mr. James Young *of* Plimouth, *in the beginning of* January *last, of the fatal Symptoms caused by a Bullet swallowed into the Lungs.*

SIR, In the beginning of *April*, 1674. one Mr. *Anthony Williamson* of *Liscard* in *Cornwal*, aged about 65 years, of a brisk, firm habit, became (after a too liberal drinking of Cyder) afflicted with the Colick, of which in four days he cured himself, by swallowing two Musket Bullets, and receiving some Carminative Clysters. On the 12. of the same month, his pain returning somewhat smarter than before, he attempted to swallow three Pistol Shot, and supposing it the easiest way, he lay on his back, and threw them all at once into his throat; where they choaking, had almost strangled him; constraining him to vomit, *&c.* When they were past down, he became seized immediately with a violent Cough, Wheasing, pain in the left side of his Breast, a great noise in respiration, more especially after a fit of Coughing, for then his Breast would hiss, like the sucking of a Pump, when the Air descends through the boxes.

These accidents so suddenly occurring, without any manifest cause, did much surprize him, and the more, because he was naturally of a sound breast; the Colick was cured by Clysters, Potions of Manna, *ol.amyg.d.* *&c.* and two of the Shot were soon ejected, *ex ano*, and maugre the other accidents, he became indifferently well, and able to walk about house.

Five or six weeks after this, those symptom s became more fierce, depaupering his spirits, prostrating his appetite, disquieting his sleep with dreams, a *Dyspnœa*, and rutling violent Cough; a straitness and load in his Breast kept him in bed, extenuated his body (which without help of Milk Clysters, was costive) he frequently fainted with sweats and a tickling sleepiness in both legs.

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Under the tyranny of this legion of ſymptoms, our Weſtern *Apollo*, Dr. *Bidgood* of *Exeter* was conſulted, who affirmed them all to be cauſed by the remaining Bullet, which paſſing through the *Larynx*, was fallen into one of the branches of the *Trachea*, where it would abide, in deſpight of any endeavours to eject it: yet to alleviate the violence of the accidents, he directed to the uſe of emollient *Eclegma's*, temperate Cordials, *&c.* by help of which, and ſome other propitious circumſtances, he not only recovered his legs, becoming able to walk, and ride a ſmall Journey, but alſo conſummated Marriage with a young woman of 25--- who afterward brought him two Children, whereof one is now alive, and very luſty; and was ſeven months gone with a third, when he died: the more wonderful if the woman were juſt to him (of which there appeareth no reaſon to doubt) becauſe a very little motion would ſo increaſe his difficulty of breathing, as to make him faint.

After Matrimony he had divers lucid Intervals, at times would be very brisk, and at others very languid, and faint, like a dying man: he continually expectorated, ſometimes grumous coagulated Blood, otherwhiles very recent; now purulent fœtid matter, then laudable *pus*. His natural averſion to Medicine cauſed him to reject what was adviſed by Dr. *Bidgood*, Dr. *Lower*, Dr. *Sprage*, *&c.* ſaving a few of the more ſlight mixtures: And although Sack had been formerly very familiar to him, he was now forced to ſhun it, and all ſtrong Drinks, becauſe they would infallibly produce a *Cardialgia*, a pulſant throbbing of the Heart, and labouring in his Breaſt: the firſt of theſe perhaps proceeded from his Conſtitution, which inclined to Choler; but the latter undoubtedly, from the effervеſcency, and warm motion, to which it enforced the Blood, which the obſtruction and preſſure the Bullet occaſioned in the *Pneumatick* organs, could not peaceably admit of: wherefore he reſolutely fixed to ſmall Drink, and ſhunned, as much as poſſible, all evitable Exerciſe, ſaving that of his

his hands, which he frequently employed in making Net-work.

In the Year 1676. he applied himſelf to our ingenious and learned Country-man, Dr *Mayow* of *Bath*, who agreed with Dr. *Bidgood*, that the remaining Bullet lodging in the Lungs, was the occaſion of all thoſe ill ſymptomes under which he laboured; but ſeemed to diſſent from his preſage, by hoping he might expectorate it: to atchieve which, he directed to have the body ſuſpended head downwards, and fumes of Storax, Benjamin, *&c.* to induce expulſive Coughing, together with concuſſions of the body, and all preceded with an opening courſe, to relax, and dilate the veſſels of the Breaſt; all which were uſed to no purpoſe, ſave to verifie Dr. *Bidgoods* Prognoſtick, that no efflation, how violent ſoever, would be able to extrude it, and inhaunce the Patients deſpair of being ever cured; from which time he never attempted it: ſo that thoſe ſymptomes before mentioned, continuing until the Winter, and then gaining conſiderably on him, eſpecially the *Hæmoptyſis*, *&c.* he languiſhed till the ninth of *December* laſt, and then died.

The tenth *Ditto* (aſſiſted by his Son-in-law) I opened the *Thorax*, in preſence of two other Chirurgions of the place, together with divers perſons of Quality, whoſe curioſity led them to ſee the examination; becauſe the Bullets being there, was ſo much doubted by many, and diſputed as impoſſible by others. In the diſſection the following particulars were obſervable,

The Body was extenuate and tabid,

The right lobes of the Lungs were replete, ſound, and well coloured.

The *Serum* in the *Pericardium* was almoſt all abſumed,

The Heart ſtrangely ſhrivelled and very ſmall.

Under the *Pericardium* (the Body being *ſupine*) we found a lump of coagulated Blood, as big as a Pigeons Egg; near which lay alſo a ſubſtance, ſhaped like an

P 2 obtuſe

obtuſe headed muſcle, having a Tendon-like tail, which inſinuated to the Pendant Lobe: Its body was above an half inch thick. Its other dimenſions and ſhape exactly like that of the figure X, of which A ſheweth the head or upper end, B the tail, which in drawing out of the rotten Lungs (being alſo corrupted) broke aſunder. Its Texture ſeemed fibrous, like that of the Kidneys, being white one half way through, the reſt of a dark red: it was very ſoft and plum, having a firm ſmooth tegument, and felt very much like a Sheeps kidney.

The left Lobe of the Lungs was cadaverous, and hollow, by an abſceſs which had diſcharged near a pint of very fœtid and purulent matter, into that ſide of the trunk where it lay immured up, by the adheſion of the Lungs on that ſide, to the *Pleura*, which with the *Diaphragma*, as far as the matter extended, was livid, and eroded.

We examined this rotten part of the Lungs, with what exactneſs and curioſity we were capable of, amidſt ſuch a crowd as were preſent; and the more troubleſome ſtench of the *Cadaver*; and found though the whole *Parenchyma* were rotten, and no firmer than coagulated Blood (with which it had very near reſemblance) yet the branches of the *Trachea* continued into it were uncorrupt, and ſound; nor in any of them could we find what we very confidently preſumed to be there, *viz.* the Bullet.

Wherefore I reſolved to ſeek it the way by which it muſt have entred; and accordingly dividing the *Trachea* at its inſertion to the Lungs, I thruſt in a bended Probe to the left branch, and there felt him, lying looſe about two inches within it, which with my fingers I eaſily expreſſed at the divided end of the pipe: to do which, I laid it bare ſo far as where the Bullet had lodged; and I proteſt, to my wonder, I found it not any way injured, or altered, by hardneſs, eroſion, *&c.* though the Bullet had divers impreſſions from the later. The

The ſanguiferous veſſels, though lacerated, and cut in the diſſection, did yield little or no Blood, either fluid or coagulate.

Thus far is true Hiſtory, and matter of fact; I muſt now beg your pardon, if I preſume to give my ſenſe, and apprehenſion of ſome of thoſe Phænomena here related.

The extenuation of the body, the abſumption of the *ſerum* in the Heart-bag, and the contraction of the Heart, were the effects of the *Tabes*; and that occaſioned by the Bullets injuring the Lungs, and pectoral veſſels.

The lump of coagulate blood found under the Heart-bag was extravaſate from the rotted veins, and arteries of the Lungs.

That ſtrange ſubſtance lodged between the *Pericardium*, and the Bullet, was either a *Polypus*, and the excreſcence of ſome part, or it was generated by nature, and ſubſtituted for a cuſhion to defend the Heart from injury, by ſo uneaſie a neighbour. That Polypuſes have been found in the Heart, is affirmed by *Nicolas Tulpius*, *Marcellus Malpighius*, *G. Garnarus*, &c. but their ſhape and texture differing vaſtly from that of ours, giveth reaſon to believe this to be none; eſpecially conſidering that they all excreſcing from the Heart, or ſome carneous part, are inſeparably united and radicated to their original, and are ſpungy; whereas this was nothing leſs, having no root, nor ſo much as an adheſion any where, ſaving at the tail; the ſmall end of which, being rotted by the Lungs, into which it continued, did eaſily divide upon my endeavour to draw it out: the body of it alſo lay looſe in the aforeſaid interſtice, and as eaſily ſlipped out, as a Wen, or a *Struma*, when the containing parts are opened. Its ſubſtance was not fungous, but of a ſoft firmneſs, like a Kidney; and in what ever circumſtances it may reſemble a Polypus, as it doth the figure of that of the Noſe, *vide N. Tulpii ob.med. lib.*1. *obſ.* 26.yet it alſo differs from all other excreſcrefcences, befides, in what hath been mentioned, in that it was not rooted in any flefhy, bony, or mufculous part; and fuch the Lungs are well known not to be: it muft therefore be the ftupendious effect of Natures induftry, and laid as a cufhion to defend the Heart, *&c.* Its compofition being fo delicately foft, and yet firm enough for fuch a purpofe: Its magnitude, fituation, *&c.* concurring alfo to confirm this opinion concerning it; befides which, I do almoft remember, and believe (though I cannot be pofitive) that the pulfant pain he had fo violently in his Breaft, toward the left fide, decreafed *gradatim*, from the time of the deglutition: if that be true, whatever the fubftance were, or its caufe, its effects were very propitious, manifefting nature to be, not only a diligent fupplier of her own defects, but as induftrious to produce ftrange and unaccountable relief, in fuch emergencies as this before us: A refembling ftory we have from *A Pareus*, *lib.* 8. *cap.* 15.

The abfcefs was without doubt from a Phlegmon of the Lungs; and becaufe for the moft part it was below, or beyond the Bullet, it proceeded rather from its obftructing, and fo ftagnating the Blood, and recrements in that Lobe, than from extravafation. What occurred of the latter, was expectorated, or remained in fuch Coagulums as that found under the Heart.

The caufe of the Bullets falling, rather into the left than the right *Ramus* of the *Trachea*, is obvious from the more fupine and direct figure thereof, correfponding with the trunk, as the figure doth manifeft: which confideration, together with the Bullets being loofe in the pipe, renders the unfuccefsfulnefs of Dr. *Mayow*'s attempt very wonderful: I am inclined to believe it was fo, either for want of a more early trial, or a more skilful tryer, than him who was employed about it. The way was ingenioufly contrived, and (as the Doctor himfelf told me) had been fuccefsfully experienced in the like occafion. Certainly, had not the diftance of the Doctors abode, and very important avocations,

tions,

tions, denied his perſonal aſſiſtance: or had any other perſon skilled in Anatomy, *&c.* been ſubſtituted, the Bullet from his own favourable ſhape, and more propitious gravity, and particularly from the ſtrong efflations they provoked, together with the aſſiſtant poſture of the body, would have been extruded. Had they inſtead of hanging him perpendicular, made him incline a little to the right ſide, to have made the left *Ramus* more prone; and at the ſame time made him diſtend the pipes by ſucking in as much breath as they could contain, their other means might have been effectual; which I am induced to preſume from the proſperous effects of the like attempt, and yet wanting many of their advantages; I mean the reverſion of a Stone, when ſticking, and not able, to paſs through the Urinary Channels. Let any Phyſicians ſeriouſly perpend the difficulty of this, with the advantages for the former, and they will juſtifie my opinion.

The eroſion of the *Pleura*, and *Diaphragme*, was from the acidity of the matter, gnawing and corrupting them; for though the *Trachea* wonderfully eſcaped ſuch impreſſions, the Bullet diſcovered on its ſuperficies, divers marks of eroſion, which all acids produce with much facility, upon the ſaccharous or ſaline parts of Lead; as is to be ſeen by immerſing it in vinegar.

And now Sir, to relieve your patience (no leſs than my own) perhaps already wearied with the prolixity of this Narrative, give me leave to conclude, with ſuggeſting, that I am of a belief (having peruſed moſt of the publick accounts of this kind) that ſcarcely a rarer accident, and accompanied with ſuch ſtupendious circumſtances, hath occurred to the preſent age than this; that an extraneous body, ſo large, ſo heavy, ſo hard, ſhould ſlip down that difficult, and unuſual way of the Weaſon, and abide ſo long in the organs of reſpiration, in ſo aged a perſon, admitting after it ſuch exerciſes, as he performed, Riding, Marriage, *&c.* that nature ſhould ſo unaccountably provide ſuch a pertinent fence ſence againſt injuries accidentally accruing, and that even the ſmalleſt Ramifications of the *Trachea*, though immerſed in ſuch a *Cadaver*, ſhould be preſerved from injury thereby. I am ſure in the voluminous Obſervations of *Schenckius*, *Horſtius*, *Riverius*, *Bartholine*, *Burnet*, &c. nor among all the ſtories in Mr. *Oldenburg*'s Tranſactions, or the *Miſcellanea Curioſa* of the *Leipſwick* Doctors, hath it a Parallel.

This, and whatever is elſe contained in this Hiſtory, as my δόξα, I ſubmit to the better ſenſe, and reaſon of the Learned, not preſuming to be poſitive in any thing, ſave in affirming my ſelf, *&c.*

JAMES YOUNG.

P. S.

For the plainer underſtanding where the Bullet lodged in the Wind-pipe, I have drawn and ſent you an exact figure of the *Trachea*, excarnified; as its to be be found in *Gerrard Blaſſius*, *Syntagma Anatomicum J. Veſlingi.* See figure Y in the III. Table.

C points to the *Trachea* divided under the Larynx.

D the right Ramus of the *Trachea.* *E* the left.

F the place where the Lungs adhered to the *Pleura.*

g g g, *&c.* the extremities of thoſe branches of the *Aſpera arteria*, divaricated into the rotten Lobe.

H the Bullet in the pipe where it was found.

ERRATA.

PAge 1. line 17. *foot.* p. 2.l.6.*joyned*, p.8.l.21.*Cete*, p.11.l.5.*is diffuſed*,p.11. l.17.*Foſtor*,p.12.l.33.*within the ſphere of its activity*,p.12.l.34.dele *as*,p.14. l.8.*ether*, p.19.l.7.*common ſights*, p.22.l.31.*Auzout*, p.23.l.19.*been produced*, p.24. l.27. add *ſee fig* 4.p.27.l.5.*of this orb*, p.27.l.21.m, p.28.l.27.*ſixth figure*, p.29. l.18 *& perige*, p.29 l.25.*B*,*B*,*E*. ib.l.26,27.*H*,*H*,*I*.p.31.l.9.*for*(.)put(,)p.32.l.28. *ſtream of bubbles*, p.35.l.29.add *fig*.9. p.38.l.28.*to get out of*, l.34. *of finding the*, p.46.l 31.*Baldwines* p.49.l.17.*downwards ſhall touch*,p.54.l.26.*Scolopendra*,p.69 l.ult.*Suns phaſe*,p.71.l.25.for 43 put 34, p.83.l.8.*to my wonder*, p.93.l.3.blot out *firſt*,p 96 l.14.*plano connexes*,p.101.l.22.for *table* put *tube*,p.101.l.30.*Seul*,p.102. l.17.*magnified*,l.23.*the paralellogram*,l.24,*page* 241, p.104.l.6. for *fluid* put *feild.*

LECTURES
De Potentia Restitutiva,
OR OF
SPRING
Explaining the Power of Springing Bodies.

To which are added some

COLLECTIONS

Viz.

A Description of Dr. Pappins *Wind-Fountain and Force-Pump.*
Mr. Young's *Observation concerning natural Fountains.*
Some other Considerations concerning that Subject.
Captain Sturmy's *remarks of a Subterraneous Cave and Cistern.*
Mr. G. T. *Observations made on the* Pike *of* Teneriff, 1674.
Some Reflections and Conjectures occasioned thereupon.
A Relation of a late Eruption in the Isle of Palma.

By *ROBERT HOOKE.* S.R.S.

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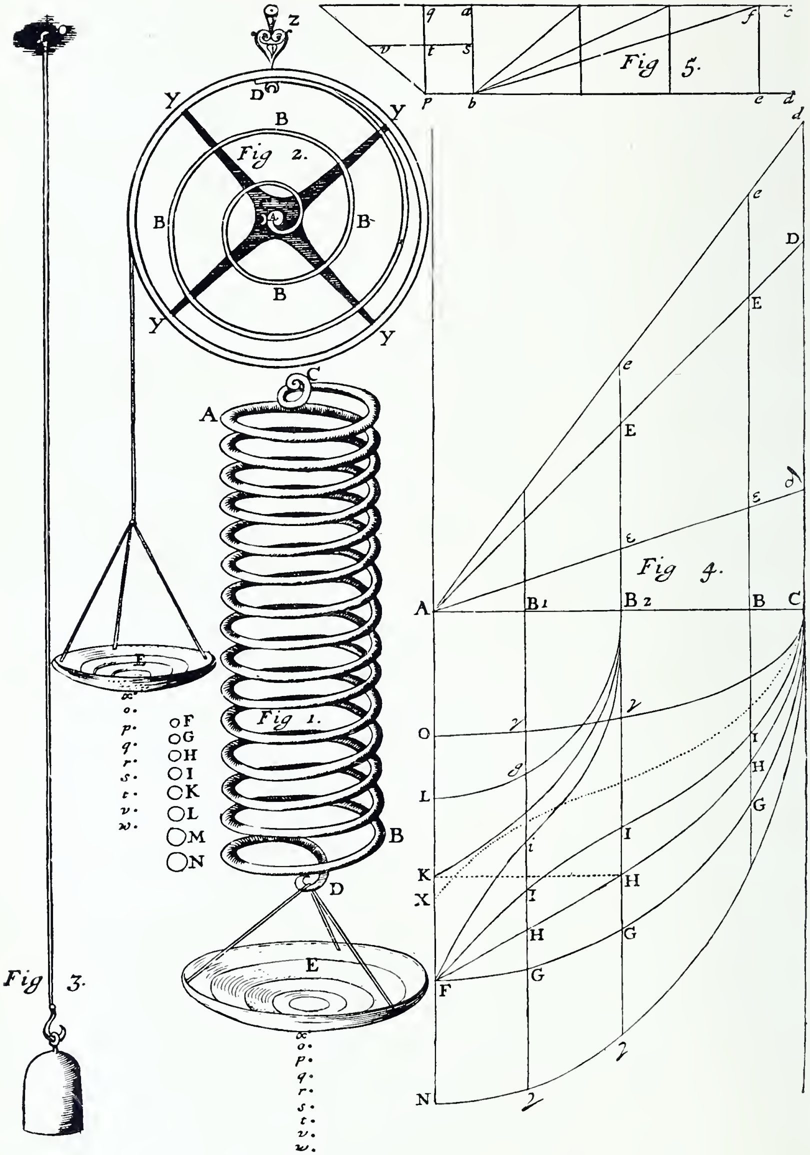


Fig 1.
Fig 2.
Fig 3.
Fig 4.
Fig 5.
A
B
C
D
E
Y
Z
F
G
H
I
K
L
M
N
O
X
B 1
B 2

Potentia Reſtitutiva,

OR

SPRING.

THe Theory of Springs, though attempted by divers eminent Mathematicians of this Age has hitherto not been Publiſhed by any. It it now about eighteen years ſince I firſt found it out, but deſigning to apply it to ſome particular uſe, I omitted the publiſhing thereof.

About three years ſince His Majeſty was pleaſed to ſee the Experiment that made out this Theory tried at *White-Hall*, as alſo my Spring Watch.

About two years ſince I printed this Theory in an Anagram at the end of my Book of the Deſcriptions of Helioſcopes,*viz.c e i i i n o s s s t t u u,id eſt,Ut tenſio ſic vis*; That is, The Power of any Spring is in the ſame proportion with the Tenſion thereof: That is, if one power ſtretch or bend it one ſpace, two will bend it two, and three will bend it three, and ſo forward. Now as the Theory is very ſhort, ſo the way of trying it is very eaſie.

Take then a quantity of even-drawn Wire, either Steel, Iron, or Braſs, and coyl it on an even Cylinder into a Helix of what length or number of turns you pleaſe, then turn the ends of the Wire into Loops, by one of which ſuſpend this coyl upon a nail, and by the other ſuſtain the weight that you would have to extend it, and hanging on ſeveral Weights obſerve exactly to what length each of the weights do extend it beyond the length that its own weight doth ſtretch it to, and you ſhall find that if

B one

one ounce, or one pound, or one certain weight doth lengthen it one line, or one inch, or one certain length, then two ounces, two pounds, or two weights will extend it two lines, two inches, or two lengths; and three ounces, pounds, or weights, three lines, inches, or lengths; and ſo forwards. And this is the Rule or Law of Nature, upon which all manner of Reſtituent or Springing motion doth proceed, whether it be of Rarefaction, or Extenſion, or Condenſation and Compreſſion.

Or take a Watch Spring, and coyl it into a Spiral, ſo as no part thereof may touch another, then provide a very light wheel of Braſs, or the like, and fix it on an arbor that hath two ſmall Pivots of Steel, upon which Pivot turn the edge of the ſaid Wheel very even and ſmooth, ſo that a ſmall ſilk may be coyled upon it; then put this Wheel into a Frame, ſo that the Wheel may move very freely on its Pivots; faſten the central end of the aforeſaid Spring cloſe to the Pivot hole or center of the frame in which the Arbor of the Wheel doth move, and the other end thereof to the Rim of the Wheel, then coyling a fine limber thread of ſilk upon the edge of the Wheel hang a ſmall light ſcale at the end thereof fit to receive the weight that ſhall be put thereinto; then ſuffering the Wheel to ſtand in its own poſition by a little index faſtned to the frame, and pointing to the Rim of the Wheel, make a mark with Ink, or the like, on that part of the Rim that the Index pointeth at; then put in a drachm weight into the ſcale, and ſuffer the Wheel to ſettle, and make another mark on the Rim where the Index doth point; then add a drachm more, and let the Wheel ſettle again, and note with Ink, as before, the place of the Rim pointed at by the Index; then add a third drachm, and do as before, and ſo a fourth, fifth, ſixth, ſeventh, eighth, *&c.* ſuffering the Wheel to ſettle, and marking the ſeveral places pointed at by the Index, then examine the Diſtances

Diſtances of all thoſe marks, and comparing them together you ſhall find that they will all be equal the one to the other, ſo that if a drachm doth move the Wheel ten degrees, two drachms will move it twenty, and three thirty, and four forty, and five fifty, and ſo forwards.

Or take a Wire ſtring of twenty, or thirty, or forty foot long, and faſten the upper part thereof to a nail, and to the other end faſten a Scale to receive the weights: Then with a pair of Compaſſes take the diſtance of the bottom of the ſcale from the ground or floor underneath, and ſet down the ſaid diſtance, then put in weights into the ſaid ſcale in the ſame manner as in the former trials, and meaſure the ſeveral ſtretchings of the ſaid ſtring, and ſet them down. Then compare the ſeveral ſtretchings of the ſaid ſtring, and you will find that they will always bear the ſame proportions one to the other that the weights do that made them.

The ſame will be found, if trial be made, with a piece of dry wood that will bend and return, if one end thereof be fi x1 in a horizontal poſture, and to the other end be hanged weights to make it bend downwards.

The manner of trying the ſame thing upon a body of Air, whether it be for the rarefaction or for the compreſſion thereof I did about fourteen years ſince publiſh in my *Micrographia*, and therefore I ſhall not need to add any further deſcription thereof.

Each of theſe ways will be more plainly underſtood by the explanations of the annexed figures.

The firſt whereof doth repreſent by A B the coyl or helix of Wire, C the end of it, by which it is ſuſpended, D the other end thereof, by which a ſmall Scale E is hanged, into which putting Weights as F G H I K L M N, ſingly and ſeparately they being in proportion to one another as 1 2 3 4 5 6 7 8, the Spring will be thereby equally ſtretcht to *o*,*p*,*q*,*r*,*s*,*t*,*u*,*w*,

B 2 that

that is, if F ſtretch it ſo as the bottom of the Scale deſcend to *o*, then G will make it deſcend to *p*, H to *q*, I to *r*, K to *s*, L to *t*, M to *u*, and N to *w*, &c. So that x *o* ſhall be one ſpace, x *p*, 2, x *q*, 3, x *r*, 4, x *s*, 5, x *t*, 6, x *u*, 7, x *w*, 8.

The ſecond figure repreſents a Watch Spring coyled in a Spiral by C A B B B D, whoſe end C is fixed to a pin or Axis immovable, into the end of which the Axis of a ſmall light Wheel is inſerted, upon which it moves; the end D is fixed to a pin in the Rim of the Wheel y y y y, upon which is coyled a ſmall ſilk, to the end of which is fixed a Scale to receive the weights. To the frame in which theſe are contained is fixed the hand or Index z; then trying with the former weights put into the Scale E, you will find that if F put into the Scale E ſinks the bottom of it x to *o*, then G will ſink it to *p*, and H to *q*, I to *r*, K to *s*, L to *t*, and z will point at 1,2,3,4,5,6,7,8 on the Wheel.

The trials with a ſtraight wire, or a ſtraight piece of wood laid Horizontal arc ſo plain they need not an explication by figure, and the way of trying upon Air I have long ſince explained in my *Micographia* by figures.

From all which it is very evident that the Rule or Law of Nature in every ſpringing body is, that the force or power thereof to reſtore it ſelf to its natural poſition is always proportionate to the Diſtance or ſpace it is removed therefrom, whether it be by rarefaction, or ſeparation of its parts the one from the other, or by a Condenſation, or crowding of thoſe parts nearer together. Nor is it obſervable in theſe bodys only, but in all other ſpringy bodies whatſoever, whether Metal, Wood, Stones, baked Earths, Hair, Horns, Silk, Bones, Sinews, Glaſs, and the like. Reſpect being had to the particular figures of the bodies bended, and the advantagious or diſadvantagious ways of bending them.

From

From this Principle it will be eaſie to calculate the ſeveral ſtrength of Bows, as of Long Bows or Croſs-Bows, whether they be made of Wood, Steel, Horns, Sinews, or the like. As alſo of the *Baliſtæ* or *Catapultæ* uſed by the Ancients, which being once found, and Tables thereof calculated, I ſhall anon ſhew a way how to calculate the power they have in ſhooting or caſting of Arrows, Bullets, Stones, Granadoes, or the like.

From theſe Principles alſo it will be eaſie to calculate the proportionate ſtrength of the ſpring of a Watch upon the Fuſey thereof, and conſequently of adjuſting the Fuſey to the Spring ſo as to make it draw or move the Watch always with an equal force.

From the ſame alſo it will be eaſie to give the reaſon of the *Iſochrone* motion of a Spring or extended ſtring, and of the uniform ſound produced by thoſe whoſe Vibrations are quick enough to produce an audible ſound, as likewiſe the reaſon of the ſounds, and their variations in all manner of ſonorous or ſpringing Bodies, of which more on another occaſion.

From this appears the reaſon, as I ſhall ſhew by and by, why a Spring applied to the balance of a Watch doth make the Vibrations thereof equal, whether they be greater or ſmaller, one of which kind I ſhewed to the right Honourable the Lord Viſcount *Brounker*, the Honourable *Robert Boyle* Eſq; and Sir *Robert Morey* in the year 1660. in order to have gotten Letters Patents for the uſe and benefit thereof.

From this it will be eaſie to make a Philoſophical Scale to examine the weight of any body without putting in weights, which was that which I mentioned at the end of my deſcription of Helioſcopes, the ground of which was veiled under this Anagram, *c e d i i i n n o o p s s s t t u u*, namely, *Ut pondus ſic tenſio*. The fabrick of which ſee in the three firſt figures.

This Scale I contrived in order to examine the gravitation of bodies towards the Center of the Earth,

B 3 *viz.* to

viz. to examine whether bodies at a further diſtance from the Center of the Earth did not loſe ſomewhat of their power or tendency towards it. And propounded it as one of the Experiments to be tried at the top of the Pike of *Teneriff*, and attempted the ſame at the top of the Tower of St. *Pauls* before the burning of it in the late great Fire; as alſo at the top and bottom of the Abby of St. *Peters* in *Weſtminſter* though theſe being by but ſmall diſtances removed from the Surface, I was not able certainly to perceive any manifeſt difference. I propounded the ſame alſo to be tried at the bottom and ſeveral ſtations of deep Mines; and D. *Power* did make ſome trials to that end, but his Inſtruments not being good, nothing could be certainly concluded from them.

Theſe are the Phenomena of Springs and ſpringy bodies, which as they have not hitherto been by any that I know reduced to Rules, ſo have all the attempts for the explications of the reaſon of their power, and of ſpringineſs in general, been very inſufficient.

In the year 1660. I printed a little Tract, which I called, *An Attempt for the explication of the Phenomena, &c.* of the riſing of water in the pores of very ſmall Pipes, Filtres, *&c.* And being unwilling then to publiſh this Theory, as ſuppoſing it might be prejudicial to my deſign of Watches, which I was then procuring a Patent for, I only hinted the principle which I ſuppoſed to be the cauſe of theſe Phænomena of ſprings in the 31 page thereof in the Engliſh Edition, and in the 38 page of the Latine Edition, tranſlated by *M. Behem*, and printed at *Amſterdam*, 1662. But referred the further explication thereof till ſome other opportunity.

The Principles I then mentioned I called by the names of *Congruity* and *Incongruity* of bodies. And promiſed a further explanation of what I thereby meant on ſome other occaſion. I ſhall here only explain ſo much of it as concerns the explication of this preſent Phænomenon. By

By *Congruity* and *Incongruity* then I underſtand nothing elſe but an agreement or diſagreement of Bodys as to their Magnitudes and motions.

Thoſe Bodies then I ſuppoſe congruous whoſe particles have the ſame Magnitude, and the ſame degree of Velocity, or elſe an harmonical proportion of Magnitude, and harmonical degree of Velocity. And thoſe I ſuppoſe incongruous which have neither the ſame Magnitude, nor the ſame degree of Velocity, nor an harmonical proportion of Magnitude nor of Velocity.

I ſuppoſe then the ſenſible Univerſe to conſiſt of body and motion.

By Body I mean ſomewhat receptive and communicative of motion or progreſſion. Nor can I have any other Idea thereof, for neither Extention nor Quantity, hardneſs nor ſoftneſs, fluidity nor fixedneſs, Rarefaction nor Denſation are the proprieties of Body, but of Motion or ſomewhat moved.

By Motion I underſtand nothing but a power or tendency progreſſive of Body according to ſeveral degrees of Velocity.

Theſe two do always counterballance each other in all the effects, appearances, and operations of Nature, and therefore it is not impoſſible but that they may be one and the ſame; for a little body with great motion is equivalent to a great body with little motion as to all its ſenſible effects in Nature.

I do further ſuppoſe then that all things in the Univerſe that become the objects of our ſenſes are compounded of theſe two (which we will for the preſent ſuppoſe diſtinct eſſences, though poſſibly they may be found hereafter to be only differing conceptions of one and the ſame eſſence) namely, *Body*, and *Motion*. And that there is no one ſenſible Particle of matter but owes the greateſt part of its ſenſible Extenſion to Motion whatever part thereof it ows to Body according to the common notion thereof: Which is, that

Body

Body is ſomewhat that doth perfectly fill a determinate quantity of ſpace or extenſion ſo as neceſſarily to exclude all other bodies from being comprehended within the ſame Dimenſions.

I do therefore define a ſenſible Body to be a determinate Space or Extenſion defended from being penetrated by another, by a power from within.

To make this the more intelligible, Imagine a very thin plate of Iron, or the like, a foot ſquare, to be moved with a Vibrative motion forwards and backwards the flat ways the length of a foot with ſo ſwift a motion as not to permit any other body to enter into that ſpace within which it Vibrates, this will compoſe ſuch an eſſence as I call in my ſenſe a Cubick foot of ſenſible Body, which differs from the common notion of Body as this ſpace of a Cubick foot thus defended by this Vibrating plate doth from a Cubick foot of Iron, or the like, throughout ſolid. The Particles therefore that compoſe all bodies I do ſuppoſe to owe the greateſt part of their ſenſible or potential Extenſion to a Vibrative motion.

This Vibrative motion I do not ſuppoſe inherent or inſeparable from the Particles of body, but communicated by Impulſes given from other bodies in the Univerſe. This only I ſuppoſe, that the Magnitude or bulk of the body doth make it receptive of this or that peculiar motion that is communicated, and not of any other. That is, every Particle of matter according to its determinate or preſent Magnitude is receptive of this or that peculiar motion and no other, ſo that Magnitude and receptivity of motion ſeems the ſame thing: To explain this by a ſimilitude or example. Suppoſe a number of muſical ſtrings, as A B C D E, *&c.* tuned to certain tones, and a like number of other ſtrings, as *a*,*b*,*c*,*d*,*e*, &c. tuned to the ſame ſounds reſpectively, A ſhall be receptive of the motion of *a*, but not of that of *b*, *c*, nor *d*; in like manner B ſhall be receptive of the motion of *b*, but not of the motion of of *a*, *c* or *d*. And ſo of the reſt. This is that which I call *Congruity* and *Incongruity*.

Now as we find that muſical ſtrings will be moved by Uniſons and Eighths,and other harmonious chords, though not in the ſame degree; ſo do I ſuppoſe that *the particles of matter* will be moved principally by ſuch motions as are Uniſons, as I may call them, or of equal Velocity with their motions, and by other harmonious motions in a leſs degree.

I do further ſuppoſe, A ſubtil matter that incompaſſeth and pervades all other bodies, which is the Menſtruum in which they ſwim which maintains and continues all ſuch bodies in their motion, and which is the medium that conveys all Homogenious or Harmonical motions from body to body.

Further I ſuppoſe, that all ſuch particles of matter as are of a like nature, when not ſeparated by others of a differing nature will remain together, and ſtrengthen the common Vibration of them all againſt the differing Vibrations of the ambient bodies.

According to this Notion I ſuppoſe the whole Univerſe and all the particles thereof to be in a continued motion, and every one to take its ſhare of ſpace or room in the ſame, according to the bulk of its body, or according to the particular power it hath to receive, and continue this or that peculiar motion.

Two or more of theſe particles joyned immediately together, and coaleſcing into one become of another nature, and receptive of another degree of motion and Vibration, and make a compounded particle differing in nature from each of the other particles.

All bulky and ſenſible bodies whatſoever I ſuppoſe to be made up or compoſed of ſuch particles which have their peculiar and appropriate motions which are kept together by the differing or diſſonant Vibrations of the ambient bodies or fluid.

C According

According to the difference of theſe Vibrative motions of the Incompaſſing bulks. All bodies are more or leſs powerful in preſerving their peculiar ſhapes.

All bodies neer the Earth are incompaſſed with a fluid ſubtil matter by the differing Velocity of whoſe parts all ſolid bodies are kept together in the peculiar ſhapes, they were left in when they were laſt fluid. And all fluid bodies whatſoever are mixed with this fluid, and which is not extruded from them till they become ſolid.

Fluid bulks differ from ſolids only in this, that all fluids conſiſt of two ſorts of particles, the one this common Menſtruum near the Earth, which is interſperſed between the Vibrating particles appropriated to that bulk, and ſo participating of the motions and Vibrations thereof: And the other, by excluding wholly, or not participating of that motion.

Though the particles of ſolid bodies do by their Vibrative motions exclude this fluid from coming between them where their motions do immediately touch, yet are there certain ſpaces between them which are not defended by the motion of the particles from being pervaded by the Heterogeneous fluid menſtruum.

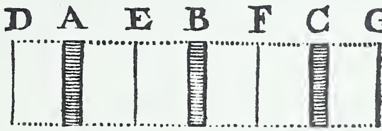
Theſe ſpaces ſo undefended by the bodies and Vibrative motion of the particles, and conſequently pervaded by the ſubtil incompaſſing Heterogeneous fluid are thoſe we call the inſenſible pores of bodies.

According to the bigneſs of the bodies the motions are, but in reciprocal proportion: That is, the bigger or more powerful the body is, the ſlower is its motion with which it compounds the particles; and the leſs the body is, the ſwifter is its motion.

The ſmaller the particles of bodies are, the nearer do they approach to the nature of the general fluid, and

and the more eaſily do they mix and participate of its motion.

The Particles of all ſolid bodies do immediately touch each other; that is, the Vibrative motions of the bodies do every one touch each other at every Vibration. For explication, Let A B C repreſent three bodies, each of theſe bodies I ſuppoſe to have a Vibrative motion on either ſide of it, A between D and E, B between E and F, and C between F and G. I ſuppoſe then that B in every one of its Vibrations doth meet A at E, and C at F, and ſo the motions are continually interchanged: That is, B communicates its motion to A at E, and A at the ſame time and place communicates its motion to B, which returning to F meets there with C, and communicates its received motion to C, which at the ſame inſtant and place communicates its own motion to B, which returns it back to E: So that the Velocity of theſe bodies is always the ſame, and each body impreſſeth on the contiguous bodies ſuch a determinate number of pulſes within a certain ſpace of time. Suppoſe for inſtance, in every ſecond of time B communicates to A and to C one million of pulſes, and hath received as many from each of them, by which means each of them doth preſerve its own ſpace of Vibration, according to the power of its Vibration, that neither of the contiguous bodies can enter into it. The extreme particles A and C are repercuſſed by the motion of the ambient Heterogeneous fluid, whereof though the bodies are of differing magnitudes, yet the body and motion of the one are equivalent to the body and motion of the other, ſo that whatever the body be leſs, the motion is quicker; and where the body is bigger, the motion is leſs. But the Particles of fluid



C 2 bodies

bodies do not immediately touch each other, but permit the mixture of the other Heterogeneous fluid near the Earth, which ſerves to communicate the motion from particle to particle without the immediate contact of the Vibrations of the Particles.

All ſolid Bodies retain their ſolidity till by other extraordinary motions their natural or proper motions become intermixed with other differing motions, and ſo they become a bulk of compounded motions, which weaken each others Vibrative motions. So that though the ſimilar parts do participate of each others motions, whereby they indeavour to joyn or keep together, yet do they alſo participate of an Heterogeneous motion which endeavours to ſeparate or keep them aſunder. And according to the prevalency of the one or the other is the body more or leſs fluid or ſolid.

All bodies whatſoever would be fluid were it not for the external Heterogeneous motion of the Ambient.

And all fluid bodies whatſoever would be unbounded, and have their parts fly from each other were it not for ſome prevailing Heterogeneous motion from without them that drives them more powerfully together.

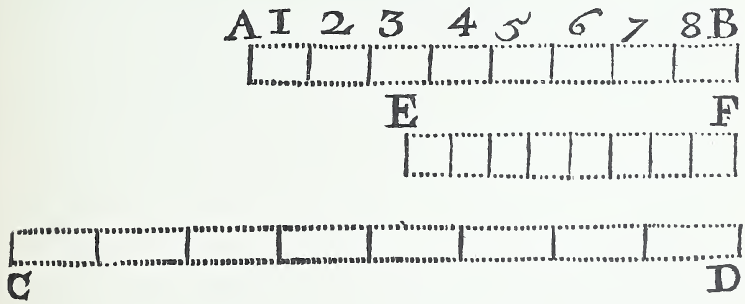
Heterogeneous motions from without are propagated within the ſolid in a direct line if they hit perpendicular to the ſuperficies or bounds, but if obliquely in ways not direct, but different and deflected, according to the particular inclination of the body ſtriking, and according to the proportion of the Particles ſtriking and being ſtruck.

All ſpringy bodies whatſoever conſiſt of parts thus qualified, that is, of ſmall bodies indued with appropriate and peculiar motions, whence every one of theſe particles hath a particular Bulk, Extenſion, or Sphere of activity which it defends from the ingreſs of any other incompaſſing Heterogeneous body whilſt in in its natural eſtate and balance in the Univerſe. Which particles being all of the ſame nature, that is, of equal bodies, and equal motions, they readily coaleſce and joyn together, and make up one ſolid body, not perfectly every where contiguous, and wholly excluding the above mentioned ambient fluid, but permitting it in many places to pervade the ſame in a regular order, yet not ſo much but that they do wholly exclude the ſame from paſſing between all the ſides of the compounding particles.

The parts of all ſpringy bodies would recede and fly from each other were they not kept together by the Heterogeneous compreſſing motions of the ambient whether fluid or ſolid.

Theſe principles thus hinted, I ſhall in the next place come to the particular explication of the manner how they ſerve to explain the Phænomena of ſpringing bodies whether ſolid or fluid.

Firſt for ſolid bodies, as Steel, Glaſs, Wood, *&c.* which have a Spring both inwards and outwards, according as they are either compreſſed or dilated beyond their natural ſtate.



Let A B repreſent a line of ſuch a body compounded of eight Vibrating particles, as 1, 2, 3, 4, 5. 6, 7, 8, and ſuppoſe each of thoſe Particles to perform a million of ſingle Vibrations, and conſequently of occurſions with each other in a ſecond minute of time,

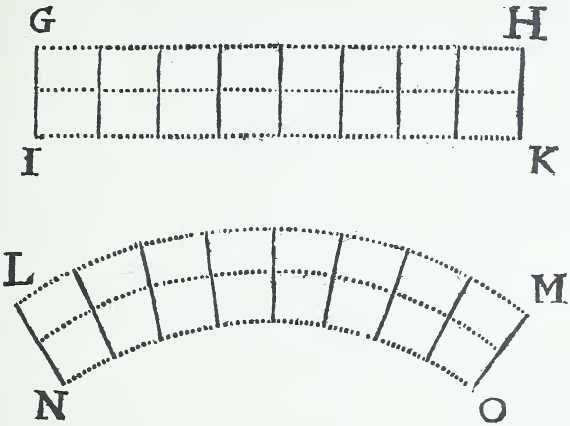
C 3 their

their motion being of ſuch a Velocity impreſſed from the Ambient on the two extreme Particles 1 and 8. Firſt, if by any external power on the two extremes 1 and 8, they be removed further aſunder, as to CD, then ſhall all the Vibrative Particles be proportionably extended, and the number of Vibrations, and conſequently of occurſions be reciprocally diminiſhed, and conſequently their endeavour of receding from each other be reciprocally diminiſhed alſo. For ſuppoſing this ſecond Dimenſion of Length be to the firſt as 3 to 2, the length of the Vibrations, and conſequently of occurſions, be reciprocally diminiſhed. For whereas I ſuppoſed 1000000 in a ſecond of the former, here can be but 666666 in this, and conſequently the Spring inward muſt be in proportion to the Extenſion beyond its natural length.

Secondly, if by any external force the extreme particles be removed a third part nearer together than (the external natural force being alway the ſame both in this and the former inſtance, which is the ballance to it in its natural ſtate) the length of the Vibrations ſhall be proportionably diminiſhed, and the number of them, and conſequently of the occurſions be reciprocally augmented, and inſtead of 1000000, there ſhall be 1500000.

Having

Having thus explained the moſt ſimple way of ſpringing in ſolid bodies, it will be very eaſie to explain the compound way of ſpringing, that is, by flexure, ſuppoſing only two of theſe lines joyned



together as at G H I K, which being by any external power bended into the form L N N O, L M will be extended, and N O will be diminiſhed in proportion to the flexure, and conſequently the ſame proportions and Rules for its endeavour of reſtoring it ſelf will hold.

In the next place for fluid bodies, amongſt which the greateſt inſtance we have is air, though the ſame be in ſome proportion in all other fluid bodies.

The Air then is a body conſiſting of particles ſo ſmall as to be almoſt equal to the particles of the Heterogeneous fluid medium incompaſſing the earth. It is bounded but on one ſide, namely, towards the earth, and is indefinitely extended upward being only hindred from flying away that way by its own gravity, (the cauſe of which I ſhall ſome other time explain.) It conſiſts of the ſame particles ſingle and ſeparated, of which water and other fluids do, conjoyned and compounded, and being made of particles exceeding

exceeding ſmall, its motion(to make its ballance with the reſt of the earthy bodies) is exceeding ſwift, and its Vibrative Spaces exceeding large, comparative to the Vibrative Spaces of other terreſtrial bodies. I ſuppoſe that of the Air next the Earth in its natural ſtate may be 8000 times greater than that of Steel, and above a thouſand times greater than that of common water, and proportionably I ſuppoſe that its motion muſt be eight thouſand times ſwifter than the former, and above a thouſand times ſwifter than the later. If therefore a quantity of this body be incloſed by a ſolid body, and that be ſo contrived as to compreſs it into leſs room, the motion thereof (ſuppoſing the heat the ſame) will continue the ſame, and conſequently the Vibrations and Occurſions will be increaſed in reciprocal proportion, that is, if it be Condenſed into half the ſpace the Vibrations and Occurſions will be double in number: If into a quarter the Vibrations and Occurſions will be quadruple, *&c.*

Again, If the conteining Veſſel be ſo contrived as to leave it more ſpace, the length of the Vibrations will be proportionably inlarged, and the number of Vibrations and Occurſions will be reciprocally diminiſhed, that is, if it be ſuffered to extend to twice its former dimenſions, its Vibrations will be twice as long, and the number of its Vibrations and Occurſions will be fewer by half, and conſequently its indeavours outward will be alſo weaker by half.

Theſe Explanations will ſerve *mutatis mutandis* for explaining the Spring of any other Body whatſoever.

It now remains, that I ſhew how the conſtitutions of ſpringy bodies being ſuch, the Vibrations of a Spring, or a Body moved by a Spring, equally and uniformly ſhall be of equal duration whether they be greater or leſs.

I have

I have here already ſhewed then that the power of all Springs is proportionate to the degree of flexure, *viz.* one degree of flexure, or one ſpace bended hath one power, two hath two, and three hath three, and ſo forward. And every point of the ſpace of flexure hath a peculiar power, and conſequently there being infinite points of the ſpace, there muſt be infinite degrees of power.

And conſequently all thoſe powers beginning from nought, and ending at the laſt degree of tenſion or bending, added together into one ſum, or aggregate, will be in duplicate proportion to the ſpace bended or degree of flexure; that is, the aggregate of the powers of the Spring tended from its quieſcent poſture by all the intermediate points to one ſpace (be it what length you pleaſe) is equal, or in the ſame proportion to the ſquare of one (ſuppoſing the ſaid ſpace infinitely diviſible into the fractions of one;) to two, is equal, or in the ſame proportion to the ſquare of two, that is four; to three is equal or in the ſame proportion to the ſquare of three, that is nine, and ſo forward; and conſequently the aggregate of the firſt ſpace will be one, of the ſecond ſpace will be three, of the third ſpace will be five, of the fourth will be ſeven, and ſo onwards in an Arithmetical proportion, being the degrees or exceſſes by which theſe aggregates exceed one another.

The Spring therefore in returning from any degree of flexure, to which it hath been bent by any power receiveth at every point of the ſpace returned an impulſe equal to the power of the Spring in that point of Tenſion, and in returning the whole it receiveth the whole aggregate of all the forces belonging to the greateſt degree of that Tenſion from which it returned; ſo a Spring bent two ſpaces in its return receiveth four degrees of impulſe, that is, three in the firſt ſpace returning, and one in the ſecond; ſo bent three ſpaces it receiveth in its whole return nine

D degrees

degrees of impulſe, that is, five in the firſt ſpace returned, three in the ſecond, and one in the third.

So bent ten ſpaces it receives in its whole return one hundred degrees of impulſe, to wit, nineteen in the firſt, ſeventeen in the ſecond, fifteen in the third, thirteen in the fourth, eleven in the fifth, nine in the ſixth, ſeven in the ſeventh, five in the eighth, three in the ninth, and one in the tenth.

Now the comparative Velocities of any body moved are in ſubduplicate proportion to the aggregates or ſums of the powers by which it is moved, therefore the Velocities of the whole ſpaces returned are always in the ſame proportions with thoſe ſpaces, they being both ſubduplicate to the powers, and conſequently all the times ſhall be equal.

Next for the Velocities of the parts of the ſpace returned they will be always proportionate to the roots of the aggregates of the powers impreſſed in every of theſe ſpaces; for in the laſt inſtance, where the Spring is ſuppoſed bent ten ſpaces, the Velocity at the end of the firſt ſpace returned ſhall be as the root of 19. at the end of the ſecond as the Root of 36. that is, of $19 + 17$. at the end of the third as the Root of 51. that is of $19 + 17 + 15$. At the end of the fourth as the Root of 64. that is of $19 + 17 + 15 + 13$. at the end of the tenth, or whole as the Root of 100. that is as $\sqrt{19 + 17 + 15 + 13 + 11 + 9 + 7 + 5 + 3} + 1$, equal to 100.

Now ſince the Velocity is in the ſame proportion to the root of the ſpace, as the root of the ſpace is to the time, it is eaſie to determine the particular time in which every one of theſe ſpaces are paſſed for dividing the ſpaces by the Velocities correſponding the quotients give the particular times.

To explain this more intelligibly, let A in the fourth figure repreſent the end of a Spring not bent, or at leaſt coun-

counterpoiſed in that poſture by a power fixt to it,and movable with it,draw the line A B C,and let it repreſent the way in which the end of the Spring by additional powers is to be moved, draw to the end of it C at right Angles the Line C δ D d, and let C D repreſent the power that is ſufficient to bend or move the end of the Spring A to C,then draw the Line D A, and from any point of the Line A C as B B.Draw Lines parallel to C D, cutting the Line D A in E, E, the Lines B E, B E, will repreſent the reſpective powers requiſite to bend the end of the Spring A to B, which Lines B E,B E,C D will be in the ſame proportion with the length of the bent of the Spring A B, A B, A C.

And becauſe the Spring hath in every point of the Line of bending A C,a particular power,therefore imagining infinite Lines drawn from every point of A C parallel to C D till they touch the Line A D, they will all of them fill and compoſe the Triangle A C D. The Triangle therefore A C D will repreſent the aggregate of the powers of the Spring bent from A to C, and the leſſer Triangles A B E, A B E will repreſent the aggregate of all the powers of the Spring bent from A to B, B, and the Spring bent to any point of the Line A C, and let go from thence will exert in its return to A all thoſe powers which are equal to the reſpective ordinates B E, B E, in the Triangles, the ſum of all which make up the Triangles A B E, A B E. And the aggregate of the powers with which it returns from any point, as from C to any point of the ſpace C A as to B B, is equal to the Trapezium C D E B, C D E B, or the exceſſes of the greater Triangles above the leſs.

Having therefore ſhewn an Image to repreſent the flexure and the powers, ſo as plainly to ſolve and anſwer all Queſtions and Problems concerning them, in the next place I come to repreſent the Velocities appropriated to the ſeveral powers. The Velocities then being always in a ſubduplicate proportion of

D 2 the

the powers, that is, as the Root of the powers impressed, and the powers imprest being as the Trapezium or the excess of the Triangle or square of the whole space to be past above the square of the space yet unpassed; if upon the Center A, and space A C, (C being the point from which the Spring is supposed let go) a Circle be described as C G G F, and ordinates drawn from any point of C A the space to be past, as from B, B, to the said Circle, as B G, B G, these Lines B G, B G, will represent the Velocity of the Spring returning from C to B, B, &c. the said ordinates being always in the same proportion with the Roots of the Trapeziums C D E B, C D E B for putting A C = to a, and A B = b, B G will always be equal to $\sqrt{aa - bb}$, the square of the ordinate being always equal to the Rectangle of the intercepted parts of the Diameter.

Having thus found the Velocities, to wit, B G, B G, A F, to find the times corresponding, on the Diameter A C draw a Parabola C H F whose Vertex is C, and which passeth through the point F. The Ordinates of this Parabola B H, B H, A F, are in the same proportion with the Roots of the spaces C B, C B, C A, then making G B to H B as H B to I B, and through the points C I I F drawing the curve C I I I F, the respective ordinates of this curve shall represent the proportionate time that the Spring spends in returning the spaces C B, C B, C A.

If the powers or stiffness of the Spring be greater than what I before supposed, and therefore must be expressed by the Triangle C d e A. then the Velocities will be the Ordinates in an Ellipse as C γ γ N, greater than the Circle, as it will also if the power be the same, and the bulk moved by the Spring be less. Then will the S-like Line of times meet with the Line A F at a point as X within the point F. But if the powers of the Spring be weaker than I supposed, then will C δ e e A represent the powers, and C γ γ O the Ellipsis of Velocity,

Velocity, whose Ordinates B γ, B γ, A O will give the particular Velocities, and the S-like Line of time will extend beyond N. The same will happen supposing the body (moved by the Spring) to be proportionately heavy, and the powers of the Spring the same with the first.

And supposing the power of the Spring the same as at first, bended only to B 2, and from thence let go B 2 E A is the Triangle of its powers, the Ordinates of the Circle B g L are the Lines of its Velocity, and the Ordinates of the S-like Line B i F are the Lines of time.

Having thus shewed you how the Velocity of a Spring may be computed, it will be easie to calculate to what distance it will be able to shoot or throw any body that is moved by it. And this must be done by comparing the Velocity of the ascent of a body thrown with the Velocity of the descent of Gravity, allowance being also made for the Resistance and impediment of the medium through which it passes. For instance, suppose a Bow or Spring fixed at 16 foot above a Horizontal floor, which is near the space that a heavy body from rest will descend perpendicularly in a second of time. If a Spring deliver the body in the Horizontal line with a Velocity that moves it 16 foot in a second of time, then shall it fall at 16 foot from the perpendicular point on the floor over which it was delivered with such Velocity, and by its motion shall describe in the Air or space through which it passes, a Parabola. If the Spring be bent to twice the former Tension, so as to deliver the body with double the Velocity in a Horizontal Line, that is, with a Velocity that moves 32 foot in a second, then shall the body touch the floor in a point very near at 32 foot from the aforesaid perpendicular point, and the Line of the motion of the body, so shot shall be moved in a Parabola, or a Line very near it, I say very near it, by reason that the

D 3 Impediment

Impediment of the medium doth hinder the exactness of it. If it be delivered with treble, quadruple, quintuple, sextuple, &c. the first Velocity it shall touch the floor at almost treble, quadruple, quintuple, sextuple, &c. the first distance. I shall not need to shew the reason why it is moved in a Parabola, it having been sufficiently demonstrated long since by many others.

If the body be delivered by the Spring at the floor, but shot by some Angle upwards, knowing with what Velocity the same is moved when delivered, and with what Inclination to the Perpendicular the same is directed, and the true Velocity of a falling body, you may easily know the length of the *Jactus* or shot, and the time it will spend in passing that length.

This is found by comparing the time of its ascent with the time of the descent of heavy bodies. The ascent of any body is easily known by comparing its Velocity with the Angle of Inclination.

Let *a b* then in the fifth Figure represent 16 foot, or the space descended by a heavy body in a second minute of time. If a body be shot from *b*, in the Line *b f* with a Velocity as much swifter than that equal motion of 16 foot in a second, as this Line *b f* is longer than *a b* the body shall fall at *e*; for in the same space of time that the oblique equal motion would make it ascend from *b d* to *a c*, will the accelerated direct motion downward move it from *a c* to *b d*, and therefore at the end of the space of one second, when the motions do equal and balance each other, the body must be in the same Horizontal Line in which it was at first, but removed asunder by the space *b e*, and for the points it passeth through in all the intermediate spaces this method will determine it.

Let the Parallelogram *a b p q* then represent the whole Velocity of the ascent of a body by an equal motion of 16 foot in a second, and the Triangle *p q r* represent the whole Velocity of of the accelerated deſcending motion, *p b* is then the Velocity with which the body is ſhot, and *p* is the point of reſt where the power of Gravity begins to work on the body and make it deſcend. Now drawing Lines parallel to *a q r*, as *s t u*, *s t* gives the Velocity of the point *t* aſcending, and *t u* the Velocity of the ſame point *t* deſcending.

Again, *p b s t* ſignifies the ſpace aſcended, and *p t u* the ſpace deſcended, ſo that ſubtracting the deſcent from the aſcent you have the height above the Line *b d*, the conſideration of this, and the equal progreſs forwards will give the intermediate Velocities, and determine the points of the Parabola.

Now having the *Jactus* given by this Scheme or Scale, appropriated to the particular Velocity, wherewith any body is moved in this or that line of Inclination, it will be eaſie to find what Velocity in any Inclination will throw it to any length; for in any Inclination as the ſquare of the Velocity thus found in this Scale for any inclination is to the ſquare of any other Velocity, ſo is the diſtance found by this Scale to the diſtance anſwering to the ſecond Velocity.

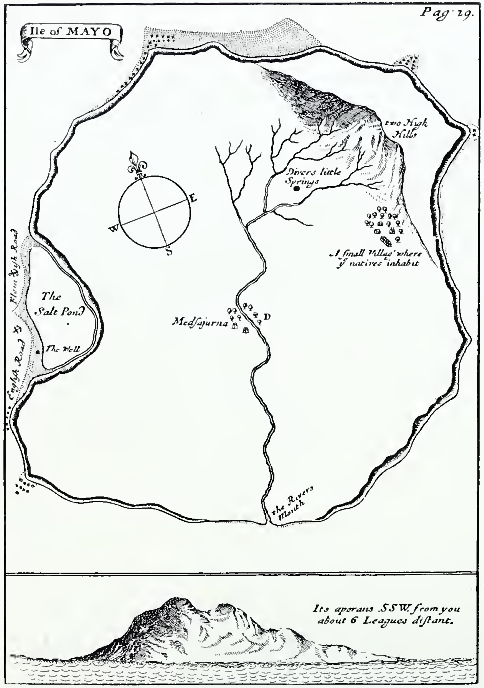
I have not now time to inlarge upon this ſpeculation, which would afford matter enough to fill a Volume, by which all the difficulties about impreſſed and received motions, and the Velocities and effects reſulting would be eaſily reſolved.

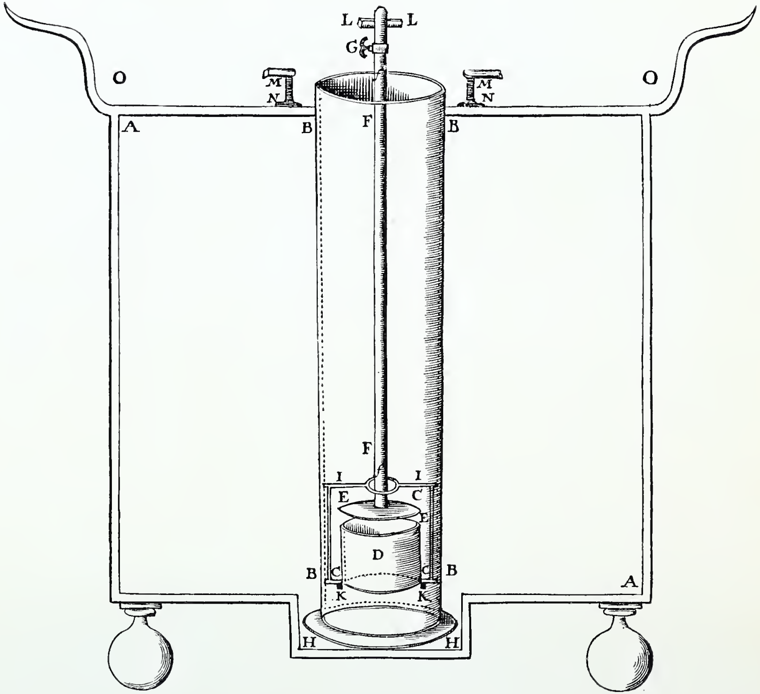
Nor have I now time to mention the great number of uſes that are and may be made of Springs in Mechanick contrivances, but ſhall only add, that of all ſpringy bodies there is none comparable to the Air for the vaſtneſs of its power of extention and contraction. Upon this Principle I remember to have ſeen long ſince in *Wadham* Colledge, in the Garden of the learned Dr. *Wilkins*, late Biſhop of *Cheſter*, a Fountain ſo contrived as by the Spring of the included Air to throw up to a great height a large and laſting ſtream

ſtream of water: Which water was firſt forced into the Leaden Ciſtern thereof by two force Pumps which did alternately work, and ſo condenſe the Air included into a ſmall Room. The contrivance of which Engine was not unknown to the Ancients, as *Hero* in his *Spiritalia* does ſufficiently manifeſt, nor were they wanting in applying it to very good uſes, namely, for Engines for quenching fire: As *Vitruvius* (by the help of the Ingenious Monſieur *Claude Perraults* interpretation) hath acquainted us in the Twelfth Chapter of his Tenth Book, where he endeavours to deſcribe *Cteſibius* his Engine for quenching fire. Not long ſince a German here in *England* hath added a further improvement thereof by conveying the conſtant ſtream of water through Pipes made of well tanned and liquored Leather, joyned together to any convenient length by the help of brazen Screws. By which the ſtream of water may be conveyed to any convenient place through narrow and otherwiſe inacceſſible paſſages.

The ingenious Dr. *Denys Pappin* hath added a further improvement that may be made to this *Cteſibian* Engine by a new and excellent contrivance of his own for making of the forcing Syringe or Pump, which at my deſire he is pleaſed to communicate to the Publique by this following Deſcription, which he ſent me ſome time ſince.

Dr. *Pappins*


Pag. 29.
Ile of MAYO
two High Hills
Divers little Springs
A small Villag where ye natives inhabit
Medsajurna
D
the Rivers Mouth
The Salt Pond
The Well
Flem Engh Road
English Road
E
W
S
Its aperans S.S.W. from you about 6 Leagues distant.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H

Dr. Pappins *Letter containing a Deſcription of a Wind-Fountain, and his own particular contrivance about the forcer of its Syringe.*

SInce the Artificial Fountain you have ſeen at Mr. *Boyles* (which was of my making upon his deſire) hath been ſo pleaſing to you as to make you deſire to ſee my deſcription thereof, I cannot doubt but the ſame will be as grateful alſo, and well received by the Publick, eſpecially when they ſhall therein find a remedy for one of the greateſt inconveniences of forcing Pumps, which are of ſo great uſe for raiſing of water, and quenching of fires. This was the occaſion of my ſending you this preſent deſcription, which would not have been thus prolix had it been only for your ſelf.

In the Figure then A A is the Receptacle or body of the Fountain careful ſodered in all places, B B is the Pump, C C the Plug or forcer, D a Pipe in the middle of the Plug, which is perfectly ſhut and ſtopped when the Plate E E is forced down upon it, E E is the Plate with a hole in the middle, upon which is ſodered a Pipe F, which ſerves for a handle to move the Plug up and down.

G is a Cock at the top of the Pipe, which ſerves to moderate the Jetto or ſtream.

H H is a Valve at the bottom of the Pump, which openeth outward for the paſſage of the water out of the Pump into the Fountain or Receptacle.

I I is a Croſs at the top of the Plug to hinder the Plate E E from being drawn or ſeparated too far

E from

from the hole D in working it to and fro.

K K are two Pins ſerving both to force down and keep open the Valve H H.

L L are two Appendices ſodered unto the top of the Pipe F F, ſerving both for a handle to the Rod of the forcer, and alſo to keep down the forcer.

M M are two other appendices or buttons faſtned at the top of the two ſmall pillars N N, ſo as to turn upon the ſame, and ſerve to haſp or button down the ends LL of the handle of the forcer that it be not driven up again.

O O is the Baſin for receiving the water that falls from the Jet or ſtream from which it may be forced again into the Fountain or Receptacle.

For charging this Machine the Baſin O O muſt firſt be filled with water, and then the Pump muſt be worked to and fro. In doing of which, when the Plug is drawn upwards the water in the Baſin runs in through the croſs (through which the Rod F F paſſes,) where finding the hole D open it fills the ſpaces of the bottom of the Pump; then the Pump being thus filled, the Plug is to be forced downwards, whereby the Plate E E being cloſely applied to the brims of the hole D hinders the water from returning back again through the ſame, but is forced through the valve H H into the Fountain A A. And by repeating this operation all the water of the Baſin O O is eaſily forced into the aforeſaid Fountain, whereby all the Air that was therein contained is compreſſed more or leſs according as more or leſs water is forced in, and kept in that compreſſion by the valve H H, which hinders the water that it cannot return through the ſame.

But when you deſire to have it return, you force down the Plug hard againſt the bottom or plate, which by the help of the aforeſaid Pins or Appendices K K force, and keep open the valve H H, and the Rod F being kept faſt down in this poſture by the aforeſaid

aforesaid Buttons or Hasps M M, upon opening the Cock G the water returneth through the valve HH, so kept open, through the hole D, and through the whole length of the Pipe F.

This way of putting a valve into the Plug of forcing Pumps will be of great use for all such as serve for supplying Towns with water, and for quenching of fire, as preventing a great inconvenience to which the common Pumps are usually subject from the Air which is apt to be generated within them, which Air upon working the said Pump remaining below the forcer, and by its Expansion when the Plug is drawn upwards, hindring the water from filling the whole Cavity beneath it, and by its Condensation when the Plug is forced downwards, losing a great part of the strength of the force, much of the effect of the said Machine is frustrated.

For preventing of which Inconvenience care is to be taken that the water in all these forcing Pumps be admitted by the top thereof as in the present Machine, whereby whatever Air shall be generated below the Plug, will readily rise into the hole D as being the highest place next the Plate E E, from whence when by the drawing up of the Plug the Plate is lifted from the brims of the hole D the Air will readily slip up, and the water as readily descend and fill all the parts of the Pump below the Plug. As I have often experimented in this present Machine.

Some Persons may object against these kind of valves, as supposing the pressure of the water to be on the wrong side thereof. But it is easie to be noted that this objection is groundless, since it is the same thing whether the Plate be pressed against the Rim of the valve, or the Rim of the valve against the plate. In common valves the Pressure of the water forceth the Plate against the Rim: But in this the Rim against the Plate; for the remaining solid Rim of the valve, being made thrice as big as the hole or Cavity thereof,

E 2 the

the preſſure of the water againſt that Rim forceth the ſaid Rim againſt the Plate in the middle three times harder than if the preſſure of the water lay only on the plate of the value, the ſame would be preſſed againſt the Rim.

To this Diſcourſe of an Artificial Fountain I thought it not improper to add an ingenious Diſcourſe of M. *James Young* of *Plimouth* conteining his own Obſervations and Opinion concerning natural Fountains and Springs.

SIR,

HAving now gained time, from my other avocations, I have drawn up thoſe obſervations. I told you I had made in my travels, which had confirmed in me the opinion of my Lord *Bacon*, that Fountains and Springs were the Percolation of the Sea; not (as your ſelf, Mr. *Ray*, *&c.* do aſſert) from the rains deſcent into the Earth, I now repreſent them to your conſideration, rather as an Apology (becauſe they ſeem rational) to excuſe, than Arguments to juſtifie and avow the preſumption of my diſſent.

The firſt ſhall be the Phænomena, I obſerved at Iſle *de Mayo*, which lieth in the Torrid Zone, about thirteen degrees and 30 minutes, North from the Equator. It's about ſix Leagues long, and four broad, the wind bloweth conſtantly North Eaſt, or thereabout, and without rain, except three weeks in *July*, when it hath many ſhowers; I here ſend you a Map of the Iſland, as exactly as I could draw it. I was there two Voyages, and each remained a full month, the beſt part of which I ſpent in hunting, and ranging the Iſland; there runneth through the middle of it a Rivulet, of very pure water; It takes its riſe from

from the bottom of two Hills, which lie on the North East end; The ſtream at the place marked D, is about fourteen foot wide and two deep; other than which there is no freſh water on the whole Iſland, except what our people dig out of the ſand between the Ocean and the ſalt Pond.

The ſaid Pond is in a large Bay, at the Weſt ſide of the Iſland, which hath from one point to another a bank of Sand, about two or three foot above water, covering the Bay like a ſtring to a Bow, the ſaid bank in the Flemiſh Road is about 150 foot wide, at the Engliſh Road it is as broad again; there is never any ſenſible ebbing or flowing of the Sea, only at full Moons, or a day before. It riſeth in high Billows, which break over the Bank, at the North end of the Pond, where it is loweſt: By which means the Pond is repleniſhed with water, which condenſeth into Salt in two days.

The Sand dividing the ſaid Pond and the Sea is very fine and looſe. Now becauſe the before-mentioned Rivulet diſembogues far from the Roads at an inconvenient place for Boats, they are conſtrained to dig Wells, in the midſt of the bank of Sand, between the Pickle of the ſalt Pond and the Sea, the manner thus: They firſt dig a pit about eight foot deep, and therein lay two Hogſheads, the one on the top of the other, the head out of both ſave the lowermoſt of the deepeſt; the ſides of both are alſo full of Gimlet holes, and the ſand laid cloſe to them: After twenty four hours they have three or four foot of very clean water in them, which being dipped out, you plainly ſee the new water ſtrain gently through thoſe holes in the ſides of the Cask: After which, in a days time, one man attending it, may draw about ten Hogſheads or more of water, a little taſting of Salt, not ſo much but that it is drinkable, and very fit to boyl meat in, and is uſed by thoſe that come there to load Cattle, for their

E 3 common

common drink. I have in the Map placed the Sign O where our Well was made.

The next obſervations, pertinent to this ſubject I made at the Iſland *Lipari*, near *Sicily*, about ſixteen Leagues from *Meſſina*; it is famous for the beſt Raiſins in the *Mediterranean*; there is on it a large Caſtle, a ſmall Town, many Vineyards, and about one hundred Families, beſides ſome *Religioſe* I judge it wants a fifth part of the bigneſs of the Iſle *de Mayo*, it is moſtly very high Land, eſpecially one Mountain, on which ſtands a Watch Tower, whence a man may ſee a monſtrous diſtance at Sea, as is confirmed by *de Ruyter*. In the relation he gives the States of *Holland*, wherein he tells them, that from that place they diſcerned the French Fleet's approach long before they could from any other part, either of their own or the other Iſland. I am ſure it is much higher than either that at the Iſle *de Mayo*, or any I have ſeen in *England*, and yet on this fair fruitful Iſland ſprings not one drop of water, the Inhabitants ſtoring themſelves with rain, which falling very frequently, they are careful to preſerve in Ciſterns, divers eſſays have been made in the moſt promiſing part of it to find Springs by digging Wells, one of thoſe which I ſaw was without doubt the deepeſt in *Europe*, I remember not the exact profundity as they related it, but I have not forgot, that throwing in a ſtone it was long ere it got to the bottom, and then returned ſuch a noiſe as it had been the diſcharge of a Muſquet.

The cauſe of this drineſs was by the people thought to be ſubterranean heats, abſuming the water, but no ſuch thing appearing, to the ſenſe of thoſe that digged the Wells, I gave no faith to that perſuaſion; they fancy ſuch heats partly from the want of water, but moſtly becauſe the four adjacent Iſlands, *Stromboli*, *Vulcano*, *Vulcanella*, and *M. Ætna*, are conſtantly burning, and very near them.

The

The obvious earth of this place is looſe, and in all apparent qualities very good, but by the heaps that had been thrown up, in digging the Wells, I ſaw the inferiour earth was clammy, or like clay, that had ſome greaſie gummous matter commixed, This the Religious told me was the very kind of Sulphur which conſtantly boyled out of the burning Cranny on *Vulcanella*; and wherewith all thoſe Iſlands abounded, not excepting their own, though it were not yet kindled.

For my third obſervation, I will go no farther than the place of my preſent abode, *Plimmouth*, in which on a kind of Piazza, commonly called the *New-key*, (a plat of ground got in from the Sea) is a Well, which (before the ever famous Sir *Francis Drake* by cutting a Rivulet of thirty miles procured us water in great plenty) was of common uſe, having (as at this day) a Pump in it; about ſeven years ſince (being before the Key was inlarged) the Well was not above eight foot from the edge thereof, over which the Sea would frequently flow, when a high out-wind and a Spring Tide concurred, I ſay this Well, though ſo near the Sea, yieldeth clean water, and as ſweet as a mixture of three parts freſh and one of ſalt water would be. About an hundred yards from that, on ground a little riſing, is a very large Well, which ſupplieth three or four Brew-houſes, by whoſe drink it is evident that the water hath not wholly quitted its ſalt. It is to be noted, that *Plimmouth* lieth on a Peninſula three miles long, and two broad, the Iſthmus about two thirds of a mile wide, and not very high from the ſurface of a full Sea. There are many Wells in it, thoſe near the Sea are ſaltiſh, thoſe farther from it the leſs ſo.

My fourth obſervation I take from the late famous French Traveller Monſieur *Taverner*, who in his firſt Volume, diſcourſing of the Coaſt of *Coromandel*, &c. he ſaith they there want freſh water, and are con-
ſtrained

ſtrained to make pits of two foot deep in the ſand by the Sea to find it.

The fifth obſervation, and which I would call the moſt ſignificant, were I aſſured of its truth, I had from a very ingenious Chirurgeon, who had uſed the *Weſt Indias*, that there is in that Sea an Iſland called *Rotunda*, of a figure agreeable to its name, which, though very ſmall, hath on it, ariſing in the middle, a Spring of a very large ſtream of water, at which our Ships frequently furniſh themſelves in their Navigation, he affirmed that it raineth there but once a year, as at the Iſle *de Mayo*; ſaying withal, that the Iſland is ſo ſhort of a proportion big enough for the ſtream, that if it conſtantly rained, it could not be ſupply enough to maintain ſo large an Efflux.

My ſixth and laſt, is the relation of Dr. *Downes* concerning *Barbadoes*, *viz.* that all their Springs were formerly very near the Sea; that up in the Country they ſupplied themſelves from the rains by digging pits in the earth, able to contain great quantities, and there preſerving it; which they did a very long time (the rains being there as unfrequent as at the Iſle *de Mayo*) and that without any ſenſible diminution by penetrating and deſcending into the earth; and to prevent the loſs thereof by the exhalations of the Sun they covered it with leaves, *&c.* but that now by digging deeper they find Springs ſo plenty that no Plantation is without one.

From all theſe obſervations the following conſectaries do mechanically reſult.

From the firſt it appeareth that ſome Springs have manifeſtly their ſource from the Sea; that ſand ſweetens tranſcolated Sea-water, and that even pickle ſtrained through it loſeth much of its ſaltneſs thereby, all which is evident from the Well therein mentioned, whoſe water could not poſſibly be other than what ſoaked in from the Pond and the Ocean.

Hence

Hence alſo is manifeſt, that conſtant and large Fluxes of water may be made for eleventh months without rain to refill the ſubterranean Ciſterns, ſuppoſed by you to ſupply them ; this appears from the River running through the Iſland, by whoſe banks I found (it being *April* when I was there, at which time they had been ten months without rain) thſh after their ſhowers it could run but little larger that it did after ſo tedious a want of them. I had forgot to intimate in the relation, that thoſe two Hommets, A. are craggy Rocks, whereon live a great number of Goats, and are conſequently very unfit, if not incapable, either to receive, or contain the Magazine for the ſupply of the Rivulet.

From the ſecond it is manifeſt, that higher Mountains of earth, and conſequently more likely to receive and contain ſufficient quantity of rain-water to beget and ſupply Springs and Rivers have not always that effect, although there was one great advantage more added here, *viz.* a clammy tyte earth in the bottom to make the ſuppoſed Ciſtern the better able to contain the ſtore. I ſay, that frequent rain to fill, high Mountains to contain, looſe pervious earth to receive, and a well luted bottom to ſupport and retain (being all the qualifications and circumſtances ſuppoſed neceſſary to make and continue Springs according to the modern Hypotheſis) though all here concurred, did notwithſtanding fail of producing that effect.

From the ſame it is alſo manifeſt, that where Springs fail, without want of the cauſes that Hypotheſis ſuppoſeth neceſſary to produce them, the occaſion hath been from an apparent defect in the other (that is the imperviouſneſs of the earth through which the water muſt paſs before a Spring can be produced) both theſe appeared at *Lipary*, where the general effect a Spring or fountain was wanting, together with the cauſes of our Hypotheſis, though thoſe of the other were

F manifeſtly

manifeſtly exiſtent, and with all the advantages neceſſary: It ſeeming to me a very rational conjecture, that the greaſie clammy Sulphur, wherewith that earth was impregnated, did by oppilating it hinder the inſinuation of the Sea into it.

From the third obſervation you have the firſt deduction confirmed, *viz.* That Springs are ſometimes manifeſtly from the Sea; That earth ſweetens Sea-water by Percolation; And that the nearer Springs are to the Sea, the more they retain of their priſtine ſaltneſs, and loſe it by ſenſible degrees, as they inſinuate farther through it.

By the fourth the ſame is confirmed.

The fifth proveth, that large ſtreams flow without any poſſibility of being ſupplied by rain, both for want of ſuch rain, and of dimenſions to receive and contain it.

The ſixth doth evidence, that rain doth not penetrate the Surface of the earth, even in a very dry parched Country, and in the Torrid Zone, and yet that Springs are under it, which at once proves ours, and refutes the other opinion; the former appears by the water in thoſe made Ponds, lying there for a long time without any ſenſible loſs thereof by its leaking into the earth: The later by the Wells near the Sea, and thoſe found ſince under that impervious Land.

He that is not altogether a ſtranger to the weight, preſſure, and Elaſticity of the air, the aſcenſion of liquors through Filters, and ſome other reſembling Phænomena, would not account the like motion of the tranſcolated water to high hills, to be an objection of any force againſt this Hypotheſis, but ſure ſuch ſolutions are no leſs beyond my ability than deſign.

Finding I have Paper enough left, I will preſume to trouble you with one rare appearance more, that occurred to one Mr. *Braſey* of this Town, an aged and

and very fat man, who by taking Spirit of Vitriol in his mornings draughts (to which he was adviſed as a remedy to aſſwage the exuberance of his belly) found that it had no effect on his body; but that a bundle of Keys, which he uſed to carry always about him, and that wonted to be very ſmooth and bright, of a ſudden became black and ruſty, though he never handled the Spirit, nor carried it in his pocket, ſo that we concurred in opinion that the ſudorous Effluvia of his body, impregnated with the Acid Spirit, had occaſioned it.

If ſo, It's very wonderful, that ſo ſmall a quantity thereof, when diluted with ſo much juice as is contained in ſuch a corpulent man, ſhould even in ſteam and the inſenſible Emanations make impreſſions on ſmooth Iron, mauger the perpetual attrition, by carrying them in his Pocket, whereby ſuch an effect (one would think) ſhould be prevented, or ſoon rubbed of.— I was going to make ſome reflections on this notable accident, but I conſider, *&c.*———

Plimmouth
May 5. 1678.

James Young.

F 2 The

THE Original of Springs is that which hath exercised the Pens of many learned Writers, and very various have been the conjectures concerning it. But amongst all I have met with I conceive none more probable than that which seems to fetch its original from the History of the Creation mentioned in Holy Writ; that is, that there is a Magazine of waters above as well as a Receptacle of waters upon or beneath the Surface of the Earth: And that the Air is that Firmament which separates between the upper and lower waters, and between these two is the circulation of waters (or bloud of the Microcosm, if I may so call it) performed. The water being sometimes by a particular constitution of the Air assisted by heat, rarified and separated into minuter parts, and so reduced into the form of Air, and thereby being divided into Particles really smaller than those of the air in compassing, and agitated with a greater degree of motion, they take up more space, and so become lighter than the Ambient, and are thereby elevated and protruded upwards till they come to their place of poise or Equilibrium in the Air: At other times by a differing constitution of the Air and deficiency of heat they lose their agitation, and many of them again coalesce, and so having less motion they condense and revert into water, and so, being heavier than the incompassing Air, descend down again to the Earth in Mists, Rain, Snow, Hail, or the like.

That there is such a Circulation I think there is none doubts, but still it remains a difficulty (with those persons that grant this) that all Rivers and Springs should have their original from the water that falls or condences out of the Air.

To persuade such persons it may not possibly be unsuccessful to mention:

First,

Firſt, That the great inundations or overflowing of Rivers manifeſtly proceed either from the Rain that immediately falls,or from the melting of Snow or Ice that hath formerly fallen on the more eminent parts of Mountains; to confirm which. Hiſtories enough might be brought were it neceſſary of *Nilus*,*Niger*,&c.

Secondly, That it hath been obſerved and computed that *communibus annis & locis*; there falls water enough from the Sky in actual Rain,Snow, or Hail upon the Surface of *England* to ſupply all the water that runs back into the Sea by the Rivers, and alſo all that may be ſuppoſed to evaporate; nay,though the quantity of the firſt be ſuppoſed twice as much as really it is. This I have been aſſured by thoſe that have both experimented and calculated it.

Thirdly, That there is not yet certainly (that I know or have heard of) any other way of making ſalt water freſh, but by Diſtillation; which, had there been ſuch an Art, it would in all probability have been made uſe of, and ſo there is little probability that the Springs at the top of a high Hill ſhould proceed from the Sea-water ſtrained through the earth. But were there ſuch a filtration known I hinted in my Attempt, publiſhed *anno* 1660 about Filtration, how ſomewhat of that kind might be explained.

Fourthly, That this Operation is conſtantly and moſt certainly performed by Nature both in exhaling and drawing up freſh ſteams and vapours from the Sea, and all moyſt bodies, and in precipitating them down again in Rain, Snow, Hail, but of the other we have no certainty.

Fifthly, I have obſerved in ſeveral places where a Tree hath ſtood upon an high Hill, ſingly and particularly at the brow of Box Hill near *Darking* in *Surry*, that the body of the Tree is continually wet, and at the root ſome quantity of water, which is always ſoaking and gliding down from the Branches and body of the Tree, the leaves, ſprigs, and branches of

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the ſaid trees collecting and condenſing continually the moyſt part of the Air, the ſame being indeed a true and lively repreſentation of a River. Nor has it been my obſervation alone, but the ſame is mentioned by divers Authors: And it is affirmed by ſome Authors, that there are ſome Iſlands in the Torrid Zone which have no other water in them than what is condenſed out of the Air by the Trees at the tops of the Hills, and converted into drops of Rain.

Sixthly, That it is generally obſerved, whereever there are high Hills there are generally many Springs round about the bottoms of them of very freſh and clear water, and often times ſome which riſe very near the tops of them, which ſeems to proceed from their great elevation above the other plain ſuperficial parts of the earth, whereby the Air being daſhed and broken againſt them, they help to condenſe the vapours that are elevated into the higher and cooler Regions of the Air, and ſo ſerve like Filtres to draw down thoſe vapours ſo condenſed, and convey them into the Valleys beneath, And hence it is very uſual in Countries where there are high Hills to ſee the tops of them often covered with clouds and miſts, when it is clear and dry weather beneath in the Valleys. And in the paſſing through thoſe clouds on the top I have very often found in them very thick miſts and ſmall rain, whereas as ſoon as I have deſcended from the higher into the lower parts of the Hills, none of that miſt or rain hath fallen there, though I could ſtill perceive the ſame miſts to remain about the top. Conſonant to this Obſervation was one related to me by an ingenious Gentleman Mr. *G. T.* who out of curioſity with other Gentlemen whilſt he lived in the Iſland of *Teneriff*, one of the *Canaries* made a journey to the top of that prodigious high Mountain, called the *Pike*. The ſubſtance of which (to this purpoſe) was, that the *Caldera* or hollow Cavity, at the very top

top thereof he obſerved to be very ſlabby and moyſt, and the earth to ſlip underneath his feet, being a very moyſt ſoft Clay or Lome like mortar. And farther, that at a Cave, not far from the top, there was a great quantity of very freſh water, which was continually ſupplied, though great quantities of Ice were continually fetch'd from thence, and carried down into the Iſland for cooling their Wines. Conſonant to which Obſervation was that which was related to me by the Inquiſitive Mr. *Edmund Hally* made in St. *Helena* whilſt he ſtayed there to obſerve the places of the Stars of the Southern Hemiſphere, in order to perfect the Cœleſtial Globe. Having then placed himſelf upon one of the higheſt Prominences of that ſmall Iſland, which he found to be no leſs than 3000 foot Perpendicularly above the Surface of the Sea next adjoyning, ſuppoſing that might be the moſt convenient place for his deſigned obſervation; He quickly found his expectation much deceived as to that purpoſe for which he choſe it; for being gotten ſo high into the Air the motion of it was ſo violent as much to diſturb his Inſtruments; but which was more, he found ſuch abundance of miſts and moyſture that it unglued the Tubes, and covered his Glaſſes preſently with a Dew; and which was yet more, the foggs and miſts almoſt continually hindred the ſight of the Stars. But upon removing to a lower ſtation in the Iſland he was freed from the former Inconveniences.

I could relate many Hiſtories of this nature, whereby it ſeems very probable, that not only Hills, but Woods alſo, do very much contribute to the condenſing of the moyſture of the Air, and converting it into water, and thereby to ſupply the Springs and Rivulets with freſh water: And I am confident, whoſoever ſhall conſider his own obſervation of this nature, and compare them with this Theory, will find many arguments to confirm it. However, *Nullius in verba*,

verba, Let Truth only prevail, and Theories ſignifie no further than right reaſoning from accurate Obſervations and Experiments doth confirm and agree with them.

Having thus delivered here ſomewhat of my own thoughts concerning Springs and Rivers, finding among ſome of my Papers a Relation, wherein a very ſtrange ſubterraneous Ciſtern is mentioned, I have here ſubjoyned it as I received it from Mr. *Thomas Alcock* from *Briſtol* who together with Sir *Humphry Hooke* was by whilſt Captain *Samuel Sturmy* made this inquiry, and who by interrogatories made to him, penn'd this Relation for him as it follows verbatim.

IN purſuance of His Majeſties Commands to me at the preſenting of my *Mariners Magazine*, I have with much diligence, ſome charge and peril endeavoured to diſcover that great Concavity in the earth in *Gloceſterſhire*, four miles from *Kingrode*, where His Majeſties great Ships ride in the *Severn*. And I find by experience that what has been reported of that place is fabulous, whilſt I thus deſcribe it.

Upon the ſecond of *July* 1669. I deſcended by Ropes affixt at the top of an old Lead Oare Pit, four Fathoms almoſt perpendicular, and from thence three Fathoms more obliquely, between two great Rocks, where I found the mouth of this ſpacious place, from which a Mine-man and my ſelf lowerd our ſelves by Ropes twenty five Fathoms perpendicular, into a very large place indeed, reſembling to us the form of a Horſe-ſhoo; for we ſtuck lighted Candles all the way we went, to diſcover what we could find remarkable; at length we came to a River or great Water, which I found to be twenty fathoms broad, and eight fathoms deep. The Mine-man would have perſwaded me, that this River Ebbed and Flowed, for that ſome ten fathoms above the

the place we now were in we found the water had (sometime) been, but I proved the contrary by staying there from three hours Floud to two hours Ebb, in which time we found no alteration of this River; besides, it's waters were fresh, sweet, and cool, and the Surface of this water as it is now at eight fathom deep, lies lower than the bottom of any part of the *Severn* Sea near us, so that it can have no community with it, and consequently neither flux nor reflux, but in Winter and Summer, as all *Stagna's*, Lakes, and Loughs (which I take this to be) has. As we were walking by this River thirty two fathoms under ground, we discovered a great hollowness in a Rock some thirty foot above us, so that I got a Ladder down to us, and the Mine-man went up the Ladder to that place, and walk'd into it about threescore and ten paces, till he just lost sight of me, and from thence chearfully call'd to me, and told me, he had found what he look'd for (a rich Mine;) but his joy was presently changed into amazement, and he returned affrighted by the sight of an evil Spirit, which we cannot perswade him but he saw, and for that reason will go thither no more.

Here are abundance of strange places, the flooring being a kind of a white stone, Enameled with Lead Oare, and the Pendent Rocks were glazed with Salt-Peter which distilled upon them from above, and time had petrified.

After some hours stay there, we ascended without much hurt, other than scratching our selves in divers places by climing the sharp Rocks, but four days together after my return from thence I was troubled with an unusual and violent Headach, which I impute to my being in that Vault. This is a true account of that place so much talk't of, described by me

Samuel Sturmy.

G Having

Having given you a Relation of ſomething very low within the bowels of the Earth, I now ſhall add,

An account of a Journey made to the higheſt part of the earthby my Ingenious Friend Mr.G.T. as I collected it out of the Memorials which he writ at the time of making it ; The particulars whereof were,

THat *Auguſt* the twentieth, 1674.about Nine in the morning, in company with Dr. *Sebaſtian de Franques*, Mr. *Chriſtopher Prancis*, Mr. *Thomas Proudfoot*, together with a Guide, and two other men with horſes to carry themſelves and neceſſary proviſion for the Journey, he ſet out from

They paſſed up a Hill, which was very ſteep, till they came to the *Pinal* or Wood of Pines. This Wood lieth very high in the Iſland, and extendeth it ſelf from one end of the Iſland to the other, and is in many places of a great Breadth, and is very frequently covered with a *Bruma*, fog, or miſt, which is ſo thick as to darken and hinder the appearance of the Sun through it, and ſo moyſt as to make one wet in paſſing through it.

Through this Wood they rode by a pretty ſteep aſcent near two Leagues, croſſing it till they came to the further or ſide, where alighting they reſted themſelves under a Pine, and Dined. And the fog, which had accompanied them through the whole Wood, here left them, and the Sun appeared.

From hence they parted about one in the Afternoon, and after an aſcent of about half a mile of very bad ſtony way they came to a ſandy way, which for about the length of a League was pretty plain ; but then they began to aſcend a ſandy hill, which for half a League farther was pretty ſteep, which having paſſed they arrived at the foot of the *Pike*.

Here they alighted, and then reſted themſelves for ſome time, then taking horſe again, they began to to aſcend the *Pike* it ſelf. This part of it was ſo ſteep that the way up it is made by ſeveral turnings and windings to and fro to eaſe and alleviate the ſteepneſs of the aſcent, which were otherwiſe unpaſſable for horſes. All this part ſeems to be nothing elſe but burnt ſtones and aſhes, which may have formerly tumbled down from the higher parts of the *Pike*.

At this place they alighted, and unloaded their horſes of the Proviſion of Victual and water which they were forced to carry with them for their own accommodation, as alſo of the Provender for their horſes. And preſently ſet themſelves to provide againſt the inconveniences of the enſuing night by getting together in the firſt place a good quantity of the wood of a ſmall ſhrub, called *Retamen*, not much unlike our Engliſh Broom, which grows there pretty plentifully, and when dry burns very well; then, having gotten wood enough, they endeavoured to ſhelter themſelves againſt the piercing cold wind by heaping up a wall of ſtones on the windward ſide, and making a good fire of the dry ſhrubs they had collected to warm themſelves.

But ſo furious was the wind which came pouring down from each ſide of the Mountain that it blew the ſmoak and aſhes into their eyes, and forced them (though much to their Regret by reaſon of the extreme piercing coldneſs of the Air) to remove their fire farther off. And to keep themſelves as warm as they could by lying down upon the ground very cloſe together. Thus they paſſed the night together as well as they could, but with very little ſleep, partly by reaſon of the cold, and partly for the continual expectation they had of the moment when their Guide would call them to be mounting up the *Pike*, which is uſually about two or three hours before day, to the end that they may get up to the top before the riſing of the Sun. For at the riſing of the Sun the Air is the moſt clear, and all the Iſlands of

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the *Canaries* round about may be then plainly diſcovered.

But at two a clock, when they ſhould have been on their Journey, the wind continued to blow with ſuch violence, that their Guide would by no means venter to go up for fear leaſt in the climbing up ſome ſteep places the wind ſhould encounter any of them, and hurl them headlong down, ſo that they were forced to continue and ſhelter themſelves in their bad Lodgings till the Sun aroſe, and had got ſome maſtery of the wind.

About ſix a clock therefore they ſet forwards on their enterpriſe, having firſt taken each of them his diſh of Chocolatte to fortifie their ſtomachs the better againſt the cold, ſo with their Bottle of Strong-water in their Pockets, and Staves in their hands, they began to mount the *Pike*, the way being juſt ſuch as they had paſſed the night before, but much more ſteep, and continued on till they came to the *Mal pays*, or ſtony way, which may be about half a mile from the place where they lay; This ſtony way lieth upon a very ſteep aſcent, and is compounded of abundance of ſtones which lie hollow and looſe, ſome of them of a vaſt prodigious bigneſs, and others of them ſmaller, in ſuch manner as if they had been thrown up there by ſome Earthquake, as the Author conjectures with very great probability. In the clambring up theſe ſtones they took great care in placing their ſteps on ſuch of them as were more firm for fear of ſlipping or tumbling ſo as to break their Legs or Arms.

With this difficulty they aſcended till they came to the Cave which he conjectures to be about three quarters of a mile diſtant from the beginning of the ſtony way.

At this Cave they found ſeveral perſons who were come thither to get out Ice to carry down into the Iſland, ſome of which were below in the Cave, digging

digging Ice which was very thick, others remained above. They found the mouth of the Cave about three yards high, and two yards broad; and being all of them deſirous to deſcend into it, by a Rope faſtned about their bodies under their armpits they were all one after another let down into it till they came to ſet their feet upon the Ice, which is about ſixteen or eighteen foot from the mouth.

The Cave is not very large, but full of water and Ice, which at the time when they were there lay about a foot under the Surface of the water, though the men that uſually go thither ſaid that at other times they found the Ice above the water, which makes many to ſuppoſe that it ebbs and flows by means of ſome ſecret entercourſe that it may have with the Sea, they averring that they have ſeen it emptying of it ſelf.

But this Gentleman ſo ſoon as ever he came down fixt his eye upon a ſtone that lay juſt above the Superficies of the water, and obſerved very diligently but could not in all the ſpace that he ſtaid there, which was half an hour, find it either increaſe or diminiſh, which makes him believe that the fulneſs or emptineſs of the water may rather proceed from thoſe thick fogs and miſts which are generally on the top, and which hinder the *Pike* from being ſeen ſometimes for twenty, thirty, nay, forty days together, except only juſt at the riſing or ſetting of the Sun, though at ſome other times it happens alſo that the Air is clearer, and the *Pike* may be ſeen perhaps for a month together. From theſe miſts he conceives at ſome times much water may be collected at the upper parts of the *Pike*, and ſoaking down may not only ſupply, but increaſe the water in the Cave; and conſonant to this Hypotheſis he obſerved whilſt he was there, that there was a continual gleeting and dropping of water in ſix or ſeven places from the ſides of the Cave, which droppings he ſuppoſes may be greater or leſs according as

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thoſe fogs do more or leſs encompaſs it, or ſtay about it a longer or ſhorter time; He judges alſo that there may be ſome other more ſecret ways both for the conveying water into and out of the ſaid Cave than thoſe droppings, but ſuppoſes them to proceed from the aforeſaid fogs. Hence he concludes when the Air is clear, and none of thoſe fogs condenſed about the Hill, the water in the Cave muſt neceſſarily decreaſe. And that wich confirmed him the more in this opinion was that when he came to the very top of the *Pike*, he found the earth under him ſo very moyſt, that it was like mud or morter, and might be made into Paſte as by experiment he found which he conjectures could no ways be cauſed by the wind or clear Air, which is rather drying and conſuming of moiſture, but muſt proceed from the fogs or miſts which are above the very top of the *Pike*.

He further took notice in the Cave that upon the ſides and top thereof there grew a ſnow-white furring like Saltpeter, which had a kind of ſaltiſh taſte, ſome of which he gathered and brought back with him to *England* to have it examined.

After about half an hours ſtay in the Cave, which they found warmer than without in the open Air, they were all pulled up again, and proceeded forward in their Journey by continuing to clamber up the ſtony way, which laſted till they came to the foot of that part of the Mountain which is called the Sugar-loaf, by reaſon that at a diſtance from the Iſland it appears of that ſhape, as it doth alſo even when you are at it. The diſtance of this place from the Cave they judged to be about half a mile, but the way much more ſteep and aſcending than the former part of the ſtony way, and extreme troubleſom to paſs, their feet ſinking and ſlipping down again almoſt as much as they could ſtride upwards, ſo that they concluded it the moſt painful of all; however, perſiſting in their endeavours, after many times reſting themſelves, they gained

gained the top, which they conceive might be about half a mile higher.

The very top they found not plain, but very Rocky and uneven, and in the middle thereof a deep hole; the outfide of this top this Gentleman conceived might be about a quarter of a mile round about on the outfide.

This hole he conceived to be the mouth of a *Vulcano* which hath formerly been in that place, for even at that time whilft they were there much fmoak afcended out of feveral holes and chinks of the Rocks, and the earth in divers parts was ftill fo very hot as to be very offenfive to their feet through their fhooes, and he obferved Brimftone thrown up in feveral places, of which he collected fome, and brought back with him to *England*.

From this place may be feen in a clear day all the fix adjacent Iflands, but the weather being then fomewhat thick and hazy, they could difcover none but the grand *Canaries*, *Palm*, and the *Gomera*, which laft, though diftant near eight Leagues from the bottom of the *Pike* feemed yet fo near unto them as if it had been almoft under them. The reft of the Iflands they could difcover whereabout they lay by means of a kind of white cloud hanging on them, but they could not difcern the Iflands through thofe clouds.

Here they tried their Cordial Waters which they carried in their Pockets, but found them not to abate of their ufual ftrength, and become cold and infipid as fair water, as feveral had pofitively averred to him that they had found it, but he conceived them to be very much of the fame nature and ftrength that they were of before they were carried up, which he fuppofes to be by reafon of their arriving at the top fo late.

After they had ftayed on the top about an hour, and fatisfied themfelves in obferving fuch things as they were able, they defcended again with very much facility,

facility, and came to the *Stancia* about eleven of the clock, where they dined, and thence about one in the Afternoon ſet forwards for the *Villa*, where they arrived that afternoon about five that Evening.

After their return they found their faces (by reaſon of the heat of the Sun, and the parching ſubtil wind) to caſt their skins.

He did not meaſure the Perpendicular height of the Hill himſelf, but ſays that he hath been informed by divers skilful Seamen, (who by their beſt obſervation have taken the height of it) that it is between three and four miles perpendicularly above the Sea.

I*N this Relation it is very remarkable :*

First, that this prodigious high Hill is the Product of an Earthquake, and ſeems heretofore to have been a *Vulcano*, or burning Mountain, like thoſe of *Ætna*, *Veſuvius*, *Hecla*, &c. though at preſent it hath only fire enough left to ſend forth ſome few ſulphureous fumes, and to make the earth of the *Caldera* or hollow pit at the very top thereof in ſome places almoſt hot enough to burn their ſhooes that paſs over it. And poſſibly in ſucceeding Ages even this little fire may be quite extinct, and then no other ſign thereof may be left but a prodigiouſly high Rock or ſpiring Mountain, which in tract of time may by degrees waſte and be diminiſhed into a Hill of a more moderate height.

Now as this Hill ſeems very evidently to be the effect of an Earthquake, ſo I am apt to believe that moſt, if not all, other Hills of the world whatever may have been the ſame way generated. Nay, not only all the Hills, but alſo the Land which appears above the face of the waters. And for this I could produce very many Hiſtories and Arguments that would make it ſeem very probable, but that I reſerve them in the Lectures which I read of this ſubject in *Greſham* Colledge in the years 1664, and 1665, which when I can have time to peruſe I may publiſh.

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Therein I made it probable that moſt Iſlands have been thrown up by ſome ſubterraneous Eruptions. Such is the Iſland of *Aſcenſion*, the *Moluccas*, &c.

Secondly, that moſt part of the Surface of the Earth hath been ſince the Creation changed in its poſition and height in reſpect of the Sea, to wit, many parts which are now dry Land, and lie above the Sea, have been in former Ages covered with it; and that many parts, which are now covered with the Sea, were in former times dry Land. Mountains have been ſunk into Plains, and Plains have been raiſed into Mountains.

Of theſe by obſervations I have given inſtances, and ſhewed that divers parts of *England* have in former times been covered with the Sea, there being found at this day in the moſt Inland parts thereof ſufficient evidences to prove it, to wit, Shells of divers ſorts of Fiſhes, many of which yet remain of the animal ſubſtance, though others be found petrified and converted into ſtone. Some of theſe are found raiſed to the tops of the higheſt Mountains, others ſunk into the bottoms of the deepeſt Mines and Wells, nay, in the very bowels of the Mountains and Quarries of Stone. I have added alſo divers other inſtances to prove the ſame thing of other parts of *Europe*, and have manifeſted, not only that the lower and plainer parts thereof have been under the Sea, but that even the higheſt *Alpine* and *Pyrenean* Mountains have run the ſame fate. Many Inſtances of the like nature I have alſo met with in Relations and obſervations made in the *Eaſt* as well as in the *Weſt Indies*,

Of all which ſtrange occurrences I can conceive no cauſe more probable than Earthquakes and ſubterraneous Eruptions which Hiſtories do ſufficiently aſſure us have changed Sea into Land, and Land into Sea, Vales into Mountains ſometimes, into Lakes and Abyſſes at other times; and the contrary— unleſs we may be allowed to ſuppoſe that the water or fluid

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part of the earth which covered the whole at firſt, and afterward the greateſt part thereof, might in many Ages and long proceſs of time be waſted, by being firſt raiſed into the Atmoſphere in vapours, and thence by the diurnal, but principally by the annual motion thereof be loſt into the *æther*, or medium through which it paſſes, ſomewhat like that waſting which I have obſerved to be in Comets, and have noted it in my *Cometa :* Or unleſs we may be allowed to ſuppoſe that this fluid part is waſted by the petrifaction and fixation of ſuch parts of it as have fallen on the Land and Hills, and never returned to fill up the meaſure of the Sea, out of which it was exhaled, for which very much may be ſaid to make it probable that the water of the earth is this way daily diminiſhed.

Or unleſs (ſince we are aſcertained by obſervations that the direction of the Axis of the earth is changed, and grown nearer the Polar Star than formerly; that the Magnetiſm or Magnetical Poles are varied, and do daily move from the places where they lately were, and that there are other great and noted changes effected in the earth) we may be allowed to conceive that the Central point of the attractive or gravitating power of the earth hath in long proceſs of time been changed and removed alſo farther from us towards our Antipodes, whence would follow a receſs of the waters from theſe parts of the world to thoſe, and an appearance of many parts above the ſurface of the water in the form of Iſlands, and of other places formerly above the Sea now in the form of Mountains, ſo to continue till by the libration or otherways returning motion thereof it repoſſeſs its former ſeat and place, and overwhelms again all thoſe places which in the interim had been dry and uncovered with the return of the ſame water, ſince nothing in nature is found exempt from the ſtate of change and corruption.

Further,

Further, it is probable that Earthquakes may have been much more frequent in former Ages than they have been in theſe latter, the conſideration of which will poſſibly make this Aſſertion not ſo Paradoxical as at firſt hearing it may ſeem to be; though even theſe latter Ages have not been wholly barren of Inſtances of the being and effects of them, to convince you of which I have hereunto ſubjoyned a Relation and account of one very newly which hapned in the Iſle of *Palma* among the *Canaries*.

Next, the clearneſs of the Air is very remarkable, which made an Iſland which lay eight Leagues off to look as if it were cloſe by. To this purpoſe I have often taken notice of the great difference there is between the Air very near the lower Surface of the Earth, and that which is at a good diſtance from it; That which is very near the earth being generally ſo thick and opacous that bodies cannot at any conſiderable diſtance be ſeen diſtinctly through it: But the farther the eye and object are elevated above this thick Air, the more clear do the objects appear. And I have divers times taken notice that the ſame object ſeen from the top and bottom of a high Tower hath appeared twice as far off when ſeen at the bottom as when ſeen at the top: For the Eye doth very much judge of the diſtance of Objects according as the Denſity of the Air between the Eye and Object doth repreſent them. Hence I have ſeen men look of Gigantick bigneſs in a fog, cauſed by reaſon that the Fog made the Eye judge the Object much farther off than really it was, when at the ſame time the viſible Angle altered not. This great thickneſs of the lower Air is ſufficiently manifeſt in the Cœleſtial bodies, few of the fixt Stars or ſmaller Planets being viſible till they are a conſiderable way raiſed above the *Horizon*.

The third remark about the moiſtneſs of the fogs, and the production of water at that height I have be-

H 2 fore

fore insisted on. Only the almost continual fogs that this Gentleman observed in the Wood they passed is very remarkable for the origine of Springs.

Nor shall I say any thing concerning the vast perpendicular height of the same, but for a close of this present collection I shall add the short account of the Eruption which lately hapned in the *Palma.*

A true Relation of the Vulcanos *which broke out in the Island of the* Palma *Novemb.* 13. 1677.

SAturday the thirteenth of *November* 1677. a quarter of an hour after Sun set hapned a shaking or Earthquake in the Island of St. *Michael de la Palma*, one of the *Canary* Islands, from the lower *Pyrenna*, and within a League of the City unto the Port of *Tassacorte*, which is accounted thirteen Leagues distant along the Coast, but more especially at or about a place called *Fuencaliente*, being seven Leagues from the Town to the Southwards. The trembling of the earth was observed to be more frequent and violent than elsewhere, and so it continued till *Wednesday* the 17. *ditto.* The People thereabouts were much affrighted, for besides the Earthquake there was often heard a thundring noise as in the bowels of the earth on a Plain called the *Canios*, which is before you come to the great descent towards the Sea, where the hot Baths stand, or the holy Fountain; likewise at the ascent from the aforesaid Plain upwards at the great and wearisom Hill, called *Cuesta Cansada*, and until the Mountain of *Goatyards*, and the same day in and about the said places mentioned, the Earth began to open several mouths, the greatest of them upon the said *Goat* Mountain, being distant from the Sea a mile and an half, and from the said opening came forth a very great

great heat and ſmell of Brimſtone; and the ſame day, an hour before Sun-ſet at one of the mouths of the wearifom Hill was a trembling thereabout with more violence than any of the four days before; and a great and black ſmoak came forth with a terrible thundring noiſe, opening a very wide mouth, and throwing out much fire, with melted Rocks and ſtones; and immediately after at another place eighty paces below hapned the like terrible noiſe and ſight, and in leſs than a quarter of an hour after there opened to the quantity of eighteen mouths towards the foot of the ſaid Mountains, and there iſſued out fire, melted Rocks, and other bituminous matter from all the ſaid mouths, and was preſently formed into a great River of fire, which took its courſe over the firſt mentioned Plain, ſlowly going down towards the ſaid holy Fountain; but it pleaſed God, being come within eight ſpaces of the Brink of the ſaid great deſcent, it turned a little on the right ſide, and took its courſe with a very great fall towards the old Port, which is that which was firſt entred by the Spaniards when they took the Iſlands.

Friday the nineteenth at two a clock in the afternoon in the aforeſaid Mountain of Goats, on the other ſide of *Taſſacorte*, there opened another mouth with much ſmoak and ſtones of fire, and ſo cloſed again. But the next day (the twentieth) it began again to ſmoak, and continued with great trembling and noiſe in the bowels of the Earth until *Sunday* the twenty firſt at noon, when with many flaſhings of fire, and a greater thundring noiſe it finiſhed that opening of that monſtrous birth, caſting up into the Air both fire and ſtones, and at night the ſmoak ceaſing, the thundring noiſe, fire and ſtones increaſed, forcing great fiery ſtones ſo high into the Air as we loſt ſight of them, and with ſuch violence ſent them upwards that according to the beſt judgmant they were five times longer in falling down,

H 3 which

which ſtones or Rocks were obſerved to be bigger than a Hogſhead, ; and what was moſt to be admired was, that theſe breaking in the Air, and changing into many ſeveral ſhapes, diſtinctly appearing, yet notwithſtanding did reunite again in falling down.

Munday the twenty ſecond it began again to caſt forth black ſmoak for two hours time, and after to thunder,and throw up fire and ſtones with great violence. *Tueſday* the twenty third at noon it ſmoaked again, and from thence until night there was terrible thundring noiſe, and caſting up of fire and ſtones more fierce than before; and about nine of the clock at night a very great trembling of the earth was felt, and preſently after followed three great ſtones of fire in the form of Globes which were forced about half a League in height, and then like Granadoes broke in the Air with very great noiſe. *Wedneſday* the twenty fourth it was for an hours time very quiet, and after it began with greater force than ever before, by reaſon that ſome of the lower and firſt mouths were partly ſtopt, with which the aforeſaid River of fire ceaſed from running, after it had dammed up the Bay of the old Port, with burnt and melted Rocks and Stones, and other matter wherewith the ſaid River had run, and had forced the Sea backward above a Muſquet ſhot at random,and near twice as much in breadth. It ran into the Sea above ſixty paces. What fell into the Sea went congealing with a great ſmoak, what came after, forced and ran over that which went before, ſo that the ſmoak was very great many paces within the Sea, as far as ſeven fathoms depth, which cauſed many men to imagine that ſome ſuch like *Vulcano* had opened under the Sea in the ſaid ſeven fathoms depth. This night it caſt up ſome ſtones like great fiery Globes as the former.

Thurſday the twenty fifth it proved yet more violent than ever with thundring noiſe and flaſhes of fire,

fire. *Friday* the twenty ſixth, the mouth that was at the foot of the Mountain began again to caſt up as much fire and ſtones as ever, and formed two other Rivers;the one taking its courſe to Leeward of the firſt River leading toward the Rocks called *de los Tacoſos*; and the other took its way to windward of the firſt, directly towards the Bathes or Holy Fountain; and in this entrance the mouth of the Monntain was obſerved to be more quiet, though it caſt up much aſhes like black ſmall ſand. What dammage appears to have been done from its beginning to this day the twenty ſixth of *November*, being of thirteen days continuance, hath been about nine or ten Country Houſes burnt, beſides Out-houſes, and great Ciſterns for water, which are the poor Peoples only Remedy in thoſe parts, and upwards of three hundred Acres of ground are quite ſpoiled, being covered with Rocks, Stones, and other Rubbiſh and Sand; and if, (which God defend) the ſaid *Vulcano* do longer continue, the damage muſt be far greater, eſpecially if any other mouth ſhould break out higher, as it is much feared, by reaſon the earth in ſome places doth open with appearances as at firſt, ſo that all about that circuit of the *Fuencalliente* will be loſt; and for what already hapned, and yet continues with much terrour, beſides the fears of more in other parts thereabout, the Inhabitants do leave their Habitations, and like poor diſtreſſed people ſeek relief at the City, and many leave the Iſland to ſeek their fortunes in the others.

From the twenty ſixth of *November*, that the aforeſaid Relation was ſent for *Teneriff* by the Chamber of this Iſland unto the General, the ſaid *Vulcano* continueth fierce and without ceaſing, rather more than leſs, with a terrible thundring noiſe, caſting up Fire, Stones, Rocks, and black Aſhes, and the three Rivers of Fire ſtill running into the Sea, and hath now dammed up all the Baths and holy Fountain, to the

the great detriment of the Iſland, that yearly received a great benefit thereby, beſides many damages dayly added to the former. Several other mouths have ſince opened in the like dreadful manner near about the ſame place, we ſee the great ſmoak by day, and hear the thunder and noiſe, like the ſhooting off of many Cannons, and by night ſee alſo much of the fire very high in the Air from this City, which is one and twenty miles from it.

We are now at the eleventh of *December*, and fear we ſhall have more to write to you by the next.

Other Letters of the thirtieth of *December* mention, that it then contined much at one as before; and ſince others of the nineteenth of *January* ſay, it is yet as dreadful as ever, and little likelihood of ceaſing; from the thirteenth of *November* that it began to the nineteenth of *January* is about ten Weeks that it hath burnt; and the laſt Letters mention abundance of Aſhes or black Sand forced into the Air, and carried all over the Iſland, falling thick like Rain, and frequently gathered in the City, in the Streets, Houſes, and Gardens, though ſeven Leagues off.

FINIS.

ERRATA.

PAge 10. *line* 15. *read* the other, *viz* the vibrating. *l.* 16. participates. *l.*17.& 18. *r.* Vibration thereof, but all Solids do exclude that menſtruum, or participate not of its motion. *p.* 14, *l.*11. *for* length *r.* number. *l.*12. *r.* occaſions will be. *p.*15. *l.*6. *r.* L M N O. *l.* 12. *r.* have of Elaſticity is. *p.* 18. *l.* 29. *r.* equal to ten. *p.*42.*l.*12. *r.* from *Oratava.* *l.*12. *r.* or South-caſt ſide. *p.*42. *l.*9. for *Prancis* r. *Francis.*

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