lacks one plate, in "Heliocopes" (see p. 131 of the tract) otherwise complete compared with copies in Bull's Bradfearn, most of which lack plates.
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.
Collège de Lecerre:

Une nouvelle méthode pour apprendre l'histoire de l'Antiquité

et l'histoire de la croissance des États.

Prévenue par diverses circonstances

M. de la Morale

Paris, 1750.
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 shown 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 Coelestis 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 Shortening 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 Equation 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. 1677. And a Postscript concerning the Invention of regulating Watches, by Springs applied to their Ballances: together with a Decade of other useful Inventions, part discovered, part described in Anagrams.

IV. L A M-
The Titles of the several TRACTS.

IV. LAMPS, 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 inconveniences 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 preparing liquors, by the by, several Theories and Explications are inferred, 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 Mosses, 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. 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’s Reflections upon those of Cassendus 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. 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. 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. i. cap. 30. Ne 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.
A TREATISE
of the
MOTION
of the
EARTH
Its
Operations
W R O N G T H O U G H T O F ITS
For Twelve
Years
1134
TO THE
TRULY HONORABLE
Sir John Cutler
K N I G H T and B A R O N E T,
My Worthy
P A T R O N.

S I R,

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, A 2 deserves
The Epistle Dedicatory.

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, contains 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 encourage me in the further prosecution of these Inquiries to approve myself.

Noble Sir,

From Gresham College,
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 Physisco-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 College 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 complied, 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 Essays or Attempts only upon several Subjects which have no dependencie or coherence 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 itself as it were, and prompt
To the Reader.

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 choose 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 man's 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 lucky hit 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 embrace 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 oustrip those that are found by Design. No man but hath some lucky hits 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,
To the Reader.

Apologies, as Method and Volumes do necessitate them to; But would rather enrich 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 man's 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 reader 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 conveniences also in this Method of Communication not less considerable than 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 ingeniously 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,
To the Reader.

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 Tears 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 always 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 doth
doth make it seem so absurd, that a man shall as soon persuade
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 al-
ready. For such Persons I cannot suppose that they should under-
stand the cogency of the Reasons here presented, drawn from
the following observations of Parallaxes; 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 prin-
ciples of Astronomy, there being already Introductions enough
for that purpose: But rather to furnish the Learned with an ex-
perimentum crucis to determine between the Tychonic and Coperni-
can Hypotheses. That which hath hitherto continued the dispute
hath been the plausibleness of some Arguments allledged 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 ex-
ceeding minuteness of the Globe of the Earth in comparison there-
with, 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 Cheddar 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 than grant that there may be Anti-
podes, believing it absolutely impossible, since they must neces-
sarily fall down into the Abyss below them: For how can they go
with their feet towards ours, and their heads downwards, with-
out 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 uni-
formity and harmony of the Celestial bodies and motions, should
be an Argument prevalent to persuade that the Earth moves a-
bout the Sun: Whereas that Hypothesis which shews how to
falve
Whence others, by reft 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 fiad, 'Tis not only those but great Geometricians, Astronomers and Philosophers have also adhered to that fide, yet generally the reafon is the very fame. For moft of those, when young, have been imbued with principles as gross and rude as thofe of the Vulgar, efcially as to the frame and fabrick of the World, which leave to deep an impression upon the fancy, that they are not without great pain and trouble obliterated: Others, as a further confirmation in their childifh opinion, have been instructed in the Ptolemaick or Tichonick Syftem, and by the Authority of their Tutors, over-awed into a belief, if not a veneration thereof: Whence for the moft part fuch persons will not indure to hear Arguments againft it, and if they do, 'tis only to find Anfwers to confute them.

On the other fide, fome out of a contradicting nature to their Tutors; others, by as great a prejudice of infitution; and fome few others upon better reafoned 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 f tand f till.

I confefs there is somewhat of reafon on both fides, but there is also fonething of prejudice even on that fide that f eems the moft rational. For by way of objection, what way of demonstration have we that the frame and conftitution of the World is fo harmonious according to our notion of its harmony, as we f uppose? Is there not a poftibility that the things may be otherwife? nay, is there not fomething of probability? may not the Sun move as Ticho f upposes, and the Planets make their Revolutions about it whilst the Earth stands f till, and by its magnetifm attracts the Sun, and fo keeps him moving about it, whilst at the fame time 0 and 8 move about the Sun, after the fame manner as b and 4 move about the Sun whilst the Satellites move about them? especially fince 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 U-niverse more conderable then the Earth we tread on. Is there not much reafon for the Hypothesis of Ticho at leaft, when he with all the accuratenefs that he arrived to with his vaf Instrument.
ments, or Riccioli, who pretends much to out-slip 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 acquies in their determination, since if we examine more nicely into the observations made by them, together with their Instruments and ways 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 alleged 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 undeniable Argument of the truth of the Copernican System; 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 always 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 thousand; 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 it was in vain to endeavour to answer that objection. For it is unreasonable to expect that the fancies of most men should be so far restrained beyond their narrow dimensions, as to make them believe the extent of the Universe so immensely 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 persuasion, the almost infinite extension of the Universe had not to me seemed 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 Ptolemy, 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 brightness 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; forty 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 always 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 always look upon it as an inquiry well worth examining; first, Whether the ways they had already attempted were not subject and liable to great errors and uncertainties; and secondly, Whether there might not be some other ways found out which should be free from all the exceptions the former were incum-bred 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 were any or no Parallax of this annual Orb; especially if it were less than 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 ways of observations were liable 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 moist 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 liable 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 than 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 largenesssoever) was capable of Divisors; whereas 'tis hardly possible for any unarm'd 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 coalesce 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 inducement 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 re-
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 ways how it might be reduced into practice, it was at length judged possible, and desirable to be tried. 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 feeling or swelling, &c.) I was destitute of such a convenience near my habitation; besides, had I had my wish, I found that 'twas liable 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 Antiopeianans 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 fame 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 ease 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 considerab, and in the later intolerable.

Having therefore examined the ways and Instruments for all manner of Astronomical observations hitherto made use of, and considered of the inconveniences 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 inconveniences 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 College, 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 fixed Stars; this must necessarily happen, especially to those fixed Stars which were nearest the Pole of the Ecliptick. And that this is so, any one may plainly perceive if he 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 first Stars of the first magnitude, whose center for demonstration sake we will suppose the Sun. Let $\gamma = \gamma$ represent the Orb in which the Earth is supposed to move about the Sun, obliquely projected on the Paper. Let $\Psi$ represent the Earth in *Capricorn*, and $\Omega$ the Earth in *Cancer*, let $\Psi = \Psi$. 12. represent the imaginary Axis of the Earth, keeping continually a parallelism to its self, and let $\Psi A E C D = \Omega$ 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 $\Psi$ and $\Omega$, 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 Ecliptick, and the Zenith points of the Earth at noon, when in *Cancer*, and at midnight, when in *Capricorn*, are all in the same Plane; and that the Axis of the Earth keeps always its parallelism, and that the Angles made by the Perpendiculars of *Gresham Colledge*, with the Axes are always 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 aforesaid Orb $A$ and $C$ as far distant from each other as the parallel Lines $A \Psi$ and $C \Omega$ 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 aforesaid, 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 $A \Psi D$, and the Angle $C \Omega D$, will give the parallactic Angle $\Psi D \Omega$ 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 aforesaid, 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 paffeth 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 Iceland, where the Zenith of the place at the times abovefaid, muft consequently pass at one time to the North fide of the Pole of the Ecliptick, and at the other on the South fide, and the Zenith of March and Sept. muft pass through the very Pole-point it felf. Now it falling out fo, that there is no considerable Star in that part of the Heavens nearer the above faid Plain, and nearer the Zenith point of Grefham 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 defigned to make this observation, finding the Zenith point of Grefham Colledge to pass within some very few minutes of the Star it felf; 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 ascension thereof being according to Riccioli 267°. 7'. 24".

And that this may be made a little plainer, let us fuppofe 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 defcribed by the Zenith of Grefham Colledge among the fixt Stars in June, and b d d d a like Circle defcribed by the faid Zenith in December, and e f f f a like Circle defcribed as above in March, and g h h h in September. It is very evident that the true diftances 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 diftances of the faid Zeniths in that part which is furthest from the faid 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 Grefham Colledge, to the Angle of Parallax in Iceland, 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 on the Radi-
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 sigus 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 Benenain, or the ultima cauda urse majoris, it being a Star of the second magnitude, and having almost as much declination as Gresham Colledge hath latitude. The principal days of doing which will be about the 4 of April, when our Zenith passes by the said Star at midnight, and the 7 of October, when it passes by it at noon or mid-day: the reason of all which will be sufficiently manifest to any one that shall well consider the preceding 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 itself much more easy to be seen in the day time. This will be very easy 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 a b c d, 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 easy to any one that shall consider that hereby, first, I avoid that grand inconvenience wherein all ancient and modern observations have been perplexed, 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 than 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 itself, 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 arises 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 eddies 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 perplexed, 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 Rays 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, or in the night or the day, the ray continually paffeth directly, and is not at all refracted and deflected from its freight 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 than 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 ways or other lyable. And notwithstanding the vast care and expense 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 joining 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 enabled 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 Glafs, and that there were some short cell to carry the Object Glafs in at the top, so as to keep it steady, when raised upward or let downwards, the light in the intermediate Rooms nor at all hindring, but rather proving of good use to this purpose for seeing the Menfurator,) 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, houfed so as to throw off the rain, and so contrived, as I could easily open or shut it by a small string no 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, fixt so as to slide upwards and downwards, as there was occasion, and by the help of a skew to be fixt in any place that was necessary: Within this Tube in a convenient cell c, was fixt the Object Glafs 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 Glafs itself was fixt into a cell or frame of Bras, so exactly fitted to it, that it went in stiff; and to fill up all the Interstitialia's, there was melted in hard Cement; this cell had a small
finall barr that cross'd under the center of the Glafs, or the aperture thereof; in which barr were drill'd two finall holes at equal distance from the middle of the Glafs, through which the upper ends of the two perpendiculars d d were fastned; and in the fixing this bra's 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 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 'fo perpendiculary 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 Eyeglafs 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 Glafs 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 Glafs f, so as that I could through the eye Glafs moved to and fro, see any part of the hole in the Table that I desired, without stirring the Stool from its fixtness. 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 Glafs, 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 surpris'd
vided by the sudden approach and swift motion of the said Star. The measuring Instrument or Menfurator was a round thin plate or circle of Brats, 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 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 part 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 croft by a rule of about 3 ½ 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 Glafs, 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 easy 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-glafs placed in the cell, and so magnifying the Objects at the Menfurator more than sixteen times, 'tis easy enough to distinguish it. But to proceed, I had one small arm m in the Menfurator, to which the Diagonal thread was fastned at the point n, which served for the more nice subdivisions into Seconds; The other Diagonal thread 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 threads 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
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 fixed in a hole, and move round in it.

This was the Menisurator 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 fixed Stars; for measuring the distances of the Satellites of Jupiter and Saturn from their disks, 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 Pulleys, 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 fixin 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 Glasses of it. Now in the time of observation the frame q r being fastened 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 \(\frac{494}{100}\) of an inch, and if o p be the subtense of 10 minutes, then \(x y\) will be the subtense of \(494\); this is so plain, simple, and easy, 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 hairs 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 Brass, such as are described in the Seventh Figure by z z, z z, which had small holes at the extremities, 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 zz, z z, and removed the plumb lines, then I laid the Mensurator 1 1 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, with that I were, near some great and solid Tower, or some great Rock or deep well, that f o 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 months. This variation also will be much more easy 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 foots: 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 Banfield 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 recom pense 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 Transfits of the bright Star in the head of Draco; and always 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 always 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 Meridien Northwards
Northwards of the Zenith point of the Menfurator, 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 Menfurator, 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, hindered 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 than I was able to make these, having since found several inconveniences in my Instruments, which I have now regulated.

Whether this Zenith so found out upon the Menfurator, 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 Glasses before I could compleat the observation) for whether it were, or were not, it is certain that it always had the same position to the true Zenith, the Object Glasses and Perpendiculars having not been in all that time removed out of the Cell, whence if the said Object Glasses were thicker upon one side then upon the other (which is very common and very seldom otherwise) and consequently deflected the ray towards the thicker side, and so made the Perpendicular of the Menfurator to lye on that side of the true Perpendicular, that the thicker side of the Object Glasses respected, yet it being always 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 Telecopical 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 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 flood North. Then I change the side of the Object Glass, and put the North side Southwards, and the South, Northwards, and observe the Tranfixus 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 easy to deduce what the false refraction of the Object Glass is, and which way it lies, 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 shinning very clear into the Room where I lay to observe, and having nothing to screen off the rays 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 clear 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 otherwife. 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 mere 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, for it affects the eye in and near the Axis of vision through
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 Glafs 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 clearly shaved away, and the naked body thereof left a very small white point.

The smallnes of this body thus discovered does very fully answer a grand objection alleged 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 vaftly bigger then the Sun, thereby hoping to make it seem so improbable, as to be rejected by all partes. 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 proving less; for the Diameter of the paralladical 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 Glafs 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 possibly 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 paralladical 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 when 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, than has hitherto been attained by the best Astronomers. The like use there may be made of it for observing any notable appulse of the α, υ, ι, θ, and ρ, 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 ways 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 itself, which perhaps were not dreamt of before, which I shall hereafter more at large describe, when further trya's 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, answerling in all things to the common Rules of Mechanical Motions: This depends upon three Suppositions. First, That all Coelestial Bodies whatsoever, have an attraction or gravitating
The attracting 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 Coelestial 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 they also, the Sun and Moon, 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 direct 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 Coelestial 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 myself 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. Paul's Church-yard, 1674.
A Philosophical History

of

Natural

Religion

Part

Second

An Essay Towards the

Establishment

of

Religion

in a

Manner

likely

to

secure

the

Public

Peace

and

Parallel

of

Our

Protestant

Religions

and

Antient

Pagan

Religions

With

Some

Inquiries

Concerning

the

Facts

and

Reasons

of

the

Christian

Religion

Vol.

I

London

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Gill

for

Thomas

Bewick

and

Charles

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1794
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SOME ANIMADVERSIONS

On the first Part of

HEVELIUS

His MACHINA COELESTIS, &c.

HAVING lately perused a Discourse of Hevelius, newly published, entituled, Johannis Hevelii Machina Coelestis, pars prior Organographiam ficte instrumentorum Astronomicorum omnium quibus Autor hactenus siderarimatus & dimensis est accuratam delineationem & descriptionem plurimis leonibus cui incisis illustratam & exornatam exhibens, &c. and finding it a Discourse about practical and mechanical Knowledge, and of that kind wherein Geometry seems to be more than 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 Physical) to consider a little the Contents thereof: And somewhat the rather too, because having heretofore communicated to him somewhat of this Subject, which I had occasion to read in this place in one of my former CUTLERIAN Lectures, I find he hath made some Animadversions and reflections thereupon.
I find then that this excellent Person hath been, for the most part exceedingly circumspect, to find out the inconveniences and difficulties that do accrue 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 vigilance, 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 $\frac{1}{3}$ 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 sonoia a secunda subdivisionem in omnibus meis machinis Astronomicis usurpo, eo quod illam multis ab hinc annis exquisitissimam expertus sum. Licet enim ejus demonstratio in Reftilines parallelogrammis propriè conveniat, nihilominus arcualibus etiam in tam exili intersec- tio quod a 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 than 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 Nonnii, 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 Division ad clarissimi Mathematici Petri Nonnii in Libello de crepusculis propositione tertia imitationem per plures quadrantis arcus introsum descriptos, & diversis modo subdivisionis procedit; et autem in hac ipsa apprime ingeniøsa Nonnii inventione aliquid autem loco expeditum est, ita ut exterior arcus in plurimas partiumculas dividatur, nec: is ordo aut numero arcuum se introsum concomitantium quem ille præsumit

sed

A 2
sed multo expeditior & perfection observètur; (I am apt to think he knew this very way, and here hints it:) Tamen quia hac subtilitas sum ad praxin devèntum est plus habeat laboris quam finis, negocid in recessu pretet quod prima fronte pollicetur, ut alibi plenus ostendemus, idcirco quod nos sudum in usu esse desit. 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 cunctus, velit ingeniosa certe & apprime utilis est distributo, quam & ego postea arcualibus graduum divisionibus in quadrantibus sextantibus & armillis, non inconcinnè aut infragiferè applicui. Licet enim demonstratio ejus in solis rectilineis superficiebus ad unguem se habeat; tamen cum quinorum vel denorum minutorum spatium in circumferentiiis 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 would be too long now to quote, it is 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 circumseption 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 significance. 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 meantime 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 metal 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, 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.
circum restitutionem Fixarum Planetarum; adminiculo esse possint; in majoribus scilicet illis distantiae capiendis: In minoribus, largior, posse aliquid praestari; sed an Instrumenta, unus Spithame radio instructa, elaborari possint multo exactius, quam optimaeque, vulgares Dioptras habentia, licet 60 pedum radio elaborata, nollem adhuc asseverare. Multa namque in Theorid videntur certissima, quae in praxi satis longe nonnumquam a vero recedunt. Si quis mihi certas observationes quasdam nonnullas distantiae & quidem Fixarum, circa Eclipticam & Equatorem existentium, illis ipsis Instrumentis, Dioptris Telescopicis instrutis habitas exhiberet: (utpote distantiam Lucidae à Palilicio; Palilicii à Polluce; Pollucis à Regulo; Reguli à Spicâ; Spicâ à Boreali; sinist. manus Serpentarii; Boreali: sinist. manus Serpentarii ab Aquilâ; Aquilâ à Marcab; & Marcab à Lucidi Arietis) vellem proptenus de rei illius certitudine & meum quale quale judicium ferre; sed antequam eas observationes obtineam, judicium suspendo. Interea utique fatero; si quis adminiculo minoris cujusdam Instrumenti observationes corporum Coelestium peragere potest, multo sane illum esse feliciorem, variis de causis, eo, qui per majora id praebere alborat. Rationes dividendi Instrumenta, diversissimae, quidem mihi probè cognitæ sunt; cægo; etiam in usum translation; nam autem sint eadem quas Clarissimus Dominus Hookius novit, ac inventit, me prorsus latet: Si illi non adversum est, rogo, ut præcipias communicet, ego ut meas intelligat rursus studo.

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 Telecopical 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 Telecopical 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 Telecopical 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 Telecopical 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 than 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 an observation more accurate, be the instrument never so large.

Now that any one may presently satisfy 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 OO, and PP, 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 thinks convenient, as aa, bb, cc, dd, ee, ff, gg, hh, ii, &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 aa and bb, between cc and dd, between ee and ff, between gg and hh, between ii and kk, between ll and mm, &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 287½ 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 curi-
osity is not only useless, but of much detriment upon many
accounts.

This is that Consideration which I could wish had occurred
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 solicitous about as-
fisting 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 Ob-
servations 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 pur-
pose, 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 ap-
proaching near that Sight, 'tis possible by Instruments large
enough, to arrive at the accurateness of a Second, in Observa-
tions 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 Tele-
copes, ceteris paribus, we shall find it much short, both as to
clearness and distinctness, and therefore even here also Tele-
copical Sights are to be preferred, as I shall sufficiently mani-
fest 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 In-
strument,
instrument, 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 observer, 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 azimuthe’s, 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 azimuth 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 premis’d 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 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 Brafs Quadrant was of three foot Radius, and so well fitted with cross Bars, and strengthened, that it was not subject to warp or bend; it had also a convenient Pedestal, and was made ease to be removed from place to place; it was suspen-
ed 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 Glass, 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-Glafs 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 consequen-
tly 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-Glafs. And therefore I do wonder at his care-
fulness to inform his Reader aright, for fear he should under-
stand a Telescope by the Tube he made use of, to keep off the adventitious light from the lower Sight, saying, pag. 99. Per Tibum 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 Glass in any of his Sights. Whether it were, that he supposed Glass to have some hid-
den, un-intelligible, and mysterious way of representing the Object, or whether from their fragility, or from their uncen-
tain refraction, or from a supposed impossibility of fixing them to the Sights, or whether from some other mysterious caufe, which I am not able to think of or imagine, I cannot
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 easy 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 distinguishing, 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 distinguishing 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 which
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 wished 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 9⁄10 of the trouble is saved, in performing the manual operation thereof, so I judge it to be much more certain, exact and plain, than 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 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 under-
fstood by the Delineation thereof, to which I shall therefore
refer the Reader. The Distances of the parallel Circles if un-
equal, may be easily set down true, according to the numbers
of natural Tangents or Secants, with a pair of Compasses, con-
trived like Beam-Compass, 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 else-
where 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 easy, 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 Qua-
drantal Dividing Plate of ten foot Radius, I divide the said
Quadrant in the faint Circle above-mentioned, 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 cu-
rious, small, round and deep holes, these are by another Line
without it, which is divided and figured the Common way,
distinguished and numbered by Figures, according to the Com-
mon 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 curi-
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-faft 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 numbering towards i. Let d d represent the moveable Arm, c c the hold-faft, fixed upon the side of that Arm, which by a small Screw pincheth and holds faft 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 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 th 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 Azimuth, which is a very great one, and which by another way altogether as easy and plain, may be avoided; of which more hereafter.

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

The second Instrument, 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 than ordinary. His way of moving and fixing the Rule of it is convenient enough, and the same with his Instrument for moving and fixing his Quadrant, but 'tis not capable of performing what he promiseth for it.

The third Instrument, which in the fourth Chapter he describes, is a Sextant of Iron, of four foot Radius, to be managed only by one Observer, by putting the Center next the eye. The whole Instrument 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 volatile 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 Observer 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 fifth 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 Bras, 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 Equilibrium, 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 Bras.

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 Bras of three foot; nor has it any other contrivance about it consideralbe, 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 part
part of Wood to be subject to failure, 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 *ceteris 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 Joynets and Plates for Divisions only being made of Metal, they being very easy 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 Joynet 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 Bras 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 inavour the obtaining of Instruments of Brass and Iron. *Nam* (says he pag. 126.) *cum longá experiens* proba tandem didicerim, *multo securior esse ex solidó prorūs metádo obteniocr Instrumema*, *tum quo majora & ampliora eo esse accuratiora & absolutora, adhaec prioribus admodum Tichonicum constructis plurima deesse quibus ditari merito debenter*, & *quod issem de causis omnino necessum sit, ut parte corrigenterur & meliorentur, tam quæ eorum materiam frustram commotionem facilitandam divisionem quam alia*
alia diversa subsidia \& adminicula, quos sicut aptius, exquisitius, promptius, minorique labore, \&c. ac temporis dispendingi possent Abris exponi observationisque peragi. Idcirco omnes curam atque operam pro tenui ingenii met facultatiumque meorum moduló à Deo concessó (religia sublimioribus ingenii atque ampliori fortunâ Viris, \&c posita, ita nos tribuens) adhibui: quo minora, tam lignea universalis ab Abris plané removerem, atque in ejus locum ex purissimo solidoque metallo, organa mihi compararem: \& quidem ejusmodi, que insigni amplitudine essent conspicua, simul commoditate regendi, simul aliquanto accuratioribus abhis divisionibus, ad paulo subtiores observationes obtinendas gaudeant. His Reafoingindeed 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 easily they were to be moved, and the more steady 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 Metall 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 bountifull of his own care, labour, and diligence; but I could have wish'd heartily that it had been some other way impoy'd. Those Instruments which he chiefly laboured to perfect, he professe to be Quadrants, Sextants, and Octants, after Ticho's manner, rejecting all other Instruments of whatsoever Figures, whether Radii, Astrolabs, Zodiacal or Aequinoctial Rings, Parallactical Instruments or Hoops, as more troublesome, and less accurate. But whether he hath in this his choice
choice been rightly advised, I shall hereafter have more occasion 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 Coelestial 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 fayes, 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 furnish'd. 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 fayes 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 praestantissima inventa meritissimo repperatur, quod etiam minora Instrumenta remotis omnibus transversalibus Lineis, in singularum minuta exunicque particularis minimas subdividé licet. He seems indeed both here and elsewhere in many other places of his Book to be highly possess with admiration of the sublimity, subtilty, and extreme usefulness of this invention, and seems very much concern'd that the Author thereof should not certainly be known, but dares not publish it upon any one positively. He fayes that one Benedictus Hederus in a Work of his which he published Anno 1643. about the new and accurate Structure of the Geometrical Astro'ab, 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
I confess I understand not their meaning nor reasoning, nor why it should be less demonstrable in lesser than 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 always a piece of a Tangent Line, that is to say, the spaces between the Parallel Circles upon the Diagonals are always to be in proportion to the difference of some Tangent Lines, and the different distance of those Circles from the Center are always 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, incapable 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 ex-primit figura in qua singula denominata per Lincolas in decem interstitia aequalia discriminatam punctis notata sunt, sicquae regula fiduciae quodcunque horum inter observandum transiens ipsum minimam gradus, quod quarabatur promit aut aliquotam ejus partem, prout ab hoc vel illo puncto removeri discernitur. Ut vero hoc etiam demonstratum hic addam ob sciosos fortè quosdam qui ea que non satis capiunt carpint sic habe.

In Figura 34. Sit A centrum Instrumenti ejusque Semidiameter A O, assumitur autem O I, Particula in qua diversio ista per lineas transversas sit ea proportione que est ad 48. qualis in meis Instrumentis ut plurimum usurpatur. Cumque A I ponatur partiam 1000000000. integri canonis majoris Rhetici, exit earundem OI 208333333 utpote pars quadragesima octava radii Arcus IE ite 2016 IV. ho horum sinus 29088779 Y I. Sinus autem secundus earundem
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For

instance:

Let B'\(\varepsilon\)C represent a Diagonal Line subtending an angul
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 BE the Tangent of 89°. to the Radius AE. and EC 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 praefertim celestis addiditorum reddam gratias, tum ob immensos in tanto apparatu sumptos erogatos, tam pratiolum conquendo supercelliorem Astronomicam, graphicè hic descripsum, tum ob indecessos labores, insonnes noctes diesque occupatisimos celestis acquirendis observationibus impensos; quorum vim ingentem, Thefærum supra aurum & margaritum pratiolum erudito orbis ante deris, plura daturus indies, verum non est ut sperem me verberis aequalè posse tua merita, qui ex privatopenu sumptos plane Regios ergastis; omnes; suscepiit non infeliciter, Herculeis Humeris (ne Atlantide dicam) formidandum.

Operis partem maximam jam evolvi, miratus inibi tanta inolis Instrumentorum ingeniosissimum regimen, & subtilissimum divisionum administrationem, cum parte diligentia conjunctam in Regulis & Dioptris solicius curandis, & quidem si hoc deesset reliquum in cañum caderet labor; quippe exiguis & vix evitabilis in Regulis aut Dioptris error, totum Instrumentum vitiaret, omnès; in sacrifici obseruationes, sed singulis immorari non licet, unum tamen est quod attingam breviter, nempe divisiones per Linæas Diagonales, circulos in limbo concentricos oblique secantes. Haec dividendi methodum jam diu receptam, ipse retines & quidem merito, circulos; hos concentricos æqualibus intervallis disjunctos habes, quod quamvis in exiguorum aut etiam mediocrium Instrumentorum limbis.
limbis latioribus aliquid erroris potest inducere in tuis tamen tanta amplitudinis instrumentis cum limbis exiguo latitudinis (quod & turcite mones) nihil quicquam erit discriminis quod in sensu occurrere posset. Hactamen occasione libet hic subjicere, quod ea de re jam oleam (circa A. 1650, aut 1651.) meditatus sum, atque apud aversariam meas nunc reperio: nempe si quis vellet minoris instrumenti limbum latium Lineis Diagonalibus sic dividere, quibus intervallis oporteat concentricos illos circulos disponere ut angulos invicem aqueae designarent illae cum transversali intersectiones calculo Trigonometrico determinare.

Divido arcus in limbo quadrantis (aliasque ejusmodi instrumenti) per circulos concentricos & rectam Diagonalem, sit latitudo limbi (RL=) L, Radius circuli intimi (AR=) R, extimi (AZ=AL=) L+R=Z continentes angulum (RAZ=) A. dividendum in partes quotlibet aequales (quarum numerus n) rectis a,b,c, &c. (quarum longitudo quieritur) facientibus ad RZ diagonalem, angulos α, β, γ, δ, &c. adeoque angulos RA a = 1/n A, RA b = 2/n A, RA c = 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 O+V. tangentem 0-V & 0+V+O+V = O. deinde cognitis angulis O & 1/n A (adeoque reliquorum a) cum trajecto lateri R habetur latus a. rempe sina, R:: sinus O. a. & pari modo ex cognitis,

\[\begin{align*}
&O_A^n \\ &O_{1/A}^n \\ &O_{2/A}^n
\end{align*}\]

modo ex cognitiis, \begin{align*}
&b \\ &c \\ &d \\
&\&e \\ &\&f
\end{align*}

Praxis sit R=1, L=0, 2. Z=1, 2. A=io', ergo O+V=179°. 50', \(\frac{0+V}{2}=89°. 55'\). tum Z+R=2, 2. Z-R=0, 2. :: \(\frac{0+V}{2}=687, 5488683.62, 5044427179°. 5'. 17''\) proxime. Ergo \(\frac{0+V+0-V}{2}=c=179°. 0'. 0''\). 

Deinde secundus sit A in Io partes quadrarum velibet sit \(\cdot\) quae-runtur igitur a,b,c,d,e,f,g,h,i, rempe.

\textit{Sin.}
Sin.a(0° 58′ 59″ 43‴) = 0.0171603. R = 1. Sin O = 0.0174511. I.01694 = a. 1694
Sin.β(0° 51′ 59″ 43‴) = 0.0168694. R = 1. Sin O = 0.0174511. I.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 (1.11010 = f. 2019
Sin.η(0° 52′ 59″ 43‴) = 0.0154152. 0.0174511 (1.13026 = 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. 2335
1.00000 = Z.

Praxis altera sit R = 1. L = 0, 1. Z = 1, 1. A = 10°. ergo et = 179, 50. \( \frac{\text{Ov}}{2} = 89° 55′ \). cujus tangens 687, 5488693, 8, 2, 1. 0, 1. 687, 5488693. 32, 7404223\( \frac{1}{2} \) = tang. 18° 15′ 1° 57′\( \frac{1}{4} \). 57″\( \frac{1}{4} \). \( \frac{\text{Ov}}{4} \) ergo \( \frac{\text{Ov}}{4} + \frac{\text{Ov}}{4} = O = 178° 10′ 1° 1° 57′\( \frac{1}{4} \). cujus complementum ad semicirculum 1° 49′ 58″. 2°\( \frac{3}{4} \). cujus sinus 0, 0319827.

ergo

\[ \begin{align*}
\sin a &= 1.43858 \cdot 2.3 = 316920 \cdot 319827 = 0.91816
\sin c &= 1.47502 \cdot 2.3 = 314013 \cdot 319827 = 0.93417
\sin y &= 1.46582 \cdot 2.3 = 311103 \cdot 319827 = 0.95119
\sin x &= 1.45582 \cdot 2.3 = 308198 \cdot 319827 = 0.97019
\sin t &= 1.44582 \cdot 2.3 = 305290 \cdot 319827 = 0.98818
\end{align*} \]

\[ \begin{align*}
302343 & \quad (1.05769 = f \quad 100720
299475 & \quad (1.06796 = g \quad 101721
296567 & \quad (1.07843 = h \quad 106821
293660 & \quad (1.0891 = i \quad 1089
290752 & \quad (1.10000 = k \quad Z
\end{align*} \]

Hactenus adversaria, ubi duos casus expendimus, nempe cum latitudi limbi ponetur pars quinta & pars decima Radii brevioris, angulus dividendus 10 minuta prima tanta fere à ærepsia, quantum feret vulgaris canon Trigonometricus: & quidem ultima unitas in ambiguo est; nunc jufto major nunc jufto minor. Radium autem (ut ego soleo) facio L (non ut plerumq; sit 10000000.) quo omnes multiplicationes & Divisiones per Radium faciendæ præcipitans: Adeo; sinu habeo pro partibus decimalibus, quibus itaq; cum opus est, eiphra praemitto quo de unius integri loco confert.

Simili
Simili processu utendum erit mutatis mutandis si latitude limbi sumatur in aliqua quavis proportione ad Radii longitudinem. Sed commodius erit (ad vitandam molestiam toties quaerendi partem proportionalem) ut sumatur angulus O commode magnitudinis (justis minutis primis determinandae absq; annexis secundis tertiisve) atq; ita quaeratur Radii maximi Z longitudine, eodem modo que 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°, 11, 57°6 sed potius 178. 10°. cuius complementum ad duos Rectos est 115°. hujus sinus in ipso canone habetur 0,0319922 & reliquorum item a, c, y, d, &c. sinus similiiter ibidem habebuntur, ut una tantum divisione opus sit pro singulis exhibendis ipsaque Radii Z. Longitudine habetur non quidem precice ut prius, i, i; sed proxima (qua itaque sumenda erit) 109996 nempe. 

\[
\begin{align*}
\sin a &= (1.49) = 317015 \times 319922 (1.00917 = a) = 917.17 \\
\sin c &= (1.48) = 314108 \times 319922 (1.01851 = c) = 934.18 \\
&c. \\
311200 &= 1.02303 = c \\
308293 &= 1.03772 = d \\
305385 &= 1.04760 = e \\
302478 &= 1.05767 = f \\
299570 &= 1.06794 = g \\
296662 &= 1.07841 = h \\
293755 &= 1.08908 = i \\
289858 &= 1.09996 = k = Z
\end{align*}
\]

Similiiter omnino res succedit si sumptis Radiis RL cum angulo A quaramus V & Radios intermedios, aut sumpto Radio L cum angulis AV quarantur R & Radii intermedii.

Verum si limbi latitutdio fit Radii non nisi pars trigessima quadragesima, quinquagesima aut adhuc minor, atq; angulus dividendum non quidem 10 minuta prima sed totidem secunda, aut minor adhuc; subtilior res est quam ut canone vulgaris Trigonometricus hic adhibatur; & que omnem sensum fugit, ipsiq; circuli concentrici distantis equalibus quantum sensus possumus distinguere invicem dispositi: quippe unus punctus pars millesima uedum decies aut centesimus millesima minor est disrepanonia quam ut sensu percipi posset. Sed nimius sum in re levi felicem mtag; esseunt enim annum ebi complectatus longa sequentium serie contrivandum, valere jubeo.
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 splendidissime tantum speculationes mentisq; ideae quecunque; 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 publifiht 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 ducentur intra extremum quadrantem alii minores numero 44. successivè sese comitantes, quorum extremus 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 Mund 

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; and
and possibly might be the same that Ticho Bracé contrived to compendifie that of Nonius.

The way then is this, described by Hevelius, pag. 141:

Quadrantes contra filiores ita à me sunt adornati, ut limbos eorum tantum in integros & semigradus distinctissimique ut hic distinctio non nemini admodum rudis videatur, sufficit tamen affutus commone & singularis minutis primis; dummodo perpendiculi ex centro appenni extrematis limbus stringens in certis particularis sit subdivisa, imo quod magis de quò non nemo sane mirabitur, non solum haec rudior limbi subdivisio sufficiens exhibendis singulis minutis primis sed etiam pro densis quinis quinetiam singularis secundis in majoribus organis si videlicet nostrum Instrumentum directorium adhibeas. Oportet ut inferior illius pars curiosissime & levissime sit levata & levigata, ut limbum totum equabilissime quidem tangat. Sed nulliibimus adhaerat; tum quovis loco liberrime pendat 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 quam libet triumtem in decem diversas; atq; ita obtinebis spatiola paulo admodum ampliora quam spatiola unius semigradus, quia intercapito 31 partium in 30 transmutata necessario sunt modice ampliores. Attamen si divisiones perpendiculi ad limbum quadrantis accedant circa extremitates perpendiculi, discrepantia illa divisionum ab invicem vis ac ne vis cognoscitur. circa medietatem vero perpendiculi satis evidenter. In medio limbo perpendiculi & divisionum parvulus index & quidem inter 15 & 16 spatium constitutur pro discernendis integris & semigradibus; quos accurate dictus index indicat, quando totum spatium perpendiculi in 30 partibus division in ipso limbo quadrantis spatium 31 partium exquisitse subtendit. Ea tamen expressa lege sit totum Instrumentum absolute ab omni parte sit constructum; quando vero iste index paxillum promotior ex sit intregul quo semigradum certissimum est indicium, observat bonum quidem adherere aut integro aut semigradui adnumeranda, si index huic vel ulti vicinior est. Cognoscitur autem minutorum numerus ex eo, quando lineola aliqua divisionum in perpendiculo cum una aliqua in limbo quadrantis procul in unam cændem coactit reddat. Nunquam enim nisi, unica lineola in perpendiculo cum altera in quadrante, si exquisiti peracta sunt.
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 Hedrus, 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 & Chafelein pour sa Majesté au Chasteau Dornans, Conseiller, & General de ses Monnoies au Conté de Bourgongne, printed at Brussels, by Françoys Vivien, 1631. (to wit 12 years before Hedreus) a Treatise in French, which he calls, La construction l'Usage & les Propriétés 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 desdits tables en une petite demi page avec son usage : & finalement la méthode de trouver les angles d'un triangle par la connaissance des costez & les costes par les angles sans l'aide 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 Bras-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 always to be kept very clean from Durt, for if a little more Durt 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 Bras 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 subtletness
fubtlenefs, but of little or no use for Astronomy, to which Heve-
lius 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 Subdi-
visions of a Degree, as Minute, as are necessary to common Sights.

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 in-
deed 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 ease. But 'tis not always 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 Qua-
drant of Brass adjusted feas 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 Essentiel part of this Instru-
ment, he takes occasion to give the description of the Turret
or Observatory which he built for it, and the several contri-
vances 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 princi-
pal use for the regulating the motion of the Sun: Such as the Solstitial and Equinoctial Altitudes, of which I hope we may expect an account in the second and third Part of his _Machina Celestis._ 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 ways 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 jointed and cramp’d together, and setted on a foundation very firm and solid, to prevent all manner of flaking and swarving. Let the rim of this be made of Brass Plates, stay’d 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 polish’d Plate of Glass, according to the way I before describ’d 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 to 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 significance 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 poyled 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 itself, 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 any
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 greateft and left 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 Registered among those glorious Ce-
lestial 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 Alphonfus, 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 Ho-
rizontal 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 fix 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 di-
ligence, 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 our many contrivances, in or-
to the making it convenient to make Observations, and he hath
not spared for cost, pains, study and industry, for the com-
pleating 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-de-
scribed, wholly impossible. For firft, a Degree upon the
Limb is but about \(\frac{1}{4}\) of an inch, and consequently a Minute is
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 Penumbra, 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 asser'ted 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 suere, ut nesciam. à quibus primum inchoare debeat. Imo

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etiam vel maxime velim, nullo tamen modo omnia & singula adeo perspicue vel delineare vel describere potero, ut universi præprimis qui similia haud ipsi met oculis usurparunt quevis recte ac plane intelligant, quinetiam credas velim ut ut alis sunt attentiores arq; hujus rei bene gnarus, aliquoties sane hocce Instrumentum visuros antequam dimidiam tantam partem debite animadventant ac ple-nissime comprehendant. Quippe & verum factur nec ipse ego, licet singula ex meo solo cerebro proderint ac conscientia fuerint, possem adeo distincte tibi eum sub aspectum ponere nisi mihi hocce organum sub oculis affindeque versaretur. Nibilotamen minus dabo operam, ut quantum fieri poterit, dilucide omnia proponam, reliqua veri exercitatis cali metaloribus ulteriorius rimanda & perquirenda committam, &c.

And so he proceeds with the Description of this Quadrant, and the Apparatus about it, and firft, 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 in 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 firft, 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 employ'd to manage these Screws, I judge them too troublesome 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, to 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 easy 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 afferts 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 very glad to have seen the Distances, which he mentions to have taken of eight fixt Stars near the Ecliptick, to wit, Lucide Arielis & Palilicii, Palilicii & Pollucis, Pollucis & Reguli, Reguli & Spica, Spica & in manu Serpentarii, in manu Serpentarii & Aquile, Aquile & Marchab, Marchab & Lucide Arielis, 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 3 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 judge.

I had many other things to have added, which have occur’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:

Caterum percipio vestrat’s non omnes mihi adsipulari
in isto Dioptrarum negotio, de quibus in machine meæ cælestis
Organographia tractavi, verum eiam Cl. Hookius & Cl. Flamsteedius aliiq; plane aliter sentiant, experimentia tamen quotidian
a me edocuit atq; eiamnum docet, rem longe aliter se habere in
magnis illis organis; quadrantibus scilicet sextantibus & octantibus
imprimis quadrantis Azimuthalibus aliiq; quadrantis regulis constructis, qua nempe adeo procliviter commoveri & inver
ti (dum Dioptra Teleoptica examinantur) imo nullo modo possunt,
ut quidem Instrumenta illa trium quatuorve pedum perpendicular
construæ. Rei cum primis in eo consÌstit, quod nullam plane ob
servacionem suscipere possint suis Dioptræ Telescopiciis nist per in
do eas examinam accuratissim; in quo tamen examine variæ
viæ, tum jugiter utrque studiosissime illud suscipiatur hallucinari data
tur. Adhæc in quadrantis Azimuthalibus, octantibus & sextantibus,
qua ratione examen istud adeo accurate nunc quam non
haud magno negotio tempore; dispensio institui posset, prosecto non-
dum capio, vis mihi persuasæ ullibis adhuc ullum alineum magnum
quoddum Instrumentum & vell 9 pedum utpote sext. octant. vel qua
drantem cum regula vel quadrant. Azim. cum pinacidiis Dioptræ
construisce, cunq; ad centrum félíci aliquo successu adhibuisse, &
quicquim solide observasse; si tentasses ac per annos aliquot obser
vationibus
vationibus continuo invigilasset sine dubio alter sentiret. Hoc negotium enim non solum in eo consistit quod stella aliquanto distinctius conspiciantur (quamquam sex ab eo qui visu pollet & exercitatus est aquē bene nudis oculis discernantur) sed in Instrumenta a omnibus parte correcte communent, an pinnacidae Telecopica Instrumentis toties ad quasvis observationes rite imponi & tuto conservari queant; de quibus quidem id omnino tempore aquē præcise fieri posse valete dubito. Quare Clarissimos illos viros humanissime rogatos volo nisi jam possideant ejusmodi vastissima organa utpote sect. oitam. & quadrantis. Azim. Dioptris Telecopicus munita, eaq; caelo continuo admoncant, suspendunt judicium paululum, donec longa annorum serie experti fuerint hanc fusisse multoties egregie elusos. Nam ex una alternate observatione quadrantis, aliquo leviori perpendiculari gaudenti obtentā, res hæc non est decidenda, sed si quis per 10 & amplius annos aedias observaverint, tum ab ideo seriam stellarum restitutionem per distantias suæperit, poterit quædam certiora in medium hanc de re proferre. De reliquo satis mirarī nec quo, eas omnes qui ejusmodi Dioptris Telecopici gaudent, nondum locorum suorum, elevationem poli ubi deget & observationes peragunt, quantum sciam recte & omnino præcise determinasse & stabilire. Hucusq; enim ad aliquot minuta integra Parisiis elevationem poli nondum est definita, alii quippe eandem observationem 48. 49. 50. 51. 52. 53. 54. 55. imo ampliorum adhuc statuerunt: sicuti legere est ex disseratione Petri Petiti de latitudine Lutetiae, sed non in his proluxius esse: ad observationes ipsas provoco, tempus aliquando debebit quorum observationes universas accuratiores fuerint, si modo nonnulli consursum sium eosq; rejicere possent. Nam video aliquos inter quos etiam Cl. El. amstedius inventit, prout ex Epistola ad Cassinum appareat, jam judicium de nostris qualibus observationibus tulisse, priusquam illum adhuc viderunt examinantur vel quicquam de ipsis cognoverunt. Nolo quidem vanus esse rerum mearam jactator, nec unquam mihi imaginatus sum rem in omni isto negotio circa silicet restitutionemellarum fixarum aequ omnino tētigisse vel tangere posse. Sed hocce penitus mihi imaginor si totum ita studia negotium Dioptris Telecopici susceπiřem, quod non solum plurimos annos examinibus tuisvissem, sed et sine dubio varia via (de qua hic non est differendi locus) ecei̇ssem. Exinde gratulor mihi me ad eam sentimentiam nondum transscripsi, ac me mea methodo universa persequi se quicquid praebītur.
praebitum Dei beneficio erit: an nihil amplius (ut putat Clarif. Flamstediust) quam haec est, & quousq; progressum suum erit cuiq; cum deinde videre usum exponere quinetiam integrum erit alium novum integrum catalogum superadditis tot ac tot centenis nonis fixis, haec est negethis alia ratione construere: Verum nondum video an cura haec molestissima, radiossima ac laboriosissima, quae non nisi multorum annorum vigilitis fuscipit & peragii potest, aliquem adhuc serio tangat. Unum aut alteram tellam ope Telescopii vel Dioptrarum Telescopiarum, dum precipue ad majores fixas earum, intercapedes supponimus correctas ad debitem locum deducere, tunc nonnunquam distantias nonnullas stellaram capere haec ludicra sunt; sed omnes conjunctim secundum longum & latum restituere, tunc ducit continuo singulis serenis diebus ac noctibus, tam altitudinem solarium quam reliquam stellarem observationibus operam dare, easq; orbi exponere ut pateat motuum harmonia atq; Instrumentorum certitudo, hoc artis hoc laboris est. Quanto observationes 20 vel 30 annorum spatio continuatas ab utraqr; parte aliquando habebimus, nimirum tam que Dioptris Telescopis quam que column modo nostris ex calo deplomtis sunt res omnino clarior erit. Interea quilibet fruatur suo ingenio, ac sua ratione pro libitum rem tentet. Honorificum nobis omnibus erit pro modo nostro à Deo concessó, rei literaria incrementum varia via promovere.

To this Letter of Hevelius I have this to answer, That the Author neither hath, had, nor can have any experience, to shew Telescopical Sights not to be as good as the Common, or that they are less applicable to large Quadrants, Sextants, Octants, or Azimuth Quadrants, or to any other Quadrants furnished with Rules, and so fixt, that they cannot be easily inverted, or turned, then they are to Quadrants or Instruments of 3 or 4 foot Radius. Nor is his Reason against them of any validity, that no Observation can be made, without a repeated previous examination and rectification of the Sights, in which, says he, notwithstanding all the care and diligence, there is a Reason of failure and mistake. For first, I say, There is less need of rectifying the Instruments or Sights, after they have been once adjusted, then of Instruments with Common Sights, all things being perfectly fixt, and so strong.
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 fame, otherwise he would never have propounded such an Objection; and indeed he seems to say as much in the following words, \emph{Qua ratione examen illum omni tempore commode \\ sine magno temporis dispendor instituit 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 preposseft 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 fame opinion he seems now to continue of. And whereas he thinks, that no tryal hath ever been made of Telecopical 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 severall, 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 fame 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 Telecopical Sights, are capable to be made to distinguish minutes, seconds, nay single thirds, if they be proportionally 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 Phæses 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 Telecopical 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 Telecopical 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 highly decried 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 hindered. Nor would I willingly be thought to depreciate 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 useful to
Astronomy and Navigation, and of such infinite tedious, 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 Instruments 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 persuaded from the use and improvement of Tele scopical 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 larget of Hevelius here described, and some of them 40, 50, may 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 strange, I will assure them, that I have contrived above 20 ways for dividing the Instrument, each of them as much different 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 accurateness is desired. I have various ways of fixing those Instruments, and appropriating them for this, that, or the other particular use. I have various mechanical ways for making and working the several parts of them with great expedition and certainty, which is a knowledge not less useful then the knowledge of the theory and use of them when made, there being so 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 business of measuring Angles. These 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 predecessors have attained the utmost perfections of any one part of knowledge, and to throw off that lazy and pernicious principle, of being contented to know as much as their Fathers, Grandfathers, or great Grandfathers ever did, and to think they know enough, because they know somewhat more then the generality of the World besides: _Reptat humi quicung; vult, Celo resit irur, Celo tentabimus ire._ Let us see what the improvement of Instruments can produce.

And now to make my Reader some amends for his patience, I shall give a Specimen of two, of each of the several parts that belong to the perfecting of celestial Instruments: And this I shall do, in the Description of an Instrument for taking all manner of Angles and Distances in the Heavens, which if increased in bulk, is capable of as great accurateness, as the Air or Atmosphere will ever permit celestial Observations to be made. Its perfection consists in seven several particulars.

1. In the Sights, which are such as may be made to discover the minutest part discoverable in an Object, they do no ways strain the eye, and are fit for all Sights, whether short-sighted or old, &c.

2. In the Divisions, which are such as will distinguish the Angle, as minutely as the Sights will distinguish the parts or Objects. 3. In the Sights, being so contrived, that with one glance of the eye, both the Objects though a Semi-circle distant, are at once distinguished and seen together.

4. In the method of setting 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 than 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 fagging. 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 0, and let n represent the Eye-Glass, whose Focus also is at 0, let s represent the point, where the eye being placed, the whole Eye-Glass n will be enlightened 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-Glasses 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 exactly
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 than 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 proportionally 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 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 Hervelius’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 easy 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 Radiins, 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 Nollinius, Vernier, or He-dreus, yet if he considers what I have now laid, 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 Coelestis, 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 than an inch, 'tis easy 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 3 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 hereof 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 3 part of the radius, let the outside 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, ground'd and fitted exactly about the above-mention'd cylinder, and as
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 express'd 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, which carries the Screw 999, and the Index-Plate t t; the contrivance of this Frame h h, is to keep the Screw 999 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, 0 0 0 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 fix'd 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 999 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 there- 
of.

Let a a a a, &c. represent the Frame of the Quadrant, consisting of 5 Bars, radiating from the Center, steadied 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 expressed in the Figure, and the thickness may be about 1 80 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 outside 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 Brasses, 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 screw'd 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.
The edge of this Brass Limb must be, by the help of the Plate c c c c, and a File or Plain, cut very exactly round, to answer the Center of the Quadrant, and the upper side thereof must be plained exactly smooth and flat, upon which Plain-side the Loop-holed Plate c c c c must move, as is visible in the Figure. This Plate at ii must be wrenched or wreithed, so that the Plain thereof must stand parallel to the Plain of the Index-Frame, and by the wreithing of it at ii, as aforesaid, there is room left for the Screw to lye obliquely, without the Screws touching the aforesaid Plate, or grating against it. The reason 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 consequently that the Teeth upon the said edge, may likewise be exactly cross or perpendicular also, and consequently that no bending of the Rule c c c c, (to the end of which the Frame of the Screw is fastned) may at all vary the Angle, nor any unequal thickness in the Limb of the Quadrant, but that the turning only of the Screw shall produce a variation, and that exactly proportionate to the number of Revolutions, and the parts thereof, shew'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 Compass of the said Screw, or the Cylinder out of which it is made is, and multiplying the said Compass 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; Suppose in the length of 4 inches, there be 83 Threads of the Screw, and that the Compass of the Cylinder of the Screw be 3\textfrac{1}{2} Centesims of an inch, I multiply the 22 by 83, the number of Revolutions, and it giveth me 76\textfrac{3}{6}, that is 76 inches, and 36 Centesims 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 demonstration
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 1, 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 Collier 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, express alone in the 8th Figure, but the manner how it lyes in the Frame, is express by p p 0 0 0 in Fig. 1 0 0 0 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 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 employ Mr. Tompion, a Watchmaker in Water-Lane near Fleetstreet; this person I recommend, as having imployn'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, it will 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 it will be easie to make a small Table, which shall shew what parts of a Quadrant, divided into Degrees, Minutes and Seconds, will be designated by the Revolutions, and parts of the Revolutions of the Screw. As for instance, If I find that 1600 Revolutions and $\frac{1}{2}$ make a Quadrant, then $17\frac{78}{8}$ Revolutions make a Degree, and $\frac{1}{2}06$ Millefems of a Revolution make a Minute, and about 5 Millefems make a Second, thence it will be easie to find: (if you observe) an Angle to contain $294\frac{35}{8}$, that is, 294 Revolutions, and 358 Millefems 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, than 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 Observer 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 Observers, 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 spoyle'd all the rest, and made the Observation become uncertain and of no use; and such Instruments as were contrived for one Observer, 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 disused, 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 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 touch each 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 sooner separated. The way then of doing it is in short this.

Joynt 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 truly polished, 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 coalesce 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 plan. 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 Joyn, 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 Joyn, which communicates the Cavities of the two Tubes. Let e e represent that part of the said Joyn 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 Joyn 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-Glafs, Sight-Threads, and Obje&-Glafs of the upper Tube, and k l and m the same parts in the lower, and whatever Ang'e the Tubes make to each other, whilst they open upon the before-mention'd Joyn, 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 Joyn of this, to represent d d the Joyn 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-Glaffes, Sight-Threads, and Obje&-Glaffes, at due Distances, the whole will be performed.

These Tubes thus fitted, will serve to take any Angle left then a Quadrant, to what exactness i 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 easy 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 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-Metal 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 liable 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 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 as certain'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 as certain'd, all the pains, care, industry, and curiosity, bestowed 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 steadly and securely fixt into the Tube, that they cannot by any means be stirr'd or removed; the manner of doing which, I suppose, is 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-Glasses, the other Thread-Sight must be left free, till by several trials 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 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
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 versa, 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 crofs 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 foever 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 exackly 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 Crosses to direct now to the Point p, and not to w, divide the Distance between the Point w, and the Point p, as exactly as you can, in half, which if you chance to hit exactly at first, 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 must again rectifie to half the Difference between s and r; now the Difference being grown yet less, you will a 3d. or 4th. time set it so exactly, as to see the Points m and e, which lie in the straight Line with the Center of the double Tube.

The 4th. thing wherein this Quadrant exceeds the Common, is for its accurateness for taking Altitudes; and this is done by the help of a Water-Level, for adjusting the exact Perpendicularity thereof. This Level may be made and fixed so exactly, that any Observer may be sure of the Level of his Instrument to a Second or two. The Level itself is nothing but a short Tube of Glass, about 6 or 8 inches long, Hermetically sealed at each end, and filled with a Liquor that will not freeze nor grow foul with standing.

The Glass, as near as can be gotten, should be Cylindrical and straight, it being the better the nearer it be to a straight, provided it have a sensible bending or swelling in the middle, the gibbous part of which should be set upwards, and a proper Cell and Box made for it of Brass.

This Glass is to be filled almost full of distill'd Water, to which about a 3d. part of good Aqua-fortis or Spirit of Niter hath been put, to keep the same from freezing, and also from growing foul, then carefully sealed up Hermetically, and placed in its Box of Brass, and with hard Cement fixed into the same, which by Screws is fixed to that side of the Quadrant, that is to lie Horizontal.

The Brass Box being thus fixed to the right side of the long fixed Tube ap ap ap, and underneath the Quadrant, so as not to hinder the free movement of the arm cc, as at xx; the next thing to be done, is by it to set the Quadrant truly Horizontal, which is thus performed.

Setting the side 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 opposite to each other, observe
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 Glasses-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 first 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 both 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 Glasses with the point of a Diamond, as 33. 22. 11. 00, which being done, it will be exceeding easy 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 00. 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 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 ff, 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 2 Pivots, 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 than \( \frac{22}{100} \) of a foot, and every Second will be almost half a Centesim 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 ascertained in any other way.

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

To prevent this, If Glasses Canes be used, there must be much care taken, and many trials 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 curvature or straightness, nor are they easily ground into a straightness or curvature by the Glass-grinder afterwards, though that can be done with some trouble. But diligence and trial will quickly find some piece or other, that will be sufficiently exact for any trial, 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 Gleass, 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 tried 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 Curvature 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 inside 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, whencesover 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 expressed 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 polished Cylinder or Globule of Glass, Crystal, 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 polished, and that the Concavity of the Plate be as smooth and well polished, 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 polished on a very large Sphere, and the Limb of it ground and polished 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 easy, by looking by the flat polished Limb, to discover the true Horizon. The only inconvenience I find in it is, that the hath some kind of sticking to the Glasses, but a small Crystal Bowl, I suppose, may remedy that inconvenience, and make it fit for use.

The 5th. thing wherein this Instrument is made to excell others,
others, is in its easiness 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 Hevelins, 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 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 re adjusted every moment. There was therefore necessary so great a judgment 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 Hevelins, 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 Trials and Observations, may be freed from this intolerable 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 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 claspseth 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 rectify'd, so as to lye in the Plain of the two celestial Objects, whether Planets or fixed 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 itself, 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
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 fixed 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 fixed stars, will be considerably shorter than the solar hours; then fit a Worm or Screw to these Teeth, that one revolution of the Worm being made in ½, 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 flank of the Worm or Screw above-mention'd; the weight that carries round this Wheel must hang upon the flank 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 Observer may at any time of his Observation either shorten or produce the length thereof, so as to make it move quicker or flower, 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 $a$ be then in the 5th. 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 $f$ 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 $f$, is fitted a Socker $h$, 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 counterpoife of Lead 8, which serveth
to counter-ballance the whole weight of the Frame and Instru-
ment upon the other, and can be screwed either nearer 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 un-
der 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 33 i i, whose circular edge 33 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 111; 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 0 0, 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 flipping up and down the Cyli-
der 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 no-
tice of a Publication, made by Christianus Hugenius Zuliche-
nim Consil. F, in his Book call'd, Horologium Osciilatiorum sine
demotu Pendulorum ad Horologia aptato demonstrationes Geome-
tricae; 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 Isochrone 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 Paraboloid, 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 foreign parts. This many of the Royal Society can bear me witness, and the publick Registers thereof do testify and make appear, and I was told by Sr. Robert Mon- ray, 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 communica\n
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 than 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 express in the 20th Figure, (though it be there very ill express, 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 21st, 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 fallly express in the 21st 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 express.) I shall not spend more time in explicating the Pinnions, r s t u, r s t u, of the 20 and 21st Figures, which are to answer.
iver 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 observed, 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 fixed fastly 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 ceiling, 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 1 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 11 of the Plate x, and the semi-circular Arms of the upper Axis, taking hold of the Points 22 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 Joyn, 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 o, 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 rectified 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,
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,
olution, 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 than 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 refractive-
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 Zodiac, 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 Appulse 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
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, than 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-
cumberfom, 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 than 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 truly found, vastly surpassing any thing performed by the best Levels yet known. To this we may add, the height of Hills, 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 captivatingly 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.

FINIS.
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.
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 leisure, 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.

Secondly,
Secondly, a way of shortening 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 78th, 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.

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.

Sixthly, the uses:
3. For making a Hand move according to the true equation 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 purports.

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 Rays from the surfaces of black Glasses, which are ground 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 facula 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 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 \( \frac{1}{16} \) 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 \( \frac{3}{4} \) 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 \( \frac{1}{8} \) part of the whole, and if that be once more reflected, the Ray will return but with \( \frac{1}{16} \) part of its first light.

This, although it be obvious, and easy 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 Glasses; others have diminished the Radiation, by covering the Glasses with a very thick and close coat of the foot of a Lamp; others, by casting the figure upon a piece of white Paper, whences 'tis reflected to the eye; Others have contrived 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 Rays; 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 wares 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 Monseur Hugelius's way, of covering the Glass with the foot of a
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 mag- nified 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 Objec-tts 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 haziness 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 ways of doing which I have represented in the adjoyning Table, wherein I have expressed ten several ways 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 subjected to lose its Figure, which all our specular Metals are very apt to do; for, by it, it would be possible to contract the Tubes for long Glasses into very short lengths, and so make them of easy use and manage.

This I attempted with several sorts of Metal, made with ₄, ₅, ₆, Antimony and Arsenick, but most of these compound Metals 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 glaze 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 superincies of the Glass. The first from the surface of the Glass before it entered; this, as it weakened the Ray, so mingling with the other reflection that came from the bottom, it created some kind of haziness and confusion, if the two superincies 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 eg the first Reflecting-plate, whose thinnest side is to c, and d e the second Reflecting-plate, whose thinnest part is towards d, which doth thereby take off the first Refraction of c eg, and so destroy the Colour's superinduced by the first. The Ray also was weakened much more from the second refraction it suffered at the unfoil'd superincies of the Glass, from the refraction of the Air, or aether, 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 superindu-
ced upon the Objectts, by the rays passing through those 
Glassxes. But this was not in all, for I found some that did 
very well answer my expectation, and I am very apt to be-
lieve, 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 leisure and opportunity I design to ex-
perience farther. But this only by the by, in relation to the 
shortening the Tubes of Telescopes for the Moon, Planets, and 
other Objectts, 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 Glassxes 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 error at a great distance from the eye being vastly mag-
nified 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 
fixty 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 superficies (γ ν, θ θ′, ζ ζ′, ξ ξ′) 
is broken into five shorter lengths (ν ν′ answering to bc, γ θ to 
gh, θ θ′ to de, ζ ζ′ to bi, ζ ξ to ef, and ξ ξ′ to ik), and 
lastly,
lastly, \( \xi \) and \( \nu \), to \( \zeta \) and \( k \), 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 shortened 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 always 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 light is directed exactly at the Sun, so that there will be little difficulty of finding it after the Glasses are fixed 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 ways 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 light 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 ab represents a concave surface of a black Glass, whose focus is o, which, for instance, we will suppose at the distance of forty foot, cd 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 e, and because the wedg-like form of this transparent plate of Glass, cd, 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 mp, where the reflection will not come to fall upon the Plate, e must be so fixed that the thinnest part of this may lie just upon the thickest part of cd, and the thickest of this over the thinnest of that, by which means both the false reflections and refractions will be removed. From e, the Rays are reflected to γθ, and from γθ to o the focus, and so through the lens, z, to the eye x. This take to be the best by Reflection; but it may be twenty other ways contrived, which I shall not now spend more time in describing, it being so ease 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 Macule, facula, and Nebula, 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 always 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 Superior Planets, and the Longitude on the Earth, than 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 pole to turn the Screw from the Centre; and that the Screw-frame may be kept down the true, 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 8th. Figure by m, by which means the Screw will be kept close, steady, and even to the outward edge of the Limb. The Letters in this 8th. Figure being the same with those of the 1st and 11th. 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-fights 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 it will be best to screw it with a small thread, otherwise it will be apt to be moved out of a straight by screwing a large thread; and the thread, whether greater or less, must be made by degrees with a pair of cutting-flocks, that may be set closer every time of screwing.

C The
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 Cylindrical 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 por-"tance 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 will it be difficult, after it is known by Observa-"tion, how many Revolutions, and what part of a Revolution answers to five whole degrees, to calculate a Table of Sub-tenses, which shall shew what part thereof goeth to make the sub-tense 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 which were 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, with great ease and certainty.

I also at the same time made another Frame with a straight Screw, which opened to five degrees only, with Tumbrels or Rowlers like a pair of dividing Compasses (after the same manner with 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 which it was fixt, and the help of a very deep Plane convex lens, whose plain side was turned downwards towards the Plate, and the convex side towards the eye, the said Frame was moveable from five degrees to five degrees, upon the whole limb of the Quadrant or Instrument, by which Instrument I could with great ease actually and accurate divide an angle into every five Seconds, and
and consequently take any angle to the accurateness of five Seconds; for, removing the Frame to the next division, less than the Angle desired, and then by the Glasses, fixing one of the Arms that had the plate, exactly over the hole or point of division, by the Screw the remaining part of the Angle could be exactly measured.

As to the method of dividing any of these, the best way will be to measure upon some Plain 1000, 1500, or 2000 foot in length, by two Rods of twenty foot long a piece, or else by Wires strained with weights, the way of which I shall shortly describe: Beginning from the very centre of the Instrument, and at the end thereof, to set up so many Deal-boards joined to the end of each other in a straight line, or else to strain a pretty big Line, which shall cut the measured line of distance from the center of the Instrument at Right-angles, and then by a Table of natural tangents, according to the distance from the centre of the Quadrant, put as Radius, to set and mark off upon those Boards or Lines the divisions of Degrees and Minutes, by Compasses or Rules, as exactly as may be, and mark them accordingly, that the Degrees may be distinguished very plainly from the Minutes: Then having adjusted the Instrument, so as to see the beginning of those Divisions through both the Tubes at once, to set both the Indices to 0, or the beginning of the divisions, then keeping the undermost of the two Tubes fixt to the same place, so as still to respect the same point or beginning of the Divisions upon the Boards or Line, by the help of the Rod to turn the Screw or Rowl, till you find the upper Tube to respect the first minute, and then the first degree, and so till you see the last minute of the five whole degrees, or whatever Angle else you design it to take in; then (for the first and third way) reckon how many whole Revolutions, and what part of a Revolution goeth to make up that whole Angle, and subdivide the same by a small Table into Minutes and Seconds, and you will presently find by the Trial, that you will be able to divide to a strange accurateness upon those Boards, by the help of your Tubes and Screw, even at the distance of 1000, 1500, or 2000 foot, and even almost to equalize the Divisions by your Compasses, when at the very Boards. And by this you
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 freight: 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 Rowers 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, which I find also is somewhat troublesome in the Instrument of three foot Radius, when 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 Animad.) at the end of the moveable Arm, is made, by unscrewing, to draw off the long Screw from touching the threads 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 Threads of the Limb of the Instrument. The only care to be taken in this action, is, that neither the Index ee be at all moved out of its posture to the Index-frame hh, 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 000, and the handle thereof pp, 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 Threads of the Limb, into which it must be steadily guided by hand, the Index ee will shew upon the Limb the number of Threads 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 5
much; A small matter of warping not creating any sensible error, 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 Analytical 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 11th. Figure, in which there are two Axes or rods of Wire that are jointed 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 situation 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 \(dd\) by the handle, till you see the Index \(ee\) on the Axis to point at those Hours, halves, quarters, or minutes you have a mind to take notice of in your Dial; by the second Index \(gg\), 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 Thread to extend from the Crofs, till it touch the Plain in the several hours, halves, 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 \(CC\) and \(DD\), which are fastned to the ends of those Rods, which serve to take hold of the four Points of the Ball, Circle, Medium, or Crof\(\) 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, Crof\(\), 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 \(rr\), taken hold of by the Hands of the Axis, I call the Points, and the other two Points \(22\), 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 Joint by bending the angle they make with each other, more or lefs, 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 Artift, to make them as they ought to be, to perform their motion with exactness. Let ab then represent one of those Rods, and cd a second, which are turned exactly cylindrical within the Collers efg 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 always cut each other in the same point, howsoever they be turned round within their Collers; nor must this point be varied, howsoever those two Axes are inclined to each other, so that though cd be inflected to lm, or no, and so make either an obtufer or acuter Angle, yet the point 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 Ecliptick, or the equation 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 Crofs 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 h, in the 22 Fig. of my Animadversions, and fixable in any posture desired, by help of a side-Screw b, such as is very commonly made use of for most Instruments that are fixed upon the end of a three leg'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 CC, steady and fixed in any posture they shall be rectified to. The Semicircular-arms CC, are to be made of very good Iron, or rather Steel, and to have a channel or groove 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 g must be made a Joynst 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 Joynst, 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 Joint be so contrived and wrought that the Axis of the two Rods may always cut each other in the centre of the medium Cross or Plate, and that the said Centre, whatever change happens to the Joint, may always 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 10th 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, which is the way I most approve of, as being strong, steady, and handsome; these are delineated in the aforesaid Figures, by $X \times$, and $X \times \times$.

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 Tropic, or of any other parallel Circle, you must rectifie the Semicircular Arms $C C$ of the Axis $B E$, to the degree of the declination of that parallel, and then 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 $x x$, give the true Ellipsis in the Orthographical projection answering to that Parallel.
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 Univerality 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 easie of taking Azimuths
and Altitudes, that the Arm of the Azimuth quadrant that is
once adjusted to the Celestial Object, should, by the aforesaid
Joynt or Instrument, be kept alwaies respecting and follow-
ing 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 Joyned into either of the Arms $r$, of the cir-
cular Plate $X$, in the 22 and 23d Figure of my Animadver-
sions, and the upper end joyned into the movable Arm, at
the fame 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 $r$ being alwaies moved in
the superficies of the Cone described, by the radiation from
the Celestial 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 $r$, being alwaies directed to the
Celestial Object, if the Arm of the Quadrant be moved per-
pendicular 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 cer-
tainly rectified to any Celestial Object; that is, by fixing Te-
lescopes or Common-fights upon the Circular-plate, so as the
Axis of them may be parallel to the Line through $r$, 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 afores-
said 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 thoroughly 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

D 2 would
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 $g q$, in proportion as the Body $k$, moves swifter or slower. And 'twill not be difficult to mark upon the Rod $g q$, 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 always 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 always be the same with the variation of the declination of the Sun, which is twenty wais 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 parallel 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 straight 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 situate 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 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 me-
chanically, and according to the truth of Geometry and Cal-
culation.

Now, in all these motions, care must be taken, to provide
that the inclination of the declination of the Sun from the E-
quinocial, be express'd 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 revo-
lution, 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 itself may perform it. This may be done
twenty waies easily enough, which I suppose will be suffi-
ciently obvious to any knowing Mechanick, and that with-
out 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
flaking 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 ease, 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
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 always 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 Sun’s 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, culminations, azimuths, and almucanters; 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 Joint, for drawing Ellipses, drilling and boring of bending Holes, for turning Elliptical and Swasth-work, till I publish my description of a Turning Engine, capable to turn all manner of Conical Lines, and Conicoidal; 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 Embo Sments 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-brown, or compressed and angular, like those of a Ram or Goat; for all manner of Swasth-work, Compress-work, &c. every of which principal parts hath a vast variety, and the compound and decompound principles have a variety almost infinite.
Appendix.

Concerning the Eclipse of the Moon, observed in London.

January the first, 1676, being at Sr. Jonas Moore 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 6h. 47'. 30". the Moon first covering it with that part of it which
which 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 Easternmost or foremost part of the Limb of the Moon began to be inlightned, whereas before the Westernmost 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 entered. 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 Sir 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 postures and magnitudes, only by gues
gues, 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 Moon's 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 controversy, 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 enlightened, 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 Moon's 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 itself 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 Moon's entering 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 inlightening 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 itself.
felf, which was indeed so very briskly bright and strong, that it did not only soon make the other light disappear, but also all the Telescopic 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.

In
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 Balance 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 Patent, and not 1. To which Clause I could no ways agree, knowing 'twas easy to vary my Principles an hundred ways, 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 it
is 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. Hugen's 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. Hugen's Pendulum-Clock, at Sea, by the help of a Ball and Socket, hung to the under side of the Deck of a Ship. But 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 itself was ingenious, and did much more than what Mr. Hugen's did expect, as I was then informed by the Right Honourable the Earl of Kincardine, the Author and perfetter of that part of the Invention. But wanting a little addition (which I concealed, and Mr. Hugen's hath not 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 other side, 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 nowhere be of use but in East
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 Premiums 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 Balance 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. Huggens 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 Experimenterd 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 Inventor, Nam ingenium 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 fusfe, 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 easy 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 centesime of the Inventions I intend to publish, though possibly not in the same order, but as I can get opportunity and leisure; 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 Architectonic Writter hath ever yet attempted, much less performed. abceedee fgg iiillianonopprr ssstuuuuuuux.

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. eeiinossstttuu.

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


6. A
6. A new Selenoscope, easy enough to be made and used, whereby the smallest inequality of the Moon's 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-Chariot for travelling far, without much wearing Horse or Rider. Discovered.


10. A new invention in Mechanicks of prodigious use, exceeding the chimera's of perpetual motions for several uses. aaaaabcdefdeeeegiiilmmmnnooppqrrrrssstuuuu.

aaeffhiiilllnrrsstuuu.

FINIS.
LAMPAS:

OR,

DESCRIPTIONS

OF SOME

Mechanical Improvements

OF

Lamps & Waterpoiles.

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, at
the Bell in St. Paul's Church-yard. 1677.
LAMPS:
OR, A
DESCRIPTION
OF SOME
Mechanical Improvements
OF
LAMPS.

He 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 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 dissolver.

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 dissolver must in time be dissolved. But to shew a way how to make the
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 of Helioscopes 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 easy 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 intolerable and diffuse: 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 choke 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 these succulencies. 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 incomparable 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
those parts of the body of steam 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 choke, 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 Selenitius 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 Selenitius 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 foamy, choaking, and stifling substance.
To make then the reason of the Phenomena observable about the lasting or stifling of the flame of a Lamp the more clear and easy 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 itself 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 streams of hot Air, and fill all the Ambient space of the Air therewith, which vapours being very much rarified, and consequently lighter than the incompeasing Air, are by the greater gravity and pressure thereof carried upwards by the Curve Lines. 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. The motion of the Particles in which is swiftest in the first, that it produceth light: next, that it produceth heat enough to make the succeeding parts of the streams 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 streams 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 iii, 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 ii 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 ii, but begin to be dissolved, and to shine as they approach the fresh Air at ii, where the dissolusion 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 by the dissolusion of the steams of the Oyl lit 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 flame dissolusion into the Air as the steams of the Oyl, and if any part of this Wick be without this Conical Superficies at 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 foot, namely, that the middle parts of the Cone of flame, being nothing but a great number of oylly 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 puzzling 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
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, ripe 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, softened, liquified, hardened, &c. that thousands of separations of conjoined 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 Hydraulicks 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 itself 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. BCDEF, 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 GH, 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 GH, exactly in the Line LM, 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.

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 Figure, 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 lie 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 CO 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

CHAPDOC
CHAP. DCC shall exactly counterpoise the said under Hemisphere CR B Z D O C, 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 AHCRCBOA 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 CZ, for if it be any whit higher as at E F, the Liquor must necessarily overpoise the aforesaid Wedge AOC, by all the weight of the Liquor
Liquor contained in FGOFZ. 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 wherefoever 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 poyfing 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 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 represents a Pedestal, which may be made with three claws or toes to make it stand the steadier and even-ner 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 Diametri-cally 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 incomp-ass the Vessel or Receptacle of the Oyl, and is shaped exactly like it. This is made strong enough of Brass,
Iron, Silver, or other material to bear the Receptacle, Poife and Oyl without bending, and hath, as I said before, two Pins or Gudgeons at C, and opposite to it Di- 
ametrically, 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 Perpen- dicularity, so as that the upper edge thereof at F F will al- ways 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 Veflel 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 pafleth through the Center or Axis of its Cavity by the Counterpoife moved on the Center C.

H H H represents that Counterpoife which is made ex- actly half the weight of the Oyl or Liquor, and the Cen- ter 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 Con- cave 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 fil- led with Oyl, the fame 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 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 hea-
vier than another. To the hollow of this Vessel is fitted a float D of Brass, Latton, Silver, Lead, &c. whose under-
side 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 quanti-
ty 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 easy to be understood by any one that shall
seriously on this Delineation consider that the float D muft
necessitate the Vessel A C B to move on its Axis B ac-
cording as its Oyl wafts, because one end thereof E being
fastned to the brim of the Vessel B, the other end O be-
ing loose will as the Oyl wafts descend towards N, whence
the end E muft hang heavier on the brim B, and conse-
quently muft move it down towards B, till the upper
side
side fig 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 wafts, 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 itself 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 between
between the top of the mouth of it next the Socket and the return thereof upwards; then by a counterpoise to suspend this Siphon that it may follow the Oyl as it wafts, 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 wafts, 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 Fuley, which Fuley 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 passes 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 Fuley 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 Fuley 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 decreases 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
terpoife L may just take off its whole gravity, and suffer it to have no degree of gravity or pressue 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 Fussey 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 use
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, do 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 fixt; 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 specify 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 Superficies
perficies of the water to the same Level, and there delivering it, have yet each of them their several properties, 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 imploled, 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 expending 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, Seareing, 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 Ciftern be once in twenty four hours recruited 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 Ciftern does no ways abate or diminish the stream of the water from the Ciftern, so the new access of other water for a supply to refill the Ciftern 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 Ciftern at any part of the depth thereof, for according as the Vessel is tapped 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 Ciftern 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 Ciftern must be tapped at four Inches deep; and if thrice that quantity in the same time, it must be tapped at nine Inches deep; and so forwards, as is already demonstrated by Mercurius, and other Authors. For since the pressure of Fluids upon the parts thereof increase, in the same proportion
with the depth below the Surface. And since the forces requisite to accelerate motions must always be in du-
licate proportion to the Accelerations, it follows, that the perpendicular depths of the Tap under the Super-
sificies of the water must be always in duplicate proportion to the Velocities required.

The plainness and certainty of this truth in Hydro-
staticks, 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 Metaphysicun*, 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, 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 Celestis, 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 Celestis 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 fæda 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 those holes; and consequently, that the motion of them is exactly according to the plain and obvious Rules of Mechanical motions. And consequently for the solving all the Phenomena of Hydrostaticks there is no need of any other Principles than the plain Mechanical Principles, which supposeth every Terrestrial Body to have a Gravity in it, which is always the same, and always communicates its Gravity to the Terrestrial Bodies subjected 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 same, and acteth continually by continual repetitions indefinitely swift. 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 must confess to be meerly Mechanical, if at least he will admit of any such thing as Mechanical motion. For I cannot conceive any Reason why the Doctor should not allow for instance the parts of a Cylinder of Lead to press 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 several parts, and laid one over another in the same 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 same weight or gravity downwards; and do communicate continually the same force, pressure, indeavour, impetus, strength, gravity, power, motion, or whatever else you will call it to the Scale. And I suppose the Doctor will grant, that if the cold Cylinder of Lead, weighing ten pounds, be divided into ten shorter Cylinders, that are each a tenth part of the whole, and do each weigh a pound alone, every one of the upper shall 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 shortened 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 yield 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 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, Crossbows, Spitting-Trunks, and the like; as likewise in the motion of Arrows shot with Bows or Ballifths; 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 occasion.
tion 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 signify 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 forever 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 encourages Ignorance and Superstition by persuading 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 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 man's Industry and Invention; I have encouragement 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 Refined Notions as do exceed Imagination and the plain deductions of Reasons therefrom.

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 Poifes 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, and
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 affer this Hylarchick Spirit. But in this he doth Canere triumphum ante victoriam, and seems to make very flight of that which he neither hath hitherto by all he hath laid 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 seriousnes and deliberation, he would not, I conceive, have believed that one that understood the Objection would be satisfied with so flight 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 Parallelipipded that doth destroy the Rotation generated by the first, but only that which entering 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 Parallelipipded, and being reflected from the second Superficies, suffereth a double Refraction in the same Superficies, the one at entering, 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 expressly 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 Prifmes. But this is contrary both to Experience, and the Laws of Reflection; for the Refractions in the Parallelipiped B are the very fame 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 E F, 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 solicii, cum sit in materia magis incerta ac inequali cuius interna contextura videatur Globulorum motus varius modi, possè mutari. For since all transparent bodies whatsoever produce the same effect, that Subterfuge of supposing some strange invisible texture in the body of Muscoey 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 Guta scilicet Pluvia, s nullus Demonstrationis Scopus subit error, actum est de Globulis Cartesianis. Sed videtur (says he) ingeniosus demonstrator non satis intellexisse scopum quo collinuneare 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
to Des Cartes Hypothesis. Neque enim satis erat probare (quod agnoseo cum seicte seite & elegant) Refractiones in gutta pluvis ita fieri, ut si in duobus pellucidi Parallelipedii Lateribus oppositis, factse essent, sed oportebat praterca evi-
cisse quod eodem modo restringatur radius in utrilque Locis quo in Parallelipedio A restringitur, hoc est ut Radius B C quamvis oblique, perpetuo tamen curvat versus candem ex-
tremitatem tam in F quam in D Parallelipedii A puta versus extremitatem E, nam in hoc caeli Rotatio ad D dissolvitur iterum ad F ut supra dixiit est; sed Demonstratio Inge-
nios Micrographi hic non attingit; sed probat secundam re-
fractionem in opposito Latere fieri ad modum refractionis in Parallelipedio Cubi Radius B N primo restringitur in D & procurrerens versus extremitatem E ibique inflexus pergit postea versus alteram extremitatem G & Refringitur in F, qua refraction 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 Ref-
raction, 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-glafs 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 Reflacting Superficies. But this is contrary to 
Experiment, he must therefore once again consider how 
to find out a Reafon why there is no colour generated, 
where, according to his Affertion, there is so great a re-
fraction, 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 us Doctrina Cartesiana de Globulis eo-
rumque Rotationibus nihil periclitetur ab hac Demonstrations.
tionem quâ 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 i t 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 entered first into 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 Im 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 1, supposing there were no Reflecting Superficies at ef. 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 paucâ me meaque opera excogitasse quibus pro persuasissimo habeo eorum motus & rotationes modis pure mechanicius 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 Nature
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 control 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; annealing of Glasses and Metals by the gradual access of the heat, so as to make them fit for stronger degrees,
degrees, or by the gradual recefs 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.

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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 easy enough, and may be adjusted to what accurateness is desired. Then the convex Superficies of the Cylindrical poife 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 poife 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 least 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 easy 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 another 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-
lar Pendulum, which is since published by Monsieur Hugenuin, 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 balance, 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.

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 Phenomenon, that hath appeared dubious, not only to me, but to many other more knowing Naturalists. I have often doubted, I confess, whether Moss, Mushrooms, and several other small Plants (which the Earth seems to produce
duce αὐτοκέφαλον) 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 everywhere 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 smallness 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 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 wondered 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 Mushrooms, 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 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 surprizing 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 smalllest, 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 Inquifitive 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 increase 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 all the like Capillary Plants their Congeners are (though generally denied to have any at all) more abundantly prolific 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 Vessels containing 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 Vessels or little boxes may remain whole, so that by your Microscope you may see their true figures and distinctions, some of them being more flattened 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 join them to the rest of my draughts of that kind. Some particulars most considerable I now give you in the 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 indiscernible 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 swept together into a heap, many of those boxes breaking together, and jostling 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 Osmond Royal, which excelleth all the other Ferns both in greatness, comeliness, and virtues, 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 smallness 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-
...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-vesels, 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 satisfy 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 wondered at the ill-favoured roughness on the un-
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 object as that appears) is flown 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 Seedvessels, 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. i, 11, 12. that all Plants have their Seeds, and consequently Ferns, 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 Omnibd 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
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.

W. C.

**Maculae in Sole.**

During this last great heat of weather in June I observed a very conspicuous Macula with its immediately 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 itself may be much hotter when such eruptions appear, those Maculae often times ending in Faculæ. 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 publicly 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 Foreigners, 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 Descavans, and my desiring to transcript that part of it which concerned this matter, as if I had requested some singular favour thereby. I answer,
First, that he knew I designed presently to have printed it with Animadversions, but he endeavoured to prevent me, designing first clandestinely 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 afferts that several discoveries of the Accuser had been vindicated from the usurpation of others. It is answer'd, 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 coin.

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 latter, 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. III.

The Watch fig. pag. 44

cum multis alys
LECTURES AND COLLECTIONS
Made by ROBERT HOOKE, Secretary of the Royal Society.

COMETAE.
CONTAINING
Observations of the Comet in April, 1677.
Fragments of several Lectures about those of 1664, and 1665.
Sir Chrs 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. Crafts.
Mr. Gallet's Letter to Mr. Caffini, together with his Observation of $\alpha$ sub $\beta$.
Mr. Caffini's Reflections upon those of Gassend, and Hevelius, and upon this.
Mr. Hallis's Letter and Observation of the same made at St. Hellen.
Mr. Caffini's Observation of the Diurnal motion of $\beta$, and other changes happening in it.

MICROSCOPILUM.
CONTAINING
Mr. Leeuwenboeck'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.Chrubine'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
Dno JOSEPHO WILLIAMSON
EQUITI AURATO,
Serenissimo CAROLO II.°
REGI,
A Consiliis Secretioribus, et a
Secretis Status,
Nec non
SOCIETATIS REGALIS
LONDINENSIS,
Ad Scientiam Naturalem promovendam
PRAESIDI
DIGNISSIMO.

NEC potui, nec debui, Nobilissime Vir, cu-

jusquam alius nomen his Chartis inscribere,
præter Tuum. Sub Te nata, Tibi vitam debent; Ti-
bi
bi quoque debeat quod lucem aspiciant. Egregius ille
Tuus animus ad instaurandam Philosophiam artesque
adeo omnes utiles, mibi homini, aliquin subtimido, andai-
ciam hujus deductionis fecit. Ego quae nunc potui, pro-
sero, magis ad Gratulationem ostendendam, quàm erudi-
tionem. Spero autem, quemadmodum sub Tuo
PRÆSIDIO majora indies Augmenta Scienti-
arum in hâc gente fiunt, ita exorituros viros doctos,
qui Tibi justa praconia laudum perseveriant; quod ego
pra tenuitate ne conari quidem audeo, quanquam cum
primis fin

Dignitatis & Honoris Tui

Societatis Regalis

Studioissimus,

ROBERTUS HOOKE.
SYNOPSIS.

THE Comet seen April 21, 1677, between the Triangle and the Cloud of γ, its tail not directly opposite to the Ω, 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 shortening 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 propose 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 Aether, above or below the Moon. Whether it wafts, or lafts 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 itself. (9.) Its Nucleus supposed dense possibly as the middle part of the Earth, of which some conjectures. Dissolved by the Aether as in our Atmosphere. (10.) Argument for the looseness of the central parts of the Earth from the variation of magneticall direction. (11.) The Nucleus of Comets possibly the same. Internal motion may weaken gravitation. Parts separeted may be agitated by the gravitation of the Ω. Tail made not so much by the particles receeding as the Stars approaching the Sun. (12.) How the Comet may first lose its Orb in the Universe, and passing through the 3 spheres.
SYNOPSIS.

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 streams. 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 celestial. 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 1567, 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 &c., and without &c. Orb in $\infty$, a sextant in 130 days, &c. This not relied on, because there may be other hypotheses to solve the phenomena; 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 hypothecis 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 waiting in the Æther; explained by fire. Dissolution by menstruum. (33.) Thence the proprieties of Comets conjecured, and the sum of the foregoing discourse repeated, being the end of a Lecture. Recourse to Tycho Brabe's Observation (34.) for making out the Comets Orb. His supposing its motion unequal without reason a shift. Mr. Horrox his hypotheces in the ninth figure a product of chance. (35.) A discourse on it, and some objections against Tycho's. (36.) Kepler's hypotheces examined by these Observations of Tycho's, found the most likely, but with some alteration. Line of Trajectory bent a little. Motion accelerated towards the Sun, retarded from it. (37.) The twifter and further off the Comet from the Sun, the less the bend explained by the tenth figure. (38.) The way of enquiring parallax by Telecopes, (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. 1664. (42.) Some other Papers about Comets added, being reflections on Mr. Descartes and Kepler's hypotheces, 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 wafting and lafting 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-
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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 Phosphorus, written for his own use, infected; 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 glafs sphere, 2. A little enlightens a large Cylinder. 3. Liquor shaken had a smoke and flaft. 4. A dry substance affirmed to have continued shining 2 years, flafted. (59.) 5. Some dust of this on a Carpet twinkled like Stars. Writing on paper with it thin'd, and fine of Sulphur and Orions. (60.) 7. The hand on which it was rubbed, thin'd, but felt no heat. (61.) It fired Gun-powder first warm'd. (62.) And white paper held over coals. Other tryal propounded, but refused. (63.) Some Experiments made on the Phosphorus Baldwin in vacuo, and in the open air. (64.) Preferved in Vacuo, but destroyed in Air. (65, 66.) Monsieur Gallet's Letter to Monsieur Caffini, acquainting him with his Observations. (67, 68.) His Observation of four spots in O. (69.) The particulars observed. (70, 71, 72.) Monsieur Caffini's Reflections on these Observations. (73, 74.) Mr. Hally's Letter to Sir Jonas Moore, containing an account of his Observations of O sub sole, three Southern Stars. The two Nubes, &c. (75, 76, 77.) Mr. Caffini's farther discoveries about the diurnal motion, and several new appearances in X. (78, 79, 80.)

A second Discourse called Microcopium, 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, Guns 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-glas 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 caufe, and which farther use of them. (93.) How to find the figure and texture of Animal and Vegetable parts. Insatience in a ligament of Beef. (94.) The figure of Muscles hinted, and an instrument stretching them before the Glafs described. (95.) A description of the Microscopes used, 1. Of the single Microscope, and its advantages and difficulties. (96.) Another more easy described, and the ways how to make and use it explained. (97.) Causes that varie the distance of objects from the Globule. The use of Selenites and Looking-glas-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, ues, and advantages described. (100.) The benefit of a Dark Room, and appropriated lights. And a digression in answer to P. Cervantes Accusation. (101.) Some Observations made with this Microscope hinted. Animalcules in the fleping of other Grains besides Pepper. Their smallness estiimated, and compared to a Whale. Muscular fabrick hinted. Milk, Blood, Fat, Sugar, Alluin, &c. viewed. (102, 103.) Mr. Young's Letter of one who trying to cure a Colick by leaden Pills, flipt one into his Lungs; grievous symptoms ensuing. (104.) Helps of skilful Physicians in vain attempted, and particularly of Dr. Mayo, 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 remarkable things taken notice of, and their caufes explained by Mr. Young. (from 107, to 112.)

COME-
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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. Paradis, 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 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 ghesled, 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'clock seemed to rise straight up-
ward, before that about half an hour after two it leaned 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 distant, were of a much greater breadth; spreading wider 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 outsides: 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 shorter, 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 shortening 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 streams rising from a burning or hot body: but the same parts of the Blaze seemed to appear and disappear 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, without 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 express'd: 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 
 \[ \frac{1}{7} \] 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
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 express 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-glases. 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 fix 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 $4\frac{1}{2}$ 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 $\beta$, 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 itself 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 sixt 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
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, shortening 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 enquiring 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 satisfy many difficulties that occur'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 oppor-
tunity. I now had; and the disadvantageous appearance of this last were very short of giving me that satisfaction in many particulars which I with'd for, and expected at first, yet since they may possibly serve for hints to others that may hereafter have better opportunity 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 Gresham-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 myself 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 com-

pound-
pounded 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 Ether, with which the space of the Heavens is filled? Whether above or below the Moon, &c.

Whether it wafts, 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 paraboloelidical 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 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 respec ted 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 alleged 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 C dense
denfe as the body of the earth; and I have not obser-
ved 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 circumftantial information,
especially if for this purpose it had been taken notice
of with a good Telescope. What the density of the in-
nermost 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 foundings
of the bottom of the Sea, yet none can bring an unden-
iable proof that the same is so solid to 25 miles deep;
much less that it is so to the center: if therefore the ex-
ternal 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 superfici-
als or shell removed) might, like these of Com-
ets expand themselves into the encompassing Æther.
Nay we find, that notwithstanding the compacted-
ness 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 Crepusc-
dlum; 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 up-
on, 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 Ala-
baster-dust in boyling, and were not so firmly united
by the bonds of gravity, and the vinculum of petrifa-
dtion,
&ion, 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 controversy 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 properties it hath itself; 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 $\AE$ther,
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 lets the other two at work to destroy each other, that it may at length prey upon both, and dissolve them both into itself; and consequently, not only the parts thus dissolved are elevated to a greater distance from the center of the Star or Nucleus, or the superfluities 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 find one Pole of the Magnet doth the end of a Needle touched on the opposite part; whence it is, I conceive, that the parts of the body of this Comet (being confounded or jumbled, as ’twere together, and so the gravitating principle destroyed) become of other natures than they were before, and so the body may cease to maintain its place in the Universe, where first it was placed. Whence instead of continuing to move round some central body, whether Sun or Planet, as it did whilst it maintained itself entire, and so had its magnetical quality (as I may so call it) unconfounded, it now leaves that circular way and by its motion (which always tends to a straight line, and would be so 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 last place, where it was before this confusion was caused in the body of it. In this line (’tis probable) it passes from one part of the Heavens to another, and so passes through the spheres of the activity of multitudes of central bodies; in the passing through which spheres, ’tis not improbable that those parts which by their dissolution are made of a nature differing from the body in the center, are rather expelled from, than attracted towards it; and so being by this dissolution rarified, and loosened from the middle, and by their acting upon one another, and dissolution of the Æther made of another nature, after they have every way dispersed themselves to a considerable distance from their proper body, are converted and driven in a way almost opposite to that expelling body, and so continue to be driven away to such a vast distance, as to make out that prodigious length of the tail or Blaze of some Comets (such as was that of 1618. which, as Kepler reports, was extended to 70 degrees from the body or head of it) till at last they are dissolved also, and commixed with the Æther within them. So that though I suppose the attractive power of the Sun, or other central body may
draw the body towards it, and so bend the motion of
the Comet from the straight line, in which it tends, in-
to 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 ve-
ry much rarified, and produce light, are of a clean con-
trary 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 levi-
ty conjoin'd 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 at-
tracting 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, imme-
diately 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 \( \Phi \)ther from \( \Phi \)ther, as I may so call differing \( \Phi \)thers; because we have not yet distinct names in use, and the reason of their conglobation, the \( \Phi \)ther 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 flower 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 encompassed 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.
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 inaccurate instruments, or from their less skill and diligence in using them, or from an imagination of the solidity, and impenetrability of the Coelestial Orbs, or from error in their calculations, or from comparing Observations made at distant places, one or both whereof were inaccurate, or from a prepossession of Tradition or common Fame, or from what other cause ever 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 Earth.
Earth, to certain degrees thereof; beyond which, by reason of the imperfections in Instruments, and Observations, and the exceeding niceness and curiosity necessary, they signify 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 shown 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. Hellen; 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 enabled 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 when
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 marked 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 carelessness or neglect of the Graver: This I noted in the two Tables of the learned and accurate Mathematician, P. Hægidius 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 31st of December, 1664, it wa
was in $4^\frac{1}{2}$ of $\pi$ in Longitude, and in $33^\frac{1}{2}$ of Southern Latitude; but by the second it is placed at the same time in $4^\circ \pi$ 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 Rigel in the first Table is placed in $30^\frac{1}{2}$ South Latitude, and in $12^\frac{1}{2} \pi$ for Longitude, but in the second in $31^\frac{1}{2}$ South Latitude, and in $31^\frac{1}{2} \pi$ 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 $12^\circ.11'.40".\pi$ 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 inaccurate, 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 guess it to be not so far removed from the Earth. Something indeed in the general might be guessed 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
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 Monsieur 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 skillfulness in the theoretical and speculative part, and add to all a love and delight in the thing itself; and even all these will signify 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 Teleoscopic 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 glasse, to observe what place the Comet posseseth 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 respective-
ly, 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 Compafes 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 Para-
llax caused by the Diurnal motion. The next enqui-
ry will be, what other ways there are of knowing its distance. Now though none could be more demonstra-
tive than the Parallax found this way by the Diurnal motion, yet there are some other which seem more easie arising
arising from the consideration of the motions that may be thought to be concern’d in the producing the appearances. And though they be wholly hypothetical, and so need some other arguments to prove the ground and principles on which they are founded, yet since there are not very many considerable ones wanting to make them probable and rational, I shall here add somewhat of my inquiries after the distance, position, motion, magnitude, &c. of these Comets by these means.

Of these ways there are several depending upon several suppositions which produce very differing effects, as to the magnitude, distance, motion, and way of the same Comet.

The suppositions are these:

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

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

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

Now according as we take this, or those of these differing suppositions, and compound them together, so will the product of them be strangely differing. Amongst the great variety of compositions of these principles or suppositions, these seem the most simple, and consequently being any otherwise proved, will best determine the true distance and way of the Comet.

First, To suppose the Earth to stand still, and the Comet to move equal spaces in equal times in a circle.

Secondly, To suppose the Earth to move in an annual Orb about the Sun, and the Comet to move through the Æther or Expansum, equal spaces in equal times in a straight line.

Thirdly, To suppose the Earth to move (as above)
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 supposition 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. Kepler’s 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 \( \pi \), and the opposite without the Orb of \( \pi \) in \( \pi \). 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 \( \pi \), was then about \( \pi \) or \( \pi \): 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 Earth's motion very probable: but the motion of this Comet is so well made out, by the contrary supposition, that I think it may be alleged 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
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 \( \infty \) in \( \infty \); and whose Semi-
diameter is about six score 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 myself 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 \( \kappa \) of \( \alpha \): 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
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 EFG the Orb of it supposed at a greater distance: let H be the center of BCD, and I of EFG, and let AC, be to CH, as AF, to FI, all the lines drawn from the point A, so as to cut the Circles BCD and EFG, shall divide those Circles EFG, and BCD, into similar segments: as let ABE be a line drawn cutting those Circles in B and E; I say, the Arch B C shall be similar to EF. 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 ABE, to which a Perpendicular raised at BBE, and produced till it cut the line AC, (produced) at HHI, it gives the Center of its Orb HHI, and the proportions of the lines AB, AC, BH = HC, or of AE, AF, EI=F I, the Angle BAC, being given by observation. So that by this Hypothesis the Phenomena 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 Phenomena 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 Ellipsis, or other such like curve, there may be divers other Hypotheses that will solve the Phenomena; so that the Comet may be supposed to have
have no motion at all as to Longitude: that is, it may be supposed to be moved in an Ellipsis, 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 Hypothefis which we take, the time of its return, if permanent, will be longer or sooner.

And these Hypothefes 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 demonstrat aliquando in quibus Cometae partibus errent, cur tam seduti a ceteris 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 confined 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 Cælum 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 alledge 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, would 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 menfbruum 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 ambient 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 dissolusion of terrestrial bodies into the Æther, the Æther being the principal fluid body, and greatest part of this dissolusion; 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 menstruum. 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
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 guess 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
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 whatever: 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 thrown: but besides, in his Hypothesis he seems to take no notice at
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 improbabilities, 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 supposition 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 fail 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 inflinced 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 Sun.
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 trajectory 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 another, 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 thread 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 thread 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 thread 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 easy 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 centesims of a degree to two degrees is not increased much more than \( \frac{2}{17} \) that is not a quarter of a centesim 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 fixed 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 use in Astronomy, deserves none of the meanest places in Geometry, by the help of which (which is much more than either of the other ways is capable of) one may easily find the true parallax of the Comet, from any four exact observations of it, made at differing times in the same place: Nor does it require so nice and accurate Instruments and Observators as are altogether necessary in the other ways. The Problem as I received it, is this.

Problema.

Datum quatuor lineis utcunque ductis (quarum nec tres sunt parallelae neque ab eodem puncto ductae) quintam ducere qua ad quatuor primo datis in tres partes sectur ratione & positione datae.


Quoniam FD, parallela est ipsi HK, ergo ut C D ad CF, ita K γ, ad γ H, & quoniam γ N, parallela est ipsi HM, ergo, ut K γ ad γ H, ita KN, ad NM, ergo ut KN, ad NM, ita C D 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, & φ O, 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 ductur linea KM, cujus tria segmenta a quatuor lineis datis intercepta sint in data Ratione R, S, T, & servata quidem
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 trajectory 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 ν, 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 Ecliptick 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 Kepler's 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 extinguish'd, diffipated, 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 rays 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 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 incurved 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 extinguished 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 annexed 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. It might have been seen several months longer, if it had been diligently followed with Telecopes, 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 applied myself 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 immediately 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 itself, though it seem so bright, is notwithstanding not so dense, but that it admits rays enough to pass through it unreflected, to lighten 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...
shadow opposite to the Sun, and therefore in probabi-

lity this would do the like, but I diligently observed

that there was no such appearance here, but the con-

trary, that is, that where the shadow should have been,

there was the lightest part of all the blaze, and con-

sequently 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 parti-
cular, and that is an internal motion of the parts which

shine, whether that motion be caused by some exter-

nal menstruum dissolving it as in fire; and Ignes fatui,
or an external motion, stroke, or impulse as in a Dia-

mond, 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 itself, as the Bononian stone,
and Bladwines Phosphorus, which seems to be so harmo-
nious (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 tre-
mulation and sound from the motion and sound of
the unison body, or string that is struck.

To
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 Filth 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 encompassed 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 itself. 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 encompassed 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 encompassed 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 smooking body in our air, or a dissolving body encompassed 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
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 waft and disperse the whole Ball, nor can I conceive that the disapparing of those blazing bodies toward the latter end, does depend upon their dissoluti

on (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 itself, 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 sustains 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.
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, so much 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 considerable, 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 imagine.
engine, 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 knowledge 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 thickened 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, Scolopendra, 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 judge of what is pertinent
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 Phosphorus 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.
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 shewing 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 stopp'd 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 otherwise
therwise stoapt 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 Crystal-line 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 Hazel 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 haft, 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 wooden-shuts, and the Candles were removed into another Room by that we were in; being left in the dark we were entertained with the ensuing Phenomena.
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 judge 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 that which was the Phenomenon I chiefly attended) though I could perceive no smoke or fumes ascend from the luminous matter, yet I could
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 environed them, they seemed like fixed Stars of the sixth or least magnitude, but twinkled also like them, discovering such a scintillation as that whereby we distinguished the fixed 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 extremely 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 Phenomenon 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 conspicuous 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, herub’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 extinguished, 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 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 Phanomena, wherewith he had so delightfully entertained us.
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 conjectured) 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 unsuccessfulness.

And accordingly, On the two and twentieth of September in the Afternoon I received 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 lefs 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 itself 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 left it should waft 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, quench it. This done the Artift 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 beimeared by the same matter, I held it over the Coals, but at a considerable distance from them, and yet this little matter nimbly took fire and burnt a hole in the Paper. And to satisfy my self that the heat did but excite the luminous matter, and that twas this its self that lighted the Paper, I held the rest of the same piece of Paper far nearer the fire and kept it there a pretty while without finding it at all scorched or discoloured. Lastly, the other part of the divided fragment of the hitherto mentioned matter, Mr. Kraft put upon the tip of a quill, and having at a distance from the fire, very well dryed and warmed some 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 guess) that powder took fire and blew up.

'Twill not perhaps be impertinent to add that on occasion of the operation I observed the Air to have on the shining substance when freely exposed to it. I took a rise to tell Mr. Kraft that I presumed it might be worth while to try whether his Phosphorus did shine by virtue of a kind of real or (if I may so call it) living flame, which like almost all other flames required the presence and concourse of the Air to maintain it, or whether it were of such a kind of nature as the Phosphorus of the learned Baldwinus, which I suspected to shine not like a flame or a truly kindled substance; but like a red hot Iron, or an ignited piece of Glass, wherein the shining parts are not repaired by fewel, as in other burning bodies, but are put by the action of the fire into so vehement an agitation as whilst it lasts suffices to make the body appear luminous. This conjecture Mr. Kraft seemed much to approve of when I told him that the way I proposed to examine his noctiluca by, was to put a little of it into our Pneumatick Engine, and Pump out the Air, whose absence:
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 examine 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 one's hand, which we had prepared to be made luminous by them. And then causing the Candles to be removed (for we chose to make trial 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 haste 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 tried 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 Air's 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 or
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 advantageous 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 afflicted 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 capable
capable of being made luminous by the same or the like operation, that is employ'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 advantageous to the Phosphorus, I shall only add here as matter of fact, (for my conjectures about Light belong to my yet unpublish'd Notes, of the Origin 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 pump'd 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 Baldwinus, 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 Letter from Johannes Carolus Gallet, L.L.D. and Provost of the Church of St. Symphorican at Avignon, directed thus.

Clerissimo Eruditissimoque viro D. Johanni Dominico Cassino Matheseos Professori Celeberrimo, Astronomo præstantissimo & Academiae Regiae scientiarum alumnno meritiissimo.

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

Mr. Gallet then acquaints Mr. Cassini with his observation of $\frac{1}{2}$ sub $\circ$ 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 threads 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 accouhtred 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 \( \frac{1}{3} \) of its Semidiameter; at length the Clouds being dispersed, the Sun being 27° 45', high, or at 10 h. 54' 9 it self marked out its own place in the disk of the \( \odot \) by its own shadow cast on the Table by the shorter tube. Then he disposed the shadow of the aforesaid thred so Parallel to the Equator, that this figure of the Sun should move between the outward ones, and that the middle should mark out the Parallel described by the Center of the Sun in motion, at the same time he took the declination of \( \odot \) from this middle Parallel and the right Ascension, 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 leffer Telescope, 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 Phæce, 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 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 vanishes, 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.

<table>
<thead>
<tr>
<th>Octob.</th>
<th>Declinat. macula principalis a paral. centri</th>
<th>Differentia temp. inter limbum Occident. &amp; maculam.</th>
<th>Tempus transits disci Solis.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Die. hora</td>
<td></td>
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<tr>
<td>1 I 0 0</td>
<td>4 44 austr.</td>
<td>1 420</td>
<td>2 I 0 0</td>
</tr>
<tr>
<td>2 I 0 0</td>
<td>2 43 austr.</td>
<td>49 0</td>
<td>2 I 0 20</td>
</tr>
<tr>
<td>3 I 0 30</td>
<td>1 21 austr.</td>
<td>35 40</td>
<td>2 I 0 20</td>
</tr>
<tr>
<td>4 I 0 35</td>
<td>0 40 austr.</td>
<td>25 40</td>
<td>2 I 0 30</td>
</tr>
<tr>
<td>6 I 0 0</td>
<td>3 0 boreal.</td>
<td>3 20</td>
<td>2 I 0 4</td>
</tr>
</tbody>
</table>

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,


The Contents of which are,

That designing to observe this passage of ☉ under ☉ he with his Tube watchfully looked for it in the Sun's place, from the 5th to the 7th. day, with a Telescope of 23 foot.
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 Equator drawn through the Sun's 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 Sun's 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 Imersions and the Emerion 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, whilst he himself took the Altitude of the Sun with his Quadrant, as in the tenth Observation.
The Order of the Observations of Mercury seen under the Sun.

<table>
<thead>
<tr>
<th>The number of the Phases observed.</th>
<th>The north Declination of ( \odot ) from the parallel of the equator through the center.</th>
<th>The difference of the time between the Transit of West Limb of the Sun, and the Transit of ( \odot ) from parent center.</th>
<th>The distance of the body of ( \odot ) under the same meridian, collected from the pendulum vibrating ( \frac{1}{2} ) of a second.</th>
<th>The hour of the observation of ( \odot ) from parent altitude.</th>
<th>The Altitude of the Sun.</th>
<th>The Declination of ( \odot ) in reference to 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 ( \odot ) appeared by the difference of time between the Transit of ( \odot ) and the West limb of the Sun by the same meridian. Then from the Declination and right Ascension of ( \odot ) given by Trigonometrical Calculation, he found out the Longitude and Latitude of it in every Observation, and the time of its true Conjunction.</th>
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<td>M. S. vib. Pend.</td>
<td>M. S. T.</td>
<td>G. M. H. M. S.</td>
<td>M. S. T.</td>
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<td>G. M. H. M. S.</td>
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<td>55</td>
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<td>258</td>
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<td>26</td>
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The time of the Transitus of the Sun. 414 2 18 34 30 0 43 0 0

the Parallel. The Diameter in a great Circle. The Diameter in

of Mercury. 31 0 1 10 0 17 30.
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 adjoined this ensuing Table, to shew how much the Heavens do differ from the Astronomical Tables.

<table>
<thead>
<tr>
<th>The time of the phases observed</th>
<th>Heckers place of the</th>
<th>The Decl. of Υ in m</th>
<th>The right Ascension of Mercury</th>
<th>The N.Lat. of Υ in m</th>
<th>The Long. of Mercury in m</th>
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<tbody>
<tr>
<td>H. M. S.</td>
<td>G. M. S.</td>
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<td>16 53 58</td>
<td>15 33 55</td>
<td>16 32 33</td>
<td>223 16 40</td>
<td>3 10</td>
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<tr>
<td>2</td>
<td>12 00</td>
<td>15 36 41</td>
<td>16 31 38</td>
<td>223 13 43</td>
<td>3 14</td>
</tr>
<tr>
<td>3</td>
<td>09 55</td>
<td>15 37 61</td>
<td>16 30 43</td>
<td>223 12 37</td>
<td>3 53</td>
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<td>4</td>
<td>03 50</td>
<td>15 38 11</td>
<td>16 30 71</td>
<td>223 10 51</td>
<td>3 55</td>
</tr>
<tr>
<td>5</td>
<td>44 10</td>
<td>15 41 03</td>
<td>16 29 72</td>
<td>223 8 54</td>
<td>4 15</td>
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<tr>
<td>6</td>
<td>55 22</td>
<td>15 41 31</td>
<td>16 28 12</td>
<td>223 7 59</td>
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<td>11 58</td>
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<td>223 6 45</td>
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<td>223 4 55</td>
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<td>26 56</td>
<td>15 45 23</td>
<td>16 26 15</td>
<td>223 3 50</td>
<td>4 58</td>
</tr>
</tbody>
</table>

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 adjoined this ensuing Table, to shew how much the Heavens do differ from the Astronomical Tables.

Tempus datum ex Tabulis Novemb. | 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 calculus. 7 8 17 0 7 38 | excessus. |
R. admodum Patris Bonif. Societatis Jesu. 7 8 17 0 7 38 | excessus. |

Upon
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 suppos'd 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 Advertifement 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 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 happened 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 public; 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. Edmond 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 Celestial 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 Majesty's 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 and
and egress of ☉ on the ☿, which compared with the like Observations made in England, will give a demonstration of the Sun's 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 hindered 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 Nubecula called by the Seafers the Magellanick Clouds, are both of them exactly like the whiteness of the milky way lying within the Antarctic 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 certify 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 assistent 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. Helene, Latitudo Australis, 15. 55.

Anno 1677.

Octobris 28. die mane apparuit intra.

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<tr>
<td>2</td>
<td>41</td>
<td>54</td>
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</tbody>
</table>

Pars aliqua corporis intra Sollem decem gradus à Nadir ad dextram circiter.

Formabat angulum contactus totus scilicet intus.

Limbus proximus dixit à limbo Solis sui Diametro.

Centrum exiit à Sole 30 grad. circiter à Nadir ad dextram.

Limbus integer factus.

Longitudo & Latitudo trium Stellarum illustrium prope polum austrinum.

<table>
<thead>
<tr>
<th></th>
<th>Long.</th>
<th>Latit.</th>
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<tbody>
<tr>
<td>Canopus</td>
<td>11 3 5</td>
<td>75 49</td>
</tr>
<tr>
<td>Centauri pes</td>
<td>25 24 m</td>
<td>42 22</td>
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<tr>
<td>Alcarnar.</td>
<td>10 31 x</td>
<td>59 18 3</td>
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</tbody>
</table>

[77]
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.

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 establishishng 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 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 arise
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 $x$ from the $\odot$, and that they are a little slower when $x$ 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 $x$ 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.

\[ \text{Differentiam inter medium locum Jovis & apparentem convertite in tempus danno singulis gradibus min. } \text{13. hoc tempus adde tempori restitutionis maculae supputato, si locus apparentes Jovis exesset medium: subtrahere vero si defecerit a medio.} \]

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 Flammino de Mezzavachi) must be made use of.
MICROSCOPIIUM:

OR,

Some new Discoveries made with and concerning Microscopes.

A Letter of the Ingenious and Inquisitive Mr. Leeuwenhoek 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 affirmations, that I might satisfy 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 justify (that they might be within compass) half the number that they believed each of them saw in the water, and even so the number of those little creatures that would thereby be proved to be in one drop of water would be so great, that it would exceed belief. Now whereas 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
have varied from the truth of it, if I had asserted that there were 8,000,000; 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 45,000 animalcules. It would follow that in an ordinary drop of this water there would be no less than 4,140,000 living creatures, which number if doubled will make 8,280,000 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 8,000,000 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. Leenhoecck 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. Leeuwenhoek 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. Leeuwenhoek 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.
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 ventured again to send you some of my farther Enquiries, 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 indefatigably 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 Globules 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 possible, 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 imagine 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 pliability receive many sorts of figures, and coming thence into a larger place, they recovered their former
former globulosity which was a very great pleasure to observe: and withal, that the Globules of blood coming many together, and growing cold thereby, came to unite, and made a matter very smooth, wherein there were no more parts distinct to be taken notice of, much after the same manner as if we supposed a Dish filled with balls of wax set over a fire, by which they would quickly be melted together, and united into one mass; by which uniting of the Globules, I concluded this to be the reason of the accident which is called the cold fire, and of that also which causes the hands or fingers to be lost by cold: but I leave this to others. And I did very clearly also discover that there were six other smaller Globules of blood contained within each of the former and larger Globulous Vesicles: and withal, I took much pains to observe the number of the same very small globules, out of which the greater Globules do consist: that at last I strongly imagined, that every of the greater Globules consisted of six smaller Globules, no less pliable than the aforesaid: for oftentimes I saw very clearly how the small Globules joyned and adapted themselves according to the figure the Vesicle or larger Globule stretched at length had taken, being themselves stretched after the same manner: and thus made one of the larger Globules stretcht out, to appear by the lesser within it stretcht also with it, as if it consisted of long threads. Moreover, I put the greater Globules into so violent a motion, that their Vesicles burst in pieces, and then the lesser Globules appeared plainly to be scattered: This first Globule I can see as plainly and great, as with the naked eye one should look upon the eggs or spawn of a Cod-fish.

About nine or ten years since Dr. Graff opened in my presence the vein of a Dog, and let out so much blood that the Dog grew faint; then he opened the Artery of another Dog, and by a pipe transfused the
blood of this second into the first, whereby the first
was recovered, the second was faint. Then the said Dr.
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 fame 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-
ystal; save only that through and between the same
drive some irregular particles for the most part round-
ed: 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 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 incredible number of small clear Globules, which makes it appear also as white as Milk itself: and thence I suppose that the whiteness of Milk hath the same cause.

I have been often minded by some, that flesh was nothing else but clotted blood; yet for all my endeavours 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 voided 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 consist 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 proceeded 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 observed 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 cuticles: and in the same manner as I have heretofore explained how our cuticle is supplied underneath, as the upper part is rubbed off in surf, so I suppose the inner.
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 fly, 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 yourself and the Society had seen in so small a quantity 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 1/4 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 one's head, and three or four times as long as thick; the which made, with the hinder part of their body, oftentimes 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 hair's breadth; the other sorts or kind of which were yet smaller, whose shape for brevity I omit; only I shall say, that oft-
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 motions 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 vinegar: Whether you have also observed these small Creatures with your Microscope, I shall be glad to understand. 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. Leeuwenhoek 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 enabled 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. Leeuwenhoek 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 itself 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 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 wick 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 wick do lie and come above the sides: this cover may be turned back on its hinges when there is occasion to raise the wick, 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 soldering 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, be-
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. Leenwenhoeck's, 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 myself for my own trials, at least for those Microscopes I make use of; what it may be for those which Mr. Leenwenhoeck 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 fastened to it a joyned arm of three or four joints, 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 swammed 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 phenomena of fluids of differing figures and refractions, the examinations of substances this way will be very apt to misinform, 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 examiner, 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. Leemmenhoeck, 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 everer 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.
But there are other substances which none of these ways I have yet mentioned will examine, and these 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 fabric 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 fabric thereof, and what causes its motion. Thus if you view a thread 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 Aristotle 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. Leeuwenhoek 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 Mi-
Microscope itself, 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 everywhere 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 polished in the Tool, the more clear and distinct doth the object appear, but to make any of a Sphere less than $\frac{1}{2}$ 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 polished, '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 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 easy 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 polished 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 jointed 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
sphere, without the superfcies 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 differ-
ing specific refraction of the glafs made use of.

By this means I have prodigiously magnified some
small bodies, infsomuch that I have been able to fee 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 land;
so prodigiously do these exceeding little Globules of
glafs 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. Leeuwenhoek
I newly mentioned, or else they may be put upon ex-
ceeding thin plates of Muscovy glafs 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 hurtng the prospect by
the interposition of the Muscovy glafs; 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 great-
est clearness and brightness that any one kind of Mi-
croscopes can possibly be imagined susceptible of, when
you have fixt one of these little Globules as I have di-
rected, 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 specific refractive, 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 published 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, 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 where
where to be accommodated with such as are good.

And of the improvement of this kind of Micro-
scope, I see no limits, especially as to the augmenting
the visible appearance of such objects as are capable of
ending the increase of light; for since 'tis demonstra-
tive 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 assign-
ed magnitude, what but the difficulty of making all
things correspondent should limit the power of such an
instrument. Now the making of this double Micro-
scope, though it be somewhat more difficult than of the
single one, yet the tractableness thereof when well fit-
ted, 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 Con-
vex 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 ex-
actly shaped, and the brighter they are polished, the
clearer do they represent it; and the longer the glasses
are distant from each other, the more is the image mag-
nified, ceteris paribus, though indeed the same thing is
performed by glasses of very differing magnitudes, due
proportions of all things about it being kept and ob-
served. For if as the distance of one object from the
object-glass is to the distance of another object from an-
other 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-Glafs, and length of the Tube: and secondly, by the augmenting that image in the bottom of the eye, and that is by the Eye-glafs; 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 seul point de l'objet; Wherein the Author pretends amongst other things to have promoted Microscopes extremely 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
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 equaling 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, Anis seeds, 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 as big as that of an hair cut transversely, and 27000 to make a Cylinder as big as the hair of one's 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 sum 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 transversely as 40000000 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 800000000000 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, Joints, 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, Flegm, &c. and found them much the same with what Mr. Leewenhoek 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 parallelepipeds, 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
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 Williamjon of Lifcard in Cornwall, 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 Piftol 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. amygd. &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 symptoms became more fierce, depaupering his spirits, prostrating his appetite, disquieting his sleep with dreams, a Dyspnæa, 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 coltive) he frequently fainted with sweats, and a tickling sleepiness in both legs.
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 despit of any endeavours to eject it: yet to alleviate the violence of the accidents, he directed to the use of emollient Ecleuma’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 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 purulent foetid 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 flight 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 effervescency, 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 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 symptoms under which he laboured; but seemed to differ from his presage, by hoping he might expectorate it: to achieve 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 verify Dr. Bidgood's 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 symptoms before mentioned, continuing until the Winter, and then gaining considerably on him, especially the Hemoptysis, &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 Bullet's being there, was so much doubted by many, and disputed as impossible by others. In the dissection the following particulars were observables,

The Body was extenuate and tabid,
The right lobes of the Lungs were replete, found, 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 Pigeon's Egg; near which lay also a substance, shaped like an
obtuse headed muscle, having a Tendon-like tail, which infinuated 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 theweth 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 absces which had discharged near a pint of very faetid 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 found; 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 hardnes, erosion, &c. though the Bullet had divers impressions from the later.
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 tenfe, and apprehension of some of those Phænomena here related.

The extenuation of the body, the assumption 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 Tulpin, Marcellus Malpighius, G. Garnatus, &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 spongy; 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 whatever circumstances it may resemble a Polypus, as it doth the figure of that of the Nose, vide N. Tulpian ob.med. lib.1. obs. 26. yet it also differs from all other ex-
crescences, besides, in what hath been mentioned, in that it was not rooted in any fleshly, bony, or muscular part; and such the Lungs are well known not to be: it must therefore be the stupendious effect of Natures industry, and laid as a cushion to defend the Heart, &c. Its composition being so delicately soft, and yet firm enough for such a purpose: Its magnitude, situation, &c. concurring also to confirm this opinion concerning it; besides which, I do almost remember, and believe (though I cannot be positive) that the pulsant pain he had so violently in his Breast, toward the left side, decreased gradatim, from the time of the deglutition: if that be true, whatever the substance were, or its cause, its effects were very propitious, manifesting nature to be, not only a diligent supplier of her own defects, but as industrious to produce strange and unaccountable relief, in such emergencies as this before us: A resembling story we have from A Parens, lib. 8. cap. 15.

The abscess was without doubt from a Phlegmon of the Lungs; and because for the most part it was below, or beyond the Bullet, it proceeded rather from its obstructing, and so stagnating the Blood, and increments in that Lobe, than from extravasation. What occurred of the latter, was expectorated, or remained in such Coagulums as that found under the Heart.

The cause of the Bullets falling, rather into the left than the right Ramus of the Trachea, is obvious from the more supine and direct figure thereof, corresponding with the trunk, as the figure doth manifest: which consideration, together with the Bullets being loose in the pipe, renders the unsuccessful of Dr. Mayow's attempt very wonderful: I am inclined to believe it was so, either for want of a more early trial, or a more skilful tryer, than him who was employed about it. The way was ingeniously contrived, and (as the Doctor himself told me) had been successfully experienced in the like occasion. Certainly, had not the distance of the Doctors abode, and very important avocations,
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 Weapon, 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:
fence 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 Leipsicke, Doctors, hath it a Parallel.

This, and whatever is else contained in this History, as my sick, I submit to the better sense, and reason of the Learned, not presuming to be positive in any thing, save in assuring 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 Blaffius, 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, diversicated into the rotten Lobe.

H the Bullet in the pipe where it was found.

ERRATA.

P

Tab. III.

Fig. vi pag. 26 l. 29

Horaxij Hypothesis
Tab. III.

Fig. 13

Fig. 14

Fig. 15

Fig. 16

Fig. 17

Fig. 18
Tab. V. Hypothesis of Chr. Wren. Eqv.
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.

LONDON,
Printed for John Martyn Printer to the Royal Society,
at the Bell in St. Paul's Church-Yard, 1678.
Potentia Restitutiva, or Spring.

He 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., e i i i n o s s t t u n, 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 one
one ounce, or one pound, or one certain weight
dothen it one line, or one inch, or one cer-
tain 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 pro-
ceed, 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 pro-
vide 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 suffer-
ning 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 In-
dex; then add a third drachm, and do as before, and
so a fourth, fifth, sixth, seventh, eighth, &c. suffer-
ing 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 fix'd 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 v w, 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 v, &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 v, 8.

The second figure represents a Watch Spring coiled in a Spiral by CABBBD, 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 yy y y, upon which is coiled 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 are so plain they need not an explication by figure, and the way of trying upon Air I have long since explained in my Micography 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 advantageous or disadvantagious ways of bending them.
From this Principle it will be easy 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 Ballistæ 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 easy 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 easy 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, cediinnoopssssstunu, namely, Ut pondus scetensfio. The fabric 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,
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 proposed 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 Phenomena 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 Phenomenon.
By Congruity and Incongruity then I understand nothing else but an agreement or disagreement of Bodies 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 Densification 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 owes to Body according to the common notion thereof: Which is, that
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 incom- passeth 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.
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 near the Earth are encompassed 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 encompassing 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 reperculated 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 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 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 join together, and make up one solid body, not perfectly everywhere 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 Phenomena of springing bodies whether fluid or solid.

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 oc-
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 occurrions 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 occurrions, 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 balance to it in its natural state) the length of the Vibrations shall be proportionably diminished, and the number of them, and consequently of the occurrions 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 easy to explain the compound way of springing, that is, by flexure, supposing only two of these lines joined together as at \( GHIK \), which being by any external power bended into the form \( LNO \), \( LM \) will be extended, and \( NO \) will be diminished in proportion to the flexure, and consequently the same proportions and Rules for its endeavour of restoring itself 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 encompassing the earth. It is bounded but on one side, namely, towards the earth, and is indefinitely extended upward being only hindered 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, conjoin 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 proportionally I suppose that its motion must be eight thousand times swifter than the former, and above a thousand times swifter than the latter. 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 Occurrences will be increased in reciprocal proportion, that is, if it be Condensed into half the space the Vibrations and Occurrences will be double in number: If into a quarter the Vibrations and Occurrences will be quadruple, &c.

Again, If the containing 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 Occurrences 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 Occurrences will be fewer by half, and consequently its intentions 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 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 easy 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 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 the
the powers, that is, as the Root of the powers impressed, and the powers impressed 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 AC, (C being the point from which the Spring is supposed let go) a Circle be described as CGGF, and ordinates drawn from any point of CA the space to be past, as from B, B, to the said Circle, as BG, BG, these Lines BG, BG, 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 CDEB, CDEB for putting AC = a, and AB = b, BG will always be equal to \( \sqrt{a^2 - bb} \), 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; BG, BG, AF, to find the times corresponding; on the Diameter AC draw a Parabola CHF whose Vertex is C, and which passes through the point F. The Ordinates of this Parabola BH, BH, AF, are in the same proportion with the Roots of the spaces CB, CB, CA, then making GB to HB as HB to IB, and through the points CIIF drawing the curve CIIF, the respective ordinates of this curve shall represent the proportionate time that the Spring spends in returning the spaces CB, CB, CA.

If the powers or stiffness of the Spring be greater than what I before supposed, and therefore must be expressed by the Triangle CDEA, then the Velocities will be the Ordinates in an Ellipse as \( C \gamma \gamma N \), 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 AF at a point as X within the point F. But if the powers of the Spring be weaker than I supposed, then will C \& \&e A represent the powers, and C \gamma \gamma O the Ellipses of Velocity,
Velocity, whose Ordinates $B_2$, $B_2$, $A_0$ 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 easier 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 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 factus 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 $ab$ 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 $bf$ with a Velocity as much swifter than that equal motion of 16 foot in a second, as this Line $bf$ is longer than $ab$ the body shall fall at $e$; for in the same space of time that the oblique equal motion would make it ascend from $bd$ to $ac$, will the accelerated direct motion downward move it from $ac$ to $bd$, 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 $be$, and for the points it passeth through in all the intermediate spaces this method will determine it.

Let the Parallelogram $abpq$ then represent the whole Velocity of the ascent of a body by an equal motion of 16 foot in a second, and the Triangle $pqrs$ 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 \textit{Jactus} given by this Scheme or Scale, appropriated to the particular Velocity, where-with any body is moved in this or that line of Inclination, it will be easy 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 enlarge 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 \textit{Wadham} Colledge, in the Garden of the learned Dr. \textit{Wilkins}, late Bishop of \\textit{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 Publicque by this following Description, which he sent me some time since.
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. Boyle's (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 soldered 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 soldered 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 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 soldered 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 have or button down the ends L L 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
aforefaid Buttons or Halsps 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,
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 containing 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 justify 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
It's apearans SSW from you about 6 Leagues distant.
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 Hogheads, 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 straining gently through those holes in the sides of the Cask: After which, in a days time, one man attending it, may draw about ten Hogheads or more of water, a little tasting of Salt, not so much but that it is drinkable, and very fit to boil meat in, and is used by those that come there to load Cattle, for their 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 Religious. 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 dryness was by the people thought to be subterraneous 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 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 inferior earth was clammy, or like clay, that had some greasy gummous matter commixed. This the Religious told me was the very kind of Sulphur which constantly boiled 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, Plimmonth, 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 enlarged) the Well was not above eight foot from the edge thereof, over which the Sea would frequently flow, when a high outwind 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 faith they there want fresh water, and are con-

strained
drained 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 consequences do mechanically result.

From the first it appeareth that some Springs have manifestly their source from the Sea; that sand sweetsens 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) that after their showers it could run but little larger than 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 tye 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 supposed 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 manifestly
manifestly existent, and with all the advantages necessary: It seeming to me a very rational conjecture, that the greasy 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 saltiness, 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 assuage 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 wanted 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 off. — I was going to make some reflections on this notable accident, but I consider, &c. —

Plimouth
May 5, 1678.

James Young.
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 blood 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 condenses 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 & locks; 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 moist 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 Dorking in Surrey, 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
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 Pikc. 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 flabby and moist, and the earth to slip underneath his feet, being a very moist 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 adjoining, 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 moisture 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 moisture of the Air, and converting it into water, and thereby to supply the Springs and Rivulets with fresh water: And I am confident, who

foever shall consider his own observation of this na

ture, and compare them with this Theory, will find

many arguments to confirm it. However, Nullius in

verba.
verba, Let Truth only prevail, and Theories signify 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 subjoined 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, penned 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 Gloucestershire, 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 persuaded 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 Stagnæ'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 three-score and ten paces, till he just lost sight of me, and from thence cheerfully 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 persuade 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, Enamedel with Lead Oare, and the Pendent Rocks were glazed with Salt-Petre 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 earthly 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 Francis, 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 moist 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 palled 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 ascend the Pike itself. 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 Vi\textit{v}etal 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 \textit{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 heap-ing 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
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 venture 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 enterprize, 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 clambering 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 fastened 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 itself.

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 gleeing and dropping of water in six or seven places from the sides of the Cave, which dropings he supposes may be greater or less according as...
thofe fogs do more or lefs encompass it, or flay 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 thofe droppings, but fupposes them to proceed from the aforesaid fogs. Hence he concludes when the Air is clear, and none of thofe fogs condensed about the Hill, the water in the Cave muft necessarily decrease. And that which 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 fo very moyft, that it was like mud or mortar, and might be made into Paffe as by experiment he found which he conjectures could no ways be caused by the wind or clear Air, which is rather drying and confluming of moisture, but muft proceed from the fogs or mifts 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 faltifh taste, some of which he gathered and brought back with him to England to have it examined.

After about half an hours flay 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 ftony way, which lafted till they came to the foot of that part of the Mountain which is called the Sugar-loaf, by reafon that at a distance from the Island it appears of that fhape, 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 fteep and ascending than the former part of the ftony way, and extreme troublesome to pas, their feet finking and flipping down again almoft as much as they could stride upwards, fo that they concluded it the moft painful of all; however, perfiting in their endeavours, after many times refting themfelves, 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 outside of this top this Gentleman conceived might be about a quarter of a mile round about on the outside.

This hole he conceived to be the mouth of a *Vulcano* which hath formerly been in that place, for even at that time whilst they were there much smoak ascended out of several holes and chinks of the Rocks, and the earth in divers parts was still so very hot as to be very offensive to their feet through their shoes, and he observed Brimstone thrown up in several places, of which he collected some, and brought back with him to *England*.

From this place may be seen in a clear day all the six adjacent Islands, but the weather being then somewhat thick and hazy, they could discover none but the grand *Canaries, Palm*, and the *Gomera*, which last, though distant near eight Leagues from the bottom of the *Pike* seemed yet so near unto them as if it had been almost under them. The rest of the Islands they could discover whereabout they lay by means of a kind of white cloud hanging on them, but they could not discern the Islands through those clouds.

Here they tried their Cordial Waters which they carried in their Pockets, but found them not to abate of their usual strength, and become cold and insipid as fair water, as several had positively averred to him that they had found it, but he conceived them to be very much of the same nature and strength that they were of before they were carried up, which he supposes to be by reason of their arriving at the top so late.

After they had stayed on the top about an hour, and satisfied themselves in observing such things as they were able, they descended 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.

In 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 sulphurous 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 shoes 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.
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 giveninstances, 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...
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 ether, 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 petrification 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 reposeth 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 subjoined 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 Coelestial 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-
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 Enencaliente, 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 noife 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 greatefl 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 wearisom 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 judgment, they were five times longer in falling down,
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.

Monday 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 smokèd 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 stop't, 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.
Friday the twenty sixthth, 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 Tacofos; 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 Mountain was observed to be more quiet, though it cast up much ashes like black small sand. What damage 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 happened, and yet continues with much terror, 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 least, 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 continued 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. Vibration thereof, but all Solids do exclude that menstruum, or participate not of its motion. p. 14. l. 11. for lengthr. 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. 7. or South-cast side. p. 42. l. 9. for Francis I. Francis.