ber of little Articles neceſſary to the Practice, the Author refers them to another Time, as more properly belonging to the Deſcription of the whole Art, than to a Memoir in which he only gives the Principles of it.

IV. *A Letter from the Reverend Mr.* James Bradley *Savilian Profeſſor of Aſtronomy at* Oxford, *and F.R.S. to Dr.* Edmond Halley Aſtronom. Reg. &c. *giving an Account of a new diſcovered Motion of the* Fix'd Stars.

SIR,

YOU having been pleaſed to expreſs your Satisfaction with what I had an Opportunity ſometime ago, of telling you in Converſation, concerning ſome Obſervations, that were making by our late worthy and ingenious Friend, the honourable *Samuel Molyneux* Eſquire, and which have ſince been continued and repeated by my ſelf, in order to determine the *Parallax* of the *fixt Stars*; I ſhall now beg leave to lay before you a more particular Account of them.

Before I proceed to give you the Hiſtory of the Obſervations themſelves, it may be proper to let you know, that they were at firſt begun in hopes of verifying and confirming thoſe, that Dr. *Hook* formerly communicated to the publick, which ſeemed to be attended with Circumſtances that promiſed greater Exactneſs in them, than could be expected in any other, that had been made and publiſhed on the ſame Account. And as his Attempt was what principally gave Riſe to this, ſo his Method in making the Obſervations was in ſome Mea-

Meaſure that which Mr. *Molyneux* followed: For he made Choice of the ſame Star, and his Inſtrument was conſtructed upon almoſt the ſame Principles. But if it had not greatly exceeded the Doctor's in Exactneſs, we might yet have remained in great Uncertainty as to the *Parallax* of the *fixt Stars*; as you will perceive upon the Compariſon of the two Experiments.

This indeed was chiefly owing to our curious Member, Mr. *George Graham*, to whom the Lovers of Aſtronomy are alſo not a little indebted for ſeveral other exact and well-contrived Inſtruments. The Neceſſity of ſuch will ſcarce be diſputed by thoſe that have had any Experience in making Aſtronomical Obſervations; and the Inconſiſtency, which is to be met with among different Authors in their Attempts to determine ſmall Angles, particularly the annual Parallax of the *fixt Stars*, may be a ſufficient Proof of it to others. Their Diſagreement indeed in this Article, is not now ſo much to be wondered at, ſince I doubt not, but it will appear very probable, that the Inſtruments commonly made uſe of by them, were liable to greater Errors than many times that Parallax will amount to.

The Succeſs then of this Experiment evidently depending very much on the Accurateneſs of the Inſtrument that was principally to be taken Care of: In what Manner this was done, is not my preſent Purpoſe to tell you; but if from the Reſult of the Obſervations which I now ſend you, it ſhall be judged neceſſary to communicate to the Curious the Manner of making them, I may hereafter perhaps give them a particular Deſcription, not only of Mr. *Molyneux*'s Inſtrument, but alſo of my own, which

which hath ſince been erected for the ſame Purpoſe and upon the like Principles, though it is ſomewhat different in its Conſtruction, for a Reaſon you will meet with preſently.

Mr. *Molyneux*'s *Apparatus* was compleated and fitted for obſerving about the End of *November* 1725, and on the third Day of *December* following, the bright Star in the Head of *Draco* (marked γ by *Bayer*) was for the firſt Time obſerved, as it paſſed near the Zenith, and its Situation carefully taken with the Inſtrument. The like Obſervations were made on the 5th, 11th, and 12th Days of the ſame Month, and there appearing no material Difference in the Place of the Star, a farther Repetition of them at this Seaſon ſeemed needleſs, it being a Part of the Year, wherein no ſenſible Alteration of Parallax in this Star could ſoon be expected. It was chiefly therefore Curioſity that tempted me (being then at *Kew*, where the Inſtrument was fixed) to prepare for obſerving the Star on *December* 17th, when having adjuſted the Inſtrument as uſual, I perceived that it paſſed a little more Southerly this Day than when it was obſerved before. Not ſuſpecting any other Cauſe of this Appearance, we firſt concluded, that it was owing to the Uncertainty of the Obſervations, and that either this or the foregoing were not ſo exact as we had before ſuppoſed; for which Reaſon we purpoſed to repeat the Obſervation again, in order to determine from whence this Difference proceeded; and upon doing it on *December* 20th, I found that the Star paſſed ſtill more Southerly than in the former Obſervations. This ſenſible Alteration the more ſurprized us, in that it was the contrary way

way from what it would have been, had it proceeded from an annual *Parallax* of the *Star*: But being now pretty well ſatisfied, that it could not be entirely owing to the want of Exactneſs in the Obſervations; and having no Notion of any thing elſe, that could cauſe ſuch an apparent Motion as this in the Star; we began to think that ſome Change in the Materials, *&c.* of the Inſtrument itſelf, might have occaſioned it. Under theſe Apprehenſions we remained ſome time, but being at length fully convinced, by ſeveral Trials, of the great Exactneſs of the Inſtrument, and finding by the gradual Increaſe of the Stars Diſtance from the Pole, that there muſt be ſome regular Cauſe that produced it; we took care to examine nicely, at the Time of each Obſervation, how much it was: and about the Beginning of *March* 1726, the Star was found to be 20″ more Southerly than at the Time of the firſt Obſervation. It now indeed ſeemed to have arrived at its utmoſt Limit Southward, becauſe in ſeveral Trials made about this Time, no ſenſible Difference was obſerved in its Situation. By the Middle of *April* it appeared to be returning back again towards the North; and about the Beginning of *June*, it paſſed at the ſame Diſtance from the Zenith as it had done in *December*, when it was firſt obſerved.

From the quick Alteration of this Star's Declination about this Time (it increaſing a Second in three Days) it was concluded, that it would now proceed Northward, as it before had gone Southward of its preſent Situation; and it happened as was conjectured: for the Star continued to move Northward till *September* following, when it again became ſtationary,

tionary, being then near 20″ more Northerly than in *June*, and no leſs than 39″ more Northerly than it was in *March*. From *September* the Star returned towards the South, till it arrived in *December* to the ſame Situation it was in at that time twelve Months, allowing for the Difference of Declination on account of the Preceſſion of the Equinox.

This was a ſufficient Proof, that the Inſtrument had not been the Cauſe of this apparent Motion of the Star, and to find one adequate to ſuch an Effect ſeemed a Difficulty. A Nutation of the Earth's Axis was one of the firſt things that offered itſelf upon this Occaſion, but it was ſoon found to be inſufficient; for though it might have accounted for the change of Declination in γ *Draconis* yet it would not at the ſame time agree with the Phænomena in other Stars; particularly in a ſmall one almoſt oppoſite in right Aſcenſion to γ *Draconis*, at about the ſame Diſtance from the North Pole of the Equator: For, though this Star ſeemed to move the ſame way, as a Nutation of the Earth's Axis would have made it, yet it changing its Declination but about half as much as γ *Draconis* in the ſame time (as appeared upon comparing the Obſervations of both made upon the ſame Days, at different Seaſons of the Year) this plainly proved, that the apparent Motion of the Stars was not occaſioned by a real Nutation, ſince if that had been the Cauſe, the Alteration in both Stars would have been near equal.

The great Regularity of the Obſervations left no room to doubt, but that there was ſome regular Cauſe that produced this unexpected Motion, which did not depend on the Uncertainty or Variety of the

Seaſons of the Year. Upon comparing the Obſervations with each other, it was diſcovered, that in both the fore-mentioned Stars, the apparent Difference of Declination from the *Maxima*, was always nearly proportional to the verſed Sine of the Sun's Diſtance from the Equinoctial Points. This was an Inducement to think, that the Cauſe, whatever it was, had ſome Relation to the Sun's Situation with reſpect to thoſe Points. But not being able to frame any Hypotheſis at that Time, ſufficient to ſolve all the Phænomena, and being very deſirous to ſearch a little farther into this Matter; I began to think of erecting an Inſtrument for my ſelf at *Wanſted*, that having it always at Hand, I might with the more Eaſe and Certainty, enquire into the Laws of this new Motion. The Conſideration likewiſe of being able by another Inſtrument, to confirm the Truth of the Obſervations hitherto made with Mr. *Molyneux*'s, was no ſmall Inducement to me; but the Chief of all was, the Opportunity I ſhould thereby have of trying, in what Manner other Stars were affected by the ſame Cauſe, whatever it was. For Mr. *Molyneux*'s Inſtrument being originally deſigned for obſerving γ *Draconis* (in order, as I ſaid before, to try whether it had any ſenſible Parallax) was ſo contrived, as to be capable of but little Alteration in its Direction, not above ſeven or eight Minutes of a Degree: and there being few Stars within half that Diſtance from the Zenith of *Kew*, bright enough to be well obſerved, he could not, with his Inſtrument, throughly examine how this Cauſe affected Stars differently ſituated with reſpect

reſpect to the equinoctial and ſolſtitial Points of the Ecliptick.

Theſe Conſiderations determined me; and by the Contrivance and Direction of the ſame ingenious Perſon, Mr. *Graham*, my Inſtrument was fixed up *Auguſt* 19, 1727. As I had no convenient Place where I could make uſe of ſo long a Teleſcope as Mr. *Molyneux*'s, I contented my ſelf with one of but little more than half the Length of his (*viz.* of about $12\frac{5}{8}$ Feet, his being $24\frac{1}{2}$) judging from the Experience which I had already had, that this Radius would be long enough to adjuſt the Inſtrument to a ſufficient Degree of Exactneſs, and I have had no Reaſon ſince to change my Opinion: for from all the Trials I have yet made, I am very well ſatisfied, that when it is carefully rectified, its Situation may be ſecurely depended upon to half a Second. As the Place where my Inſtrument was to be hung, in ſome Meaſure determined its Radius, ſo did it alſo the Length of the Arch, or Limb, on which the Diviſions were made to adjuſt it: For the Arch could not conveniently be extended farther, than to reach to about $6\frac{1}{4}^{\circ}$ on each Side my Zenith. This indeed was ſufficient, ſince it gave me an Opportunity of making Choice of ſeveral Stars, very different both in Magnitude and Situation; there being more than two hundred inſerted in the *Britiſh* Catalogue, that may be obſerved with it. I needed not to have extended the Limb ſo far, but that I was willing to take in *Capella*, the only Star of the firſt Magnitude that comes ſo near my Zenith.

My Inſtrument being fixed, I immediately began to obſerve ſuch Stars as I judged moſt proper to

give me light into the Cauſe of the Motion already mentioned. There was Variety enough of ſmall ones; and not leſs than twelve, that I could obſerve through all the Seaſons of the Year; they being bright enough to be ſeen in the Day-time, when neareſt the Sun. I had not been long obſerving, before I perceived, that the Notion we had before entertained of the Stars being fartheſt North and South, when the Sun was about the Equinoxes, was only true of thoſe that were near the ſolſtitial Colure: And after I had continued my Obſervations a few Months, I diſcovered, what I then apprehended to be a general Law, obſerved by all the Stars, *viz.* That each of them became ſtationary, or was fartheſt North or South, when they paſſed over my Zenith at ſix of the Clock, either in the Morning or Evening. I perceived likewiſe, that whatever Situation the Stars were in with reſpect to the cardinal Points of the Ecliptick, the apparent Motion of every one tended the ſame Way, when they paſſed my Inſtrument about the ſame Hour of the Day or Night; for they all moved Southward, while they paſſed in the Day, and Northward in the Night; ſo that each was fartheſt North, when it came about Six of the Clock in the Evening, and fartheſt South, when it came about Six in the Morning.

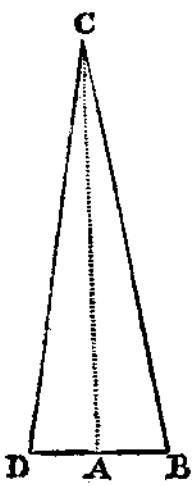
Though I have ſince diſcovered, that the *Maxima* in moſt of theſe Stars do not happen exactly when they come to my Inſtrument at thoſe Hours, yet not being able at that time to prove the contrary, and ſuppoſing that they did, I endeavoured to find out what Proportion the greateſt Alterations of Declination in different Stars bore to each other; it being very

very evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneaux*'s Obſervations, that γ *Draconis* altered its Declination about twice as much as the fore-mentioned ſmall Star almoſt oppoſite to it; but examining the matter more particularly, I found that the greateſt Alteration of Declination in theſe Stars, was as the Sine of the Latitude of each reſpectively. This made me ſuſpect that there might be the like Proportion between the *Maxima* of other Stars; but finding, that the Obſervations of ſome of them would not perfectly correſpond with ſuch an Hypotheſis, and not knowing, whether the ſmall Difference I met with, might not be owing to the Uncertainty and Error of the Obſervations, I deferred the farther Examination into the Truth of this Hypotheſis, till I ſhould be furniſhed with a Series of Obſervations made in all Parts of the Year; which might enable me, not only to determine what Errors the Obſervations are liable to, or how far they may ſafely be depended upon; but alſo to judge, whether there had been any ſenſible Change in the Parts of the Inſtrument itſelf.

Upon theſe Conſiderations, I laid aſide all Thoughts at that Time about the Cauſe of the fore-mentioned Phænomena, hoping that I ſhould the eaſier diſcover it, when I was better provided with proper Means to determine more preciſely what they were.

When the Year was compleated, I began to examine and compare my Obſervations, and having pretty well ſatisfied my ſelf as to the general Laws of the *Phænomena*, I then endeavoured to find out the Cauſe

Cauſe of them. I was already convinced, that the apparent Motion of the Stars was not owing to a Nutation of the Earth's Axis. The next Thing that offered itſelf, was an Alteration in the Direction of the Plumb-line, with which the Inſtrument was conſtantly rectified; but this upon Trial proved inſufficient. Then I conſidered what Refraction might do, but here alſo nothing ſatisfactory occurred. At laſt I conjectured, that all the *Phænomena* hitherto mentioned, proceeded from the progreſſive Motion of Light and the Earth's annual Motion in its Orbit. For I perceived, that, if Light was propagated in Time, the apparent Place of a fixt Object would not be the ſame when the Eye is at Reſt, as when it is moving in any other Direction, than that of the Line paſſing through the Eye and Object; and that, when the Eye is moving in different Directions, the apparent Place of the Object would be different.



I conſidered this Matter in the following Manner. I imagined C A to be a Ray of Light, falling perpendicularly upon the Line B D; then if the Eye is at reſt at A, the Object muſt appear in the Direction A C, whether Light be propagated in Time or in an Inſtant. But if the Eye is moving from B towards A, and Light is propagated in Time, with a Velocity that is to the Velocity of the Eye, as C A to B A; then Light moving from C to A, whilſt the Eye moves from B to A, that Particle of it,

it, by which the Object will be diſcerned, when the Eye in its Motion comes to A, is at C when the Eye is at B. Joining the Points B, C, I ſuppoſed the Line CB, to be a Tube (inclined to the Line BD in the Angle DBC) of ſuch a Diameter, as to admit of but one Particle of Light; then it was eaſy to conceive, that the Particle of Light at C (by which the Object muſt be ſeen when the Eye, as it moves along, arrives at A) would paſs through the Tube BC, if it is inclined to BD in the Angle DBC, and accompanies the Eye in its Motion from B to A; and that it could not come to the Eye, placed behind ſuch a Tube, if it had any other Inclination to the Line BD. If inſtead of ſuppoſing CB ſo ſmall a Tube, we imagine it to be the Axis of a larger; then for the ſame Reaſon, the Particle of Light at C, could not paſs through that Axis, unleſs it is inclined to BD, in the Angle CBD. In like manner, if the Eye moved the contrary way, from D towards A, with the ſame Velocity; then the Tube muſt be inclined in the Angle BDC. Although therefore the true or real Place of an Object is perpendicular to the Line in which the Eye is moving, yet the viſible Place will not be ſo, ſince that, no doubt, muſt be in the Direction of the Tube; but the Difference between the true and apparent Place will be (*cæteris paribus*) greater or leſs, according to the different Proportion between the Velocity of Light and that of the Eye. So that if we could ſuppoſe that Light was propagated in an Inſtant, then there would be no Difference between the real and viſible Place of an Object, altho' the Eye were in Motion, for in that caſe, AC being infinite with Reſpect to AB, the Angle ACB (the

ference between the true and visible Place) vanishes. But if Light be propagated in Time (which I presume will readily be allowed by most of the Philosophers of this Age) then it is evident from the foregoing Considerations, that there will be always a Difference between the real and visible Place of an Object, unless the Eye is moving either directly towards or from the Object. And in all Cases, the Sine of the Difference between the real and visible Place of the Object, will be to the Sine of the visible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

If Light moved but 1000 times faster than the Eye, and an Object (supposed to be at an infinite Distance) was really placed perpendicularly over the Plain in which the Eye is moving, it follows from what hath been already said, that the apparent Place of such an Object will be always inclined to that Plain, in an Angle of $89^\circ\ 56'\frac{1}{2}$; so that it will constantly appear $3'\frac{1}{2}$ from its true Place, and seem so much less inclined to the Plain, that way towards which the Eye tends. That is, if A C is to A B (or A D) as 1000 to one, the Angle A B C will be $89^\circ\ 56'\frac{1}{2}$, and A C B $= 3'\frac{1}{2}$, and B C D $=$ 2 A C B $= 7'$. So that according to this Supposition, the visible or apparent Place of the Object will be altered $7'$, if the Direction of the Eye's Motion is at one time contrary to what it is at another.

If the Earth revolve round the Sun annually, and the Velocity of Light were to the Velocity of the Earth's Motion in its Orbit (which I will at present suppose to be a Circle) as 1000 to one; then tis easy

to

to conceive, that a Star really placed in the very Pole of the Ecliptick, would, to an Eye carried along with the Earth, ſeem to change its Place continually, and (neglecting the ſmall Difference on the Account of the Earth's diurnal Revolution on its Axis) would ſeem to deſcribe a Circle round that Pole, every Way diſtant therefrom $3' \frac{1}{2}$. So that its Longitude would be varied through all the Points of the Ecliptick every Year; but its Latitude would always remain the ſame. Its right Aſcenſion would alſo change, and its Declination, according to the different Situation of the Sun in reſpect to the equinoctial Points; and its apparent Diſtance from the North Pole of the Equator would be $7'$ leſs at the Autumnal, than at the vernal Equinox.

The greateſt Alteration of the Place of a Star in the Pole of the Ecliptick (or which in Effect amounts to the ſame, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit) being known; it will not be difficult to find what would be the Difference upon this Account, between the true and apparent Place of any other Star at any time; and on the contrary, the Difference between the true and apparent Place being given; the Proportion between the Velocity of Light and the Earth's Motion in its Orbit may be found.

As I only obſerved the apparent Difference of Declination of the Stars, I ſhall not now take any farther Notice in what manner ſuch a Cauſe as I have here ſuppoſed would occaſion an Alteration in their apparent Places in other Reſpects; but, ſuppoſing the Earth to move equally in a Circle, it may be gathered from what hath been already ſaid, that a Star which

is neither in the Pole nor Plain of the Ecliptick, will ſeem to deſcribe about its true Place a Figure, inſenſibly different from an Ellipſe, whoſe Tranſverſe Axis is at Right-angle to the Circle of Longitude paſſing through the Stars true Place, and equal to the Diameter of the little Circle deſcribed by a Star (as was before ſuppoſed) in the Pole of the Ecliptick; and whoſe Conjugate Axis is to its Tranſverſe Axis, as the *Sine* of the Stars Latitude to the *Radius*. And allowing that a Star by its apparent Motion does exactly deſcribe ſuch an Ellipſe, it will be found, that if A be the Angle of Poſition (or the Angle at the Star made by two great Circles drawn from it, thro' the Poles of the Ecliptick and Equator) and B be another Angle, whoſe Tangent is to the Tangent of A as Radius to the Sine of the Latitude of the Star; then B will be equal to the Difference of Longitude between the Sun and the Star, when the true and apparent Declination of the Star are the ſame. And if the Sun's Longitude in the Ecliptick be reckoned from that Point, wherein it is when this happens; then the Difference between the true and apparent Declination of the Star (on Account of the Cauſe I am now conſidering) will be always, as the Sine of the Sun's Longitude from thence. It will likewiſe be found, that the greateſt Difference of Declination that can be between the true and apparent Place of the Star, will be to the Semi-Tranſverſe Axis of the Ellipſe (or to the Semi-diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick) as the Sine of A to the Sine of B.

If the Star hath North Latitude, the Time, when its true and apparent Declination are the ſame, is before

fore the Sun comes in Conjunction with or Oppoſition to it, if its Longitude be in the firſt or laſt Quadrant (*viz.* in the aſcending Semi-circle) of the Ecliptick; and after them, if in the deſcending Semi-circle; and it will appear neareſt to the North Pole of the Equator, at the Time of that *Maximum* (or when the greateſt Difference between the true and apparent Declination happens) which precedes the Sun's Conjunction with the Star.

Theſe Particulars being ſufficient for my preſent Purpoſe, I ſhall not detain you with the Recital of any more, or with any farther Explication of theſe. It may be time enough to enlarge more upon this Head, when I give a Deſcription of the Inſtruments *&c.* if that be judged neceſſary to be done; and when I ſhall find, what I now advance, to be allowed of (as I flatter my ſelf it will) as ſomething more than a bare Hypotheſis. I have purpoſely omitted ſome matters of no great Moment, and conſidered the Earth as moving in a Circle, and not an Ellipſe, to avoid too perplexed a *Calculus*, which after all the Trouble of it would not ſenſibly differ from that which I make uſe of, eſpecially in thoſe Conſequences which I ſhall at preſent draw from the foregoing Hypotheſis.

This being premiſed, I ſhall now proceed to determine from the Obſervations, what the real Proportion is between the Velocity of Light and the Velocity of the Earth's annual Motion in its Orbit; upon Suppoſition that the *Phænomena* before mentioned do depend upon the Cauſes I have here aſſigned. But I muſt firſt let you know, that in all the Obſervations hereafter mentioned, I have made an Allowance for the Change of the Star's Declination on Account of the Preceſſion of

the Equinox, upon Suppoſition that the Alteration from this Cauſe is proportional to the Time, and regular through all the Parts of the Year. I have deduced the real annual Alteration of Declination of each Star from the Obſervations themſelves; and I the rather chooſe to depend upon them in this Article, becauſe all which I have yet made, concur to prove, that the Stars near the Equinoctial Colure, change their Declination at this time 1″ ½ or 2″ in a Year more than they would do if the Preceſſion was only 50″, as is now generally ſuppoſed. I have likewiſe met with ſome ſmall Varieties in the Declination of other Stars in different Years, which do not ſeem to proceed from the ſame Cauſe, particularly in thoſe that are near the ſolſtitial Colure, which on the contrary have altered their Declination leſs than they ought, if the Preceſſion was 50″. But whether theſe ſmall Alterations proceed from a regular Cauſe, or are occaſioned by any Change in the Materials &c. of my Inſtrument, I am not yet able fully to determine. However, I thought it might not be amiſs juſt to mention to you how I have endeavoured to allow for them, though the Reſult would have been nearly the ſame, if I had not conſidered them at all. What that is, I will ſhew, firſt from the Obſervations of γ *Draconis*, which was found to be 39″ more Southerly in the Beginning of *March*, than in *September*.

From what hath been premiſed, it will appear that the greateſt Alteration of the apparent Declination of γ *Draconis*, on Account of the ſucceſſive Propagation of Light, would be to the Diameter of the little Circle which a Star (as was before remarked) would ſeem to deſcribe about the Pole of the Ecliptick, as 39″ to 40″,4. The half of this is the Angle A C B (as repreſented

ſented in the *Fig.*)This therefore being 20″, 2, A C will be to A B, that is, the Velocity of Light to the Velocity of the Eye (which in this Caſe may be ſuppoſed the ſame as the Velocity of the Earth's annual Motion in its Orbit) as 10210 to One, from whence it would follow, that Light moves, or is propagated as far as from the Sun to the Earth in 8′ 12″.

It is well known, that Mr. *Romer*, who firſt attempted to account for an apparent Inequality in the Times of the Eclipſes of *Jupiter*'s Satellites, by the Hypotheſis of the progreſſive Motion of Light, ſuppoſed that it ſpent about 11′ Minutes of Time in its Paſſage from the Sun to us: but it hath ſince been concluded by others from the like Eclipſes, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypotheſis, is as it were a *Mean* betwixt what had at different times been determined from the Eclipſes of *Jupiter*'s Satellites.

Theſe different Methods of finding the Velocity of Light thus agreeing in the Reſult, we may reaſonably conclude, not only that theſe *Phænomena* are owing to the Cauſes to which they have been aſcribed; but alſo, that Light is propagated (in the ſame *Medium*) with the ſame Velocity after it hath been reflected as before: for this will be the Conſequence, if we allow that the Light of the Sun is propagated with the ſame Velocity, before it is reflected, as the Light of the *fixt Stars*. And I imagine this will ſcarce be queſtioned, if it can be made appear that the Velocity of the Light of all the *fixt Stars* is equal, and that their Light moves or is propagated through equal Spaces in equal Times, at all Diſtances from them: both which points (as I apprehend) are ſufficiently proved from the apparent Alteration

ration of the Declination of Stars of different Luſtre; for that is not ſenſibly different in ſuch Stars as ſeem near together, though they appear of very different Magnitudes. And whatever their Situations are (if I proceed according to the foregoing Hypotheſis) I find the ſame Velocity of Light from my Obſervations of ſmall Stars of the fifth or ſixth, as from thoſe of the ſecond and third Magnitude, which in all Probability are placed at very different Diſtances from us. The ſmall Star, for Example, before ſpoken of, that is almoſt oppoſite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamſteed*'s Catalogue) was 19″ more Northerly about the Beginning of *March* than in *September*. Whence I conclude, according to my Hypotheſis, that the Diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick would be 40″, 2.

The laſt Star of the great Bear's-tail of the 2d Magnitude (marked η by *Bayer*) was 36″ more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greateſt Alteration of Declination of a Star in the Pole of the Ecliptick would be 40″, 4, exactly the ſame as was before found from the Obſervations of γ *Draconis*.

The Star of the 5th magnitude in the Head of *Perſeus* marked τ by *Bayer*, was 25″ more Northerly about the End of *December* than on the 29th of *July* following. Hence the *Maximum* would be 41″. This Star is not bright enough to be ſeen as it paſſes over my Zenith about the End of *June*, when it ſhould be according to the Hypotheſis fartheſt South. But becauſe I can more certainly depend upon the greateſt Alteration of Declination of thoſe Stars, which I have frequently obſerved about the Times when they become ſtationary,

nary, with reſpect to the Motion I am now conſidering; I will ſet down a few more Inſtances of ſuch, from which you may be able to judge how near it may be poſſible from theſe Obſervations, to determine with what Velocity Light is propagated.

α Perſei Bayero was 23″ more Northerly at the beginning of *January* than in *July*. Hence the *Maximum* would be 40″, 2. *α Caſſiopeæ* was 34″ more Northerly about the End of *December* than in *June*. Hence the *Maximum* would be 40″, 8. *β Draconis* was 39″ more Northerly in the beginning of *September* than in *March*; hence the *Maximum* would be 40″, 2. *Capella* was about 16″ more Southerly in *Auguſt* than in *February*; hence the *Maximum* would be about 40″. But this Star being farther from my Zenith than thoſe I have before made uſe of, I cannot ſo well depend upon my Obſervations of it, as of the others; becauſe I meet with ſome ſmall Alterations of its Declination that do not ſeem to proceed from the Cauſe I am now conſidering.

I have compared the Obſervations of ſeveral other Stars, and they all conſpire to prove that the *Maximum* is about 40″ or 41″. I will therefore ſuppoſe that it is 40″$\frac{1}{2}$ or (which amounts to the ſame) that Light moves, or is propagated as far as from the Sun to us in 8′ 13″. The near Agreement which I met with among my Obſervations induces me to think, that the *Maximum* (as I have here fixed it) cannot differ ſo much as a Second from the Truth, and therefore it is probable that the Time which Light ſpends in paſſing from the Sun to us, may be determined by theſe Obſervations within 5″ or 10″; which is ſuch a degree of exactneſs as we can never hope to attain from the Eclipſes of *Jupiter*'s Satellites.

Having thus found the *Maximum*, or what the greateſt Alteration of Declination would be in a Star placed in the Pole of the Ecliptick, I will now deduce from it (according to the foregoing Hypotheſis) the Alteration of Declination in one or two Stars, at ſuch times as they were actually obſerved, in order to ſee how the Hypotheſis will correſpond with the *Phænomena* through all the Parts of the Year.

It would be too tedious to ſet down the whole Series of my Obſervations; I will therefore make Choice only of ſuch as are moſt proper for my preſent Purpoſe, and will begin with thoſe of γ *Draconis*.

This Star appeared fartheſt North about *September* 7th, 1727, as it ought to have done according to my Hypotheſis. The following Table ſhews how much more Southerly the Star was found to be by Obſervation in ſeveral Parts of the Year, and likewiſe how much more Southerly it ought to be according to the Hypotheſis.

1727. D.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.	1728. D.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.
	"	"		"	"
October 20th --	4 1/2	4 1/2	*March* - 24	37	38
November - 17	11 1/2	12	*April* - - 6	36	36 1/2
December - 6	17 1/2	18 1/2	*May* - - 6	28 1/2	29 1/2
- - - 28	25	26	*June* - - 5	18 1/2	20
1728			- - - 15	17 1/2	17
January - 24	34	34	*July* - - 3	11 1/2	11 1/2
February - 10	38	37	*Auguſt* - 2	4	4
March - - 7	39	39	*September* - 6	0	0

Hence

Hence it appears, that the Hypotheſis correſponds with the Obſervations of this Star through all Parts of the Year; for the ſmall Differences between them ſeem to ariſe from the Uncertainty of the Obſervations, which is occaſioned (as I imagine) chiefly by the tremulous or undulating Motion of the Air, and of the Vapours in it; which cauſes the Stars ſometimes to dance to and fro, ſo much that it is difficult to judge when they are exactly on the Middle of the Wire that is fixed in the common Focus of the Glaſſes of the Teleſcope.

I muſt confeſs to you, that the Agreement of the Obſervations with each other, as well as with the Hypotheſis, is much greater than I expected to find, before I had compared them; and it may poſſibly be thought to be too great, by thoſe who have been uſed to Aſtronomical Obſervations, and know how difficult it is to make ſuch as are in all reſpects exact. But if it would be any Satisfaction to ſuch Perſons (till I have an Opportunity of deſcribing my Inſtrument and the manner of uſing it) I could aſſure them, that in above 70 Obſervations which I made of this Star in a Year, there is but one (and that is noted as very dubious on account of Clouds) which differs from the foregoing Hypotheſis more than 2″, and this does not differ 3″.

This therefore being the Fact, I cannot but think it very probable, that the *Phænomena* proceed from the Cauſe I have aſſigned, ſince the foregoing Obſervations make it ſufficiently evident, that the Effect of the real Cauſe, whatever it is, varies in this Star, in the ſame Proportion that it ought according to the Hypotheſis.

But leaſt γ *Draconis* may be thought not ſo proper to ſhew the Proportion, in which the apparent Altera-

tion of Declination is increaſed or diminiſhed, as thoſe Stars which lie near the Equinoctial Colure: I will give you alſo the Compariſon between the Hypotheſis and the Obſervations of *η Urſæ Majoris*, that which was fartheſt South about the 17th Day of *January* 1728, agreeable to the Hypotheſis. The following Table ſhews how much more Northerly it was found by Obſervation in ſeveral Parts of the Year, and alſo what the Difference ſhould have been according to the Hypotheſis.

1727.	d.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.	1728.	d.	The Difference of Declination by Obſervation.	The Difference of Declination by the Hypotheſis.
		"	"			"	"
September	14	29 1/2	28 1/2	*April*	16	18 1/2	18
	24	24 3/4	25 1/2	*May*	5	24 1/2	23 3/4
October	16	19 3/4	19 3/4	*June*	5	32	31 1/2
November	11	11 1/2	10 3/4		25	35	34 1/4
December	14	4	3	*July*	17	36	36
1728							
February	17	2	3	*Auguſt*	2	35	35 1/2
March	23	11 1/2	10 1/2	*September*	20	26 1/2	26 1/2

I find upon Examination, that the Hypotheſis agrees altogether as exactly with the Obſervations of this Star, as the former; for in about 50 that were made of it in a Year, I do not meet with a Difference of ſo much as 2″, except in one, which is mark'd

mark'd as doubtful on Account of the Undulation of the Air, *&c.* And this does not differ 3″ from the Hypotheſis.

The Agreement between the Hypotheſis and the Obſervations of this Star is the more to be reguarded, ſince it proves that the Alteration of Declination, on account of the Proceſſion of the Equinox, is (as I before ſuppoſed) regular thro' all Parts of the Year; ſo far at leaſt, as not to occaſion a Difference great enough to be diſcovered with this Inſtrument. It likewiſe proves the other part of my former Suppoſition, *viz.* that the annual Alteration of Declination in Stars near the Equinoctial Colure, is at this Time greater than a Preceſſion of 50″ would occaſion: for this Star was 20″ more Southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the Preceſſion was but 50″. But I may hereafter, perhaps, be better able to determine this Point, from my Obſervations of thoſe Stars that lie near the Equinoctial Colure, at about the ſame Diſtance from the North Pole of the Equator, and nearly oppoſite in right Aſcenſion.

I think it needleſs to give you the Compariſon between the Hypotheſis and the Obſervations of any more Stars; ſince the Agreement in the foregoing is a kind of Demonſtration (whether it be allowed that I have diſcovered the real Cauſe of the *Phænomena* or not;) that the Hypotheſis gives at leaſt the true Law of the Variation of Declination in different Stars, with Reſpect to their different Situations and Aſpects with the Sun. And if this is the Caſe, it muſt be granted, that the Parallax of the fixt Stars is much ſmaller, than hath been hitherto ſuppoſed by thoſe,

who have pretended to deduce it from their Obſervations. I believe, that I may venture to ſay, that in either of the two Stars laſt mentioned, it does not amount to 2″. I am of Opinion, that if it were 1″, I ſhould have perceived it, in the great number of Obſervations that I made eſpecially of γ *Draconis*; which agreeing with the Hypotheſis (without allowing any thing for Parallax) nearly as well when the Sun was in Conjunction with, as in Oppoſition to, this Star, it ſeems very probable that the Parallax of it is not ſo great as one ſingle Second; and conſequently that it is above 400000 times farther from us than the Sun.

There appearing therefore after all, no ſenſible Parallax in the fixt Stars, the *Anti-Copernicans* have ſtill room on that Account, to object againſt the Motion of the Earth; and they may have (if they pleaſe) a much greater Objection againſt the Hypotheſis, by which I have endeavoured to ſolve the fore-mentioned *Phænomena*; by denying the progreſſive Motion of Light, as well as that of the Earth.

But as I do not apprehend, that either of theſe Poſtulates will be denied me by the Generality of the Aſtronomers and Philoſophers of the preſent Age; ſo I ſhall not doubt of obtaining their Aſſent to the Conſequences, which I have deduced from them; if they are ſuch as have the Approbation of ſo great a Judge of them as yourſelf. I am,

Sir, Your moſt Obedient

Humble Servant

J. BRADLEY.

POSTSCRIPT.

AS to the Obſervations of Dr. *Hook*, I muſt own to you, that before Mr. *Molyneux*'s Inſtrument was erected, I had no ſmall Opinion of their Correctneſs: the Length of his Teleſcope and the Care he pretends to have taken in making them exact, having been ſtrong Inducements with me to think them ſo. And ſince I have been convinced both from Mr. *Molyneux*'s Obſervations and my own, that the Doctor's are really very far from being either exact or agreeable to the *Phænomena*; I am greatly at a Loſs how to account for it. I cannot well conceive that an Inſtrument of the Length of 36 Feet, conſtructed in the Manner he deſcribes his, could have been liable to an Error of near 30″ (which was doubtleſs the Caſe) if rectified with ſo much Care as he repreſents.

The Obſervations of Mr. *Flamſteed* of the different Diſtances of the Pole Star from the Pole at different Times of the Year, which were through Miſtake looked upon by ſome as a Proof of the annual *Parallax* of it, ſeem to have been made with much greater Care than thoſe of Dr. *Hook*. For though they do not all exactly correſpond with each other, yet from the whole Mr. *Flamſteed* concluded that the Star was 35″ 40″ or 45″ nearer the Pole in *December* than in *May* or *July*: and according to my Hypotheſis it ought to appear 40″ nearer in *December* than in *June*. The Agreement therefore of the Obſervations with the Hypotheſis is greater than could reaſonably be expected, conſidering the *Radius* of the Inſtrument, and the Manner in which it was conſtructed.