

## CRITICISMS AND DISCUSSIONS.

### THE REV. JAMES BRADLEY ON THE MOTION OF THE FIXED STARS.

(Reprinted from the *Philosophical Transactions* of 1727.)

[The theory of the relativity of time and space, which is at present uppermost in the minds of physicists, has come into the foreground mainly through the differences of measuring at large distances the time it takes light to reach the observer's eye which is further complicated by the motions of his own standpoint. This happened for the first time in the history of science in the year 1726 when Mr. Bradley discovered that the fixed stars possessed a definite and peculiar motion of their own which was due to the motion of the earth around the sun and depended on the time it takes the light to reach the earth.

This classical exposition of his experiments was published in the form of a letter sent to the *Phil. Trans.* (Vol. XXXIV, p. 637) and has naturally become quite inaccessible. There is probably only one complete file of the *Transactions* west of the Alleghanies, the fortunate possessor of which is the Chicago Public Library. Considering the rarity of this essay we deem it proper to republish it and render it accessible to our readers. We do not doubt the very way in which Mr. Bradley approaches the problem will throw much light on the principle of relativity. In fact this essay will prove sufficient to explain its far-reaching significance, the need of its invention and the limitations of its use. A consideration of the foundation of this principle and the history of its origin will clear it of the mysticism with which its recent representations have surrounded its statements.—P. C.]

*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 some time ago, of telling you in Conversation, concerning some Observations, that were making by our late worthy and ingenious Friend, the honorable *Samuel Molyneux* Esquire, and

which have since been continued and repeated by myself, 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 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 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 ob-

servicing about the End of November 1725, and on the third Day of *December* following, the bright Star at the Head of *Draco* (marked  $\nu$  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 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 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 anything 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 Star's 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* 1725, 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 done 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, 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  $\nu$  *Draconis* yet it would not at the same time agree with the Phaenomena in other Stars; particularly in a small one almost opposite in right Ascension to  $\nu$  *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  $\nu$  *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 Phaenomena, and being very desirous to search a little farther into this Matter; I began to think of erecting an Instrument for myself 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  $\nu$  *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, thoroughly examine how this Cause affected Stars differently situated with 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 made use of so long a Telescope as *Mr. Molyneux's*, I contented myself with one of but little more than half the Length of his (viz. of about  $12\frac{1}{2}$  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 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  $6\frac{1}{2}^{\circ}$  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

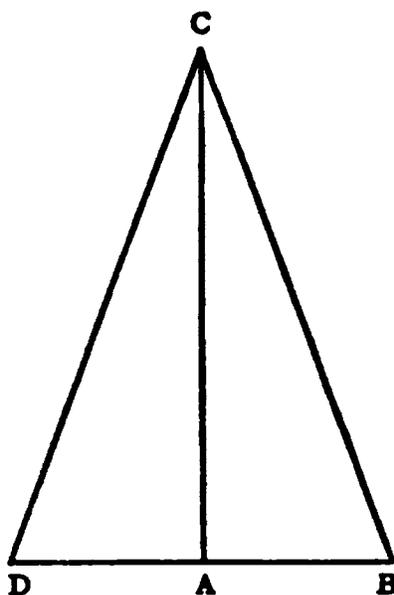
small ones; and not less than twelve, that I could observe through all the Seasons of the Year; they being bright enough to be seen in the Day-time, when nearest the Sun. I had not been long observing, before I perceived, that the Notion we had before entertained of the Stars being farthest North and 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 farther 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 evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneux's* Observations, that  $\nu$  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 at 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 Phaenomena, 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 myself as to the general Laws of the Phaenomena, I then endeavoured to find out the 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 Phaenomena 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 CA to be a Ray of Light, falling perpendicularly upon the Line BD; then if the Eye is at rest at A, the Object must appear in the Direc-

tion AC, 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 CA to BA; then Light moving from C to A, whilst the Eye moves from B to A, that Particle of 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, although the Eye were in Motion, for in that case, AC being infinite with Respect to AB, the Angle ACB (the Difference 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 Ob-

ject (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 AC is to AB (or AD) as 1000 to one, the Angle ABC will be  $89^{\circ} 56' \frac{1}{2}$ , and  $ACB = 3' \frac{1}{2}$ , and  $BCD = 2ACB = 7'$ . 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 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, the Difference 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 Star's 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 Star's 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 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 myself 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 not 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 Phaenomena 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  $1\frac{1}{2}$ " 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  $\nu$  *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  $\nu$  *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 ACB (as represented in the Fig.) This therefore being 20", 2, AC will be to AB, 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' and 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 *Phaenomena* 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 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 *♄ 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  $\eta$  by *Bayer*) was 36" more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greatest Altera-

tion 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  $\nu$  *Draconis*.

The Star of the 5th magnitude in the Head of *Perseus* marked  $\tau$  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, 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.

$\alpha$  *Persei Bayero* was  $23''$  more Northerly at the beginning of *January* than in *July*. Hence the *Maximum* would be  $40''$ , 2.  $\alpha$  *Cassiopeæ* was  $34''$  more Northerly about the End of *December* than in *June*. Hence the *Maximum* would be  $40''$ , 8.  $\beta$  *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 *Feb.*; 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 *ν 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 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
Oct. 20	4†	4†	Mar. 24	37	38
Nov. 17	11†	12	April 6	36	36†
Dec. 6	17†	18†	May 6	28†	29†
Dec. 28	25	26	June 5	18†	20
1728			June 15	17†	17
Jan. 24	34	34	July 3	11†	11†
Feb. 10	38	37	Aug. 2	4	4
Mar. 7	39	39	Sept. 6	0	0

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 *ν Draconis* may be thought not so proper to shew the proportion, in which the apparent alteration 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

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
Sept. 14	29†	28†	April 16	18†	18
Sept. 24	24†	25†	May 5	24†	23†
Oct. 16	19†	19†	June 5	32	31†
Nov. 11	11†	10†	June 25	35	34†
Dec. 14	4	3	July 17	36	36
1728			Aug. 2	35	35†
Feb. 17	2	3	Sept. 20	26†	26†
Mar. 21	11†	10†			

it was found by Observation in several Parts of the Year, and also what the Difference should have been according to the Hypothesis.

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 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 regarded, since it proves that the Alteration of Declination, on account of the Precession of the Equinox, is (as I before supposed) regular thro' all Parts of the Years; 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 *ν Draconis*; which agreeing with the Hypothesis (without allowing anything 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.

## THE PRINCIPLE OF LEAST ACTION.\*

### REMARKS ON SOME PASSAGES IN MACH'S MECHANICS.

Ernst Mach in his *Mechanics*<sup>1</sup> remarks,<sup>2</sup> with reference to the integral variational principles of Hamilton and of least action, that *other* such principles are possible, which idea has been suggestive to myself, and, as I have obtained some results which throw light on Mach's suggestions, I will try to describe the results here in not too technical language.<sup>3</sup>

#### I.

We must first of all notice a slight historical inexactitude in Mach's treatment of the principle of least action. "Maupertuis," we are told,<sup>4</sup> "enunciated, in 1747, a principle which he called '*le principe de la moindre quantité d'action.*'" Maupertuis<sup>5</sup> laid before the Paris Academy on April 15, 1744, a memoir in which he explained the reflection and refraction of light by a hypothesis substituted for Fermat's principle of least time.<sup>6</sup>

Maupertuis, like a good follower of Newton, accepted the emission hypothesis of light, and, according to P. Stäckel,<sup>7</sup> the contra-

\* Philip E. B. Jourdain, an English scholar who has devoted his life to research in the line of modern logic, mathematics and pure mechanics, submits to us some remarks on Mach's *Science of Mechanics*. He is a devoted and zealous student of Mach's works and is as familiar with them as a theologian with his Bible. Being also well acquainted with the work of Georg Cantor, Peano and Bertrand Russell he is especially fitted to explain the theoretical aspect of pure mechanics. We are confident that his lucubrations serve a good purpose and therefore deem it wise to submit them to specialists by giving them space in our columns.

<sup>1</sup> *Die Mechanik in ihrer Entwicklung historisch-kritisch dargestellt*, 4th ed., Leipzig, 1901, pp. 395-413; Engl. transl. by T. J. McCormack under the title *The Science of Mechanics, a Critical and Historical Account of its Development*, 3d ed., Chicago, 1907, pp. 364-380. (This translation will be referred to as *Mechanics*, and the above German edition as *Mechanik*.)

<sup>2</sup> *Mechanik*, pp. 399, 402, 413; *Mechanics*, pp. 368-369, 371-372, 380.

<sup>3</sup> Cf. note on p. 78 of my paper "On the General Equations of Mechanics," *Quarterly Journal of Mathematics*, 1904, pp. 61-79.

<sup>4</sup> *Mechanik*, p. 395; *Mechanics*, p. 364.

<sup>5</sup> Cf. *Mechanik*, pp. 484-485; *Mechanics*, pp. 454-455.

<sup>6</sup> *Mechanik*, pp. 454-457; *Mechanics*, pp. 422-425.

<sup>7</sup> *Encykl. der math. Wiss.*, IV, I, (1908), p. 49, note 125. Stäckel wrongly refers to the *Berlin Mem.*, 1745, p. 276, for Maupertuis's application of the principle of least action to the motion of light.