XIV. *On the parallax of certain fixed stars. By the Rev.* John Brinkley, *D. D. F. R. S. and Andrews professor of astronomy in the university of Dublin.*

Read March 5, 1818.

THE attention of the Royal Society has been lately called to the subject of the parallax of the fixed stars, by the astronomer royal; and as this has been occasioned principally by the results of observations which I have made at the Observatory of Trinity College, Dublin, I have taken the liberty of offering a few remarks relative to, and connected with this subject.

The Royal Society did me the honour to publish an extract of a letter to the late Dr. MASKELYNE on the parallax of α lyræ; since which time, in pursuing my observations, I have met with apparent motions in several of the fixed stars, the cause of which I was unable to explain, unless by attributing them to parallax. Among these stars α aquilæ exhibited the greatest change of place.

The results of my observations have been published in the 12th volume of the Transactions of the Royal Irish Academy

I there detailed my reasons for supposing that I could not have been misled by any source of error in the instrument, or in the mode of observing; and I trust, whatever may be the final result as to this subject, that I shall not be considered as having too hastily adopted the explanation by parallax.

I remarked, in the essay to which I have referred, my reasons for not being surprised that the Greenwich circle did not immediately confirm my results. However, after several years observations, Mr. POND was led, although he had found discordances of a similar kind, but much less than mine, to doubt the explanation by parallax, and certainly with good reason, as two instruments that might be supposed equally adapted for the examination, seemed to give different results; in consequence of which he took measures for submitting the matter to another kind of trial. He most laudably applied to the Royal Society, as visitors of the Royal Observatory; and by their assistance, and by the advantage of vicinity to the first artists, he has been enabled to put up his fixed telescopes.

Thus, unless unforeseen difficulties shall be found to exist, this question is likely to be soon decided; and certainly on many accounts, it is a most interesting question.

It is now about sixteen months since Mr. POND informed me of his doubts respecting the conclusion I had drawn from my results, and from that time I have anxiously looked to every observation that tended to confirm my conclusion, if just, or invalidate it, if wrong.

The last two years have been very unfavourable for astronomical observations; so that my opportunities always, in consequence of the cloudy atmosphere, very few, have been during that time fewer than usual. However, I have been able to obtain some results that I shall notice farther on, which appear to coincide with my former ones as to α aquilæ, in a remarkable manner; and it is to this star that we are, I think, to look for the final decision of the question. As to α lyræ and arcturus, my results have not been so uniform as I had expected from my former observations; but as to α cygni, my recent observations are consistent with my former ones, in exhibiting the same discordance between the summer and winter observations as before, which appeared to me to point out a parallax for that star, but less than for any of the other three stars. The results of the observations of Mr. Pond, by the fixed telescope, as given in the last volume of the Transactions, appear, at first sight, very decisive against the existence of any visible parallax in α cygni; but in considering these observations, a difficulty suggests itself, which if founded, will render the result deduced from α cygni and β aurigæ quite inconclusive. This, and some other points relative to the fixed telescopes, will be noticed farther on.

If it shall appear that I have been deceived by a constant source of error in my instrument as to these stars, it will be of much importance to investigate that source; and although at present I can form no conjecture as to any cause, yet, when it shall be found actually to exist, it will be incumbent on me to endeavour most strenuously to investigate the cause; and in so doing, I conceive I shall render a most acceptable service to astronomers. It will be shown, whether it be a cause that will be likely to affect other instruments. It appears at present, from the results of Mr. Pond, that the

Greenwich circle is subject to a similar cause of error (supposing the discordance should not arise from parallax), and that this cause has been diminished, if not entirely done away, by reducing the internal air to the same temperature as the external.

However, from all the consideration that I have been enabled to give the subject, I am led to entertain doubts of the fitness of an instrument similar to the Greenwich mural circle, for this delicate enquiry. I do not allude to the objection stated by Mr. POND, since, as he justly observes, that is obviated by keeping the telescope fixed to the same place on the circle during a period of observations, as was the case in the observations of 1813, and as to those mentioned in the Appendix. And in respect to Mr. POND's paper, and its Appendix, as given in the first part of the Transactions for 1817, it appears to me doubtful, whether the results, if they could be exactly obtained (that is, if the elements from which they are deduced were exact), may not be such as to furnish a discordance explained by a parallax nearly equal to mine, or whether the results might not be entirely against parallax. My reasons for entertaining these doubts, will appear in the following remarks respecting the elements used in computing the index error, in instruments similar to the Greenwich mural circle.

The polar distance of a star, as observed by a mural circle, requires, besides the corrections for refraction, aberration, annual variation, &c. also the application of the index error.

This index error is determined by the mean of results deduced from observations of stars of the standard catalogue.

Let $i =$ index error.

$d =$ mean polar distance of a star of the standard catalogue deduced from observation.

$c =$ mean polar distance of the same star in the catalogue.

Then

$$i = d - c$$

Let $o =$ observed polar distance.

$r =$ refraction.

$p =$ parallax.

$a =$ aberration of light.

$n =$ nutation.

$s =$ semiannual equation.

$v =$ annual variation.

Then $i = o + r + p + a + n + s + v - c$ these quantities being applied with proper signs.

Now i partakes of the error or uncertainty of each of these quantities.

1. Let us suppose that there is no error from the observation or construction of the instrument; that is, let us suppose o exact.

2. As to refraction. Any uncertainty in the quantity of refraction affects the index error, and therefore the required polar distance of a star, although that star should be in or near the zenith. Thus the determination of the polar distance of a star in the zenith, will partake of any uncertainty in the refraction of the lower stars used for the index error.

Let us see to what this may amount as to the index error by a single star.

BRADLEY's refractions, by which the Greenwich observations have hitherto been computed, differ from the French refractions as follows.

$$\left.\begin{array}{l}\text{The French refraction at } 45^{\circ}=57,''5\\ \text{BRADLEY's refraction at } 45=56,\ 9\end{array}\right\}\begin{array}{l}\text{therm. } 50\text{, and}\\ \text{barom. } 29{,}60\end{array}$$

and the mean diff. $= 0'',6 \times$ tan. zen. dist. nearly.
But this will not affect the index error, as it equally affects r, &c.

But the effects of the change of temperature as computed by BRADLEY's, and by the French refractions, have an important concern in this enquiry.

To deduce the actual refraction from the mean refraction for height of thermometer $= t$, the mean refraction is multiplied by $\frac{400}{350+t}$ according to BRADLEY.

According to the French tables, the multiplier is $\frac{500}{450+t}$ at least sufficiently nearly so for the 30 standard stars of Mr. POND.

The difference then between BRADLEY's refraction and the French refraction from the change of temperature, is nearly $\frac{t-50}{2000} \times$ mean refraction.

Now Procyon is one of the standard stars; and when this star passes the meridian in June, soon after mid-day, we may suppose FAHRENHEIT's thermometer at 70°; and when this star passes the meridian in December, near midnight, the thermometer may be at 30°. The mean refraction of this star at Greenwich, is 58″ nearly. Therefore the refraction computed by the French table may, in summer, exceed that computed by BRADLEY's table by 0,″58, and the contrary may take place in winter. Hence an uncertainty in the index error, which as deduced from Procyon, might occasion a difference in the zenith distance of (ex. gr.) α lyræ in summer and winter, $= 1'',16$, bearing a considerable proportion to the supposed discordance in summer and winter. This is an extreme case: the index correction, as deduced from other stars, would not be so much affected. The polar star below the pole, is likely to be often used, and might occasion an uncertainty of about $1''$ under similar circumstances.

When I call this uncertainty, I suppose the matter is entirely doubtful as to the preference to be given to either of the formulæ.

Bradley's law of change from temperature was deduced from his astronomical observations, but other astronomical observations do not contradict the law of the French formula; which has also been confirmed by physical experiments, and seems more to be depended on.

It appears then that an incorrect law may materially affect the index error, and occasion incorrect results.

It therefore seems of the first importance with respect to a mural circle, to ascertain with exactness the law of variation of refraction from change of temperature; otherwise errors will be mixed up in all the conclusions.

It will not be possible to deduce easily, by the results from Bradley's refractions, the results that the French refractions would give.

For this purpose it will be necessary not only to know the mean temperature at the observations of the star, whose north polar distance is required, but also the temperature at the observations of stars by which the index error is computed, which in fact is much the same as to recompute the index error,

I know not how far this may have been a source of inaccuracy in the north polar distances (p. 388, Phil. Trans. 1815), from the French refractions.

They seem to have been merely deduced from the column of N. P. D. by BRADLEY's refraction, and the mean heights of the barometer and thermometer, as given in page 386.

3. As to *p*, or the effect of parallax, we are not certain that many of the standard stars may not have a parallax in declination, amounting to a fraction of a second. This therefore so far will render the index error uncertain.

4. As to *a*, or the effect in declination of the aberration of light.

The maximum of aberration, pretty generally adopted of late years by astronomers, is $20'',25$. The researches of the Chevalier DELAMBRE have principally led to this. The maximum formerly used was $20''$. The former is probably more exact, but by no means certainly so. It is even possible that the maximum of aberration may be so low as $20''$, or $19'',8$. The strongest argument for $24\frac{1}{4}''$ is derived from the researches of M. DELAMBRE, respecting the reflected light from Jupiter's Satellites;* which certainly cannot be considered conclusive as to the direct light of the stars.

It seems reasonable to conclude, from an examination of Dr. BRADLEY's paper on aberration, that this matter requires farther examination, and that there is an uncertainty amounting to a quarter of a second.† If so, the index error com-

* DELAMBRE, Astron. Tom. 3, p. 105

† See note (A) at the end of this paper.

puted by the pole star may be uncertain 0″,2 or 0″,3, in July, and the same in an opposite direction in January. Add to this, it has not been usual for astronomers to consider the variable velocity of the earth in its orbit. The effect of this in N. P. D. as to stars, the Æ'. of which are nearly 3^s or 9^s is always insensible, but not as to stars, the Æ'. of which are nearly 0^s and 6^s, and are also far from the ecliptic. This quantity is nearly

$$=\frac{ab.\ \text{in N. P. D}}{60}\cdot\cos.\,(\odot\ \text{Long.} \sim 9^s\ 9^\circ.)$$

and therefore in the pole star may amount in July to 0″,34. Hence, from these two causes, the uncertainty in the aberration of the pole star in declination in July may be = 0″,6. The joint effect of these causes will be 0 in October and in January.

The index error computed by the pole star when below the pole in July, will be opposite to the above, and thus the index errors so computed at the same time may differ 1″,2.

5. As to n, or the nutation, according to some astronomers, the nutation in declination,

$$= 7''.85\ \text{Sin.}\,(Æ - ☊) + 1'',15\ \text{Sin.}\,(Æ + ☊) \quad - \ - \quad (1)$$

according to others,

$$8'',42\ \text{Sin.}\,(Æ - ☊) + 1'',23\ \text{Sin.}\,(Æ + ☊) \quad - \ - \ - \quad (2)$$

M. Laplace * made it (as will appear by the proper reductions) so great as

$$8'',76\ \text{Sin.}\,(Æ - ☊) + 1'',35\ \text{Sin.}\,(Æ + ☊ \quad - \ - \quad (3)$$

(1) is the nutation according to Lambert's tables. The maximum of formula (3) was deduced from the mass of the moon, $=\frac{1}{58,6}$, (the earth being unity), as determined by the

* Mec. Cel. Tom. 2, p. 350.

tides at the port of Brest. LAPLACE* afterwards modified this quantity, in consequence of the determination of DELAMBRE, as to the lunar equation of the solar tables; of the determination of Dr. MASKELYNE, as to the nutation itself from BRADLEY's observations; and of the determination of M. BURG, as to the parallax of the moon. These three determinations agree in deducing nearly the same mass of the moon, and induced LAPLACE to adopt $\frac{1}{68,5}$, and then the nutation will be nearly that in formula (2), which is nearly the same as that of DELAMBRE.†

LAPLACE has farther considered this subject,‡ and finds, according to a high degree of probability, that the maximum is between $9'',31$, and $9,''94$.

From the above there is evidently room for some uncertainty, which uncertainty may be doubled, by taking two stars differing 180° in right ascension.

M. DELAMBRE, although he thinks the maximum in aberration is settled, supposes the mass of the moon still subject to some uncertainty.§

6. As to *s*, or the semiannual equation as it is called. This $= 0'',48 \text{ Sin.} (2 \odot - Æ)$. Here, on account of the smallness of the quantity of this equation, there is no room for any material uncertainty.

There is also another equation omitted by astronomers, viz. $0'',04 \text{ Sin.} (2 ☽ - Æ)$. This cannot occasion a greater difference than $0'',08$, and therefore scarcely need be noticed even among the minute objects of this enquiry.

* Mec. Cel. Tom. 3, p. 159.

† Astr. Tom. 3, p. 156.

‡ Conn. des Temps. 1818, p. 361

§ Conn. des Temps. 1810, p. 462.

7. As to v, or the variation in declination. This consists of two parts, one the effect of the precession of the equinoxes, and the other of the proper motion of the star. The former seems determined with sufficient accuracy. Also as far as regards the stars of the standard catalogue of Mr. Pond, the latter seems pretty well agreed on among astronomers. But here arises a question of some importance: is the proper motion of each star uniform? It is assumed to be so in computing it by two results separated by a long interval. A series of results sufficiently accurate, and separated by intervals sufficiently long, have not yet been obtained to ascertain this important point.

A star of the 6th magnitude, 1 Leporis, seems to furnish an instance of a variable proper motion, by a comparison of the observations of Bradley and M. Piazzi. There is nothing against a variable proper motion in our theories of the nature and motions of the fixed stars. Hence, another source of uncertainty in computing the index error.

8. Lastly, as to c, or the mean polar distance in the standard catalogue. This is subject to two uncertainties. The original error in the catalogue, and an uncertainty in the annual variation, as mentioned in the last article.

Notwithstanding all the care that has been used by Mr. Pond in perfecting his standard catalogue, it may contain small inaccuracies, as will easily be apprehended from the observations in the preceding articles.

The uncertainties to which the index error is liable from the above causes, are independent of those to which the observation to which it is applied, is also subject. It may be said, that these uncertainties tend to correct each other, and that the uncertainty remaining, after taking a mean of results from several stars, will be too small to be regarded. This indeed may be said as to the index error when applied to observations of the sun, moon, or planets; but not, I think, when it is applied to investigations relative to the parallax of the fixed stars, annual variation of north polar distance, exact determination of the quantity of aberration and nutation, and these, it will be allowed, are objects of great importance in the present improved state of astronomy.

Indeed, with respect to the parallax of the fixed stars, several of these objections may be obviated by a proper selection of the standard stars. Thus the uncertainties of refraction may be avoided by using only stars near the zenith. The objection in the 8th article may be partly obviated by using the same stars for ascertaining the index error at the two periods of greatest and least parallax, and so of other uncertainties. No error as to parallax arises from neglecting the unequal motion of the earth in its orbit, as far as regards the index error computed by the same stars at the two periods. But this selection of stars will be limiting the use of the instrument, and the advantage of a mean of a number of observations lost; and in fact, with respect to the index errors used in determining the N. P. D. of α Lyræ, α Aquilæ, and α Cygni, as given by Mr. POND, Phil. Trans, 1817, no particular selection of stars, with a view to these points, seems to have been made.

It may also perhaps be suggested, that the mural circle may be used without applying index error, as was done with respect to the observations given in the appendix of Mr. Pond's first paper. But the knowledge of the stability of the index error during six or eight months, depends on the reductions by the standard stars; and therefore the above sources of uncertainty remain. Mr. Pond remarks, that between July and March the index error may have oscillated a small fraction of a second on each side the mean, and not more; so that I think no important conclusion can be deduced from the results in that appendix.

The differences between the exterior and interior temperature may have tended to exaggerate the discordance between the summer and winter observations made at Greenwich; but it may appear that sufficient observations have not been made to ascertain this point, when we consider the many other sources of uncertainty. As far as I have examined into this matter, with respect to my own observations, I cannot suppose any of my discordances materially affected by the difference of exterior and interior temperature. The room containing the circle at Greenwich is much smaller, and less lofty than the room of this Observatory, which contains both the circle and transit instrument.

I hope I have so expressed myself, that I shall be understood to mean, that I consider the results of observations hitherto made by the Greenwich circle inconclusive as to the existence or non-existence of parallax, merely from the uncertainty of the elements used in the reductions, not from any errors of the observations, or from any defects in the construction of the instrument.

I more particularly offer to the consideration of astronomers the preceding remarks, as in the present state of astronomy, the relative fitness of instruments for ascertaining with precision the smaller motions, whether real or apparent, of the fixed stars, is an object of importance.

In instruments similar to that belonging to the Observatory of Trinity College, Dublin, the index error is found by reversing the instrument, the position of the vertical axis being ascertained by a plumb line. Thus the determination of the index error is not materially affected by any of the uncertainties above referred to. Therefore, by its principle, this instrument should appear particularly adapted for enquiries relative to the annual parallax, annual variation, &c. &c.

From the fixed telescopes we are probably to look for the final decision of the question of parallax. At first sight these seem to offer a very simple and certain criterion. However, a little consideration will point out probable sources of difficulty. Suppose the star under examination be compared with a star opposite in Æ, or with one as nearly so as can be conveniently had. Besides the uncertainty respecting the annual variation, even the uncertainty in the quantity of aberration may tend in some degree to conceal the parallax, unless the minimum of aberration in declination of each star be at the same time, and the observations are made pretty equally on both sides of this time. The star β Aurigæ has been judiciously chosen by Mr. POND to compare with α Cygni. A more proper star could not have been chosen; yet here the effect of an uncertainty in the maximum of aberration, amounting only to $\frac{1}{4}$ of a second, will have a sensible effect.

If we suppose the maximum only $20''$, as I believe the maximum used by Mr. POND is $20''\frac{1}{4}$, his winter distance for the observations given would be increased $0'',2$, and his summer distance decreased by about the same quantity; which would make his results differ in the same direction as they should do by the effect of parallax. I do not intend by this that any argument in favour of parallax can be deduced from his results, but only to show the effect of small uncertainties.

There may be uncertainty as to the stability of the instrument during the interval which elapses between the successive observations of α Cygni and β Aurigæ, which is sometimes necessarily of several days.

This is the point before alluded to; and there appears, on examining Mr. Pond's results as to α Cygni and β Aurigæ, indications of such an instability, and that to an amount that may do away the conclusion he has drawn from these observations.

The seconds of the micrometer for the same star should be the same in summer and winter, after the usual reductions, supposing no uncertainties in the elements of these reductions, supposing no parallax, and supposing no derangement in the instrument. Now, referring to Mr. Pond's paper, the seconds for α Cygni are decreased by about $5''$ in summer, and those of β Aurigæ increased by nearly the same quantity. This may be concluded to arise from a derangement in the instrument by the change of temperature, as Mr. Pond has mentioned no other cause. The effect of an increase of temperature, therefore, appears to be to decrease the seconds of α Cygni, and to increase those of β Aurigæ. Applying this to observations of the same day in winter, α Cygni passes the meridian near noon, and β Aurigæ near midnight, or at least late in the evening. An increase of temperature, therefore, relative to α Cygni takes place, and the seconds in α Cygni become less than they would have been had the temperature remained the same as in the night. The sum of the seconds of α Cygni and β Aurigæ is diminished by this cause, and it would be increased by the effect of parallax. Hence this cause tends to conceal the effect of parallax in winter. In summer the passages of α Cygni and β Aurigæ are reversed as to noon, and the sum of the quantities increased by temperature and decreased by parallax.

This explanation, if justly founded, will have a tendency to diminish the value of stars nearly opposite in Æ, which Mr. POND so judiciously selected, and by which he avoided any uncertainty from differences of parallax. As to δ Cygni, the winter observations, Mr. POND remarks, are far from satisfactory ; and they seem too few and too discordant to decide any thing, even supposing we were certain of the annual variation of δ Cygni, and that it had no visible parallax.

I shall now proceed to state briefly the results of my observations up to the present time, which appear to point out parallax as to α Cygni, α Aquilæ, and α Lyræ ; also the results of observations of γ Draconis.

α Cygni

The winter observations of this star cannot be materially affected by any uncertainty in the maximum of aberration, being made nearly equally on both sides of the time when parallax is greatest, and aberration = 0. But the summer observations being generally made after the time when aberration in declination = 0, the effect of a less maximum of aberration is to increase parallax. I have therefore used for my recent observations $20''\frac{1}{4}$, and corrected my former ones, which were computed with $20''$ max. of aberration; thus using the most unfavourable quantity.

Summer Z. dist. Jan. 1, 1815.

	N°. Ob.	° ′ ″
1811 & 1812	23	8 45 45,71 + ,74 p
1814	10	45,97 + ,72 p
1815	20	46,12 + ,48 p
1817	14	45,22 + ,68 p
	67	

Winter Z. dist. Jan. 1, 1815.

	N°. Ob.	° ′ ″
1810-13	24	8 45 47,12 — ,76 p
1814-15	12	46,79 — ,80 p
1816-17	16	46,57 — ,76 p
	52	

The correct means of the preceding results being taken by attributing to each a weight proportional to the number of observations, we obtain

$$8^\circ\ 45'\ 46'',86 - ,77\ p = 8^\circ\ 45'\ 45'',77 + ,63\ p$$

$$\text{or } p = \frac{1,09}{1,40} = 0'',78$$

or $2\ p = 1'',56$, the angle subtended by the diameter of the earth's orbit at the star.

α Aquilæ.

The stars β and γ Aquilæ pass the meridian within a few minutes of the passage of α Aquilæ; and as they are much inferior in brightness to that star, and differ less than 3 degrees in declination from it, I considered that if I could observe the three stars on the same day, the comparisons of the observations in winter and summer would furnish much information relative to the parallax of α Aquilæ.

As the stars pass so nearly together, there was not sufficient time to read off the three microscopes for each observation; I therefore, for some time, read off only the bottom microscope for γ, to be compared with the reading of the bottom microscope for α, and the three microscopes for α, giving up the observation of β. Afterwards, I only read off the bottom microscope for α, and thus was enabled to observe β. Unfortunately from the few observations to be obtained in October and November, when the sun approaches these stars, I have not succeeded hitherto in obtaining a sufficient number of observations; but my summer observations appear very satisfactory, in agreeing with the result from the former observations of these stars, which were made in the autumn of 1813, and with Mr. POND's north polar distances; whereas the summer zenith distance of α Aquilæ has been uniformly less than the winter zenith distance of that star. So that, as far as I have gone with this kind of trial, the results have been very strong in favour of the parallax of α Aquilæ. As in my recent observations of this star, only the bottom microscope has been used, I have deduced results from all my former observations of α Aquilæ from the bottom microscope only.

The conclusion as to the parallax of this star does not differ materially from my former one, where the three microscopes were used.

Summer Zenith dist. Jan. 1, 1817.

	N°. Ob.	by bottom microscope.
1808-1812	38	$44^{\circ}\ 59'\ 36'',38 + ,38\ p$
1814	21	$36,92 + ,21\ p$
1815-1816	22	$36,10 + ,30\ p$
1817	25	$36,54 + ,26\ p$
	106	

Winter Zenith dist. Jan. 1, 1817.

	N°. Ob.	by bottom microscope.
1808-1812	38	$44^{\circ}\ 59'\ 38'',51 - ,40\ p$
1813-1814	24	$39,20 - ,47\ p$
1816-1817	20	$38,36 - ,45\ p$
1817-1818	20	$37,57 - ,44\ p$
	102	

The correct means give

$$44^{\circ}\ 59'\ 36'',47 + ,30\ p = 44^{\circ}\ 59'\ 38'',36 - ,44\ p$$

$$p = \frac{1,89}{,74} = 2'',53$$

or $2\ p = 5'',0$ by 208 observations.

α Lyræ.

The following are the results of my observations of α Lyræ. My former observations are here reduced to what they would be by the French refractions, and the other observations have been reduced, taking the maximum of aberration $= 20''\frac{1}{4}$. Both circumstances tend to diminish in a small degree the parallax; but the result from all my observations gives the double parallax above $\frac{1}{2}$ a second less than I should have expected from my former observations. Whether the discordance I had found was to be attributed to parallax, or any other cause, I had expected the new results would not materially differ from my former conclusions. Although it has happened otherwise, yet an examination of the different results will, I conceive, be found not to contradict my former remarks respecting the accuracy to be attained by my instrument.

Summer Zenith dist. Jan. 1, 1811.

	N°. Ob.	° ′ ″
1808-1813	65	14 46 19,35 + ,78 p
1814	20	19,87 + ,78 p
1815	20	19,86 + ,74 p
1816	11	20,46 + ,77 p
1817	12	19,62 + ,62 p
	128	

Winter Zenith dist. Jan. 1, 1811.

	No. Ob.	° ′ ″
1808-1813	61	14 46 20,96 — ,79 p
1814-1815	20	21,00 — ,78 p
1815-1816	14	19,47 — ,68 p
1816-1817	15	20,88 — ,76 p
1817-1818	24	20,06 — ,76 p
	134	

The correct means give

$$19'',63 + ,76\ p = 20'',64 - ,77\ p$$

$$\text{or } p = 0'',66$$

or $2\ p = 1'',32$, the result of 262 observations of α Lyræ.

γ Draconis.

		°	′	″
Of this star, the mean of 53 observations in winter gives mean Z. D. Jan. 1 1814.	=	1	52	17,55
59 observations in summer give	=	1	52	17,92

This result is in a direction contrary to parallax, and therefore had I compared the differences of zenith distances of this star and α Lyræ, in summer and winter, the result would have given me a greater parallax for α Lyræ.

This conclusion is quite opposite to that of Mr. Pond, and seems to me a point of much difficulty to be explained. However, from the mean of my late results as to α Lyræ, I am inclined now, to consider my former argument deduced from γ Draconis of less weight than I had attributed to it, not thinking the observations of γ Draconis sufficiently numerous.

I have thus stated the results of my observations, and the conclusions that seem to follow as to the parallax of the respective stars. The many causes that may lead, if not to actual error, at least to a high degree of uncertainty, induced me in the paper alluded to, to speak with hesitation as to my explanation. The observations of Mr. POND, as far as they go, seem to invalidate that explanation, particularly as to α Cygni and α Lyræ.

It is by observation alone that the decision can be made. No conjecture as to the relative distances of the stars can be of any material weight. The conjecture, in itself probable, that the brightest stars are nearest to us, seems opposed by another conjecture, also by itself probable, that those stars are nearest which have the greatest proper motion.

Some of the brightest fixed stars have scarcely any sensible proper motions, while those of some much smaller are very perceptible. The two stars, 61 Cygni, have each an annual proper motion of about $5'',3$ in right ascension, and of $3''$ in declination. These stars are of about the 6th magnitude, and one a little brighter than the other.

This great proper motion seemed to render it probable, that these stars are sufficiently near to us, to have a visible parallax. I accordingly made observations on one of them, but found nothing satisfactory.

Also 40 Eridani, which is of the 5th magnitude, has so great a proper motion, that we might conjecture it to be nearer to us than many of the brighter stars.

The uncertainty, therefore, respecting the relative distances, as deduced from their degrees of brightness, weakens conclusions against parallax drawn from differences of north polar distances of stars having nearly the same right ascension, and north polar distance.

It would be an interesting circumstance, could the existence of visible parallax in any one star be ascertained, and placed beyond doubt, by the joint results of two separate instruments. The comparison of my summer and winter observations of α Aquilæ indicating so great a parallax, induces me to expect that as to this star it may yet be accomplished.

Mr. Pond suggests that the effects of refraction may occasion some uncertainty as to this star. This can only arise from irregularity of refraction; and it seems scarcely possible that the mean of 100 observations can be sensibly affected thereby. My refractions have been computed from the internal thermometer placed on the instrument: had they been computed by the external thermometer, the difference between the summer and winter zenith distances of α Aquilæ would have been lessened about $0'',3$. As α Aquilæ passes the meridian near noon in winter, there is seldom much difference then between the external and internal thermometer here.

If the discordance which I have found between the summer and winter zenith distances had arisen from the different temperatures at the two seasons, it might have been expected that Aldebaran, Capella, α Orionis and Procyon would have been much more affected by this cause; as in winter they pass the meridian at night, and in summer in the day time; and therefore as to these the observations are made in the extremes of temperature.

To many, the time and labour spent in this minute enquiry, may appear wasted. Some however will justly appreciate the exertions that have been made here and at Greenwich. Several attempts to observe the parallax of the fixed stars have failed since the time of Dr. HOOKE, and Mr. FLAMSTEAD,* and if this should end like the rest, it will be some satisfaction to have ascertained, beyond doubt, certain limits; and also, probably to have occasioned these limits to be still farther circumscribed by the observations of Mr. POND, in the event of his not confirming my conclusions.

* See note (B).

Observatory of Trinity College,
Dublin, Feb. 20, 1818.

Note (A).

Upon examination of Dr. BRADLEY'S* account of the aberration, it will appear, I think, that the maximum of aberration deduced therefrom, cannot be depended on to $\frac{1}{4}$ of a second. Dr. BRADLEY afterwards mentions, in his paper on the nutation, that he had revised his computation, and states $20''$ as the result nearest the truth. The result from the eclipses of Jupiter's satellites, as deduced by M. DELAMBRE, is $20\frac{1}{4}$. The limit of the probable error of this latter determination is not easily known; but it appears to me that we ought to adopt the result of Dr. BRADLEY'S revision, rather than any conclusion *we* can deduce from the data in his first paper. We have not the original observations to refer to; and it is to be remarked, that he puts down all the maxima of changes of

* Phil. Trans. xxxv, 637, or Old Abridg. vi, 149.

declination (D—D′) in seconds, without fractional parts, and thence deduces for each star the maximum of aberration.

	D—D′	2 *a*	2 *a* †
γ Draconis	39″	40,4	40,3
β Draconis	39	40,2	40,2
η Ursæ Maj^s.	36	40,4	40,4
α Cassiopeæ	34	40,8	41,1
τ Persei	25	41,0	41,4
α Persei	23	40,2	40,2
35 Camel.	19	40,2	40,2
Capella	16	40,0	39,7
	Mean	40,40	40,44

I re-computed the maximum of aberr. from column D—D′, and find the results as in column 2 *a* †, which differ a little from Bradley's results in column 2 *a*, but differ considerably from M. Zach's results (Conn. des Temps. 1810, p. 459) as to τ Persei and 35 Camel. However, M. Zach seems not to have attended to Dr. Bradley's remark respecting τ Persei, and therefore, as to this star, his result is erroneous; and his different result from 35 Camel. must have been an error of computation.

It is evident both from the consistent results, and Dr. Bradley's remarks, respecting the annual variation, which he deduced from observation, that his observations from whence the above results have been deduced, must have been extremely accurate. The changes of nutation (evident by his observations, but at that time unknown to him) are included in the annual variation; and hence no source of error on this account, except from error of observation: therefore we may conclude that had Dr. BRADLEY used the fractional parts of the seconds, the maximum would have been very accurate. We cannot now estimate the effect of this omission; we can only see that it is probable it has had a sensible effect on his conclusion; and we may suppose this to have been corrected by his subsequent revision. His words in the paper * on nutation are, "I have again examined my observations that were most proper to determine the transverse axis of the ellipse, which each star seems to describe, and have found it to be nearest to 40″; which number I therefore make use of in the following computations." He had at first concluded it to be $20''\frac{1}{4}$. On the whole then, it seems that 20″ is the result deduced from the direct light of the fixed stars, and $20''\frac{1}{4}$ from the solar light reflected from Jupiter's satellites. It is highly probable that future observation will find these quantities exactly equal. At present there exists an uncertainty.

Note (B.)

The results of the attempts of HOOKE and FLAMSTEAD are remarkable; the former reasoned justly on inaccurate observations, and the latter wrong on exact ones; and both imagined they had discovered a parallax. HOOKE, who erected at Chelsea a fixed telescope, 36 feet long, for observing γ Draconis, found a change of place agreeing with a considerable parallax. The great mechanical skill of Dr. HOOKE, the length of his telescope, and the precautions he took, seemed to leave no doubt.

Dr. BRADLEY, in his paper on the aberration, expresses great

* Phil. Trans. xlv, 1; or Old Abridg. x, 32.

surprise at the erroneous results of HOOKE's observations; which he, Dr. BRADLEY, had considered as exact, till they were contradicted by Mr. MOLYNEAUX's observations, and by his own. He says, "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 30″ (which was doubtless the case), if rectified with so much care as he represents."

It may be remarked here, that the results of the observations of α Cygni with the fixed telescope of Mr. POND, as given in the last volume of the Transactions, would in themselves appear to indicate a considerable parallax in that star, and thus produce an error similar to that of HOOKE. But Mr. POND guarded against such a source of error, by using two stars; and therefore no derangement of the instrument could affect his results, except as far as it might take place between two succeeding observations.

FLAMSTEAD's instrument, which he has described in his letter to Dr. WALLIS,* was a mural arch of 140°, by which he could observe all stars visible in his hemisphere; and observe below the pole all circumpolar stars that were not above $11^\circ\frac{1}{2}$ from the pole. He deduced the index error of his instrument by observations of the pole star corrected for refraction; not at first considering any correction for parallax as necessary: but he soon found a correction necessary, which he attributed to the effect of parallax. In this he was singularly mistaken. The result of his observations was, that the diameter of the circle described by the pole star about the pole, was 1′ 20″, or 1′ 30″ greater in summer than in winter.

* Wallis Oper. tom. iii. p. 701.

This we now know to be the effect of the aberration of light. Thus FLAMSTEAD's observations give the maximum of aberration 20″, or $22''\frac{1}{2}$. The near agreement of this with BRADLEY's result is much greater than could have been expected from FLAMSTEAD's instrument; but the remarkable circumstance is, that FLAMSTEAD should have been so much mistaken in his mathematical application; and that WALLIS, who interested himself much in the question of parallax, did not point out his mistake: it can only be attributed to the great age of WALLIS, who was then in his 83rd year.