

XVIII. *Concerning the Latitude and Longitude of the Royal Observatory at Greenwich; with Remarks on a Memorial of the late M. Cassini de Thury. By the Rev. Nevil Maske-lyne, D. D. F. R. S. and Astronomer Royal.*

Read Feb. 22, 1787.

M E M O I R E

Sur la jonction de Douvres à Londres.

Par M. CASSINI DE THURY, Directeur de l'Observatoire Royal; de la Société Royale de Londres, &c.

**I**L est intéressant pour le progrès de l'astronomie que l'on con-  
naisse exactement la différence de longitude et de latitude  
entre les deux plus fameux Observatoires de l'Europe; et quoi-  
que les observations astronomiques faites depuis un siècle offrent  
un moyen assez exact pour parvenir à cette recherche, il paraît  
cependant que l'on n'est point d'accord sur la longitude de  
Greenwich à onze seconds près, et sur sa latitude à quinze  
secondes.

L'on a reconnu par les opérations trigonométriques exécutées  
en France, au Nord, et au Perou, que sur l'étendue d'un degré  
du méridien ou de 57 mille toises, l'on se trompait à peine  
de dix toises, ce qui a été prouvé par des bases mesurées à l'ex-  
trémité des suites de triangles; ainsi sur la distance de Douvres  
à Londres, qui est de 49800 toises ou environ, on ne pourrait se  
tromper de 120 toises, qui repondent à onze secondes en longitude.

M. CASSINI a déjà publié, dans le livre de la Méridienne Vérifiée, les opérations par lesquelles l'on a déterminé la distance de Calais à la grosse tour de Douvres de 18241 toises par un premier triangle, et de 18243 toises par un second triangle; on aurait cette distance avec une plus grande exactitude en observant les angles conclus à *Douvres, qui sont fort aigus*. M. CASSINI a découvert des côtes de France plusieurs objets sur les côtes d'Angleterre, qui seront visibles de la tour de Douvres; et sur cette première base on établirait une suite de quelques triangles jusqu'à Londres, dont le nombre et la grandeur dépendent de l'exposition des objets compris dans la direction de Douvres à Londres.

M. CASSINI ne doute point que ce projet ne soit agréé d'un Souverain qui aime les sciences, qui non content des découvertes du célèbre COOK vient d'ordonner un second voyage autour du monde, et que la Société Royale ne charge un de ses Membres de l'exécution; et dans le cas où ses occupations l'empêcheraient de s'y livrer, qu'elle ne permît à M. CASSINI de s'en charger. L'honneur qu'elle lui a fait de l'associer à un corps aussi respectable serait un titre pour lui accorder sa confiance. M. CASSINI a profité du voyage du Roi en Flanders en 1748 pour joindre les triangles de la méridienne à ceux de SNELLIUS en Hollande; en 1762 il a prolongé la perpendiculaire de Paris jusqu'à Vienne en Autriche. La branche qui s'étendra jusqu'à Londres fera la troisième, et formera la jonction des deux plus belles villes de l'Europe.

THE preceding Memorial of the late M. CASSINI DE THURY was put into my hands by Sir JOSEPH BANKS, our President, on the 28th of April, 1785, desiring me at the same time to give an answer to it. Happy if I can solve the doubts entertained by the late Royal Astronomer of France concerning the latitude and longitude of this Royal Observatory, and at the same time do justice to the memories of my learned predecessors, and to myself, I shall give an account of the principal operations that have been performed here for ascertaining those points, and then add my own remarks to elucidate the subject and reconcile the difficulties in question.

Had Dr. BRADLEY lived longer, for the benefit of astronomy, to publish his valuable observations, or had they been since published by another hand, which unfortunately they hitherto have not, these remarks might have been unnecessary, and perhaps even the occasion for them might never have occurred; as it would have then appeared upon what foundation the latitude of this Observatory had been established, and what differences of meridians between Greenwich and the other principal Observatories of Europe resulted from the observed eclipses of Jupiter's satellites and other celestial phenomena.

However, having formerly been apprised by Dr. BRADLEY himself of several particulars of moment relative to his observations, and particularly of the method which he used for settling his latitude and refractions, after he became possessed of the new instruments in 1750, and being assisted with some of his manuscript calculations, with the addition of my own observations, I flatter myself I can throw the light wanted on the

question, and obviate the principal difficulty, that relative to the difference of latitude of Greenwich and Paris, and reduce the difference of meridians within smaller limits, notwithstanding Dr. BRADLEY'S original observations had been removed from this Observatory, in which they were made, before I came here, and have not yet been restored to it.

Dr. BRADLEY having been furnished by Government in the year 1750 with a brass mural quadrant of eight feet radius, constructed by that excellent artist Mr. JOHN BIRD, an instrument far superior to any before used in the practice of astronomy, assiduously observed the pole star and other stars lying to the north of the zenith with it for upwards of three years, and then removed it to the opposite side of the wall, making it change place with the iron quadrant of the same radius constructed by Mr. GRAHAM, likewise an excellent instrument, though inferior to this, and commenced a regular series of observations of the sun, planets, and fixed stars, which have been ever since continued in the same manner. Moreover, the temperature of the air, shewn by the barometer and thermometer, is affixed to each observation; and the zenith point of the quadrant settled from time to time by the help of a zenith sector of  $12\frac{1}{2}$  feet radius, turned alternately contrary ways, the same with which Dr. BRADLEY had before made his two useful and admirable discoveries of the aberration of light and the nutation of the earth's axis.

By the observations of the pole star and other circumpolar stars, above and below the pole, Dr. BRADLEY got the apparent zenith distance of the pole; by the apparent and equal zenith distances of the sun at the two equinoxes, having at the same time opposite right ascensions, as found from comparing his observed transits over the meridian with those of

fixed stars, after the manner used by Mr. FLAMSTEED for deducing the right ascensions of the fixed stars, he found the apparent zenith distance of the equator, which lessened by parallax and added to the apparent zenith distance of the pole gave a sum less than  $90^\circ$  by the sum of the two refractions belonging to the pole and meridian zenith distance of the equator. But he remarked, that the difference of refractions, belonging to these zenith distances, would come out the same within 2 or 3'' by any of the best tables then extant, whether deduced solely from observations, or partly from observations and partly from theory. The sum and difference of refractions answering to the pole and equator being thus given, the refractions themselves are given, the greater of which added to the apparent zenith distance of the equator gives the latitude of the place, and the less refraction added to the apparent zenith distance of the pole gives the co-latitude.

He afterwards, from the consideration that the refractions at the pole and equator may be taken without sensible error as the tangents of the zenith distances, according to Mr. THOMAS SIMPSON's theory of refractions in his *Mathematical Dissertations*, divided more accurately the sum of the refractions at the pole and the equator into the just parts answering to each zenith distance, and thereby found the latitude with more exactness. In this manner he found the latitude of the Royal Observatory to be  $51^\circ 28' 39''\frac{1}{2}$ , and the mean refraction at  $45^\circ 3'$  to be 57'', the barometer standing at 29,6 inches, and the thermometer of FAHRENHEIT's scale at  $50^\circ$ .

But, not to let a matter of so much consequence rest on my assertion or memory, when further proof can be given of it, I have by me, in the hand-writing of Dr. BRADLEY, among other particulars, his calculations of the latitude of the Obser-

vatory from his observations, according to the manner above explained; in which he first states it at  $51^{\circ} 28' 38''$ , and finally more correctly in these words. "The apparent zenith distance of the equator, by the mean of 20 observations in 1746-47,  $51^{\circ} 27' 28''$ . The mean apparent distance of the pole, by the observations made between 1750-52,  $38^{\circ} 30' 35''$ . Sum  $89^{\circ} 58' 3''$ . Sum of refractions  $1' 57''$ . Polar refraction  $0' 45''\frac{1}{2}$ . Equatorial refraction  $1' 11''\frac{1}{2}$ . Latitude  $51^{\circ} 28' 39''\frac{1}{2}$ . Co-latitude  $38^{\circ} 31' 20''\frac{1}{2}$ ."

The latitude of the Observatory being thus settled, as well as the quantity of refractions for all stars passing the meridian between the pole and the equator, Dr. BRADLEY readily inferred from his observations the true distance of all such stars from the north pole, which, compared with their zenith distances observed below the pole, gave the refractions at those lower altitudes. Finally, by comparing the refractions together observed in extreme degrees of heat and cold, he deduced the law of their variation as affected by heat and cold; and thus at length he inferred his elegant rule for determining the refraction in all circumstances, that it is to  $57''$ , in the direct compound ratio of the tangent of the apparent zenith distance lessened by 3 times the refraction to the radius, and of the height of the barometer in inches to 29,6 inches, and in the inverse ratio of the degree of height of FAHRENHEIT'S thermometer increased by 350 to 400.

But it may be proper to confirm this rule for refractions also from the same manuscript of Dr. BRADLEY, which I before cited for confirming the latitude, by the following passage, which immediately follows the other. "Suppose the mean refraction at  $45^{\circ} 3' = 57''$ , and  $y = 350$ ; then  $y + z$ : bar. ::  $77''$ : refr. at  $45^{\circ} 3'$ ."

"Rad.

“ Rad. : tan.ZD :: 3' :  $m$  . . . . Rad. :  $\overline{\tan.ZD - m}$  :: refr. at  $45^{\circ} 3'$  :  $r$  ”  
 “ [the refraction required].” It is easy to see that this rule agrees with the other: for putting  $t = 50$ , and barometer = 29,6, the first analogy, putting the barometer down in tenths of an inch, is  $350 + 50 = 400 : 296 :: 77'' : 56'',98$  for the refraction at  $45^{\circ} 3'$ , or  $57''$  within  $\frac{1}{300}$ th of a second. The second analogy serves to give the treble refraction nearly, called  $m$ . Whence it is evident, the last analogy coincides with the rule above given.

This valuable rule was first communicated by myself to the public in vol. LIV. of the Philosophical Transactions, p. 265. and in p. 49. and 129. of the first edition of Tables requisite to be used with the Nautical Almanac, together with a Table of the mean Refractions deduced from it, with the first Nautical Almanac, that of 1767, published by order of the Commissioners of Longitude in 1766; and again, at page the 5th of the Explanation and Use of the Astronomical Tables, annexed to the first volume of my Observations made at the Royal Observatory from 1765 to 1774, published by order of the President and Council of the Royal Society, with two tables in that work, containing the mean refractions and decimal multipliers for reducing them to any given temperature of the air indicated by the barometer and thermometer. The words in page the 5th of the said preface are as follows. “ The  
 “ astronomical refractions and latitude of the Observatory were  
 “ settled with the greatest accuracy by Dr. BRADLEY, from his  
 “ observations of the circumpolar stars, with the brass mural  
 “ quadrant, during the three years that it was turned to the north,  
 “ and of the sun and stars in the subsequent years after it was  
 “ removed to point to the south. The following elegant rule  
 “ was

“ was the result of his observations, that the refraction at any  
 “ altitude is to 57 seconds, in the direct compound ratio of the  
 “ tangent of the apparent zenith distance lessened by 3 times  
 “ the refraction to the radius, and of the altitude of the baro-  
 “ meter in inches to 29,6 inches, and in the reciprocal ratio  
 “ of the height of FAHRENHEIT'S thermometer increased by  
 “ the number 350, to the number 400. Tables XXII. and  
 “ XXIII. were adapted to this rule; the first containing the  
 “ mean refractions answering to 29,6 inches height of the  
 “ barometer and 50 degrees height of the thermometer; and  
 “ the second table containing decimals for multiplying the  
 “ mean refraction in order to find the correction, which applied  
 “ to it will give the actual refraction, the same as would have  
 “ been produced by the rule with somewhat more trouble.  
 “ Dr. BRADLEY supposed the horizontal parallax of the sun  
 “  $10\frac{1}{2}$  seconds, in the calculations from which he inferred the  
 “ refractions; and I have been informed, that he determined  
 “ the latitude of the Observatory  $51^{\circ} 28' 39''\frac{1}{2}$ . But, had he  
 “ made use of the true parallax  $8''8$  or  $8''\frac{3}{4}$ , as found by the  
 “ two late transits of Venus over the sun, he would have made  
 “ the refraction at the altitude of  $45^{\circ}$  to be  $56''\frac{1}{2}$  instead of  
 “  $57''$ , and the latitude of the Observatory exactly  $51^{\circ} 28' 40''$   
 “ instead of  $51^{\circ} 28' 39''\frac{1}{2}$ . But his rule for refractions can-  
 “ not be corrected for all altitudes, without examining his ob-  
 “ servations of refractions made at various times.”

On comparing this extract with M. CASSINI'S Memoir, I cannot but express my surprize, that he should not have adverted to a passage containing so direct an application to the grounds of his Memorial, in a publication of such notoriety, and of so old a date as 1776; had he done so, I cannot but think, he would never have hazarded such an opinion as that advanced by him  
 in



in his Memoir of an uncertainty of 15'' in the latitude of Greenwich; but he might have been induced to believe, that the latitude of this place had been well determined.

For further confirmation of the certainty of the astronomical refractions, and latitude of the Observatory, as settled by Dr. BRADLEY, it may be proper to add, that the Greenwich brass mural quadrant underwent a trial, which all astronomical instruments ought to be submitted to, but which very few ever have been, on account of the difficulty and nicety of the operation, namely, an examination of the total arc; when it was found by Dr. BRADLEY to be an accurate quadrant, the arc appearing at one trial to differ only a fraction of a second from 90°, and another time, after an interval of above six years, to be a perfect quadrant. See p. 24. of BIRD'S Method of constructing Mural Quadrants, published by the Board of Longitude in 1768. In like manner he had before examined the total arc of the iron quadrant, first put up by Mr. GRAHAM, for the use of Dr. HALLEY, in the year 1725, by means of a level, and found it to be 16'' less than a quadrant. See BIRD'S Method of constructing Mural Quadrants, p. 7. and Memoires of the Royal Academy of Sciences at Paris, for 1752, p. 424. But this quadrant was, in the year 1753, re-divided by Mr. BIRD, and, in this respect, probably rendered as accurate as the other. See BIRD'S Method of constructing Mural Quadrants, p. 24.

Dr. BRADLEY made a curious use of the new set of divisions, soon after they were laid upon the quadrant, to re-examine the error of the total arc laid down originally by Mr. GRAHAM (which by the plumb-line and level he had found to be 16'' less than a quadrant in 1745) according to the following passage contained in the manuscript before cited.

“ August

“ August 12, 1753, I measured with the screw of my micrometer the difference of the arcs (of  $\frac{64}{90}$ ) as set off by Mr. GRAHAM originally, and by Mr. BIRD when he put on a new set of divisions upon the old quadrant, and I found that Mr. GRAHAM'S arc was less than Mr. BIRD'S by  $\frac{8}{40}$  divisions of my micrometer, which to a radius of 96 inches answers to 10'',6; so that the whole arc of 96 differs from a true quadrant 15'',9, which is the same difference that I formerly found by means of the level, &c.”

Let me further add, that Dr. BRADLEY had informed me, that he had found the same refractions, latitude of the Observatory, and obliquity of the ecliptic, by both quadrants, making a proportionable allowance, in the use of the iron quadrant, for the error of 16'' in the total arc in proportion to the zenith distance of the object before it was new divided.

The Rev. Dr. HORNSBY, F. R. S. Savilian Professor of Astronomy at Oxford, to whose care Dr. BRADLEY'S original observations have been committed, in order to their being printed and published, having favoured me with calculations of the latitude of the Royal Observatory from observations of the pole star made with both quadrants, from a manuscript of Dr. BRADLEY, I think it proper to give it a place here, not only as a very curious paper, but also as strongly confirming the latitude of this place before stated.

*Transcribed from a loose Paper of Dr. BRADLEY.*

“ The mean zenith distances of the pole star above and below the pole, corrected by refraction, aberration, &c. and reduced to January 1751, O. St. as collected from the observations made after the new quadrant was balanced, Nov. 24, 1750.

Number of obser- vations.	Above the pole.	Number of obser- vations.	Below the pole.	to
23	36° 29' 46",66	8	40° 33' 2",95	Jan. 15, 1751
22	45,42	26	3,37	Aug. 27, 1751
23	45,13	27	2,14	May 4, 1752
19	44,63	28	1,67	Nov. 10, 1752
28	45,00	23	1,74	May 31, 1753
9	44,59	8	1,54	July 26, 1753
124	36 29 45,24	120	40 33 2,24 36 29 45,24	

77 2 47,48  
38 31 23,74  
1,74

Error of collimation

Co-latitude 38 31 22, 0

77° 1' 15,8

Collimation 38 30 37,9  
Refraction + 1,4  
+ 45,5  
38 31 22,0  
By new quad. 51 28 38,0

Refraction.

42" +  
49 -

36° 29' 2½"  
40° 32' 13½"

4° 3' 10,8  
+ 6,6

6½  
Refraction

4 3 11  
+ 6½

4 3 17,4

4 3 17½

2 1 39 - the mean distance  
from mean pole Jan. 1751.

The apparent zenith distance of the pole, by the mean of 310 observations, is

38° 30' 36" allowing - 2" for the error of the line of collimation.

Refraction + 45½

38 31 21½

Latitude 51 28 38½

Co-lat. 38 31 21½ by the new quadrant.

38 31 18½ by the old quad, new divisions.

Apparent zenith distances of the pole observed with the iron quadrant.				
1753	Apparent zenith distance of the pole.	Barom.	Thermom.	
			in.	out.
		Inches.		
Sept. 13	38° 30' 40,1	30,10	64	65
23	41,5	29,88	61	60
Oct. 2	42,5	29,67	56	53
5	41,0	30,02	57	57
11	41,1	29,48	61	57
19	40,2	29,82	50	43
31	40,8	29,65	42	34
Nov. 5	42,1	29,69	45	39
16	40,5	29,39	42	35
19	42,2	29,59	40	35
20	40,9	29,84	40	33
24	40,0	30,00	43	35
29	40,0	29,80	38	30
Dec. 3	39,3	30,16	39	33
8	37,4	30,06	35	27
17	41,5	29,50	50	49
30	38,0	30,11	33	22
Mean Refraction	38° 30' 40,5 + 46,4	29,81	47	41½
Col.	- 8,4			
Pol. corr.	38° 31' 18,5			

So far the manuscript.

Thus the latitude by the brass quadrant being  $51^{\circ} 28' 38''\frac{1}{2}$ , and by the iron quadrant with new divisions  $51^{\circ} 28' 41''\frac{1}{2}$ , the mean by both quadrants is  $51^{\circ} 28' 40''$ , or only half a second greater than settled in another manner, according to the manuscript of Dr. BRADLEY in my possession. Also the apparent zenith distance of the pole with the mean refraction  $45''\text{,}4$  being  $38^{\circ} 30' 36''\text{,}1$  by the brass quadrant, and  $38^{\circ} 30' 33''\text{,}1$ , by the iron quadrant, the mean by both is

$38^{\circ}$

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38° 30' 34'',6, or only  $\frac{4}{10}$ ths of a second less than by Dr. BRADLEY's manuscript cited before.

On my promotion to the Royal Observatory in 1765, finding its latitude to have been so accurately settled by Dr. BRADLEY before me, I might have thought myself dispensed from making any particular or very laborious observations for that purpose; however, I confirmed it by my own observations to great nearness, *viz.* within 1 or 2'', at the same time that I was establishing a new catalogue of the principal fixed stars, continually observed here for settling the right ascensions of all other celestial objects with the transit instrument. The result in brief is as follows.

I first settled the relative right ascensions of about 30 of the brightest fixed stars, and lying nearest the equator, by a great number of observations with the transit instrument, referring them to  $\alpha$  Aquilæ as the fundamental star, whose right ascension I assumed from Dr. BRADLEY's determination. Hence, by observed transits of the sun and the same stars, in the spring and autumn, when his daily motion in declination was at least 16' or two-thirds of the greatest, I inferred the sun's right ascensions relative to the right ascensions of those stars settled in the manner just mentioned. Also from the sun's observed zenith distances taken with the brass mural quadrant on the same days, and corrected by refraction, parallax, and error of line of collimation, with Dr. BRADLEY's obliquity of the ecliptic, and latitude of the Observatory, I computed the sun's declinations, and thence the right ascensions corresponding to them.

Now, if the assumed right ascension of  $\alpha$  Aquilæ, and thence those of the other stars were affected with some small error, as might be supposed, the sun's right ascensions deduced from the observed transits would differ the same way from the

truth at both seasons of the year, *viz.* by the unknown error of the assumed right ascension of  $\alpha$  Aquilæ; but his right ascensions inferred from his observed zenith distances would be affected contrary ways at the two opposite seasons of the year, by the unknown errors in the refractions, parallaxes, latitude of the place, and obliquity of the ecliptic. Hence, the mean of the two corrections of the sun's right ascension, found from the observed declinations about the vernal and autumnal equinox, would be the true correction of the assumed right ascension of  $\alpha$  Aquilæ; and the difference of the same corrections would, by an easy calculation, shew how much the computed declinations were too great or too little for the truth, and consequently what the true declinations were, and what the true zenith distance of the sun was, when in the equator, or the latitude of the place, on supposition that Dr. BRADLEY'S refractions were truly stated; for any small uncertainty in the obliquity of the ecliptic, as stated by him, could not affect this result, which was deduced equally from observations of the sun in north and south declination, when the same error of the obliquity would affect the sun's right ascensions deduced from the observed declinations contrary ways. I took the sun's parallax from the 24th of my tables annexed to my observations, constructed upon a horizontal parallax  $8''{,}84$ , which I had deduced from the observations of the first transit of Venus, that in 1761, and differing insensibly from  $8''{\frac{1}{2}}$ , which I deduced from the observations of the total durations of the transit between the internal contacts observed at Wardhus and Otaheite in 1769, consequently more correct than the horizontal parallax of  $10''{\frac{1}{2}}$  used by Dr. BRADLEY. It is also evident, that the true zenith distance of the equator thus found, diminished by Dr. BRADLEY'S mean refraction, will be the apparent

zenith distance of the equator, affected only by the mean refraction.

I shall now give the apparent zenith distance of the equator, and the true latitude of the Observatory, resulting in this manner from my observations of six years, from the autumnal equinox of 1765 to that of 1771, in which I allowed 0'',9 for the correction of the error of the line of collimation, additive to the observed zenith distances, as I found from a revision of my calculations of the zenith distances of stars taken with the mural quadrant, compared with the like taken with the zenith sector in 1768.

Years of the observations,	Number of days of observations.	Apparent zenith distance of equator, taking the sun's horizontal parallax 8'',8.	True latitude of the Observatory, according to Dr. BRADLEY's refractions and the sun's horizontal parallax 8'',8.
Autumnal equinox of 1765 and vernal of 1766	52	51° 27' 28,6	51° 28' 40,1
Autumnal equinox of 1766 and vernal of 1767	38	30,4	41,9
Both equinoxes of 1768 — —	46	32,2	43,7
Both equinoxes of 1769 — —	48	28,7	40,3
Both equinoxes of 1770 — —	20	29,3	40,8
Both equinoxes of 1771 — —	42	29,7	41,3
Mean from six years observations — —		51 27 29,8	51 28 41,3
Mean found by Dr. BRADLEY, with ☉'s horizontal parallax 10'' $\frac{2}{3}$ — —		51 27 28	51 28 39,5
But if 1'',2 be added to reduce them to the ☉'s horizontal parallax 8'',8, Dr. BRADLEY's result will be changed to — —		51 27 29,2	51 28 40,7
Differing from my determination above only		0,6	0,6

In further confirmation of the latitude of the Observatory, I shall now adduce eight years observed zenith distances of the sun in the solstices, being deduced from a number of observations taken at and near the solstices, and corrected for line of collimation, refraction, parallax, and nutation.

Years

Years of observations.	Summer solstitial zenith distance reduced.	N <sup>o</sup> of days of observations.	Winter solstitial zenith distance.	N <sup>o</sup> of days of observations.	Half sum or latitude of the place.	Half difference or obliquity of the ecliptic.
1765	28° 0' 30",2	6	74° 56' 46",1	5	51° 28' 38",2	23° 28' 8",0
1766	33,6	7	48,8	2	41,2	7,6
1767	32,0	6	{uncertain. from an accident.}	—	—	—
1768	29,7	6	44,9	7	37,3	7,6
1769	31,8	8	46,2	5	39,0	7,2
1770	30,7	7	44,1	6	37,4	6,7
1771	32,0	8	42,2	9	37,1	5,1
1772	31,8	12	Line of collimation altered by applying an achromatic object-glass to the telescope.			
Mean Latitude	28° 0' 31,5 51° 28' 40"		74° 56' 45,4		51° 28' 38,4	23° 28' 7,0 On Jan. 1, 1769.
23° 28' 8,5 mean obliquity of ecliptic on Jan. 1, 1769.						

This last obliquity 23° 28' 8",5 is deduced in the manner used by Dr. BRADLEY, and is more to be depended on than the other 23° 28' 7",0 deduced from both solstices, on account of the less certainty of the lower refractions, from which, however, it only differs a second and a half.

Thus all the observations of the sun and circumpolar stars accord to 1" or 2" with the latitude of the Observatory settled by Dr. BRADLEY, making use of his refractions.

I shall now determine the latitude independent of Dr. BRADLEY's refractions, and infer the higher refractions at the same time, from a comparison of my observations of the apparent zenith distance of the equator before set down with Dr. BRADLEY's observations of the apparent zenith distance of the pole, both taken with the same excellent brass mural quadrant, in the same manner as Dr. BRADLEY deduced them from



from the apparent zenith distance of the equator observed with the iron quadrant compared with the apparent zenith distance of the pole observed with the brass quadrant, according to the extract from the manuscript in my possession before cited.

The mean apparent zenith distance of the equator, by my observations of six years from 1765 to 1771, was related before  $51^{\circ} 27' 29''{,}8$ . The mean apparent zenith distance of the pole was found by Dr. BRADLEY from 1750 to 1752 to be  $38^{\circ} 30' 35''$ . Their sum  $89^{\circ} 58' 4''{,}8$  taken from  $90^{\circ}$  leaves  $1' 55''{,}2$ , the sum of the refractions at the two zenith distances. Saying then, as  $1' 56''{,}7$  the sum of the refractions by Dr. BRADLEY'S rule, to  $1' 55''{,}2$  the sum by observation, so are  $1' 11''{,}4$  and  $45''{,}3$  the respective refractions at the two apparent zenith distances of the equator and pole by Dr. BRADLEY'S rule, to  $1' 10''{,}5$  and  $44''{,}7$  the two refractions at those zenith distances, which added to them give the co-latitude  $38^{\circ} 31' 19''{,}7$ , and the latitude  $51^{\circ} 28' 40''{,}3$ . And as  $1' 56''{,}7 : 1' 55''{,}8 ::$  so is  $57''$  the refraction at the apparent zenith distance  $45^{\circ} 5'$  by Dr. BRADLEY :  $56''{,}27$  the true refraction at that zenith distance, or not half a second differing from Dr. BRADLEY'S, but more to be depended on as deduced from observations made with the brass quadrant only, and calculated from a parallax of the sun nearer to the truth.

But if the apparent zenith distance of the pole be made use of, resulting from a mean of 310 observations made with the brass quadrant, according to Dr. BRADLEY'S manuscript, communicated by Dr. HORNSBY, from the whole of his observations from 1750 to 1753, *vis.*  $38^{\circ} 30' 36''$ , the sum of this and  $51^{\circ} 27' 29''{,}8$ , the apparent zenith distance of the equator found by myself with the iron quadrant, or  $89^{\circ} 58' 5''{,}8$  taken from  $90^{\circ}$  leaves  $1' 54''{,}2$ , the sum of the two refractions

at the pole and equator. Whence the refraction at the pole will be found in like manner as before  $44''$ , $3$ , and that at the equator  $1' 9''$ , $9$ , and the latitude  $51^{\circ} 28' 39''$ , $7$ , and the refraction at the apparent zenith distance of  $45^{\circ} 3' = 55''$ , $8$ , which is  $1''$ , $2$  less than Dr. BRADLEY's determination, and  $1''$ , $2$  greater than deduced from Mr. HAWKSBEЕ's experiment of the refraction of the air hereafter cited. It will be shewn in the sequel, that the latitude thus found does not at all depend on the truth of the total arc, but only supposes the instrument proportionally divided at the points answering to the pole and the equator.

From the whole then I conclude, that the latitude of the Royal Observatory at Greenwich is firmly established from Dr. BRADLEY's observations and my own at  $51^{\circ} 28' 40''$ , probably without the error of a single second.

Let us now inquire into the latitude of the Royal Observatory at Paris. M. LE MONNIER, in the Memoires of the Royal Academy of Sciences for 1738, and in his *Histoire Celeste*, has examined into the latitude of the Royal Observatory at Paris, resulting from the observations of the principal French astronomers, and assuming the refraction at the height of the pole at Paris to be  $50''$ , which is  $2''$  less than DOMINICO CASSINI's table gives, and the same which Dr. BRADLEY's rule gives, he finds the latitude of their Royal Observatory as follows:

From the observations of M. PICARD	-	$48^{\circ} 50' 10''$
_____ of M. DE LA HIRE	-	$48 50 12$
_____ le Chev. DE LOUVILLE		$48 50 8$
_____ M. Maraldi	-	$48 50 14$

His

His own observations in 1738, after examining and making an allowance for the error of the total arc of his quadrant - - - - - 48 50 14

His further observations in 1740, making allowance for the error of the total arc of his quadrant, and considering the effect of the state of the air indicated by the thermometer upon the refractions 48 50 15

In the Memoires of the Royal Academy of Sciences for 1744, M. CASSINI DE THURY (the author of the memoir) finds from his own observations, with the same refractions - - - - - 48 50 12

In the Memoires of 1755, the Abbé DE LA CAILLE, from a nice and accurate calculation of his observations made at the College of Mazarine, at Paris, and the Cape of Good Hope, deduces new tables of refraction suitable to each place, and states their respective latitudes, and thence that of the Royal Observatory at Paris - - - - - 48 50 14

Hence the ancient observations of M. PICARD, M. DE LA HIRE, and the Chevalier DE LOUVILLE give 48 50 10

The modern and more accurate observations of M. MARALDI, M. LE MONNIER, M. CASSINI DE THURY, and the Abbé DE LA CAILLE, give - 48 50 14;

which is now generally made use of by the French astronomers as the true latitude of their Royal Observatory; and from the near agreement of so many diligent observers and able astronomers cannot be supposed to differ above 2 or 3'' from the truth. The difference of this and 51° 28' 40'', the latitude of the Royal Observatory at Greenwich above stated, is 2° 38' 26'', the true difference of latitude of the two Observatories, which,

from what has been said of the observations on which the respective latitudes were founded, cannot be supposed to differ above 3 or 4'' from the truth. What then becomes of the uncertainty of 15'' supposed by the late M. CASSINI?

The same difference of latitude I find nearly from a comparison of my own observations of  $\gamma$  and  $\beta$  Draconis, taken with the zenith sector in 1768, with those of the Abbé DE LA CAILLE in 1750 and 1756, given in his *Fundamenta Astronomiæ*, after making the proper allowances for aberration, precession, and nutation, and correcting my observations by Dr. BRADLEY's refraction, and the Abbé DE LA CAILLE's by his table, and making allowance for the distance of the Abbé DE LA CAILLE's Observatory from their Royal Observatory; *viz.*  $2^{\circ} 38' 25'',4$  from  $\gamma$  Draconis, and  $2^{\circ} 38' 26'',1$  from  $\beta$  Draconis; the mean being  $2^{\circ} 38' 25'',7$ , differing only  $0'',3$  from that stated above; but from Dr. BRADLEY's observations  $2^{\circ} 38' 24'',9$ , and  $2^{\circ} 38' 27'',2$ , mean  $2^{\circ} 38' 26'',0$ . It is too well known to astronomers to need my pointing out, that the best method of determining the difference of latitude of places, differing but little in latitude, is by such differences of zenith distances of stars passing near the zeniths, as the two above cited, observed at both places, in the same manner as the amplitude of the celestial arc is observed for finding the length of a degree of the meridian by comparison with geometrical measures.

The question now will be, upon what foundation was the late M. CASSINI's supposition of an uncertainty of 15'' in the latitude of Greenwich built? This appears evidently to have been upon a passage in the Abbé DE LA CAILLE's researches into the astronomical refractions and latitude of Paris, contained in the Memoires of the Royal Academy of Sciences for

1755, p. 578, 579, where M. DE LA CAILLE takes the differences of zenith distances of 14 stars observed by Dr. BRADLEY (in correspondence to the same observed by himself at the Cape of Good Hope, for determining the moon's parallax in declination) published in the Memoires of the Royal Academy of Sciences for 1752, and the same observed by himself at Paris, after his return from the Cape, and correcting them for the difference of the refractions at the respective zenith distances, according to his own table of refractions, and the known apparent motions of the stars, finds the mean  $2^{\circ} 37' 23''{,}9$ , which added to  $48^{\circ} 51' 29''{,}3$ , his latitude at the College of Mazarine, gave him  $51^{\circ} 28' 53''{,}2$  for the latitude of Greenwich, exceeding Dr. BRADLEY's latitude by 13 or 14''.

Now the legitimacy of this conclusion depends upon a supposition that both instruments measured the true angle, or that their total arcs were justly laid off, and that the Abbé DE LA CAILLE's table of refractions is just. The first indeed has been proved with respect to Dr. BRADLEY's quadrant, but never has been attempted with respect to the Abbé DE LA CAILLE's sextant; for the examination which the Abbé made of his instrument by parts for every  $7^{\circ}\frac{1}{2}$  (see Memoires of the Royal Academy of Sciences for 1751 p. 405.), could not determine the error of the whole arc, as the difference from the truth might be insensible upon such small arcs, and the examination seems to have been intended to find the differences of these small arcs from one another rather than from the true arc which they represent. We may therefore be allowed to doubt of the truth of this circumstance. This doubt will be further strengthened by several particulars which I shall adduce.

1. The apparent altitude of the pole at the Royal Observatory  $48^{\circ} 51' 12''$ , resulting from the Abbé DE LA CAILLE's

observations, exceeds  $43^{\circ} 51' 4''$  the mean of the observations of Mess. MARALDI, LE MONNIER, and CASSINI DE THURY, by  $8''$ , and at the same time his refractions for that altitude exceed what they adopt by the same quantity. 2. His refractions are greater than all other tables give, DOMINICO CASSINI's, FLAMSTEED's, NEWTON's, BRADLEY's, MAYER's, SIMPSON's, and Lord MACCLESFIELD's. The latter I have by me in a manuscript of Dr. BRADLEY, being what he used to correct his observations by, before he had been enabled to determine the refractions with the new mural arc. They were deduced from a brass quadrant of 5-foot radius made by Mr. SISSON, still remaining in the Observatory at Sherburn-Castle, and are the more to be esteemed because the divisions of the instrument had been submitted to the *strictest re-examination*, whereby, in the opinion of Dr. BRADLEY, it was probably rendered *as perfect in its kind as any extant, or as human skill could at that time produce*. See Dr. BRADLEY's Letter to Lord MACCLESFIELD, Phil. Transf. vol. XLV. p. 5. The refractions in this table are less than Dr. BRADLEY's by  $2''$ ,  $4$  at the altitude of  $45^{\circ}$ , and  $4''$  at the altitude of  $20^{\circ}$ . MAYER's refractions agree almost exactly with Dr. BRADLEY's, and are entitled to much weight, having been determined by a 6-foot mural arc constructed by Mr. BIRD. 3. The refractions were found by the French Academicians at the polar circle, according to M. MAUPERTUIS's Book on the Figure of the Earth, to agree nearly with DOMINICO CASSINI's table. Hence it may be inferred, that the refractions in a warmer climate, as France, should be less than according to the same table, and therefore much less than according to M. DE LA CAILLE's, and approaching to Dr. BRADLEY's, which are a little less than M. CASSINI's. 4. M. LE MONNIER, after his return from the polar circle, with a quadrant examined at the zenith and horizon, and after making allowance for the error thereby inferred in the total arc, ob-

served a great many refractions of stars under the pole, with the state of the thermometer, and sometimes of the barometer also, as recorded in his *Histoire Céleste*. These I calculated formerly, and found the refractions observed in very hot and very cold weather, compared together, to follow the same rate of increase and decrease, according to the changes of temperature, as Dr. BRADLEY has assigned; and, reducing the observed refractions to the mean temperature, I found them agree nearly with Dr. BRADLEY's. 5. The refractive power of the air about its mean temperature was carefully observed by Mr. HAWKSBEЕ, as related in his *Physico-Mechanical Experiments*, and the ratio of the sine of incidence to that of refraction out of air into a vacuum found to be as 999736 to 1000000. Hence the astronomical refraction at the altitude of  $45^{\circ}$  should be  $54''{,}6$ , only  $2''{,}4$  less than Dr. BRADLEY's, and  $2''$  less than the same when his higher refractions are new calculated with the true parallax of the sun, and  $1''{,}2$  less than I have before shewn to result from my observations of the apparent zenith distance of the equator compared with Dr. BRADLEY's of the apparent zenith distance of the pole, both taken with the same brass mural quadrant, but  $12''$  less than the Abbé DE LA CAILLE's.

From all these facts, I think, I may be allowed to conclude, that the Abbé DE LA CAILLE's refractions are not just, but considerably too large; and, consequently, as there can be no doubt of the care or diligence used by this astronomer in his observations and calculations, that the total arc of his instrument is too large for the radius, and, as I shall shew presently, gives the measures of the zenith distances too small.

But it may be asked, are then all the observations of this great astronomer, with their results, the fruit of so much labour and pains, to be considered as uncertain or lowered in their value in proportion to the error of his instrument? I am happy to answer,

answer, that the very ingenious method which he used of getting his refractions from the comparison of the sum of the apparent altitudes of the poles at Paris and the Cape with the sum of the apparent zenith distances of stars passing the meridian between the two places, has fortunately, without his being aware of it, given him the refractions affected with the error of the arc of the instrument, and consequently proper for correcting his observations; for, if the instrument be supposed ill divided, any error in the divisions will naturally be thrown upon the refractions; and, if the total arc is too large for the radius, the stars will appear to approach the zenith by the error of the divisions as well as the refractions, and the refractions in the table will come out too large, but still suitable to the instrument, because a correction is necessary to be added to the observed zenith distance, on account of the error of the instrument, as well as of the true refractions, and the table deduced from the instrument gives the sum of the two corrections together, without determining them separately.

Hence his table of refractions, though well adapted to his instrument, may be very unfit to be applied to any other. His latitudes of his observatories and his declinations of the stars will not lose any of their certainty, at least within the limits of the zenith distances measured by his sector, *viz.*  $60^{\circ}$ . And this accounts for a circumstance, at first sight rather extraordinary, that his declinations of stars should agree so nearly (generally within  $5''$  of Dr. BRADLEY's, as Dr. BRADLEY himself remarked) though his refractions made use of were so very different.

Having now shewn, that the Abbé DE LA CAILLE's refractions are too great, and only fit to be applied to his own instrument, it will be easy, by a just calculation, to reconcile the  
before



before mentioned zenith distances of 14 stars, observed at Greenwich by Dr. BRADLEY and by the Abbé DE LA CAILLE at the College of Mazarine at Paris, with the established latitudes of the two Observatories, nearly; in doing which I shall claim the same right to correct Dr. BRADLEY's observations by his table of refractions, as I have allowed the Abbé DE LA CAILLE to be intitled to correct his observations by his table of refractions; which, I think, will be allowed me, after what I have said of the manner in which the Greenwich refractions were deduced and the instruments made use of. The difference of latitude of the College of Mazarine and the Royal Observatory at Greenwich will then come out by the several stars, as follows;  $2^{\circ} 37' 12'',7$ ,  $16'',0$ ,  $13'',8$ ,  $13'',7$ ,  $18'',4$ ,  $17'',7$ ,  $19'',7$ ,  $23'',2$ ,  $17'',7$ ,  $17'',0$ ,  $13'',9$ ,  $12'',4$ ,  $5'',6$ ,  $11'',9$ . The mean is  $2^{\circ} 37' 15'',2$  (or  $8'',7$  less than the Abbé DE LA CAILLE's result in his method of calculation, which I have shewn to be inadmissible) and added to  $48^{\circ} 51' 29'',3$ , the latitude of the Abbé DE LA CAILLE's Observatory gives  $51^{\circ} 28' 44'',5$  for the latitude of the Royal Observatory at Greenwich, only  $4''\frac{1}{2}$  more than established by Dr. BRADLEY's observations and my own; a sufficient agreement, especially considering that many of the stars were at great distances from the zenith, and that no account has been made of the temperature of the air at the times of the observations. The proper method, however, of settling the difference of latitude of two Observatories is by stars near the zenith, as I observed before; and the difference of the latitudes of the two Observatories of the College of Mazarine and Greenwich, by the Abbé DE LA CAILLE's observations of  $\beta$  and  $\gamma$  Draconis compared with mine, was  $2^{\circ} 37' 10'',4$ , and compared with Dr. BRADLEY's  $2^{\circ} 37' 10'',7$ , the first of which, added to the Abbé DE LA CAILLE's latitude, gives

$51^{\circ}$

$51^{\circ} 28' 39''.7$ , and the other  $51^{\circ} 28' 40''$ , for the latitude of the Royal Observatory at Greenwich, exactly agreeing with that deduced immediately from the observations made at this place.

The same result nearly follows from M. CASSINI DE THURY'S own observations of the zenith distance of the sun at the summer solstice of 1755, contained in the Memoires of the Royal Academy of Sciences for that year, compared with Dr. BRADLEY'S, which latter was communicated to me by the late JOHN HOWE, Esq.; for, by M. CASSINI'S observations, the solstitial altitude of the sun's upper limb, corrected by the difference of refraction and parallax, according to DOMINICO CASSINI'S table, which happens to agree with the same difference by my tables at this height, was  $64^{\circ} 53' 36''$ , from which  $15' 47''$  being subtracted for the semi-diameter of the sun according to MAYER'S tables, there remains  $64^{\circ} 37' 49''$ , the true altitude of the sun's center, and consequently the sun's true zenith distance  $25^{\circ} 22' 11''$ . But the same was found from Dr. BRADLEY'S observations, by my tables of refractions and the sun's parallax,  $28^{\circ} 0' 32''.8$ . The difference  $2^{\circ} 38' 21''.8$  or  $2^{\circ} 38' 22''$  is the difference of latitude of the two Observatories, which added to  $48^{\circ} 50' 14''$ , the latitude of the Royal Observatory at Paris, gives  $51^{\circ} 28' 36''$  for the latitude of the Royal Observatory at Greenwich, or only  $4''$  less than before stated from the Greenwich observations, the difference lying the contrary way to that which the Abbé DE LA CAILLE carried the latitude of Greenwich, by improperly applying his own table of refractions to the Greenwich observations as well as to his own.

The Abbé DE LA CAILLE having, in the sequel of his memoir, inferred the difference of latitude of Gottingen and the College of Mazarine from 22 stars observed by M. MAYER with

with a 6-foot mural quadrant of BIRD's construction, correspondent to the same observed by himself, I shall make some remarks on this comparison, because it appears to me to afford a strong argument to shew that the Abbé DE LA CAILLE's refractions are too great; and that MAYER's, which agree with Dr. BRADLEY's, are just. The Abbé, after correcting the zenith distances of 22 stars observed at both places by his own table of refractions, finds the difference of latitude of Gottingen and Paris, by a mean, to be  $2^{\circ} 40' 35''$ ,<sup>1</sup>, which added to  $48^{\circ} 51' 29''$ ,<sup>3</sup>, the latitude of the College of Mazarine, gives him the latitude of M. MAYER's Observatory  $51^{\circ} 32' 4''$ ,<sup>4</sup>. He adds, that some observations of the pole star sent to him by M. MAYER would give the latitude of Gottingen  $19''$  less than he has established it, as just mentioned. Now I find, that if M. MAYER's observations of the pole star, as well as of the stars to the south of the zenith, be corrected by M. MAYER's table of refractions, and the Abbé DE LA CAILLE's observations by *his* table of refractions, the latitude resulting from M. MAYER's observations of the pole star will agree to  $2''$  with that resulting from the difference of latitude by the stars to the south; for subtracting  $19''$  from  $51^{\circ} 32' 4''$ ,<sup>4</sup>, the latitude which the Abbé DE LA CAILLE has assigned to Gottingen in the manner above-mentioned, there remains  $51^{\circ} 31' 45''$ ,<sup>4</sup> the latitude which he found by the pole star; to which adding  $52''$ ,<sup>8</sup>, the refraction at the mean height of the pole star according to the Abbé DE LA CAILLE, the sum  $51^{\circ} 32' 38''$ ,<sup>2</sup> must be the apparent height of the pole by Mr. MAYER's observations, which diminished by  $45''$ ,<sup>6</sup>, M. MAYER's refraction, gives the true latitude of M. MAYER's Observatory  $51^{\circ} 31' 52''$ ,<sup>6</sup>. But by the difference of the Abbé DE LA CAILLE's and M. MAYER's zenith distances of the 22 stars to

the south, corrected each by their own table of refractions, I find the difference of latitude  $2^{\circ} 40' 32''$ ,0,  $28''$ ,9,  $25''$ 1,  $27''$ ,9,  $22''$ ,6,  $28''$ ,3,  $32''$ .0,  $26''$ ,7,  $27''$ ,2,  $24''$ ,2,  $25''$ ,4,  $31''$ ,3,  $23''$ ,2,  $23''$ ,6,  $28''$ ,9,  $18''$ ,4,  $16''$ ,7,  $22''$ ,0,  $23''$ ,8,  $21''$ ,1,  $27''$ ,3,  $27''$ 3, the mean of which is  $2^{\circ} 40' 25''$ ,6, which, added to  $48^{\circ} 51' 29''$ ,3, the latitude of the College of Mazarine, gives the latitude of Gottingen  $51^{\circ} 31' 54''$ ,9, or only  $2''$ ,3 more than I have deduced above from M. MAYER's observations of the pole star rightly corrected, and only  $0''$ ,9 less than is set down in M. MAYER's tables, which he expressly says, p. 48. of the precepts to his solar and lunar tables, published by myself for the Commissioners of Longitude in 1770, was deduced from his own observations.

I have before, when I shewed the Abbé DE LA CAILLE's refractions to be considerably too great, at the same time vindicated them as fit for his instrument, because he deduced them in a manner which gave him the apparent elevation of objects above their true place by the sum of refraction and the error of his instrument, if his instrument measured the zenith distances too small, as I had concluded it did. The like remark may be applied to Dr. BRADLEY's table; for his refractions at the pole and equator, having been determined with one and the same quadrant, at one time turned to the north to observe the apparent zenith distance of the pole by means of the polar star and other circumpolar stars, and afterwards to the south to observe the apparent zenith distance of the equator, in the manner before explained, must necessarily be the true refractions, if the instrument measured the true angle; and the sum or difference of the true refractions and the errors of the instrument for these zenith distances, in case the instrument did not measure the true angle; and therefore equally proper to correct his observations, whether the total arc was just or not.

Moreover, from the two refractions thus found at the equator and pole, the refractions of the circumpolar stars at their passing the meridian above the pole were computed by Dr. BRADLEY, from the hypothesis that the refractions at considerable altitudes are as the tangents of the zenith distances; which rule is pretty accurately true with respect to the real refractions, and would vary but little from the truth for the apparent refractions, which would be the sum or difference of the true refractions and the errors of the arc, in case the total arc erred from the truth by a very small quantity, not exceeding 10 or at most 20 seconds. The observed zenith distances of the stars above the pole being corrected by the refractions thus computed, and subtracted from the known co-latitude, gave their true distances from the north pole, which added to the co-latitude gave their true zenith distances under the pole; and this diminished by the observed zenith distance would give the refraction under the pole, or the sum or difference of the refractions and the errors of the instrument belonging to their respective zenith distances; and thus his whole table would exhibit the sum or difference of the true refraction and error of the instrument. Hence the latitude of Greenwich established by Dr. BRADLEY, with his quadrant, as well as the latitudes of the Observatories at the College of Mazarine and the Cape of Good Hope, settled by the Abbé DE LA CAILLE with his sextant, and the declinations of the stars and the obliquity of the ecliptic found by both will be very near the truth, independent of the justness of the total arcs, although their respective refractions may be suitable only to their own particular instruments. But, for the reasons before given, I apprehend, the Abbé DE LA CAILLE's refractions to be much too large, and Dr. BRADLEY's to be very near the truth.

I shall now close my enquiry into the latitudes of Greenwich and Paris, and Dr. BRADLEY's and the Abbé DE LA CAILLE's refractions, by a remark naturally arising from my comparison of, and endeavours to reconcile, their observations, which I desire to submit to the consideration of astronomers, it not having, that I know of, been made before; that a table of refractions should be made for every vertical instrument from observations made with itself turned alternately north and south; and that the table, so made, applied to observations made with it, will give the true zenith distances, whether the total arc of the instrument be accurately just, or affected with a small error, or however unequally it be divided below the pole, provided the divisions are equal between themselves in the part of the instrument lying between the equator, the zenith, and the pole.

It remains to give some account of the longitude of Greenwich, or rather of the difference of meridians of Greenwich and Paris, in reply to the late M. CASSINI's doubts on the subject. This had been settled by Dr. BRADLEY at  $9' 20''$ , as he informed me himself, and that he had deduced it from eclipses of Jupiter's first satellite observed at both places, and that he had found it come out the same both from the immersions and emersions. This quantity had been inserted in the table of latitudes and longitudes of places, prefixed to Dr. HALLEY's tables, on the authority of Dr. BRADLEY, so long ago as the year 1749, the date of the publication of those tables, and was generally admitted by astronomers till the year 1763, when the late Mr. JAMES SHORT, F. R. S. computed it from the four transits of Mercury over the sun in 1723, 1736, 1743, and 1753, observed at Paris, London, and Greenwich, to be  $9' 16''$ . See Philosophical Transactions, Vol. LIII. p. 158. In

the year 1776, I requested the late Mr. WARGENTIN, the learned Secretary of the Royal Academy of Sciences at Stockholm, and Author of the improved Tables for computing the Eclipses of Jupiter's Satellites, who collected observations of them from the principal Observatories of Europe, in order to the further improvement of the tables, to inform me what difference of meridians of Greenwich and Paris resulted from my last ten years observations of the eclipses of the first satellite of Jupiter compared with those made by M. MESSIER at Paris. In the answer which he favoured me with, inserted in the Philosophical Transactions, Vol. LXVII. p. 162. he set down the result of the comparisons of eight corresponding immersions and nine emersions observed on both parts, by myself and M. MESSIER, from which he deduced the difference of meridians of the Royal Observatories of Greenwich and Paris  $9' 35''$ . By two corresponding immersions and nine corresponding emersions, observed at both Royal Observatories, he found  $9' 21''$ . From the observations made between 1761 and 1764 he found  $9' 28''$ . By the observations made before 1700,  $9' 21''$ . And, from a comparison of mine and the Parisian observations, with the intermediate help of his own made at Stockholm,  $9' 26''$ : and from the whole he inferred the difference of meridians to be  $9' 25''$ .

Twelve years having elapsed since Mr. WARGENTIN's comparison, I was desirous to see what would result from the further observations made during that time, and applied to the Comte CASSINI, the respectable heir of the late M. CASSINI DE THURY and his successor at the Royal Observatory, and to the celebrated M. MESSIER, to favour me with such of their observations of the eclipses of the first satellite of Jupiter as had been made correspondent to mine. These they immediately sent me in  
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the most obliging manner, by which I am enabled to make further inferences concerning the difference of our meridians, as exhibited in the two following tables; in reference to which it is to be understood, that all the observations at Greenwich were made with a 46-inch achromatic telescope of 3,6 inches aperture, except a few otherwise noted as observed with a 6-foot Newtonian reflector, whose aperture is 9,4 inches; and once with a 2-foot Gregorian reflector, whose aperture is 4,5 inches; and once with an 18-inch Gregorian reflector, with an aperture of 4,4 inches, furnished with new metals of Mr. EDWARDS'S brilliant composition, described in the Appendix to the Nautical Almanac of the present year, which reflects as much of the incident light as an achromatic telescope transmits; and that in making out the columns, intitled difference of meridians corrected, I have subtracted 7'' from the immerfions and added as much to the emerfions observed with the 6-foot reflector, and added 13'' to the immerfions and subtracted as much from the emerfions observed with the 2-foot reflector, to reduce them to what they should have been probably observed at with the 46-inch achromatic telescope, and added 5'' to the time of the emerfion observed on Sept. 5, 1784 at the Royal Observatory at Paris, with a 5-foot reflector of DOLLOND, to reduce it to the 3½ feet achromatic telescope.



*and Longitude of the Royal Observatory at Greenwich.* 183

Difference of meridians of the Royal Observatories at Greenwich and Paris, by observations of eclipses of Jupiter's first satellite, observed at both places.

By Immersions.			Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
	Difference of meridians.	Diff. of meridians corrected.		
1779, Jan. 11	9 22	9 29	6 F.	Limbs undulating.
18	9 38	9 45	6 F.; air a little hazy.	Air very clear.
1780, Jan. 14	9 5	9 5	Air very clear.	
Mar. 18	9 16	9 16	Air very clear.	Air hazy.
25	9 8	9 8	- -	Air a little hazy.
1785, Oct. 1	9 11	9 11	{ In contact with Jupiter's body.	A little hazy.
Mean of 6 imm.	9 17	9 19		
By Emersions.			Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
	Difference of meridians.	Diff. of meridians corrected.		
1773, Nov. 1	9 10	9 3	6 F. Air a little hazy.	
1775, Feb. 15	10 44	10 37	6 F. Twilight.	Air very clear.
Mar. 17	9 43	9 36	6 F. Air a little hazy.	Air very clear.
1778, Mar. 13	10 46	10 46	—Bright moonshine.	
Apr. 12	9 46	9 46	Air hazy.	
1779, Mar. 30	10 43	10 43	- -	Air hazy.
Apr. 1	9 56	10 9	{ 2 F. reflector. Air very clear.	
1781, May 24	9 49	9 49	Air very clear.	
31	9 8	9 8	- -	Air very clear.
1782, Aug. 29	9 27	9 27	Air very clear.	
1783, Aug. 2	8 54	8 54	Air very clear.	{ The sat. very near Jupiter's disk.
Oct. 26	9 45	9 45	{ Air very clear, but Jupiter low.	{ Limbs undulating much.
1784, Sept. 5	9 4	9 9	Air very clear.	{ 5-foot reflector by DOLLOND, magni- fying 450 times.
1785, Nov. 9	9 26	9 26	{ 18 inch reflector, new metals.	
16	9 45	9 45	- -	Hazy.
Mean of 15 emer.	9 44,4	9 42		
Mean of 6 imm.	9 17	9 19		
Mean of both means	9 31	9 30½		

Before the 24th of May, 1781, a  $3\frac{1}{2}$ -feet achromatic telescope of DOLLOND, of 42 lines aperture, that was but an indifferent one, was made use of at the Royal Observatory at Paris. From that time a very good one was employed of the same size and aperture.

Difference of meridians of the Royal Observatory of Greenwich and the Hôtel de Clugny at Paris, 2'' of time East of the Royal Observatory, deduced from observations of eclipses of Jupiter's first satellite observed at both places.

By Immerfions.			Circumstances of the observations at Greenwich.	Circumstances of the observations at Hôtel de Clugny.
	Difference of meridians.	Diff. of meridians corrected.		
1775, July 15	9 55	9 55	- -	Air very clear.
Aug. 7	9 6	9 13	6 F.	Air very clear.
1776, Sept. 10	9 22	9 22	Jupiter a little hazy.	
1777, Sept. 6	9 33	9 33	- -	Air very clear.
Nov. 7	9 23	9 23	Air hazy.	Air very clear.
1779, Jan. 11	10 18	10 25	6 F.	Air very clear.
18	9 23	9 30	6 F. air a little hazy.	Air very clear.
Dec. 22	8 10	8 10	Air very clear.	A little hazy.
1780, Jan. 7	9 39	9 39	Air very clear.	Air very clear.
14	9 40	9 40	Air very clear.	Air very clear.
Mar. 18	9 2	9 2	Air very clear.	Air very clear.
25	9 24	9 24		
1781, Feb. 26	8 48	8 48	J's limb undulates.	
Mar. 5	8 59	8 59	- -	Air very clear.
Apr. 13	9 16	9 16	- -	Jupiter ill defined.
1783, July 8	9 58	9 58	- -	Air very clear.
1786, Sept. 4	9 19	9 19	Air very clear.	
Dec. 30	9 31	9 31	Air very clear.	Very hazy.
Mean of 18imm.	9 22,5	9 23,7		

By Emerfions.		Circumstances of the observations at Greenwich.		Circumstances of the observations at the Hôtel de Clugny.
	Difference of meridians.	Diff. of meridians corrected.		
1775, Feb. 15	9 31	9 24	6 F. Twilight.	Air very clear.
22	9 1	8 54	6 F.	Air very clear.
Mar. 17	9 13	9 6	6 F. Air a little hazy.	Hazy.
1776, Jan. 26	9 15	9 15	- -	
1777, Feb. 4	8 51	8 51	Air very clear.	Air very clear.
Mar. 24	9 13	9 13	Air very clear.	Air very clear.
31	9 22	9 22	Air very clear.	
1778, Feb. 25	8 48	8 48	- -	Air hazy.
Mar. 13	9 15	9 15	- -	A little hazy.
Apr. 5	9 17	9 17	Air hazy.	Air very clear.
12	10 11	10 11	- -	Air very clear.
May 21	9 29	9 22	6 F.	Air very clear.
1780, Apr. 19	9 28	9 28	Air very clear.	
May 28	9 37	9 37	- -	Air very clear.
1781, May 31	9 10	9 10	- -	Air a little hazy.
June 16	9 25	9 25	- -	Air a little hazy.
1783, Aug. 2	9 23	9 23	Air very clear.	Air very clear.
25	9 40	9 40	Air very clear.	
Oct. 3	9 30	9 30	Air very clear.	Air hazy.
1785, Nov. 9	9 14	9 14	{ 18-inch reflector, new metals.	
18	9 12	9 12	Air very clear.	
1786, Jan. 3	9 58	9 58	Air a little hazy.	Air hazy.
Mean of 22 emer.	9 22	9 20,7	M. MESSIER'S achrom. telescope is of $3\frac{1}{2}$ feet focus, 40 lines aperture, with magnifying powers of 70 and 140.	
Mean of 18 imm.	9 22,5	9 23,7		
Mean by both Royal Observ. west of Hôtel de Clugny	9 22	9 22		
Diff. of merid. of the Royal observatories	9 20	9 20		

Hence the difference of meridians of the two Royal Observatories, by the observations made in the Royal Observatories themselves, is  $9' 30''$ ; and by the observations made by

M. MESSIER, at the Hôtel de Clugny, and reduced to the Royal Observatory is  $9' 20''$ . The mean of both results is  $9' 25''$ . But if greater weight be given to the latter determination than to the former in the ratio of 2 to 1, on account of the series of M. MESSIER's observations being the most complete, the difference of meridians will be  $9' 23''$ .

M. DU SEJOUR, in the Memoires of the Royal Academy of Sciences for 1771, found  $9' 20''$ , as well from the beginning as end of the solar eclipse of 1769. M. MECHAIN, the learned editor of the *Connoissance des Temps*, informs me, that from the immersions of Celeno and Maia at the moon's limb, on March 5th last year, he has found by calculation from M. MESSIER's observations compared with mine  $9' 19''$ , 9 and  $9' 17''$ , 9, or by a mean  $9' 18''$ , 9; but, by his own observations compared in like manner, he makes it a little more than  $9' 20''$ . He regulated his clock by corresponding altitudes; but M. MESSIER corrected his by a transit instrument, which, however, has no meridian mark. For the present, I infer, we may take the difference of meridians  $9' 20''$ , as being within a very few seconds of the truth, till some more occultations of fixed stars by the moon, already observed, or hereafter to be observed, in favourable circumstances, and carefully calculated, shall enable us to establish it with the last exactness. To collect and calculate such observations I have not leisure at present; but the field of calculation is equally open to the celebrated astronomers of Paris, the observations made at this place being now published annually.

The extensive geometrical operations recommended by the late M. CASSINI DE THURY, and commenced under the direction of Major-general ROY, F. R. S. by his exact measure of a base on Hounslow-Heath, may also, when completed, determine

mine the difference of meridians of Greenwich and Paris to great exactness. But they do not seem to me likely to throw any new light on the difference of latitude of the two Observatories, because the uncertainty we are still under about the true figure and dimensions of the earth, and the irregular attractions arising from the irregular external figure and unequal density of the internal parts of the earth would prevent us from drawing any accurate conclusions, or such as we could confide in, from those geometrical measures, with respect to so large a quantity as  $2^{\circ} 38' 26''$  the difference of latitude; and, at all events, it must be less exact, as it is less direct, to determine the difference of latitude of two places from the measured distance of the two parallels compared with the length of a degree in the intermediate latitude, inferred from former measures of degrees, which were themselves determined with the help of astronomical observations, than to infer it from the immediate astronomical observations made at the two observatories, in the manner I have already deduced it.

NEVIL MASKELYNE,  
Astronomer Royal.

Greenwich,  
February 21, 1787.

