



## LXIII. A short account of the improvements gradually made in determining the astronomic refraction

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I gave him the same quantity as before (viz. ol. tereb. rect. et syr. croci āāჃj). This produced violent retchings, tenesmus, strangury, and great pain in the back; the urine was also a little tinged with blood. The strangury and tenesmus continued nearly a week, and the patient was not able to work for several days after. As he had not voided any portion of worm with the last dose, I concluded that he was quite well, but requested he would call on me again in about two months. He called last week, and I advised him to try his old remedy (a drachm of jalap), which had its usual effect, in bringing away a large quantity of the worm. I fear I shall not be able to induce him again to try the ol. tereb., from the severe symptoms which it produced when he last used it.

Aug. 27, 1810.

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LXIII. *A short Account of the Improvements gradually made in determining the Astronomic Refraction.* By T. S. EVANS.

THE principal object which the astronomer has in view, is to determine the real places of the heavenly bodies, from the apparent ones observed from a point situated on the earth's surface. In general, it is necessary to reduce them to what they would have been found, were the observer situated in the sun's centre: and it is very seldom that they do not require to be reduced to some other point. Various equations and corrections are of course necessary for this purpose, but none of greater importance than the *refraction*, which is caused by the atmosphere that surrounds the earth, and produces in every ray of light that traverses it, a greater or less deviation from its rectilinear course, according to the density of the air, and the altitude of the object above the horizon. Perhaps there is nothing that has opposed so great an obstacle to the improvement of astronomy as refraction, and nothing requires greater attention by every one who makes observations of any accuracy, since there are very strong reasons for presuming that it is different, in some degree, in almost every different situation. Most of the principal astronomers from Tycho Brahe down to the present time have done something which tended to improve the method of finding it: but further observations and experiments are still wanting, for there is, even now, an uncertainty of several seconds in it, at low altitudes. To bring under one point of view, and in the compass of a small sketch,

sketch, the various endeavours of these illustrious men, is the humble attempt of this short essay; which, it is hoped, will have the desired effect of stimulating others, who possess the means, to the consideration of the subject, that we may shortly be enabled to discover its quantity with the greatest accuracy, at all altitudes, and under all changes of the atmosphere.

There appears to be but little doubt that the astronomic refraction was known to the ancients, since it is expressly mentioned by Ptolomy, although not made use of in his calculations\*. He says, near the end of the eighth book of the *Almagest*, that in the rising and setting of the heavenly bodies there are changes which depend upon the atmosphere: and he mentioned it more at length in a work on optics which unfortunately has not been handed down to us†.

Alhazen, an Arabian writer, who is generally supposed to have lived about the year 1100, and to have taken the greater part of his optics from the works of Ptolomy, speaks also decidedly of it, and shows the manner of convincing ourselves of its existence by experiment‡.

“Take,” he says, “an armillary sphere which turns round its poles, and measure the distance of a star from the pole of the world when it passes near the zenith in the meridian, and when it is rising or setting near the horizon, and you will find the distance from the pole less in the latter case.” He then demonstrates that this must arise in consequence of the refraction, but he does not state its quantity.

In the collection of observations made by Bernard Walter, published by Willebrord Snell, in the year 1618, it is stated, the observations were so exact that they pointed out to Walter the quantity by which the altitudes of the stars and planets were increased on account of the refraction.

Tycho Brahe§, however, appears to have been the first who asserted, with any degree of accuracy, that the refraction elevates the heavenly bodies rather more than half a degree when in the horizon. But either his instruments or his observations were not sufficiently correct to determine it with certainty for all degrees from the zenith to the horizon: and accordingly where these failed the rest was supplied by conjecture. He believed that the sun’s refraction was 34’ in the horizon, and that it became insensible at 45° of altitude. For the stars, however, he assumed an en-

\* La Lande’s *Astronomy*, 2163, 3d edit. *Encyclop. Yverd. art. Refraction.* *Encycl. Meth. do.*

† La Lande’s *Astronomy*, as above. *Smith’s Optics*, p. 58. *Remarks.* *Priestley’s Hist. Opt.* 4to, p. 18.

‡ *Encyclop. Yverd. art. Refraction.*

§ *Progymn.* p. 15.

tirely different quantity, viz. 30' in the horizon: but this, according to him, terminated at only 20° of altitude\*.

The following is the manner in which it is related that Tycho made this discovery†. He had determined with one or two instruments, extremely well made, the latitude of the place, by observations of polaris above and below the pole. He determined it also by the sun's altitude in both solstices, and found it four minutes less by the latter. At first he doubted the goodness of his instrument, and therefore constructed with the utmost care as many as ten others of different sizes and forms, but they all gave nearly the same result. He could no longer attribute this difference between the two determinations of the latitude to any defect in the observations, and therefore endeavoured, by an attentive consideration of the subject, to find out the cause of this curious phenomenon. At length he supposed it could only arise from the refraction, which elevated the sun at the winter solstice, having then only 11° of altitude above the horizon. This conjecture agreed very well with the principles of optics; but still Tycho Brahe could scarcely persuade himself that the refraction was sufficiently large to produce so great a difference: on this account he made other instruments of ten feet diameter, whose axis corresponded exactly with the pole of the world; and with these he measured the declination of the stars out of the meridian‡. He then found, that even in summer, the refraction, although insensible at the meridian altitude of the sun, was very considerable near the horizon; and that the defect was about half a degree in the horizon.

A copy of Tycho Brahe's Table§ of Refraction for a star is given in the margin.

In this state did the refraction continue for many years. Even Riccioli|| in 1665 supposed it nothing at about 26' of altitude: but he thought the moon had only 29' of horizontal refraction in summer; the sun 30', and the stars 30' 37".

| Alt. | Refract. |
|------|----------|
| 0°   | 30' 0"   |
| 1    | 21.30    |
| 2    | 15.30    |
| 3    | 12.30    |
| 4    | 11.00    |
| 5    | 10.00    |

\* Mem. de l'Acad. av. s. renouv. tom. v. p. 82. Long's Astronomy, vol. i. p. 254, where a comparison is given of his Table with those of Newton and Flamsteed.

† Encycl. Method.

‡ The greater part of these very curious and ingenious instruments are given in his *Astronomice instauratæ Mechanica*, printed at Wandesburg in 1598. This work is now become extremely rare, and to be met with only in a few of the great public libraries: on which account M. Jeaurat had the plates engraved again upon a reduced scale, and published in the Memoirs of the Academy of Sciences of Paris for the year 1763, p. 120.

§ Progymn. p. 79. 104. Street's Astr. Carol. p. 119. Long's Astr. vol. i. p. 254. Maria Cunitia *Urama Propitia*. p. 286, fol. 1630.

|| Astr. Reformat. Astr. ref. Tabul. p. 47.

# On Refraction.

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Table continued.

It was not till after the year 1672, that a tolerably near table of refraction made its appearance, when the elder Cassini took the subject into consideration\*. What led to this was the voyage of Richer to Cayenne in that year, upon the utility of which some very excellent remarks were made by Cassini, showing how far observations made in a situation so near the equator tended to confirm or disprove certain theories derived from observations made in Europe†. Several very useful deductions were drawn from a comparison of those made both at Paris and Cayenne; among others the refraction was settled upon more accurate elements than heretofore‡, and a new Table computed, for the first time, of its quantity for all degrees, up to the zenith; an abridgement of which is given in the margin.

| Alt. | Refract. |
|------|----------|
| 0°   | 30' 0"   |
| 6    | 9.0      |
| 7    | 8.15     |
| 8    | 6.45     |
| 9    | 6.0      |
| 10   | 5.30     |
| 11   | 5.0      |
| 12   | 4.30     |
| 13   | 4.0      |
| 14   | 3.30     |
| 15   | 3.0      |
| 16   | 2.30     |
| 17   | 2.0      |
| 18   | 1.15     |
| 19   | 0.30     |
| 20   | 0.0      |

From the relation of his grandson, it appears, however, that Cassini had at one time computed three tables of refraction for all altitudes: one for winter, another for summer, and a third for spring and autumn: but several doubts having been suggested to him respecting this arrangement, although in appearance conformable to nature, and principally the observations of Richer at Cayenne, where the refraction was found little different from that at Paris, he changed his opinion; and, judging that since the great difference of heat of the torrid zone, from that of the temperate, which we inhabit, does not cause sensible differences in the refraction; therefore, the greatest heat or cold of our climate could not change it much; and he then fixed upon one table, which was that used by the astronomers of the Royal Observatory of Paris, up to the year 1745§.

| Alt. | Refract. |
|------|----------|
| 0    | 32.20    |
| 1    | 27.56    |
| 2    | 21.4     |
| 3    | 16.6     |
| 4    | 12.48    |
| 5    | 10.32    |
| 10   | 5.28     |
| 15   | 3.36     |
| 20   | 2.39     |
| 30   | 1.42     |
| 40   | 1.10     |
| 45   | 0.59     |
| 50   | 0.50     |
| 60   | 0.34     |
| 70   | 0.21     |
| 80   | 0.10     |
| 90   | 0.0      |

It was always thought, before the time of Cassini, that the refraction did not extend its influence higher

\* Mem. de l'Acad. avant son renouvel. tom. v. p. 81.

† In the observations of Picard made in various parts of France, in the year 1674, there are several for ascertaining the refraction; and a table is given from them for each degree of altitude up to 22°.—*Mem. de l'Acad. av. son renouvel.* tom. iv. p. 111.

‡ Mem. de l'Acad. avant son renouvel. tom. v. p. 105.

§ Ibid. 1745.

than  $45^\circ$  of altitude: and he is generally considered as the first who proved that it reached all the way to the zenith\*. He also supposed that near the equator the horizontal refraction was less than in our climate by about one-third; that this difference decreased as far up as  $60^\circ$ , after which it was the same nearly for both climates.

From this discovery it followed, as a natural consequence, that the refraction must be greater near the pole than at Paris: and this was shortly afterwards proved to the Academy by the publication of a work expressly on that subject†. The king of Sweden, being in 1694 at Tornea in West Bothnia, near the latitude of  $65^\circ 45'$ , and observing that the sun did not set there in the summer solstice, sent the following year some mathematicians to make more certain and exact observations of this curious phenomenon. They are contained in this book, and Messrs. Cassini and De la Hire‡ concluded from them, that in the latitude of  $65^\circ 45'$  the horizontal refraction must be  $58'$ , or nearly double of that at Paris.

According to an observation made by some Dutchmen§ who passed the winter of 1596–7 in Nova Zembla, in latitude  $76^\circ$  north, the sun, which had entirely disappeared the 14th of November, began to rise again the 24th of January, viz. six days sooner than was expected according to astronomical calculations||. If so, when the sun has been two or three months under the horizon, as the Dutchmen observed in 1597, the cold becomes dreadful, and perhaps the refraction increases prodigiously. M. le Monnier assures us, that he found by the observations printed in 1599, that the 24th and 27th of January 1597, there were more than  $4\frac{1}{2}$  degrees of refraction: that he could neither explain these observations, reject them as doubtful, nor suppose any error, as was done by most of the other astronomers, Kepler, Cassini, Scotto, and, lastly, M. le Gentil¶, who maintained that there were errors in the observations, and accordingly read a memoir on the subject. If it were not so difficult a task to winter in these high latitudes, we might expect such observations as would remove all doubt

\* Mem. de l'Acad. 1700, p. 112.

† “*Refractio solis in occidui*,” &c. Holmiæ. 4to, 1695. These observations in Lapland were made by Messrs. Spole and Bilberg.

‡ In two papers of remarks on these observations published by them in the Mem. de l'Acad. 1700, p. 37.

§ Smith's Optics, p. 61. Remarks. Dr. Jurin's Notes on Varenius's Geography, vol. i. p. 441.

|| Leipsic Acts, 1679.

¶ Voy. dans les Mers des Indes, tom. i. p. 595; tom. ii. p. 832.

on the subject; and, perhaps, bring others to light of as great or greater importance\*.

The refraction of the north being so considerable, is very useful to the inhabitants, who are deprived of the sun's light during many months; as it makes the sun rise much earlier, and set much later to them, than it otherwise would.

About the year 1725, Mr. Flamsteed, the English Astronomer Royal, published his table † computed from his own observations: and this was the one commonly used in England for many years afterwards.

Sir Isaac Newton also constructed one ‡ from theory, which was first published by Dr. Halley in the Philosophical Transactions, No. 368, for 1721. He made the horizontal refraction  $33' 45''$ ; whereas Mr. Flamsteed's was only  $33' 0''$ .

But although the refraction might be determined within a few seconds at all altitudes by observation; yet, the law of its increase from the zenith to the horizon was a subject that occupied the principal mathematicians and astronomers for more than a century §. Newton having discovered the general principles of attraction, found that the refraction was a consequence of this law of nature; and that it arose from the attraction of the atmosphere on the particles of light. On this principle the curve which a ray of light describes might be determined; since it is successively attracted by different layers of the atmosphere, increasing in density as they approach the earth, and, consequently, bending the ray more and more from the right line which it described in the vacuum previous to its reaching the atmosphere. There are many authors who have endeavoured to find from theory the curve described by this ray in its course, by the assumption of various hypotheses: but perfection and our attempts to arrive at it, as is well observed by the elder Cassini in discoursing on this subject, are like the progress of certain curves and their asymptotes. The principal of these writers on the subject are, Bernouilli<sup>1</sup>, Boscovich<sup>2</sup>, Bouguer<sup>3</sup>, Cassini<sup>4</sup>, Des Cartes<sup>5</sup>,

\* Encyclop. Meth. art. *Refraction*.

† Hist. Célest. vol. i. p. 396; also Hodgson's Math. vol. i. p. 367. Long's Astronomy, p. 254.

‡ Long's Astr. p. 254.

§ In 1714, Cassini published in Mem. de l'Acad. for that year, some methods of finding the refraction by observation, and of determining its quantity by theory. He has also given a table of it for the first  $30^\circ$  of altitude, computed, first, according to a rectilinear, and, secondly, according to a circular hypothesis which he there assumes.

<sup>1</sup> Hydrodyn. 1738, p. 221.

<sup>2</sup> Oper. tom. ii.

<sup>3</sup> Prix de 1729. Mémoires, 1739, p. 407; 1749, p. 75.

<sup>4</sup> Epist. ad Montanari, 1665. Refrassioni è Parallosse, &c. 1671. Mem. for 1714, and his Astr. vol. i. p. 11. Paris, 1740, in 2 vols. 4to.

<sup>5</sup> Dioptrique, 4to. Paris, 1637.

De la Grange<sup>6</sup>, Euler<sup>7</sup>, Gregory<sup>8</sup>, Hodgson<sup>9</sup>, Huygens<sup>10</sup>, Kramp<sup>11</sup>, Lambert<sup>12</sup>, Laplace<sup>13</sup>, Mayer<sup>14</sup>, Newton<sup>15</sup>, Oriani<sup>16</sup>, Thomas Simpson<sup>17</sup>, Brook Taylor<sup>18</sup>, Heinsius<sup>19</sup>, Tobias Mayer<sup>20</sup>, La Hire<sup>21</sup>, d'Alembert<sup>22</sup>.

It was conjectured by many of the early writers, that the refraction was subject to variations depending upon the weather: but it then amounted to little more than a conjecture, on account of the indifferent manner in which astronomic instruments were divided. Picard found by meridian altitudes of the sun in 1669, that it was greater in winter than in summer. He found also that it was less by day than by night. In the observations given at the end of his journey to Uraniburg\*, to settle the latitude of that place, and its difference of longitude from Paris, for the purpose of comparing the observations of Tycho Brahe with those made at the Royal Observatory of Paris, he found the horizontal refraction for the first limb of the sun that made its appearance above the horizon there  $33' 2''$ , and for the second  $32' 37''$ . So that in the small interval of time that the sun took to rise, the refraction was diminished 25 seconds by the warmth arising from the sun's presence.

A quadrant being also directed by him from the top of Mount Valerian towards the summit of the church of Notre Dame at Paris, he found the depression  $20'$ ; but the sun had scarcely risen, when it was increased to  $22'$ ; exhalations being raised by the sun's presence, and the medium between Paris and Mount Valerian become more equal; whereas, before the sun rose, the air of Paris was more dense than that of Mount Valerian†.

The density of the atmosphere being the immediate cause of the refraction, it was very natural to suppose that it must decrease as this density became less; whether by causes which diminished its weight, or by the expansion produced by heat: and, indeed, astronomers were not long after this,

<sup>6</sup> Nouveaux Mémoires de Berlin, vol. iii.      <sup>7</sup> Mem. de Berlin, 1754, tom. x.

<sup>8</sup> Astronomy, vol. i. p. 358. edit. of 1715, in 8vo.

<sup>9</sup> Mathematics, vol. i. p. 367. Fluxions. p. 133.

<sup>10</sup> Traité de la Lumière, p. 44. Dioptrica, 4to, 1703.

<sup>11</sup> Analyse des Refract. Astr. et Terres. 4to. Strasburg, 1799.

<sup>12</sup> Les Propriétés Remarquables de la Route de la Lumière. A la Haye, 1759. Another edition in German, 1773.

<sup>13</sup> Mécanique Céleste, vol. iv. p. 231.

<sup>14</sup> Tables, 1770.

<sup>15</sup> Principia, b. i. sect. 14.

<sup>16</sup> Ephem. de Milan, 1788.

<sup>17</sup> Mathematical Dissertations, 1743.

<sup>18</sup> Methodus Incrementorum, 4to. Lond. 1715. , Propos. 27, p. 108.

<sup>19</sup> Dissertatio de Computo refractionum Astron. 4to. Leipsig, 1749.

<sup>20</sup> De Refractionibus Astronomicis, 4to. Altorf. 1781.

<sup>21</sup> Mem. de l'Acad. pour 1702, p. 52.

<sup>22</sup> Opusculs Mathématiques, tom. viii. p. 297.

\* Mem. de l'Acad. av. s. ren. tom. i.      † Encycl. Meth. art. *Refraction*.  
before



before they discovered that very sensible differences were occasioned by these circumstances.

But all the honour of introducing corrections on account of the variation of density in the atmosphere, as indicated by the barometer and thermometer, is due to Messrs. Lowthorpe and Hauksbee; the former of whom, in 1698, proved by a very simple experiment, in the presence of the Royal Society, that the refractive power of air is directly proportional to its density\*: and the latter, by repeating and extending the same course of experiments in the year 1708, with the machinery pointed out by the former, found that the variations of refraction, depending on the barometer, are proportional to the alteration of height of the mercury in the tube: and by a series of these experiments, he furnished us with a table of the corrections which it is necessary to make on account of the changes of heat indicated by the thermometer. These experiments, although not quite conclusive on the subject, were yet made with as much accuracy and care as the nature of the machinery, and the state of experimental philosophy of that time, would admit. An example is also given, towards the end of his paper, on the mode of applying them to correct the refraction. By these, Hauksbee found that a volume of air expressed by unity, when the thermometer was at  $130^{\circ}$  above zero, became, at  $50^{\circ}$  below, one-eighth more dense: or, which is the same thing, that the air lost one-eighth of its density, for an elevation of 180 degrees of Fahrenheit's thermometer; which is exactly the difference of heat between melting ice and boiling water. But although this one-eighth, as will be shown hereafter, was too small; yet it laid the foundation for other experiments, since made by several philosophers, by which the quantity of expansion has been determined more accurately.

We have already shown that the refraction near the pole is greater than in our climate†; the degree of cold being more intense. It was also found to be less in the torrid zone, where the heat is greater than in Europe. Bouguer made a variety of observations at Peru‡, the result of which he has given us. In 1740, he came down into an island situated in the river of Emeralds, called Isle of Inca, where he determined the refraction from  $1^{\circ}$  to  $7^{\circ}$  of altitude: and the table which he computed therefrom, shows the refrac-

\* Hauksbee's Exper. 4to, 1709, p. 175.

† It was, however, found by Capt. Phipps, in his voyage to the North, in 1773, that the refraction in latitude  $80^{\circ}$  was the same as in England. But this was in summer.

‡ Vide Mem. Ac. 1739, and his Fig. de la Terre.

tion to be about one-seventh less than in Europe\*. The horizontal refraction he found to be  $27'$ : but at  $6'$  of altitude it is  $7' 4''$ ; and at  $45^\circ$  it is  $44''$ . Bouguer then gives a table† for Quito, which is more elevated above the level of the sea. M. le Gentil‡ found it greater at Pondicherry in India, although in the torrid zone.

The refraction diminishes when we are elevated above the level of the sea. Bouguer observed § the quantity of it at Chimborazo, 2388 toises above the level of the sea, and found it in the horizon only  $19\frac{1}{2}'$ . At the cross of Pit-chinca, 2044 toises above the sea, he found it  $20' 48''$ ; at Quito, 1479 toises above the sea,  $22' 50''$ : but at the level of the sea  $27'$ . These observations, when joined with the theory, produced the following rule; That if we take the excess of 5158 toises above the elevation of the place, with regard to the level of the sea, the refraction will be as the square root of this excess. Thus the square root of 5158 toises is  $27'$ , for the horizontal refraction at the level of the sea, in the torrid zone: and the square root of the excess of 5158 above the elevation of the place will be its horizontal refraction. The quantity 5158 is the height above which the refractive matter no longer produces any sensible effect, at least in the torrid zone||.

But although by this time considerable attention had been paid to the subject, yet great differences were to be found in the tables then most in use. Thus at the altitude of  $30^\circ$ , according to Flamsteed, the refraction was  $1' 23''$ ; Newton  $1' 30''$ ; Cassini  $1' 42''$ ; and de la Hire  $1' 55''$ ; leaving an uncertainty of more than half a minute: and it must have been very mortifying to an observer, after having taken the utmost pains to avoid errors of two or three seconds, to find his reduced observations liable to so great an error, according to the choice of his table of refraction.

It is indeed rather extraordinary, that in a memoir published by Cassini de Thury, among those of the Academy for 1745, he attempted to reconcile a number of observations with each other, by considering the state of the thermometer only, without at all noticing that of the barometer; although at that time Hauksbee's experiments had been published about 37 years.

He concludes his paper, as is very natural to suppose, without being able to make the observations agree: nor does it clearly appear that the French noticed the above-

\* This Table is in the memoir above cited.

† Mem. 1749. Conn. des Mouv. Célest. p. 1765. ‡ Mem. 1774. Voyage, tom. i. § Mem. p. 1749. || Encycl. Method. art. Refr.

mentioned

mentioned experiments made by Hauksbee till about the year 1749 \*. It is also worthy of remark, that although the necessity of introducing corrections on account of the alterations of the barometer and thermometer were likewise shown to be absolutely necessary by Dr. Halley †, and the circumstance mentioned, and in some degree admitted by Le Monnier ‡, yet it does not appear that he followed the advice of his illustrious contemporary, but merely endeavoured, as Cassini did, to reconcile his observations with the state of the thermometer at the time of making these observations, without taking the barometer into account §.

[To be continued.]

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LXIV. *Some Particulars respecting the Thunder-storm at London, and in its Vicinity, on the 31st of August 1810.*  
By Sir H. C. ENGLEFIELD, Bart. F.R.S. and F.S.A.

To Mr. Tilloch.

SIR, As the stroke of thunder, which was felt in London at about half after two o'clock in the morning of the 31st of August last, was, perhaps, the most violent and awful ever experienced in this country, you may not think the following account of it from an eye-witness, and who was very near the spot where it fell and did mischief, unworthy of insertion in your Journal.

I was with three friends in a coach standing at a house where we had supped. The house-door was still open, and there was a strong light from a large lustre in the hall, full on the coach, and two very bright lamps at the door of the house. This circumstance was in favour of our seeing the nature of the light distinctly; for, had we been in the dark, its excessive brightness would have so dazzled our eyes as to prevent all distinct vision. As we got into the coach there was a small mizzling rain, and a very strong flash of distant lightning in the N.E., but no thunder that we could hear. The servants at the door said there had been much distant lightning for an hour or two.

The sky over head appeared very dark, but the lights prevented accurate observation of it. We were just seated in

\* Mem. de l'Acad. 1749, p. 106.—Probably this was on account of some reflections made by him on the French philosophers who repeated his experiments before the Royal Academy of Paris, and failed in their results.—Vide his book, p. 196.

† Philosophical Transactions 1720, No. 364.

‡ Hist. Céleste, 4to. Paris, 1741.

§ See the whole of his *Discours prelim.* prefixed to the work before cited.