INSTITUT DE FRANCE
Académie des sciences

Comptes Rendus
Physique

Andrew T. Young

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Volume 23, Special Issue S1 (2022), p. 179-212

Published online: 21 February 2023
Issue date: 27 October 2023

https://doi.org/10.5802/crphys.125

Part of Special Issue: Astronomie, atmosphères et réfraction
Guest editors: Pierre Léna (Professeur émérite, Observatoire de Paris et Université Paris Cité, membre de l'Académie des sciences) and Luc Dettwiller (Université Jean Monnet Saint-Etienne, CNRS, Institut d Optique Graduate School, Laboratoire Hubert Curien UMR 5516, F-42023, SAINT-ETIENNE, France)

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Les Comptes Rendus. Physique sont membres du Centre Mersenne pour l'édition scientifique ouverte
www.centre-mersenne.org
e-ISSN : 1878-1535

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Astronomie, atmosphères et réfraction / *Astronomy, Atmospheres and Refraction*

Relations among atmospheric structure, refraction, and extinction

Relations entre la structure de l'atmosphère, la réfraction et l'extinction

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Abstract. The refraction and extinction in the Earth's atmosphere depend on the atmosphere's structure, so it was natural to try to infer that structure from optical observations. Efforts to extract structure from observed refractions led to proof that this is possible only below the astronomical horizon. Direct studies of the real atmosphere show complicated, variable structure. The complex history of relations between structure and refraction is outlined by citing some important works.

Résumé. La réfraction et l'extinction dans l'atmosphère terrestre dépendent de sa structure, donc il était naturel d'essayer de déduire celle-ci à partir d'observations optiques. Partant des réfractions observées, les efforts pour en tirer cette structure ont conduit à prouver que cela n'est possible que sous l'horizon astronomique. Des études directes de l'atmosphère réelle révèlent une structure variable et compliquée. On expose l'histoire complexe des études concernant les relations entre structure atmosphérique et réfraction en citant des travaux majeurs.

Keywords. Refraction, Extinction, Atmospheric Structure, Polytropes, Lapse Rate, Inverse Problems, Standard Atmosphere.

Mots-clés. Réfraction, extinction, structure de l'atmosphère, cas polytropiques, gradient thermique, problèmes inverses, atmosphère standard.

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1. Introduction

1.1. *Generalities*

Atmospheric refraction displaces the images of objects seen through the Earth's atmosphere from the objects' actual positions; so optical position measurements must be corrected for this effect. As the displacements depend on the structure of the intervening air, it is useful to understand this

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relationship. If atmospheric structure is well known, it can be used to correct for refraction; on the other hand, if refraction can be measured well, it can be used to infer atmospheric structure.

However, this is more difficult than it seems at first glance. This article outlines the long history of attempts to define and solve this problem, and explains its current state. The subject is too complicated to go into details, but they can be found in the references cited. Readers unfamiliar with astronomical terminology should consult Smart's classic introductory text [1], which devotes a chapter to astronomical refraction (i.e.,the refraction of astronomical objects.)

These same problems arise in geodesy and civil engineering, where the effects are described as "terrestrial refraction". The terminology is different, but the physics is the same. The relation between astronomical and terrestrial refraction is treated in Section 14 of this paper.

Another article [2] in this special issue describes other varieties of refraction phenomena.

1.2. *Beginnings*

The refraction and extinction of light in the Earth's atmosphere have been noticed for millennia, as the distortion of the setting Sun and its relatively dim image at the horizon are naked-eye phenomena. It was known in antiquity that celestial objects at the horizon appear higher in the sky than they should, and this was attributed to refraction by the atmosphere, just as a coin hidden within an opaque vessel becomes visible when the vessel is filled with water.

The history of the subject until the 1850s is reviewed by Bruhns [3], with emphasis on works of the Classical era. The table-top demonstration described above appears in the *Catoptrics* [4] attributed to "Euclid" — whether or not this was the author of the *Elements* is debated — and is repeated in very similar form in both Ptolemy's *Optics* [5] and in the work on the circular motion of celestial bodies by Cleomedes [6]. Ptolemy and Cleomedes are Bruhns's first two references.

Unfortunately, Smith (who translated and commented on Ptolemy's *Optics* [4, 5]) did not understand astronomical coordinates, and called ecliptic latitude, geographic latitude, altitude, and declination all "latitude," which garbles the "explanations" in his notes to Ptolemy's work. It also appears that the instrument used by Ptolemy was an armillary, and not (as Smith says) an astrolabe. However, it's clear from Smith's translations that Ptolemy thought celestial objects were displaced upward by "refraction … at the surface that separates the air and the ether". Ptolemy said the refraction might be computed "if the distance of the interface between the two media were known." Perhaps this is the beginning of an attempt to connect refraction with atmospheric structure.

Both Ptolemy and Cleomedes were much taken by the analogy between refraction at this imagined surface and that seen at the surface of water in the vessel holding the disappearing coin. Cleomedes [6] remarked that "many such things appear in the air, and especially around the Sea. For it could happen that the ray leaving our eyes is bent on entering wet and humid air and reaches the Sun now hidden below the horizon." And he then described the tabletop experiment with "a gold ring … put into a cup or some similar vessel," thinking that "it could be that something similar happens in humid and watery air." This may be the origin of the long-accepted story that refraction was caused by "vapors at the horizon."

Another common error, already asserted by Ptolemy, was to suppose that refraction caused the Moon Illusion. This mistake is still popular today. Actually, refraction makes the apparent sizes of objects near the horizon smaller, not larger; the Illusion is entirely a perceptual phenomenon that has nothing to do with optics.

1.3. *Refraction measurements*

After a handful of qualitative observations confirming that astronomical refraction exists, the first measurement of its size at the horizon was made by Walther [7]. He first observed it at the sunset of March 6, 1489, with later measurements on 12 Dec. 1490, 9 Sept., 1491, and 12 Dec. 1503. Much of this has been translated into English [8], along with Kepler's comments.

Systematic measurements of refraction were made by Tycho Brahe, whose empirical refraction tables were published by Kepler [9] after Tycho's death. Tycho had three refraction tables: one each for the Sun, the Moon, and the stars, because he suspected that the refraction might depend on the distance or the brightness of the refracted object. His tables for the Sun and Moon were systematically in error because of a wrong correction for parallax, but his table for the stars agrees within 2 or 3 minutes of arc with modern values. Tycho could not measure refraction at altitudes above 20°, where the actual refraction is about 2.6 arc minutes.

Tycho had to correct his solar data for the horizontal parallax (see [1, §117]) of the Sun. As he could not measure the solar parallax independently, he adopted the traditional value of 3 minutes of arc, which had been found by the well-known method of Aristarchus of Samos. This value is about 20 times larger than the true one. Because both the refraction and the parallax increase from zenith to horizon, and the parallax correction is nearly constant in the region where Tycho could measure refraction, he could not detect this error, and his refraction values for the Sun are all too large.

Tycho's stellar refractions were measured by comparing altitudes of circumpolar stars at upper and lower culminations (cf. [1, pp. 68–69]). But to extract refractions from such differences, one needs to know the actual declinations of the stars; this requires the observer's latitude. And to find the latitude, the apparent altitude of the Pole has to be corrected for refraction. (Tycho avoided this problem by assuming that the refraction was negligible above 45° altitude, so he regarded the altitude of the Pole as unaffected by refraction.) Further progress in determining refractions observationally required additional information about both the refraction law and atmospheric structure.

1.4. *The sine law*

The first step toward a theory of atmospheric refraction was the discovery of the sine law of refraction, which is well explained in [1, §34]. Unfortunately, its origin is still debated [10].

1.4.1. *Ibn Sahl*

The constant ratio of sines for two media first appears in an Arabic manuscript in the Tenth Century [11]. But it comes from thin air: neither empirical nor theoretical reasons are offered to justify it. Furthermore, the manuscript consists entirely of geometrical arguments relating ray paths to conic surfaces of revolution. The focusing properties of parabolic and ellipsoidal mirrors are worked out; then the sine ratio is introduced *ex nihilo* and used to show that hyperboloidal refracting surfaces could also focus light. Though arrangements for drawing conic sections on flat paper are proposed, no way to produce three-dimensional conic surfaces is mentioned.

As the manuscript is devoted to geometrical properties of conics, it appears that its author began by studying the focusing properties of elliptical and parabolic mirrors, and then wondered whether something similar could be done with hyperbolae. By following arguments similar to those he had used for reflective surfaces, he discovered that refractive optics could produce similar results *if* this peculiar ratio could be kept constant. As his motivation in composing the manuscript was to produce "burning glasses," he rearranged the presentation for refractive optics to begin with the conclusion (our sine law), and derive the focusing property at the end. This would account for the complete lack of justification for the constant ratio, and the lack of any discussion of optical materials or methods of making optical surfaces [10]. (A similar process seems to have been followed by James Gregory [12] in his belated rediscovery of the law in 1663.)

1.4.2. *Thomas Harriot*

In the 1950s, it was noticed that Thomas Harriot had actually found and used the sine law around the end of the 16th Century [13,14]. His discovery was entirely empirical, and just required more accurate measurements than earlier workers had made. These observations were made in 1597 and 1598, and used by Harriot in the next few years. He certainly had the sine law by 1601 [15]. It seems he refrained from publishing the discovery, for fear of attracting the attention of malevolent authorities. He died in 1621, without publishing the law.

1.4.3. *Willebrord Snel*

Snel's work is better documented. His outline for a manuscript on refraction, long thought to be lost, was discovered in 1935 [16]. What was found is clearly only an outline for a book on refraction; there are big gaps that were not filled in. Snel planned to have a large section on atmospheric refraction, and to discuss the height of the atmosphere. Vollgraff [17] quotes some additional bits from the outline (which he mistakenly calls an "index"): Snel described a distorted sunset, invoking "vapores crassos" as the cause of the flattening. Enough second-hand accounts of Snel's work exist to allow a reasonable reconstruction [18] of how he found the sine law empirically. However, he died about five years after learning it, with his manuscript unpublished.

1.4.4. *Descartes*

Finally, in 1637, Descartes published the sine law — though without using sines. (All these early workers described refraction in terms of ratios of lengths; the sine function was explicitly introduced later.) Many translations are available, e.g., [19]. The law was quickly checked by other workers, and accepted as correct. Its first application to astronomical refraction was Cassini's [20].

2. The homogeneous model

According to Ptolemy [5], astronomical refraction occurs at "the surface" that separates the air from the "ether." This supposed surface was assumed to be real until the 18th Century, though it is hard for a modern reader to imagine it. As late as 1719, de Mairan [21] was still quoting from a physics textbook that the air "extends above the Earth up to a height of about two or three leagues, where neither winds nor clouds occur; its surface must be very smooth, like that of all fluids that are not agitated".

Below this surface, the air was supposed to be uniform, and incapable of producing any refraction — except, perhaps, for those obscure "vapors near the horizon." This, then, was the picture to which Cassini applied the sine law.

2.1. *Cassini's refraction table*

In the late 1640s, Giovanni Domenico Cassini began working with Count Malvasia, a rich amateur astronomer with a private observatory who was interested in astrology. Cassini's job was to help the Count produce improved ephemerides. He soon found that better observations were needed, and was able to get an enormous "heliometer" installed in the huge Basilica of San Petronio, the largest building in Bologna (and still the sixth largest church in Europe). This instrument consisted of a small hole in the roof of the vault, 27 meters above the floor, which has a meridian line 66.8 meters long inlaid in the paving of the left nave. Together they formed a gigantic pinhole camera that allowed Cassini to measure both the altitude and the diameter of the Sun throughout the year. Near the summer solstice, the scale of the solar image on the floor is about 7 arcsec/mm, and the smaller (E-W) axis of the solar image is about 27 cm.

In his first report on these solar altitudes [22], Cassini showed how the refraction, the solar parallax, the obliquity of the ecliptic, and the latitude of the observer are all involved in understanding the apparent position of the Sun. With a precision on the order of 10 seconds of arc, his observations were about 15 times better than Tycho's naked-eye measurements, and the error in Tycho's refraction tables, caused by his use of a solar parallax nearly 20 times too large, became obvious. Cassini realized that better refraction corrections were required.

Although the sine law was well known by the 1650s, Cassini took care to check it experimentally himself before using it to calculate refraction. He then applied it to the refraction at the assumed upper surface of the homogeneous atmosphere. The process is very simple, as it involves only geometry and the definitions of the trigonometric functions; see [23] or [24, pp. 93–94] for an elementary treatment in modern notation. Cassini's table was printed in Malvasia's new Ephemerides [20, p. 173].

The model has two parameters: the refractive index of air, and the height of the uniform atmosphere. Cassini did not express his calculations in mathematical form, but instead described the process verbally, using "the ratio of the sines" instead of the modern term "refractive index." Also, he had no way to fit the two parameters simultaneously, and so resorted to a trial and error method, which was partly explained half a century later by his son [25].

He first tried to find a height of the "refractive medium" that would make the observed and computed refractions agree at some low altitudes; and "as he found that supposing this height to be 2000 toises, the two refractions agreed," he adopted this height for the calculation of the table. (The 2000 toises was obviously an arbitrary round number that Cassini considered good enough for computing the table.) As this height ("which is only a league") was considerably less than the minimum height of 6 leagues that anyone had proposed ("there are others who go up to 18 or 20"), "The atmosphere would only be refractive in a small part of its extent, and in its lowest layers, or, if one wants, the refractive material would be different from the atmosphere." But by 1714 [25] it was becoming clear that the homogeneous model was the cause of this discrepancy, and that a density varying with height would produce better refraction tables.

2.2. *Objections to the table*

A few years after the publication of Cassini's first table, Riccioli published a book [26] in which he raised objections to it. Part of his disagreement was due to his misunderstanding of how refraction is mixed up with the solar parallax; but part was due to the peculiar run of Cassini's computed refractions at the horizon, which was quite different from Tycho's empirical table. In Tycho's table, the *differences* of successive pairs of values steadily increase with increasing zenith distance; but in Cassini's table, the difference in refraction between 88 and 89 degrees is considerably larger than that between 89 and 90 degrees. Riccioli suggested this might be a typographical error in Cassini's table.

Cassini sent a response to Riccioli's criticism in a letter to Geminiano Montanari, who promptly published it [27]. This letter was so important that it was reprinted (with two others) in 1692 [28], and English abstracts of all three were printed in *Phil. Trans.* in 1672 [29]. Those English abstracts explain the process that Cassini had used to correct the observed height of the Pole and those of the Sun at the solstices for refraction and parallax, and the laboratory experiments on refraction that convinced him of the truth of the sine law. The resulting refraction tables "make it evident" that solar refractions are appreciable even in the summer, and continue all the way to the zenith, "which hitherto hath alwaies been denyed." In responding to Riccioli's remark about the intervals between the computed refractions nearest the horizon, Cassini [27, 28] wrote (in Latin, of course) the following:

"I have calculated the refractions again trigonometrically from the principles in the published letters for the same degrees of altitude, and likewise again came out those which are arranged in the table; hence there is no error, either typographical or trigonometrical, but it really follows from the laws of astronomical refractions published by me. The real reason why the difference of refractions from the first to the second degree is bigger than from the horizon to the first, is that at the first, up to an altitude of a degree, the inclination of the visual ray to the upper surface of the air is changed only 15 minutes, and from the first to the second degree of altitude is changed about five and thirty minutes, as is deduced geometrically by me by means of the ratio of the height of the air to the Earth's semidiameter, but so much variation of the inclination to the surface of the air in the second degree of altitude having been made produces a bigger ratio of the differences of the angles to the differences of the sines at a larger zenith distance, which produces the bigger difference of the refractions at the horizon, of course, as you will understand more clearly in my theory of refractions, which I am working on."

Actually, Kepler had encountered exactly this same problem 62 years earlier [8]; but neither he nor Cassini realized that this problem is inherent in the homogeneous atmosphere. Because the rays are straight within that atmosphere, they meet its refracting surface at an angle that is independent of the path length in air. Consequently, the model's refraction is the same a few minutes of arc above the astronomical horizon as at the same distance below it, and is completely symmetrical about the astronomical horizon. So the computed refraction has an extremum at the horizon, and is nearly constant near altitude zero degrees.

2.3. *Why was Cassini's table useful?*

Although Cassini's computed refractions were later disparaged as unreliable, the values in his table are in fact within a second of arc or two of the refractions tabulated in Allen's *Astrophysical Quantities* from the zenith to 15° altitude, and are still within 1 or 2 per cent of modern values out to 82° zenith distance. These errors are an order of magnitude smaller than the observational errors in the data Cassini was using, so his refraction table was perfectly adequate for his purposes, except for errors of about 3 minutes of arc at the horizon. The surprising accuracy of Cassini's table was pointed out by Delambre [30]; and Ivory [31] noticed that "if we use accurate elementary quantities in the computation, it will determine the refractions to the extent of 74° from the zenith with the same degree of exactness as any of the other methods, without even excepting the formula of Laplace."

How could the crude homogeneous model have given such good results — especially considering that the height Cassini used was too small by a factor of two?

Figure 1 shows the geometry. Well above the horizon, the astronomical refraction depends weakly on the height h of the refracting surface, and is nearly a function of just the refractivity of air. At the altitude where $\sin z_o = (1 - h/R)$, the refraction becomes strongly dependent on h. Cassini chose $h/R = 6.095 \times 10^{-4}$, which puts the boundary between these two regions at an altitude of 2°. (If he had chosen the right height, which is twice as big, this altitude would be $\sqrt{2}$ times larger, close to the crossover altitude of 2.6° found in [24, p. 108].)

Because the refraction within about 87° of the zenith is very insensitive to the adopted vertical structure [24], and Cassini fitted his model to a refraction of $5'\,28''$ at 80°, all his refractions from the zenith to ten degrees altitude were scaled to a well-determined observational value at a larger zenith distance than any of his stellar and planetary observations, or any of the more accurate meridian altitudes of the Sun at San Petronio. His adopted refraction at the horizon and atmospheric height had very little influence on computed values over most of the sky.

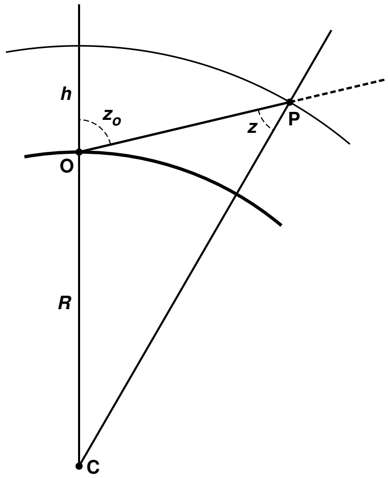


Figure 1. Geometry of a ray (the straight line **OP**) in the homogeneous model. The heavy arc through **O** represents the surface of the Earth, with center at **C** and radius R. The apparent zenith distance of the ray is z_o at the observer, **O**, and a slightly smaller angle z at **P**, a height h above the surface. The law of sines in triangle **OPC** makes $\sin z = (R/(R+h))\sin z_o$; so if $h \ll R$, $z \approx z_o$. The refraction does not depend much on h as long as $R/(R+h)$ is closer to unity than is $\sin z_o$. But if $\sin z_o$ is nearly unity, this approximation fails, and the refraction depends on h.

The small flattening of the Sun at the horizon shows that the ray curvature is usually small compared to the Earth's curvature (see [24, pp. 86–88]). So it is instructive to ignore ray curvature and just consider the geometry of straight rays. This approximation is still better when the upper atmosphere is considered, as the refractive index n is exactly unity at the top of the atmosphere, and is not quite 1.0003 even at sea level; neglecting curvature is equivalent to assuming $n \equiv 1$ everywhere.

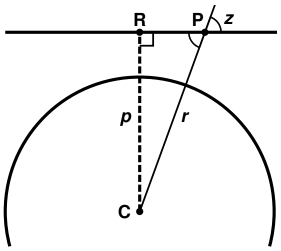


Figure 2. Geometry of a straight ray (the straight line **RP**). The dashed line **CR** is the perpendicular from the center of the Earth **C** to the ray. At any point **P** on the ray, the local zenith distance of the ray is the angle z.

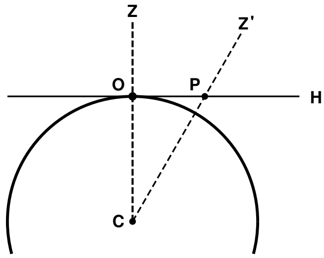
Figure 2 shows the geometry for a ray that passes through the upper atmosphere. The length p of the perpendicular **CR** from **C** to the ray is $r \sin z$, where r is the distance of a point **P** on the ray from **C**. Wherever on the ray **P** is located, the local zenith distance z varies so that the product $p = r \sin z$ remains fixed at the perigee distance of the ray.

Notice that the invariance of p makes the local zenith distance of the ray at **P** decrease as the point's distance r from the center of curvature increases. That means that z is always smaller in the upper atmosphere than the zenith distance of the ray at a ground-based observer. Furthermore, the change in zenith distance is larger for rays near the observer's horizon than for those near the zenith (see Figure 3).

2.4. *The refractive invariant*

When the small deviation of the refractive index from unity is taken into account, the invariant quantity is not the simple geometric invariant $r \sin z$, but becomes $nr \sin z$. This was noticed by Bouguer [32] and by Euler [33]. (For a derivation in modern notation, see [24].) A remarkable consequence of this invariant is that the differential equation for the refraction is easily obtained

Figure 3. Geometry of a straight ray at the observer's horizon. The observer, at **O**, sees the horizon at **H**, and the zenith at **Z**. At **P**, the curve of the Earth shifts the local zenith to **Z′**, so the local zenith distance of the ray is less than 90°. Because an observer at **P** would see the horizon at **O**, the zenith distance of the ray at **P** is just the complement of the dip of the horizon there.



by taking the logarithmic derivative of the invariant and setting it equal to zero. So we can write the refraction directly as the integral

$$\int_1^{n_o} \tan z \frac{\mathrm{d}n}{n} \tag{1}$$

where n_o is the value of the refractive index at the observer, and 1 is its value above the atmosphere.

Figure 4. Integrand of the refraction integral near the horizon. The top of the atmosphere is at the left edge; the ground is at the right. The abscissa is a linear function of density, starting at $n = 1$. (Z.D. = zenith distance.)

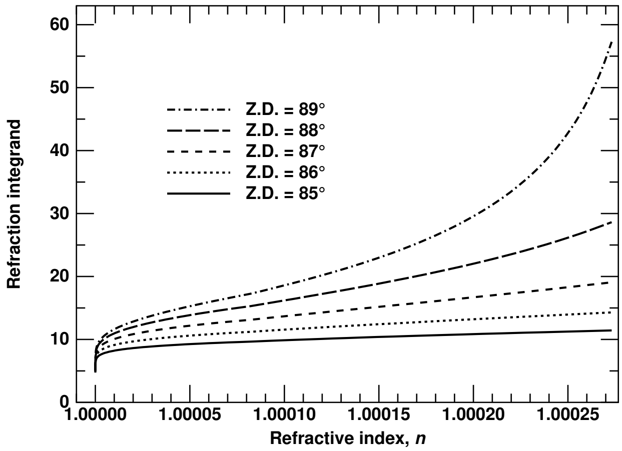


Figure 4 shows the integrand of this integral as a function of the refractive index n for a few zenith distances near the horizon, using the Standard Atmosphere model. At small and moderate zenith distances (see [24] for examples), the integrand is nearly constant, which is why Cassini's simple calculation gave good values for $z_o < 80°$, in spite of his poor choice for the height of the atmosphere.

However, Figure 4 shows that the weighting with height changes drastically in the last few degrees above the horizon. At 85°, the integrand remains nearly linear, but is hardly constant, being about 40% larger at the surface than in the upper atmosphere. With increasing zenith distance, the integrand bends up steeply near the surface, and the average value corresponds to a height of just a few km; this explains why Cassini had to use a small height to fit measured refractions near the horizon.

3. The demise of the homogeneous model

Subsequent workers tried to extend Cassini's method by allowing refraction to occur throughout the atmosphere instead of at a single height. However, without knowing either the density distribution of the air or a way of calculating the ray curvature, they could only assume some shape for the ray and use that to determine its local zenith distance and hence the differential of refraction. For example, in 1714 [34], Cassini's son Jacques assumed the rays are arcs of a circle (corresponding approximately to a linear decrease of density with height). This model produced better agreement with observed refractions near the horizon, and moved the top of the atmosphere up a factor of 3.5; but it was still much lower than estimates of the height of the atmosphere from twilight.

The situation improved considerably 15 years later, when Pierre Bouguer derived the proper differential equation for the ray. He was trained by his father in mathematics and hydrography; in 1729, he won the Grand Prix of the Royal Academy of Sciences for a monograph on determining the altitudes of stars at sea [35], in which he showed how to relate the ray shape to the density distribution in a spherical atmosphere. This allows the refraction to be written as a definite integral. Assuming that the ray makes the same angle with the local vertical at each level, the rays become logarithmic spirals.

Like Cassini II, Bouguer was struck by the great effect of the lower atmosphere; they both supposed that the "refractive material" had to be something other than air. Bouguer was misled by confusing the refractive index with the "refractive power" (i.e., the refractivity), which he correctly assumed must be proportional to density. This led him to try a refractive index proportional to some power of distance from the center of curvature, which amounts to adopting a polytropic model. Like Cassini, he then had to find his two parameters (the refractive index at the surface and the exponent in the power law) from the refractions at the horizon and some higher altitude. "But nothing is easier than to discover these two quantities," says Bouguer; he develops an iterative solution, and then uses his parameters to construct a refraction table.

Bouguer's big advance was to relate the refraction directly to the density distribution of the "refractive material." In addition, he provided a table of the geometric dip of the horizon, and a refined dip table that took refraction into account. But as Bouguer adopted a density model close to the one chosen by Jacques Cassini, he too found a small value for the height of the atmosphere — which reinforced his conviction that air could not be the "refractive material."

Because of the competence Bouguer displayed in his prize essay, he was one of the three men chosen by the Academy to measure the length of a degree of latitude near the equator in 1735. This expedition was sent to Peru; Bouguer went to Quito (now the capital of Ecuador), and measured both refraction and gravity there, and on the slope of a nearby volcano, over a kilometer above Quito, which itself is nearly 3 km above sea level. The uniform model had predicted that refractions should be much larger at these heights, where the observer would be closer to the supposed refracting surface of the atmosphere; but to everyone's astonishment, the refractions were substantially *less* than those measured at sea level. Bouguer's preliminary account, in a letter [36] sent from Quito in July, 1737, was read to the Academy in 1739.

His final report [32] was delivered to the Academy 10 years later. It contains the detailed measurements of refraction and their analysis, using the methods of his 1729 prize essay. By showing that a continuously varying refractive index explained not only the run of measured refraction with angular altitude, but also the different refractions measured from sea level to the tops of high mountains, Bouguer completely destroyed the homogeneous model atmosphere.

4. Other linear models

Bouguer's models, like the one proposed by Cassini II, assumed a linear decrease of refractivity with height, and a definite top to the refracting medium. However, this conflicted with the "geometric" (i.e., exponential) — as opposed to the "arithmetic" (i.e., linear) — decrease with height required by hydrostatic equilibrium, as well as the evidence from twilight and other phenomena that the atmosphere must extend much higher. Because of uncertainties about the effects of temperature on the refractivity of air, in addition to doubts about air itself as the "refractive medium", several other people tried to improve on these early linear models.

The first was Thomas Simpson. In the 1743 book [37] where he explained his method of evaluating definite integrals by parabolic approximations, he also discussed refraction (though without citing any previous theoreticians). Simpson produced a refraction table for altitudes less than 24° that is within a few seconds of modern values, but shows appreciable systematic variations; his method of computing it is sufficiently complicated that it never seems to have been put into practical use. However, he did appreciate the irrelevance of atmospheric structure for the run of refraction above a few degrees of altitude: "... the refraction in high altitudes, it has been proved, will be but little affected by different Laws of Density, and therefore come out very near the same, compute them according to what Hypothesis you will; even so near, that if the Refraction at any Altitude not less than about 7 Degrees be truly given from Experiment, the Refractions, computed from thence, for any higher Altitude, will never differ from one another by more than about 2 Seconds."

The next person to study the linear model was James Bradley, the third Astronomer Royal (after Halley), who discovered the aberration of light and the nutation of the Earth. He was an assiduous and careful observer, but never reduced and published his extensive observations, which were only published decades after his death in 1762. However, he corresponded extensively with many other astronomers, and his method of computing refractions became known as "Bradley's rule." It was first made public by Nevil Maskelyne in 1763 and 1764 [38, 39], and discussed by him further in 1787 [40]. He says that Bradley found the rule by applying Simpson's analysis to his own refraction measurements. The table computed by Maskelyne from Bradley's rule seems to be the first to use accurate corrections for temperature and pressure, though several earlier workers had published separate tables for summer and winter.

Meanwhile, Tobias Mayer, a self-taught mathematician who had become Professor of Economics and Mathematics at Göttingen in 1751, had devised his own way of computing refraction in the linear model. While working as a cartographer earlier, he had produced an accurate lunar map based on micrometric measurements, which allowed him to study the libration of the Moon. Apparently his interest in lunar observations led him to develop the famous theory of lunar distances, which was adopted by Maskelyne and the Nautical Almanac for navigation at sea; the refraction theory was part of this effort. Like Bradley, Mayer died in 1762 before his full theory was ready for the press, and his tables were prepared for publication by Maskelyne [41]. Unfortunately, that version only states the refraction formula with the vague remark that it was "deduced from theory". In Maskelyne's later discussion of Bradley's work [40], he noted that "Mayer's refractions agree almost exactly with Dr. Bradley's," so they evidently came from a very similar model.

The actual details of the derivation were not revealed until the publication of the correspondence between Mayer and Leonhard Euler by Eric Forbes in 1971 [42]. In this, one finds that Mayer, much like Simpson, had worried about the conflict of the linear model with the hydrostatic law; in a letter of 6 Jan. 1752, Mayer wrote: "Now, although this hypothesis is an open contradiction of the truth, the resulting formula for the refraction nevertheless agrees very closely with observations, and this makes me think that it would not really help much if one were to choose straightaway another and better expression for the density of the air. I have already seen Mr. Bouguer's treatise ten years ago ...." [This is evidently Bouguer's prize essay from 1729.] Forbes noticed that Mayer's formula is too small near the horizon; this is a well-known problem with the linear model, which corresponds to an unrealistic lapse rate (the negative of $\mathrm{d}T/\mathrm{d}h$) near the surface.

The inclusion of temperature effects in the refraction formulae of Bradley and Mayer came several decades before the development of the ideal-gas law. The understanding of these effects took more than a century [43], largely for lack of adequate instruments and suitable laboratory techniques. Daniel Fahrenheit did not invent the mercury-in-glass thermometer until 1714; earlier devices were affected by changes in barometric pressure, and by the nonlinear variation of the vapor pressures of liquids like water and alcohol with temperature.

Even so, a vague understanding that heating makes gases and vapors expand goes back to antiquity; so there was an awareness that the "degree of heat" must somehow be taken into account. But ideas about atmospheric refraction developed while the concepts of heat and temperature were still immature: the notion of "phlogiston" was proposed about the time of Cassini's first refraction table. Phlogiston gradually gave way to "caloric" in the last years of the 18th Century [44]; and that in turn was superseded by kinetic theory during the 19th Century. So, in the early years of the 18th Century, the rules for temperature corrections to astronomical refraction tables should be regarded as empirical adjustments made to fit the observed measurements of refraction.

An early attempt made by Leonhard Euler [33] luckily involved imagining a temperature scale such that at a constant pressure, "the numbers indicated by the thermometer … would always be reciprocally proportional to the densities of air." This corresponds to the modern ideas of an absolute temperature scale, and an ideal gas — both of which were at least half a century in the future in 1754. Euler also pointed out the importance (and the geometric interpretation) of the refractive invariant.

In the same paper, Euler assumed that the ray has the shape of a negative power law, which would be approximately true if the density of the atmosphere decreased as a power law of the height. He then compounded the error by guessing that the height where the atmosphere has half its temperature at the surface might "surpass a German mile" — 7 or 8 km — so he tried 2 miles, and later, 3. He also began the tradition of expanding the refraction in powers of the tangent of the zenith distance, which was continued by Lambert [45].

Bruhns [3] says that (because of Euler's errors) "no one has ever made tables from his formulas; they would have given worse values than the much simpler Bradley's formula." On the other hand, Bruhns liked Lambert's work; but Lambert gave a "proof" (based on oversimplified assumptions about the atmospheric density profile) that refraction cannot alter the apparent size of an object — did he ever notice the flattening of the setting Sun? — and that therefore the existing reports of mirages were false. It later turned out that the apparent utility of power series in $\tan z$ would prove deceptive.

All these attempts to construct useful refraction tables without knowing either the gas laws or the temperature distribution in the atmosphere must be considered empirical ways of smoothing observed measurements of refraction. They contributed little to understanding atmospheric structure. However, they did indicate that temperature variations were important.

5. The exponential model

5.1. *Avoiding the temperature problem: Newton*

But without a quantitative model for the effects of temperature, little progress was possible. However, the discovery that the density of air is proportional to its pressure, published by Boyle in 1662 and by Mariotte in 1676, allowed the principle of hydrostatic equilibrium to be applied to the refraction problem. This idea was first used by Isaac Newton in the 1690s — but without contributing to the advancement of astronomy. Although his refraction table was eventually published by Halley [46], Newton never explained how it was computed.

The mystery remained unresolved until 1835, when Francis Baily published the correspondence between Newton and Flamsteed [47]. It began in 1694, when Flamsteed told Newton he had found the horizontal refraction to be $33'$, decreasing to $23'$ a degree higher; but the existing theories based on a uniform atmosphere made this difference only $5'$ rather than $10'$ — the problem both Kepler and Riccioli had noticed. "It seems only the medium, in which the refractions are made, is not equable as supposed by those who build their tables upon theories. This subject deserves your consideration: I desire your thoughts of it at your leisure," Flamsteed wrote. Newton replied that he blamed the discrepancy on "the different heat of the air in the lower region;" adding that he also thought "the refraction in all greater altitudes is varied a little by the different weight of the air discovered by the baroscope."

A month later, Newton sent Flamsteed a preliminary refraction table based on a linear decrease of refractivity with height. A few weeks later, Newton wrote that this table was "not so accurate as it may be made ..., and when I have set it right I will send you a new copy of it." After another month, Newton wrote that "The theorem of refractions I sent you has this fault, that it makes the refracting power [i.e., the refractivity gradient] of the atmosphere as great at the top as at the bottom. This has put me upon thinking on a new theorem .... But the calculation is intricate." This was followed by his comments in January, 1695, that "the dense vapors, which always stagnate upon the surface of the sea and often fenny places, cause a strong refraction."

"To make a new table of refractions has taken up almost all my time ever since the holidays: and I have hitherto lost my pains in fruitless calculations, by reason of the difficulty of the work. For considering that such a table is the foundation of astronomy, and very necessary for your great work, and that you have taken so great pains in providing materials for it, I was desirous to complete it ...." The next month, Newton wrote that "I have been, ever since I wrote to you last, upon making a new table of refractions and have not yet finished it: 'tis a very intricate and laborious piece of work; ... I have found that, if the horizontal refraction be $34'$, the refraction in the apparent altitude of $3^{\mathrm{gr.}}$ will be $13'\,3''$: and if the refraction in the apparent altitude of $3^{\mathrm{gr.}}$ be $14'$, the horizontal refraction will be something more than $37'$." [But $14'$ and $33'$ were Flamsteed's observed values, which he asserted were "incontestable".]

Newton then went on to speculate about effects of "sea-vapors and fen-vapors" as being really due to "condensation of the air by cold." Finally, in March, 1695, Newton sent Flamsteed the finished table of refractions. But Baily did not find this table in Flamsteed's papers; the only value mentioned in the letter is $13'\,20''$ at $3°$ altitude, which agrees with Newton's table published by Halley in 1721.

Evidently Newton was aware that temperature effects were important; but, lacking a quantitative theory for them, he had to do the best he could with just pressure. And Flamsteed knew that result was inconsistent with his own careful measurements of refraction, which he continued to use instead of Newton's calculated values. Newton, who had done the best he could, considered Flamsteed ungrateful for his hard work; and relations between them rapidly deteriorated. Newton was so disgusted that he never mentioned refraction again.

The publication of this correspondence allowed Biot [48–50] to determine that Newton's enigmatic refraction table was actually based on an isothermal atmospheric model that took account of the variation of air density with pressure. Biot reverse-engineered Newton's procedure, and even correctly predicted that Newton's lost calculations would be found in the Portsmouth collection of Newtoniana; they were finally published in 1974 [51].

5.2. *Dealing with the isothermal model: early attempts*

The first person to *publish* an exponential model was Brook Taylor, who devoted the final section of *Methodus Incrementorum* [52] to refraction. He got as far as deriving the differential equation for the refraction, but never actually calculated numerical values. Solving the exponential problem proved too difficult even for Euler [33], who made several unfortunate approximations in his attempt. However, he did introduce the idea of approximating the refraction at small zenith distances by a multiple of $\tan z$, followed by some small correction terms. A little more progress in this direction was made by Lambert [45], who improved the approximations.

Later, Lagrange [53] made further improvements, based largely on de Luc's studies of the density of air. In 1787, Oriani [54, 55] extended Lagrange's approach, and produced the first numerical values of the intractable integral of $\exp(-t^2)$, obtained by trapezoidal-rule quadrature. Both Lagrange and Oriani used an awkward mathematical notation, denoting common base-10 logarithms (which they call "tabular") by *L.*, even before complicated expressions, without using parentheses; and denoting "hyperbolic" (i.e., natural) logs with a lower-case l. Oriani carried the series-expansions farther than Lagrange did, though making the clumsy typesetting more difficult to read than Lagrange's, and writing the whole paper in Latin, which was beginning to decline in use.

5.3. *Oriani's theorem*

Despite these drawbacks, Oriani managed to deal with the isothermal model numerically, and to show the properties of its representations in series of terms that are odd powers of $\tan z$. He found that the first term was adequate from the zenith to 50° Z.D., and that the first two terms suffice for z as large as 65 or 70°. Furthermore, he showed that as these two terms depend only on the conditions at the observer, "This expression depends on no hypothesis about either the law of heat in the atmosphere or about the density of the air at various distances from the surface of the Earth." (This demonstration is known as "Oriani's Theorem.") Finally, he managed to extend his refraction table to 80 or 85° by including the third and fourth terms; but at 86° his expansion began to diverge. Because of the "imperfect analysis", he recommended taking values within 5° of the horizon directly from observations.

It's not clear what accuracy Oriani had in mind in setting these limits; however, when he specified angles, he only gave values precise to the nearest second of arc. Later workers who used his two-term approximation have usually claimed it was useful to 75° Z.D., even when they worked to much finer precision. As late as 1907, Bemporad [56] compared Oriani's approximation with Bessel's *Tabulae Regiomontanae*, finding that they agreed to a hundredth of a second even at 75° Z.D. But Bessel, like Oriani, used an isothermal model atmosphere; so this comparison proves only that both astronomers could calculate correctly. Anyhow, Simpson [37] had already found that the details of the atmospheric model were unimportant above an altitude of 7°.

A more practical test is to compare the Oriani approximation to more exact refraction values. Today, it is known [23] that Oriani's two-term approximation differs from the refraction calculated for the Standard Atmosphere by 41 milliarcseconds (mas) at 70°, and by 122 mas at 74° Z.D. In fact, the isothermal model differs from the standard one by only 26 mas at 75°, and just over $0.1''$ at 79°. Such comparisons explain the popularity of Oriani's theorem.

In fact, it has become *too* popular. The idea that astronomical refraction is proportional to the refractivity at the observer has become embedded in astronomical folklore. It is a useful approximation over most of the sky, but is never exact; it usually fails by about 10% at the horizon, but can be wildly in error when the nocturnal inversion is strong. Unfortunately, many astronomers have mistakenly assumed it is true at the visible horizon, where it is even worse than at the astronomical one; see [23] for examples.

5.4. *Kramp and Laplace*

Oriani had to employ numerical integrations, leaving the further analysis of the isothermal model to later workers. In 1799, Christian Kramp published a monograph [57] on refraction in which he managed to evaluate various moments of a negative exponential, advancing the theory of the gamma function in the process. This showed him that Newton's table (as published by Halley) must have been calculated theoretically for an exponential atmosphere.

In the detailed explanation of refraction, Kramp derived the refractive invariant, and expressed the refraction as a series with powers of $\tan z_o$. He devoted a chapter to the mathematical properties of factorials; he used the integral expression for the gamma function to show that $\int_0^\infty \exp(-t^2)$ is half the square root of π (or, as he put it, "the integral ... reduces to a quarter of a circle.") This set the stage for a detailed discussion of refraction in an exponential atmosphere, followed by a discussion of actual observations of large refraction (including the Dutch observations at Novaya Zemlya). If the scale height falls to 1880 toises, the ray curvature equals that of the Earth, and the horizontal refraction is infinite. This seems to be the earliest step toward understanding ducts, which underlie various spectacular refraction phenomena: the Novaya Zemlya phenomenon, the Fata Morgana, and other types of superior mirages. Further work in this direction was done by Bouguer [32], Kummer [58], and others.

These preliminaries allowed Kramp to calculate refraction at the horizon, using the error function. This led him into a long discussion of Bradley's rule, which of course is inconsistent with the exponential model. The horizontal refraction naturally led him to discuss terrestrial refraction, and the ratio of Earth's curvature to ray curvature. Of course, as his isothermal model made the ray curvature a little too big, he found that this ratio must be less than 7 even on hot days, and usually about 5.5.

Kramp's discussion benefits from an understanding of the effect of temperatures, although he assumed a constant temperature in his model. But Charles's law allowed him to find absolute ray curvatures for different temperatures. Unfortunately, his text is difficult for a modern reader, because of his peculiar notation as well as his use of centesimal instead of sexagesimal angular units. There are also several typographical and other errors. Still, there is a lot of substance to his work.

While Kramp was working out the exponential atmosphere, Laplace was writing the *Mécanique Céleste*. Its fourth volume, which appeared in 1805 [59], deals with refraction, which Laplace regarded as due to a central force acting on the "luminous molecule". This allowed him to treat it as a problem in orbital mechanics. The result is somewhat more readable than Kramp's treatment, though it still uses centesimal angles. The main drawback of Laplace's treatment is his insistence on expressing everything in terms of a supposed law of attraction that acts on his luminous molecules, which introduces the expression

$$\sqrt{1+\frac{4K}{n^2}\cdot\rho} \tag{2}$$

wherever a modern reader expects the refractive index of air. Fortunately, this is all explained in detail in Nathaniel Bowditch's English translation [60]; in footnote (3844) on p. 453 of the cited volume, Bowditch simplifies the notation slightly in his equation numbered [8192b]. As Laplace already defined a quantity n as the velocity of light, that letter is not available to label the refractive index; he used i instead. Fortunately, Bowditch spends several pages of footnotes at the beginning of the chapter explaining the correspondences between the "emission" and the "undulatory" theories of light, noting that the only property actually used is the sine law, which is "the results of observation."

Laplace concentrated on the exponential model; but he recognized its weakness — an unrealistic increase in refraction toward the horizon — and devised a treatment for it. He also improved on Oriani's derivation of the tangent power series for refraction.

6. Hybrid models

Laplace seems to be the first person to see that because the linear model underestimates the horizontal refraction, and the exponential model overestimates it, some combination of the two would give a better refraction table than either one alone. While earlier efforts by Simpson, Bradley, and Mayer to adjust the low-altitude behavior of their linear models in this region were merely numerical, Laplace modified the structure of his atmospheric model, thereby connecting the observed refractions at low altitudes to atmospheric structure. However, his hybrid model turned out to be unphysical: it had a super-adiabatic lapse rate near the surface.

The mixed-model idea was applied more carefully by James Ivory in a long series of papers in *Phil. Mag.* volumes 57–68 (1821-26), in parallel with disputes about refraction with Thomas Young in the *Quarterly Journal of Science, Literature, and the Arts*. Their papers are too numerous to cite here; see the on-line bibliography [61] for a list. The disputed matters included the convergence of series, approximations for refraction in various atmospheric models, and the refraction at low altitudes. In general, Young's arguments seem slipshod and carelessly rushed into print, while Ivory's discussions are well thought out and insightful. In particular, Young [62] claimed that atmospheric structure could be inferred from observed refractions, while Ivory [31, 63] showed that a simple formula derived from Cassini's homogeneous model gave results practically indistinguishable from Laplace's results at zenith distances less than 80°.

A notable feature of Ivory's work was to consider various models that had not been treated before. He realized that the models of Cassini and Simpson/Bradley/Mayer both gave too little refraction at the horizon, and that the isothermal model of Kramp and Laplace gave too much; so he considered a broader class of models, with Cassini at one extreme and Laplace at the other. Today, we would call these polytropic models: the homogeneous model has polytropic index zero; the linear model has index 1; and the isothermal model has an index of infinity. And the total heights of these model atmospheres are proportional to the polytropic index plus one. (Polytropes are discussed further in Section 8.) By considering the heights of the various models, Ivory decided that the observed horizontal refraction corresponds to an atmosphere 4 times the height of Cassini's, with a lapse rate corresponding to about 7.8°C/km. However, he already knew that the observed lapse rate is about 6°C/km, which would produce too large a horizontal refraction. He attributed the discrepancy to a non-uniform decrease of temperature with height.

In 1823, Ivory published an 87-page paper [64] on refraction, summarizing the history of the subject as well as his own work. After describing the various (polytropic) models then in use, he says: "If we reflect that all these atmospheres will agree in giving the refractions actually observed by astronomers as far as 70° or 80° from the zenith, it is natural to think that the one which likewise coincides with nature at the horizon, will deviate but little from the truth in the intermediate 10°." On the basis of Gay-Lussac's balloon ascent, he notes that "the elevation for depressing the thermometer one degree will come out equal to 95 fathoms: and if we suppose that the same rate prevails in all parts of the atmosphere, the whole height will be … nearly 29 miles. The observations of twilight show that this is less than the true altitude; and hence we must infer, that the thermometer falls at a slower rate in the higher, than in the lower, parts of the atmosphere." Ivory took this decrease to be gradual; but today we know it is due to the stratosphere — which his comment might be seen as predicting.

To produce this decreasing lapse rate, Ivory introduces a parameter f on p. 449 that increases the polytropic index at great heights. At the surface, the index $m = 4$; high in the atmosphere, m becomes large, and the upper atmosphere is nearly isothermal. This flexibility gave him a physically consistent model atmosphere that would also match the observed refraction at the horizon, the measured lapse rate in the lower atmosphere, and the twilight observations. All these conditions were satisfied with $f = 2/9$. On comparing this model atmosphere with actual measurements of the temperatures and pressures measured on various mountains and in Gay-Lussac's balloon ascent, "It appears that, although the refractions near the zenith are affected in a degree hardly perceptible by the peculiar constitution of the atmosphere, yet, near the horizon, they depend entirely on the same arrangement of the strata of air indicated by terrestrial experiments."

On pp. 466–467 of this paper, Ivory discusses the convergence of the series expansions he has used. He points out that the series for the refraction integral "belongs to that class called semi-convergent; which converge indeed to a certain degree in their first terms, but afterwards become divergent." This term had been invented by Legendre a few years earlier, but was still unfamiliar to the English scientists; so Ivory's warning was generally ignored. (The modern term for such series is "asymptotic.") This behavior means that many common assumptions about the sizes of terms in a series are incorrect, and that the approximations commonly used in evaluating refraction integrals often turn out not to be valid. For example, although it is obvious that the expansion of the refraction in a power series in $\tan z$ must fail near the horizon, where $\tan z$ becomes infinite, it is not so obvious that the series actually diverges at *every* zenith distance, even very near the zenith.

Indeed, Ivory discusses this further on p. 472, where he says "The first two terms do not contain f; and they give that part of the refractions near the zenith, which has no dependence upon the constitution of the atmosphere." Of course, this explains why Oriani found this to be true for the tangent series; but Ivory does not mention Oriani. Ivory goes on to point out that f can be adjusted "so as to make the formula represent some very exact observations made at low altitudes, from 2° to 7° above the horizon. With regard to altitudes less than 2°, it is not clear that the astronomical refractions do not participate of the extreme irregularity that attends the terrestrial refractions".

Ivory's disputes with Young continued the next year. A notable contribution was Ivory's letter [65] in which he again used polytropic models, and introduced what we would call adiabatic atmospheres, which had been studied by Dalton. Ivory was puzzled by the discrepancy between the ratio of specific heats found in the laboratory and the expected value of 4/3; he knew that this plays a part in determining the speed of sound. He continued his study of adiabatic polytropes in 1825 [66], noticing that the measured atmospheric lapse rate is significantly sub-adiabatic, and that the sound speed agrees with the laboratory ratio of specific heats. He adopted a new value (−267°C) for the absolute zero temperature. And he concluded that the horizontal refraction is between 34′ and 35′ at 50°F, making the lapse rate between one degree in 84 fathoms [corresponding to 6.5°C/km — the value enshrined in our Standard Atmosphere for the past century] and 4.6°/km, saying "The true atmosphere is undoubtedly contained between these limits."

In 1826, Ivory [67] carefully compared his theory with observed refractions near the horizon, concluding that "no table of refractions hitherto published can safely be trusted to at altitudes less than 2°. And this conclusion is corroborated by the parallel instance of the terrestrial refractions, which are found to vary from a certain limit through all degrees of magnitude, and even to change from positive to negative. ... The boundary which separates the refractions irreducible to any degree of regularity, from those that can be theoretically computed, at least with tolerable regularity, ... seems to be placed at an altitude of 2° or $1°\frac{1}{2}$ above the horizon."

Finally, in his Bakerian lecture of 1838 [68], Ivory reviewed the whole subject, emphasizing that "a great share of that part of the astronomical refraction which depends upon the constitution of the atmosphere, must be ascribed to the initial rate at which the density decreases," and that "as far as 74° from the zenith ... all tables of refraction may be computed by Cassini's method." But between there and the horizon, the lapse rate near the ground becomes increasingly important. He tried hard to find evidence in refraction measurements for higher-order terms in the temperature profile, but succeeded only in showing that even the second-order term in a series expansion (representing a change in lapse rate with height near the surface) could only affect the refraction within two degrees of the horizon by a few seconds of arc. He concluded that improving the knowledge of the temperature profile near the ground would require "many good observed refractions at altitudes less than 5°."

However, in the same year, Arago (who had plenty of practical experience in measuring altitudes near the horizon) pointed out [69] that "Astronomers who have tried, even a single time in their life, to determine the value of horizontal refraction, know how little it is permissible to count on the results. Ordinarily, the edge of the sun is the aim point; but near the horizon this edge is so strongly indented, so vividly colored, so jagged; besides, these diverse irregularities are so changeable that the observer does not know where to put the wire of the reticle, at what point, at what height to fix the telescope on the graduated limb of the instrument which he uses." In short, it is hopeless to expect to obtain reliable measurements within a few degrees of the horizon. (These sunset structures are discussed further at the end of Section 12.)

7. Realistic models

As Ivory's final models have a lapse rate at the surface very close to the mean value obtained from direct measurements, they represent "mean refraction" as well as can be expected. What they lack is a way to include changes within the lowest kilometer caused by local weather conditions, which would require a means of computing refraction from actual, measured atmospheric states. This had actually been provided by Biot in 1836 in two notes in *C.R.* [48, 70] and a detailed monograph [71] in the *CdT*.

Biot was led to these improvements by reading the Newton–Flamsteed correspondence published by Baily the year before [47]. While reconstructing Newton's procedure from the clues in those letters, he not only discovered that Newton had used an exponential (i.e., isothermal) atmosphere; he also found a remarkable theorem about the vertical magnification of objects at the astronomical horizon. (Biot had a long-standing interest in that subject; he had discussed the flattening of the Sun and Moon at the horizon in his 1810 textbook.) Biot used calculus; but a simpler proof is possible with just geometry [24].

In this work, Biot repeatedly emphasizes that the refraction observed at sea level is mainly determined by the structure of the lower atmosphere, and that the structure of the upper layers has no appreciable influence. Consequently, the observed refraction cannot be used to estimate the height of the atmosphere, nor to indicate the way in which the densities and temperatures decrease at heights greater than four or five thousandths of the Earth's radius (i.e., 25 or 30 km).

After discussing the validity of the Oriani approximation, Biot went on to show a simple way to evaluate the refraction integral numerically: he changed the variable of integration to the local zenith distance of the ray at each height in the atmosphere. By using 13 suitably chosen density levels, he reproduced Ivory's calculated refraction at the horizon with an accuracy better than 1.25″. He gave the details of the calculation to illustrate this example. As additional test cases, he worked out the isothermal model and the arithmetic-decrease model (i.e., the one used by Simpson and his followers), and discussed Bradley's rule. This case is very unrealistic; its lapse rate is about three times the value measured near the surface by direct observations. Despite this large deviation from the real atmosphere, it produces very nearly the same refraction from the zenith to 80° as Ivory's model. (This is another illustration of the Oriani theorem.) But at larger zenith distances, it gives calculated refractions that are too small; Biot showed that this conclusion can be predicted from his theorem about the magnification at the horizon, saying "These defects of Bradley's rule have long been recognized, but I do not know that anyone has indicated the true cause."

In his concluding discussion, Biot said that "if … some day, exact measures allow us to obtain more precise and more numerous elements of the real state of the lower layers of the atmosphere, … one could, by the method of quadratures explained in the present memoir, use these results in the calculation of refraction." Over a century later, this actually happened after Auer and Standish [72, 73] re-invented Biot's transformation of the refraction integral. Their way of evaluating this integral is not as direct as Biot's, and fails when the ray curvature is near the Earth's curvature, which makes their Newton–Raphson iterations fail to converge.

In 1838, Biot published a note [74] in which he used Gay-Lussac's balloon measurements to show how his numerical method could be applied to data on the real atmosphere. He also produced another long monograph on atmospheric structure [75], in which he again emphasized the uselessness of refraction observations for inferring the structure of the upper parts of the atmosphere, "as the refractions observable down here [at sea level] are sensibly independent of the manner of superposition which one could attribute to them." In 1854 and '55, an extensive debate about the relation between low-altitude astronomical refraction and terrestrial refraction raged through the pages of volumes 39 and 40 of *C.R.*; Biot summed it up in a review [76] in which he pointed out that the "caprices" of refraction near the horizon were beyond the abilities of theory to predict, but that empirical refraction data in this region provide information about the *lowest* layers of the atmosphere.

8. A wasted century

Biot's understanding of refraction was far ahead of his time. Despite his best efforts, astronomers and geodesists continued to treat refraction as a nuisance to be eliminated, rather than a problem to be understood; and their traditional approach was to use Lambert's divergent series of odd powers of the tangent. Subsequent workers continued to expand unrealistic analytical models of the atmosphere in these divergent series, whose complexity led farther and farther from any understanding of the problem. Instead of following Biot's advice to deal numerically with real atmospheric conditions, they stuck to series expansions based on analytical idealizations.

One trend was to study various polytropic models intermediate between the extremes of Simpson's order 1 and the infinite-order isothermal model. (The term *polytrope* was not introduced to atmospheric physics until 1916, when Robert Emden [77] described the basic properties of these models, in which the density is proportional to a power of the pressure; if the variation of gravity with height is neglected, a polytropic region has a constant lapse rate. In 1923, he gave a rather superficial account of their use in refraction calculations [78].) Among the first was Fabritius [79], who remarked that "one can draw the conclusion that the constitution of the atmosphere in the highest layers will be without appreciable influence, even on the horizontal refraction." But most assumed a particular lapse rate, and derived series expansions for it. For example, Bauernfeind (1864) developed the refraction for an order 5 polytrope [80], carrying the expansions to the 28th order terms. (He later extended this treatment to terrestrial refraction.)

Further efforts of this sort were made by Radau, an amateur mathematician who evidently enjoyed exploring series expansions [81, 82]. He worked out the polytropes of orders 4, 5, and 6, as well as several more elaborate models. His intricate series expansions were endorsed by the Paris Observatory, and were inordinately influential on later workers. Unfortunately, they were based on Lambert's expansion [45] of the refraction integral in odd powers of $\tan z$, which Ivory [64] had shown to be divergent.

In particular, such series were used in Simon Newcomb's chapter on refraction in his very influential textbook [83]. Although Radau realized that thermal inversions in the lowest few meters were common, that they increased the refraction near the horizon, and even that a low-lying thermal maximum could trap nearly horizontal rays, Newcomb wrongly claimed that "astronomical refraction is little influenced by the diminution of temperature at low altitudes," and ignored inversions. Furthermore, Newcomb's formulae for the refraction are so complicated that he lost some coefficients and made numerical errors [23].

Somewhat more useful efforts attempted to incorporate the nocturnal inversions that had been measured by James Glaisher [84]. Early attempts to include such features in refraction models were made by Egon von Oppolzer [85] and by Banakhevich [86]; these involved a simple surface-based inversion corresponding roughly to the planetary boundary layer. Perhaps the last such efforts were those of Garfinkel [87, 88], who still represented both the model and the refraction analytically. But by that time, direct instrumental probing of the boundary layer had already revealed much more complex structure (cf. [89, 90]); and the distortions of the Sun at the horizon similarly showed the importance of thermal inversions [91–98]. (These features are discussed in Section 12.) More recent atmospheric measurements [99, 100] show extremely complex boundary-layer structure that does not resemble the simple analytical models used by astronomers.

9. Analytical solutions to the inverse problem

9.1. *Bruns and Hausdorff*

Instead of using a trial-and-error search to find an atmospheric model that could explain observed astronomical refractions, Heinrich Bruns, who succeeded Carl Christian Bruhns as director of the Leipzig observatory, developed a highly original approach [101] to astronomical refraction. He recognized that the choice between $(n-1)$ and (n^2-1) as the expression for refractivity could be made to simplify the analytical development, and also that the expression nr was a better choice of integration variable than the radius of curvature r itself. These simplifications allowed him to obtain differential corrections to the parameters of a provisional model from the deviations of its calculated refractions from actual observations of refraction: "it is possible, by reversing the course always followed until now, to obtain the vertical temperature changes ... numerically from refraction observations, as far as the nature of the matter allows."

Of course, this is the crucial difficulty, as Biot had shown; but Bruns apparently never read Biot's work. Still, Bruns realized that the higher layers "make only a small contribution to" the refraction. He also pointed out that the expansion in odd powers of $\tan z$ is only semiconvergent. Nevertheless, he found an expansion of the refraction as a sum of rational functions of $\cot z$ (which is equivalent to rational terms in $\tan z$), which can be expressed as a sum of partial fractions. By limiting the development to one or two terms, he could represent standard refraction tables better than many of the previous refraction theories.

All of these fits led to model atmospheres that ended near 30 or 40 km. Nevertheless, he concluded that "the partial-fraction formula with two terms has enough flexibility to satisfy all requirements." Although his models terminate with zero temperature and pressure at the top, he concluded that "from refraction observations, one can draw no conclusion about this part of the atmosphere, which makes only an inappreciable contribution ...."

His student Felix Hausdorff began his career by studying the refraction and airmass integrals, using methods developed by Bruns (who, in turn, had been a student of Kummer's). Hausdorff began with a thesis [102–104] in which he transformed the refraction integral into a set of general functions that could represent the refraction in *any* possible atmosphere. When this proved to be impractical (because the problem was ill-posed), he then tried the same trick with the extinction [105]. This was an even worse fiasco, partly because the existing extinction data were not monochromatic. (Today this complication of the broadband extinction problem is described as the Forbes [106] effect; but Hausdorff apparently did not know of the work of Forbes, and instead referred to the radiometric measurements of S. P. Langley.) After this wasted effort, Hausdorff abandoned the refraction problem and took up topology.

9.2. *Modern efforts*

In the 20th century, the idea of inferring atmospheric structure from refraction observations was revived by meteorologists [107, 108]. The idea seemed appealing because it is difficult to measure air temperatures very accurately [109, 110]. However, in spite of the apparent sensitivity of terrestrial refraction to lapse rates, the required measurement accuracy [111] was hard to reach in practice. In the 1980s, careful theoretical studies [112–115] showed that the inverse problem is hopelessly ill-conditioned above the astronomical horizon, but is well-posed below it, provided that the atmosphere does not contain a duct. Apparently, it is the infinite value of $\tan z$ at the perigee point on a ray that makes the refraction integral invertible; this concentrates the weight function at the perigee, and prevents the integration over the whole path from smoothing away the dominant contribution from that height.

A low-lying thermal inversion can produce a duct. If the observer is in the duct, ray trapping can produce superior mirages and other complex refraction displays. In this case, the trapped rays will be horizontal at heights that approach the top of the inversion; then it is possible to infer the thermal structure up to the top of the duct from the miraged images [116], if the heights and distances of the miraged objects are known. Modern computing power has made the necessary numerical ray-tracing practical.

10. From analytical to numerical integrations

Biot's 1836 paper [71] in the *CdT* showed how simply the refraction can be integrated numerically. Unfortunately, astronomers were accustomed to doing everything analytically in the 19th Century, and Biot's method was almost completely ignored. But, as series expansions grew more and more complicated, a few workers suggested that numerical quadratures were a better way to calculate refraction. Perhaps the first attempt was made by Julius Bauschinger [117], who made observations to 88° Z.D., and computed refractions for a model with a nocturnal inversion of 2°C in the lowest 200 m, which he divided into 10 m layers.

But a more explicit appeal was made by Azeglio Bemporad in 1907 [56, 118]. In these and later works, he emphasized that direct numerical quadrature is easier than the labor of evaluating all the terms in elaborate series expansions: "If one considers the efforts that have been spent on the countless analytical treatments of refraction theory, especially the most complete ones of Gyldén and Radau, it seems justified to ask whether the derivation of astronomical refraction is not easier to reach by *plain numerical calculation (mechanical quadrature) by directly applying the results of atmospheric physics*, than by the digression through a hypothesis that today one can completely avoid for the region below 10 km height, and which will soon be entirely dispensable following the newest advances in sounding-balloon technology." This statement was then published in French [119], making it available in three languages.

A few years later, Comstock [120] used "mechanical quadratures to compute directly from the differential expression for the refraction" for a range of conditions, from an unstable surface layer to a surface-based inversion "over a snowclad earth." Out to 75° Z.D., the computed refractions differ by "amounts that rarely exceed a few hundredths of a second of arc." But at the horizon, "variations amounting to several minutes of arc may easily result from plausible changes in the distribution of temperature at different levels in the atmosphere." These large variations almost disappeared at 4° altitude, where the range of refractions "due to varying conditions in the lower two kilometers of the atmosphere is reduced from nine minutes to less than one second of arc."

After Biot's method was rediscovered by Auer and Standish [72, 73] and adopted by the RGO [121, 122], numerical integration has been widely used to calculate refraction for a variety of atmospheric models, including many with thermal inversions [23, 116]. As the computing power formerly confined to mainframes became available on desktop and even laptop machines, several other numerical methods (e.g., [123]) have come into use.

11. Extinction

Extinction is the removal of energy from an incident beam of light by scattering (angular redistribution out of the beam) and true absorption. It was first studied by Bouguer in his pioneering work [124] on photometry. He discovered that the transmitted light decreases exponentially; the exponent is the product of an extinction coefficient and a measure of the amount of attenuating medium (for which he coined the term "airmass"), and roughly determined the transmission of the atmosphere. He also introduced a remarkable simplification for calculating airmass: as the refraction is only half a degree at the horizon, he decided to neglect the ray curvature, and simply computed the length of the atmospheric path as though it were a straight line.

More detailed discussions of the airmass function were made by Maurer [125] and Hausdorff [105]; but no real progress was made until Bemporad's treatment in 1904 [126], which remained the standard through most of the 20th Century, despite Link's insightful discussions [127–129]. Currently, there is a table and approximation [130] based on the Standard Atmosphere [131].

11.1. *Refraction and extinction*

Atmospheric extinction is closely related to refraction in several ways. The most obvious is the mathematical similarity between the refraction and airmass integrals, which was first noticed by Laplace [59, 60]. A century later, after wave theories had replaced the "emission" models of Newton and Laplace, Lord Rayleigh [132, 133] noticed that the refractivity of air is intimately connected with molecular scattering out of the incident beam of light, and hence with the molecular part of the atmospheric extinction. There are other connections as well.

11.1.1. *Laplace's extinction theorem*

Just as refraction is an averaged value of $\tan z$, airmass is an averaged value of $\sec z$ along the ray. So, just as refraction near the zenith is nearly proportional to $\tan z_o$, the relative airmass (normalized to unity at the zenith) is nearly proportional to $\sec z_o$. But the weighting function in the airmass integral involves the density, while the weighting in the refraction integral involves the refractivity (or density) *gradient*. However, Laplace noticed that in an isothermal atmosphere, the density (which is nearly proportional to the refractivity) is a declining exponential function of height; so the refractivity and density gradients also contain that same exponential factor. Therefore, the refraction is just proportional to $\sin z_o$ times the airmass factor.

How well does this rule work when the atmosphere is not isothermal? Figure 5 shows the product of airmass and $\sin z_o$ as a function of the refraction, calculated for zenith distance z_o in three model atmospheres. The points on the curves 1, 2, and 3 degrees above the astronomical horizon are joined by thin dashed lines numbered 1–3. The line for the isothermal model is straight, as Laplace's theorem predicts. The lines for the adiabatic and Standard Atmosphere models deviate from this straight line only within a few degrees of the horizon.

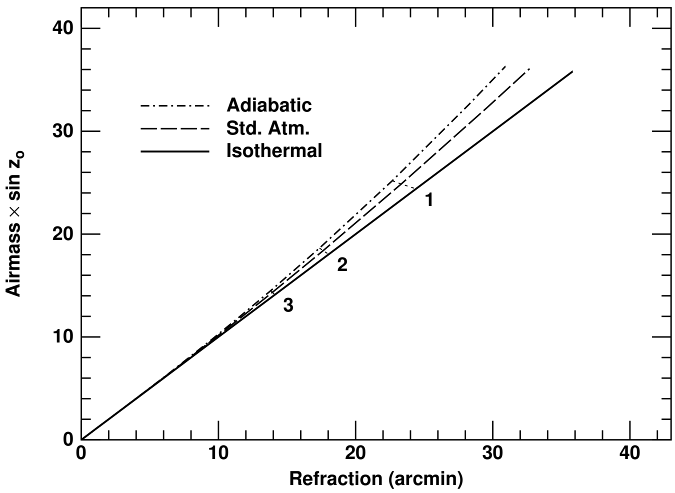


Figure 5. Laplace extinction plots for three model atmospheres. The zenith is at the origin; the horizon is at the upper right end of each line.

11.1.2. *Molecular scattering*

The refractive index and scattering are connected by the molecular polarizability. Rayleigh had first found the λ^{-4} scaling from a dimensional argument based on the elastic-solid model of the luminiferous ether, but needed the electromagnetic theory of light to find the strength of the scattered waves. While the refractivity is proportional to the mean polarizability of the molecules, the scattering (and hence the extinction) is proportional to the mean-square, rather than the straight mean. This complicates the relation between molecular scattering and refractive index of air, because the commonest molecules in air (N_2 and O_2) are anisotropic.

The isotropic part of the polarizability produces scattered waves that are coherent in the forward direction; and because the frequencies of visible light are much lower than the resonant absorptions of the valence electrons, the coherent sum of the incident and scattered waves is also retarded in phase. This retardation makes the refractive index of air greater than unity. The anisotropic part produces additional, incoherent scattering that is isotropic in distribution because of the random orientations of the molecules. As a result, the polarization of the light scattered at 90° to the incident beam is incomplete; so the polarization observed in the laboratory at right angles to the illumination provides a measure of the anisotropy. But the observed depolarization also depends on the polarization of the illuminating beam — a complication that was overlooked by several workers around 1980, when incorrect extinction coefficients were published for air, based on experiments with (polarized) lasers. These difficulties, as well as the spectral distribution of the scattered light, were explained in a review article [134, 135].

More recently, it became possible to measure the extinction coefficients of gases directly in the laboratory. The main components of air were measured, and found to be in good agreement with the complete theory for Rayleigh scattering [136, 137]. For nitrogen and argon, the theory agrees with measurement to a fraction of 1%. The measurements for oxygen show a small additional extinction at higher pressures due to the collision-induced O_2–O_2 absorption near 532 nm wavelength. These results were independently verified [138, 139], and appear to be quite reliable.

11.1.3. *Molecular absorption*

Classical dispersion theory treats absorbing molecules as damped harmonic oscillators; this model explains the dispersion of refractivity with the well-known Sellmeier dispersion formula, and is described in most textbooks on physical optics. This approach is satisfactory when the strong absorptions are far removed in frequency from the spectral region of interest, which is often an acceptable approximation in visible light, because the main absorptions of air are in the ultraviolet. However, some molecules also produce both discrete absorption lines and broad continua in visible light.

Worse yet, many of the absorbing species are far from uniformly mixed: the ozone is mainly concentrated in the stratosphere, while water vapor is mostly near sea level. The effective airmass factors for these absorbers are quite different from those for the well-mixed gases like nitrogen and oxygen. An approximate treatment for these components is available [140], but because their distribution in height is extremely variable, one should really use detailed sounding profiles to calculate correct airmasses for them.

However, there are two situations in which molecular extinction measurements can be used to retrieve vertical structure. The first is the Umkehr method for studying stratospheric ozone [141]. Although the ground-based observer is far below this ozone layer, observations of zenith spectral radiance provide an indirect proxy for the illumination of the upper atmosphere as the edge of the Earth's shadow sweeps through it. This effectively uses rays that are horizontal in the ozone region to provide a well-posed problem that is a useful supplement to satellite-based backscatter measurements.

The second case concerns water vapor. Because water is a polar molecule, it has a pure-rotation absorption spectrum; its strongest line is the R(0) line at 1.35 cm wavelength. This is a fundamental resonance line, so it has a strong dispersion signature in the refractivity spectrum, decreasing the refractive index for millimeter waves, and increasing it at microwave wavelengths of centimeters. (In addition, there are strong vibration-rotation transitions in the near infrared, whose effects have been measured and modeled in [142–146].) The refractive effects of water vapor are large in the 10–15 cm wavelength band used by navigational satellites, and can be separated from ionospheric effects if two or more frequencies are used; see [147] for a recent review of this technique. The additional delay due to water vapor appears as a path-length increase; the "mapping function" used in the radio-science community is really the same thing as the "airmass" function invented by Bouguer. as suggested in [24] and demonstrated in [148]. Because the position of each satellite is known from its ephemeris, the solution to the integral equation for airmass is well-posed and allows some vertical structure to be determined.

11.1.4. *Aerosol extinction*

Still more unpredictable is the highly variable extinction produced by aerosols. A great variety of suspended material occurs in the atmosphere: dust raised from the ground by boundary-layer turbulence; salt particles from evaporated sea spray; photochemically produced materials such as ammonium sulfate and sulfuric acid droplets; pollen grains, spores, bacteria, leaf fragments, airborne spiders and their webs (collectively called "biosol"); meteoric and volcanic dust; ice crystals, cloud droplets, and other "hydrometeors"; and other materials. Most of these arise from or are produced near the surface, and occur primarily in the lowest kilometer or two. Some of the particles are conservative scatterers, but others have complex refractive indices and absorb. Droplets are mostly spherical, but crystals and many biosols are irregular and cannot be described by Mie theory.

Many of the particles are hygroscopic, or even deliquescent, and are very sensitive to changes in relative humidity [149]. Below about 30% R.H., they are dessicated and shrivel up, presenting small optical cross-sections. Between 30% and about 70%, they gradually expand. Above 70% humidity, the deliquescent particles form aqueous solutions in equilibrium with the vapor content of the ambient air. These droplets grow rapidly as the relative humidity approaches 100%. Quantitative models exist [150] to describe the optical properties of various common aerosols, and have been incorporated into computer codes such as MODTRAN.

The aerosol opacity is generally concentrated toward the surface, with a typical scale height of a kilometer or so. This is the region of the atmosphere that dominates the refraction and extinction within a few degrees of the horizon; consequently, it is hopeless to try to infer atmospheric structure from extinction data. The usual procedure is to adopt some aerosol model, and to subtract the molecular extinction from observed values, assuming that the difference is the aerosol component. This can then be used to check the parameters of the aerosol model. Fortunately, the aerosols do not contribute to the refraction.

11.1.5. *Interactions between refraction and extinction*

Originally, refraction corrections were needed to obtain geometric ("true") positions of stars and planets from observed places. [One must be careful to avoid the term "apparent places," which is used by positional astronomers to denote directions from the Earth's center that include the effects of precession, nutation, parallax, and aberration, but *not* refraction.] Tables of refraction corrections and airmasses always use the refracted zenith distance of an object as the argument, which was very convenient for reducing observed altitudes, but is not suited to correcting photometric measurements for extinction. In photometry, one almost always computes a true zenith distance from catalog data and time, and then must convert this to an observable zenith distance that is not actually measured. Many people have mistakenly used computed zenith distances to find the airmasses for photometric observations without allowing for refraction. This error must also be avoided in using airmass tables and formulae to predict insolation for solar energy applications. Formulae are available for computing airmasses from times and geometric positions [151].

Perhaps the most important effect of extinction on measurements of refraction is the statistical bias pointed out by Henry Atkinson [152]: refraction is usually measured by observing circumpolar stars at lower transit, where extinction is so large that "no star can be seen at 89° of zenith distance, unless the air possess a degree of transparency far, very far, beyond what it possesses in its mean state", and "experience shows that at 85° or 86° of zenith distance small stars frequently cannot be observed." So the measured refractions near the horizon are biased toward times of high barometric pressure that tend to produce stronger than average thermal inversions, and unusually large refraction.

Furthermore, Atkinson recognized that this is just the part of the sky where refraction depends on atmospheric structure near the ground. He attempted to demonstrate this by calculating refraction for models with perturbed boundary layers, but died before the work could be completed. However, his incomplete manuscript was enough to show that realistic changes in the lowest 50 feet [15 m] could easily explain observed irregularities in refraction near the horizon [153, 154].

Because both aerosol and Rayleigh extinction decrease with increasing wavelength, objects appear redder near the horizon than high in the sky, as is obvious at sunset. That increases the effective wavelength of low objects observed in broadband light, and so makes their observed refraction a little smaller, because of the dispersion of atmospheric refraction. So atmospheric reddening produces color-dependent effects in ground-based astrometric measurements. In particular, variations in aerosol reddening can make the effective wavelength of visual observations at the horizon vary from about 450 nm in extremely clean conditions to 750 nm or more in very dirty air; the refraction of the Sun's upper limb can vary by 10 or 15 seconds of arc, even for a fixed temperature profile.

These interactions make it impractical to infer atmospheric structure from astronomical refraction and/or extinction observations. However, it may be possible to calculate refraction to useful accuracy from measured thermal profiles, if the measurements have enough resolution.

12. Real atmospheric structure

At first glance, it might appear that vertical resolution is not very important, because of the averaging of atmospheric structure in the refraction integral at positive altitudes. But the averaging is very nonlinear; in fact, the atmospheric layers where rays that reach the observer are nearly horizontal contribute much more to the refraction than do layers with considerably different lapse rates. So the averaging emphasizes the layers where the ray bending is largest, and the result is appreciably more refraction than would be calculated for a smoothed profile having the same average lapse rate.

Although radiosonde profiles are sometimes called "high-resolution", they are in fact samples from a profile smoothed by the thermal inertia of the temperature sensors, which are thousands of times denser than the air near the surface. A sensor must come into thermal contact with several times its own mass of air to reach equilibrium with the gas. So, as the sensors are typically a millimeter or two in diameter, they inevitably produce profiles smoothed over several meters of height [155]. For a typical time constant of 4 seconds near sea level and the nominal ascent speed of 5 m/s, this smoothing length is about 20 meters. The lag of thermistor response [156] also produces a systematic error in the reported heights of the temperature data, which refer to levels about 20 meters below the pressure data.

For example, compare the detailed profiles with simultaneous rawinsonde data in Figure 1 of [157]: much reproducible fine structure was measured with fast-response sensors on a moving carriage, but was completely unseen by the rawinsonde. These authors pointed out that "such observations have not been reported ... because the temperature and humidity sensors used in radiosondes have had inadequate response for the usual rates of ascent of balloons ...". They also found that very steep thermal inversions (typically 5°C in 5 m) occur in "sheets" between convectively unstable "layers" with nearly adiabatic lapse rates. This fine structure is completely smoothed out in radiosonde data. Such structures were independently confirmed to be "ubiquitous" up to 27 km [158], with "practical consequences for all the propagation phenomena (light beams, radio waves, sounds ...) within the atmosphere." Special balloon soundings using fine-wire sensors [159] show that the convective layers bounded by thin turbulent sheets can be hundreds of meters thick.

Boundary-layer structure that eludes radiosondes, but is obvious in refraction near the horizon, appears as Arago's [69] "jagged" deformations of the solar limb observed from a moderate height. These low-Sun phenomena are well known from the photographs made at Castel Gandolfo [97], which is 450 m above the sea. At that height, the dip of the sea horizon exceeds the solar diameter, so these features appear in the zone of sky below the astronomical horizon, where each feature in the temperature profile has a corresponding feature in the solar limb [160]. Each zigzag in the limb should match a thermal feature between sea level and 450 meters in the temperature profiles; yet the radiosonde data that accompany some of the pictures show no hint of the numerous photographed features, which are mostly mock mirages [98] due to thermal inversions. (Other references to these features were cited at the end of Section 8.) These refraction phenomena were not generally understood at the time they were photographed, so the interpretations in the book [97] are incorrect, as explained in [23]; detailed interpretations are available on the Web [161].

Clearly, the simple analytical descriptions of atmospheric structure used in the 19th Century were very unrealistic. Yet we still are using such a model: the Standard Atmosphere. How bad is it?

13. The Standard Atmosphere

13.1. *A little history*

The ISO Standard Atmosphere that has been widely used to calculate modern refraction and airmass tables is essentially the same as the 1976 version of the U. S. Standard Atmosphere [131] — which was (apart from some changes at great heights that have no effect on refraction and extinction) — the same as the 1966 version. Indeed, the troposphere and lower stratosphere tables can be traced back to the very first U. S. Standard Atmosphere that was introduced in the 1950s as a slight modification of the ICAO Standard Atmosphere produced in the late 1940s, which itself was a minor variant of the NACA atmosphere of 1925. (The differences among the various standards are given in Minzer's review [162].) All these model atmospheres start at 15°C, and all have exactly the same tropospheric lapse rate of 6.5°C per kilometer of height. This lapse rate is responsible for the behavior of refraction and airmass in those few degrees above the astronomical horizon where Oriani's Theorem is useless, and atmospheric structure actually matters. So where did this lapse rate come from?

At the end of the first World War, practical problems in aircraft engineering and long-range gunnery that had arisen during the Great War remained unsolved. These problems lay in the density profile of the atmosphere, which is what underlies refraction tables as well. However, the people who decided to solve these problems were not astronomers or surveyors. So, apparently without consulting anyone interested in atmospheric refraction or extinction, a handful of experts agreed among themselves to adopt a particular temperature profile, beginning at 15°C at sea level, and decreasing linearly by 6.5° for every kilometer of height up to 11 km, which had been proposed by Albert Toussaint, Engineer of the Aerotechnical Institute of the University of Paris, (St.-Cyr l'Ecole), and later its Director, as well as chairman of the French National Committee charged with the examination of all new inventions relating to aviation. Toussaint's paper [163] was primarily concerned with the performance of supercharged aircraft engines.

His temperature profile was based on a paper by Dr. Pericle Gamba, of the Geophysical Observatory of Pavia, who had published several papers on atmospheric structure, balloon soundings, and related matters. One of these — Toussaint did not say which one — summarized information about atmospheric temperature measurements in different countries. Toussaint was looking for a simple way to summarize this information, and chose the formula that has become the Standard Atmosphere temperature model. In describing this model, Dr. Willis Ray Gregg (at that time, Chief of the Aerological Section of the U. S. Weather Bureau) wrote [164]:

"With the advance of aeronautics and the science of artillery, engineers and specialists in these fields have come to require a specific knowledge of the varying states of the atmosphere from the ground to very great elevations. This has led to the introduction of a conventional term commonly known as *the standard atmosphere*, which pretends to specify the normal or average condition. As is well known, the 'standard atmosphere' is never found; that is to say, at no time or place do 'standard' or average conditions of all the meteorological elements at all altitudes simultaneously occur. Nevertheless, it is proper, and in certain fields (especially those of aviation and ordnance) it is necessary, to adopt so-called 'standard' values, and it is desirable to have these represent as closely as possible true mean values."

After describing Toussaint's model, Gregg continued: "Although the adopted rate of temperature decrease is arbitrary, the resulting values nevertheless agree quite well with annual means as published by various investigators for Europe and the United States .... Prof. Toussaint remarks:

> It has been found preferable to take a linear law rather than to seek an equation approximate to Prof. Gamba's curve, for the following reason:

> In order to define the standard atmosphere, what is needed is not an exact representation of that curve, but merely a law that can be conveniently applied and which is sufficiently in concordance with the means adhered to ....
>
> The deviation is of some slight importance only at altitudes below 1,000 meters, which altitudes are of little interest in aerial navigation. The simplicity of the formula largely compensates this inconvenience."

But of course this lowest kilometer that is "of little interest in aerial navigation" is also the kilometer that is most important for refraction and airmass calculations near the horizon. On the other hand, the trouble it causes is only appreciable *very* near the horizon. How much practical harm does it cause?

13.2. *The inconvenience*

Figure 7 of [23] shows that a rather drastic change to the lowest kilometer (raising the temperature at 200 m by 10°C) makes less than a minute of arc change to the refraction at an altitude of 1°. Who cares what the refraction is at this altitude? As Atkinson pointed out, only a few bright stars are even visible there, because the extinction is so large.

Refraction and extinction are important for planning solar-power installations. But in most situations, the Sun is occulted by trees, buildings, and other objects well above an altitude of one degree. In any case, the extinction at low altitudes is so great that most of the little power received by solar panels is scattered light from the sky, not direct light from the Sun.

The artificial simplicity of the Standard Atmosphere makes it useful mainly as a reference test case for checking the accuracy of refraction and airmass calculations. However, it is so easy that it cannot detect the weakness of programs that use the Auer–Standish algorithm [73], which fails in more realistic cases.

Furthermore, using the Standard Atmosphere produces a systematic bias, similar to the effect pointed out by Atkinson. The Standard is meant to represent the average of all possible conditions: day or night, rain or shine, clear or cloudy. But stellar astronomers only care about clear nights; they cannot observe during the daytime or under overcast skies. Similarly, solar-power output depends heavily on cloudless daytime conditions. The requirements for clear skies favor high-pressure air masses, which produce subsidence inversions and tend to accompany low relative humidities. So these applications of refraction and airmass formulae really should employ non-Standard atmospheric profiles, if they are to be useful in the part of the sky around 80° Z. D.

13.3. *Newer models*

Since the mid-1970s, NASA has produced and maintained a more detailed set of standard atmospheric models, described in various technical reports that are available from the website at http://www.sti.nasa.gov [165]. These provide finer-grained tables and formulae that specify atmospheric models for particular times and places.

A related approach to refraction near the horizon by considering climatological information has been used by Marcel Tschudin [166], who analyzed observations of refraction at low altitudes (including the observations Argelander made for Bessel), taking diurnal and seasonal cycles of boundary-layer structure into account.

14. Other atmospheric refractions

As atmospheric structure is only important near the horizon, it might be that refraction effects other than astronomical refraction could give more information. The idea that terrestrial refraction might help predict astronomical refraction was already discussed in the 1850s [76]. Decades later, both Hunter [167] and Livieratos [168] had the same idea, but in the other direction: to use astronomical refraction as a guide to terrestrial refraction. As the ray bending that displaces the apparent positions of terrestrial objects certainly is the same as the lower atmosphere's contribution to astronomical refraction, it is tempting to suppose that the terrestrial refraction is some calculable fraction of the astronomical refraction.

However, this overlooks two things. First, the terrestrial refraction is the apparent angular displacement of some relatively nearby object, all right; but it isn't equal to the bending of the ray by refraction. And second, the apparent angular displacement depends on the *distance* to the object. To see how astronomical and terrestrial refractions are connected, it's useful to consider a more general situation, in which the refracted object is only a finite distance from the observer. This actually occurs for objects such as meteors, rockets, artificial satellites, and even the Moon. Figure 6 shows the geometry for an observer at **A** who sees an object at **C**. (For simplicity, the figure uses Cassini's approximation that the refraction occurs at a single point, **B**.)

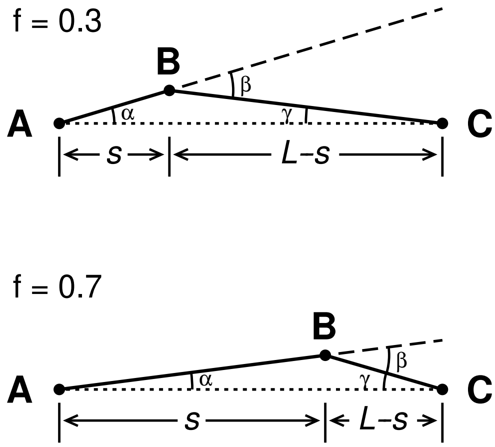


Figure 6. Geometry of a ray (the heavy line **ABC**) refracted by a small angle β at **B**, for two positions of this bend. The dashed line is the extension of **AB**. An observer at **A** would see **C** along the dotted line in the absence of refraction, but sees it refracted by an angle $\alpha < \beta$ due to the bending at **B**. **A** and **C** are separated by a distance L; the bend, **B**, is a fraction $f = s/L$ of this distance from **A**.

As the bending angle β is an exterior angle of the triangle **ABC**, it is the sum of the apparent refraction α and the parallactic angle γ ; so this case is often called *parallactic refraction* [169]. The size of the parallactic correction to β is proportional to f, the ratio of the distances of the refractive bend and the refracted object from the observer — i.e., s/L in the figure. In astronomical refraction, the refracted objects (stars) are infinitely far away, so the parallactic correction vanishes, and the refraction is just the total bending in the atmosphere, β. In terrestrial refraction, the object is usually at about the same height as the observer, so the refraction is uniformly distributed along the line of sight, and $f = 0.5$ — that is, half of the total bending is observed at each end of the line of sight. (This is why surveyors prefer to make simultaneous measurements reciprocally at both **A** and **C**.)

The parallactic correction to refraction is rarely mentioned in textbooks on positional astronomy. It is a small correction to the position of the Moon that must be taken into account in dealing with stellar occultations. On the other hand, it's a huge effect in geodesy and surveying; indeed, surveyors routinely assume that half of the bending is the apparent refraction of an observed position. However, when the line of sight is closer to the ground at one end than at the other, the refraction is usually not distributed evenly along it, and allowances should be made for the lack of symmetry.

A combination of astronomical and terrestrial refractions occurs in determining the rising and setting times of celestial objects. The position of the sea horizon varies several minutes of arc because of variations in dip; but the dip of the horizon is not quite like terrestrial refraction, because the apparent horizon is not a fixed object; its distance depends on refraction. If the apparent horizon is land, its terrestrial refraction must be allowed for. And the astronomical refraction of the celestial object is larger than these terrestrial effects. McCluskey's treatment of these effects [170, 171] is clearer than the more convoluted description in the *Explanatory Supplement* [122].

15. Conclusions

It is well established that astronomical refraction is practically independent of atmospheric structure at zenith distances less than 75 or even 80°, where it is almost completely specified by the refractivity at the observer and the height H of the homogeneous atmosphere. Oriani's theorem provides the mathematical explanation for this region. The physical explanation is that the atmosphere is so thin, compared to its radius of curvature, that the weighted average of $\tan z$ is very nearly its value at H, which differs only slightly from $\tan z_o$. In this region, the bending of the ray is too small to affect the run of z with H, and even Cassini's homogeneous model gives nearly correct refraction values.

Therefore, in this small-bending region that includes the zenith, observations of refraction give no useful information at all about atmospheric structure. Furthermore, Oriani's theorem underestimates the size of this region, because it is based on a divergent series whose alternating terms begin to be mutually cancelling near 80°. Thus the good behavior of the refraction continues even to zenith distances where the $\tan^5 z$ term begins to look significant.

And, just as refractions depend on a weighted mean of $\tan z$, airmass factors are a weighted mean of $\sec z$, which, like the tangent, diverges at arguments near 90°. So extinction measurements in the small-refraction region are tightly linked to refractions, and are similarly uninformative about the atmosphere.

For zenith distances between 75° and 85°, there is a weak dependence on the ray curvature; but this curvature is mostly in the lowest part of the atmosphere. This introduces a third parameter: the mean lapse rate (or density gradient) in the lowest kilometer or two.

Only in the last two or three degrees above the astronomical horizon is there a little sensitivity to boundary-layer structure. But this structure is so smoothed out in integrations over the whole atmosphere that the inversion problem is very ill-posed. It is well-determined only for rays that are horizontal over part of their path: that is, either below the astronomical horizon, or in superior mirages. Furthermore, atmospheric extinction and reddening, combined with dispersion, hinder measurements of stellar refraction in this zone even at night.

In the daytime, refraction is always rapidly changing due to changes in boundary-layer structure, and stars are not detectable near the horizon. Only the distortions of the rising or setting Sun show the real complexity of structure below eye level; so this part of the boundary layer can be studied with astronomical refraction at sunrise and sunset. However, terrestrial refraction can be inverted to give the structures of strong thermal inversions less than a few dozen meters above the observer, if sufficiently accurate altitudes are measured.

Conflicts of interest

The author has no conflict of interest to declare.

Acknowledgements

I thank Luc Dettwiller, Éric Frappa, Stephen McCluskey, Marcel Tschudin, and Siebren van der Werf for useful discussions, and for bringing many important references to my attention, and librarians at San Diego State University and several other institutions for providing access to them.

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