‡1 The Southern Limits of the Ancient Star Catalog and the *Commentary* of Hipparchos

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A Full speed ahead into the fog

A1 The Ancient Star Catalog (ASC) appears in books 7 and 8 of Claudius Ptolemy's classic work *Mathematike Syntaxis*, commonly known as the *Almagest*. For centuries, wellinformed astronomers have suspected that the catalog was plagiarized from an earlier star catalog[2] by the great 2nd century BC astronomer Hipparchos of Nicaea, who worked primarily on the island of Rhodes. In the 20th century, these suspicions were strongly confirmed by numerical analyses put forward by Robert R. Newton and Dennis Rawlins.

A2 On 15 January 2000, at the 195th meeting of the American Astronomical Society in Atlanta, Brad Schaefer (then at Yale, later Univ of Texas [later yet: Louisiana State University at Baton Rouge]) announced a result that was instantly hailed from the floor as "truly stunning" by *JHA* Associate Editor Owen Gingerich. Schaefer had shown, to a very high statistical likelihood, that the ASC was observed mostly by Ptolemy. According to Schaefer, the first three quadrants of right ascension in the southern sky were certainly observed by Ptolemy (Hipparchos being completely ruled out); while the fourth quadrant was probably Hipparchos', although Ptolemy could not be ruled out. Schaefer had arrived at this result by applying a complex and delicate statistical test to the southern limit of the ASC — those stars that transit the meridian just above the southern horizon. Since the southern limit of observability changes markedly with the observer's latitude, and since Ptolemy and Hipparchos were five degrees apart in latitude, this kind of test should be an easy way to determine authorship of the catalog.

A3 I flew to Atlanta specifically to hear Brad's lecture, because of my longstanding interest in the problem. My interest had been piqued by Schaefer's pre-posting of an abstract of his methods on HASTRO[3] in the weeks leading up to the meeting. After his lecture, I raised to him a question about Gamma Arae, one of the very southern stars in the Hipparchos *Commentary on Aratos and Eudoxos* that is also in the ASC; I pointed out that under Schaefer's proposed atmosphere, after applying the effects of atmospheric extinction, it would have a magnitude of 6.7 from Rhodes — making observation impossible for Hipparchos. In a private chat after the lecture, I warned Schaefer that that there was more than just one star that would give him problems in this regard. So when no paper on the subject was published in the months that followed, I figured that he had taken a good look at the *Commentary*, realized the obvious, and quietly let the whole thing drop.

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[2]The Hipparchan original is now lost, but is attested to have existed by Pliny — and also (by implication) attested by Ptolemy himself: *Almagest* 7.1 states "the fixed-star observations recorded by Hipparchus, which are our chief source for comparisons, have been handed down to us in a thoroughly satisfactory form." (Toomer 1998, 321.) The "thoroughly satisfactory form" referred to cannot be the *Commentary*, because Ptolemy immediately goes on to attest about 80 stars observed by Hipparchos, and 39 of them do not appear in the *Commentary*.

[3]The internet discussion group on the history of astronomy, HASTRO-L, can be subscribed by sending an email to listserv@WVNVM.WVNET.EDU and in the body of the message, put "subscribe HASTRO-L". The discussion archives can be found at
http://www.wvnet.edu/htbin/listarch?hastro-l&a:scmcc.archives.

A4 It turned out that my optimism was unjustified. In February 2001, the *Journal for the History of Astronomy* published Schaefer's paper on the southern limit of the ASC as its lead article, a Titanic piece of 42 pages,[4] bouyed by Unsinkable statistical results. And not once in those pages did Schaefer mention the Hipparchos *Commentary*, much less the very serious implications it holds for the southern limit of the ASC. But the *Commentary* has been waiting for 22 centuries, like an iceberg unseen in the foggy night.

B Why the Southern Limit is Firmly Hipparchan

B1 The problem here is both simple and unevadable. There is only one surviving complete work by Hipparchos, his *Commentary on Aratos and Eudoxos*, a work that mentions (often with partial positions) some 400 stars. And the southern limit of the Ancient Star Catalog is almost exactly the same as the southern limit of Hipparchos' *Commentary* (see figure 1). Therefore, both works must have been observed from the same latitude. And since we know without doubt[5] that the *Commentary* was observed by Hipparchos on Rhodes, the ASC must have been observed from there, too. Or, to put it another way: if the *Commentary* and the ASC were indeed observed from five degrees apart in latitude, then we should expect to see — glaringly in figure 1 — a five-degree gap in the southern limits of the two works. There is no five-degree gap; the actual gap is zero. Therefore, any procedure that eliminates Hipparchos as the observer of the ASC, based on a statistical analysis of the southern limit, will also eliminate Hipparchos as the observer of the *Commentary*, too: a known incorrect result.

B2 After his lecture, Schaefer posted my comment about Gamma Arae on HASTRO (2000-1-27), and refuted it by (a) wildly mis-computing its probability of observation at 1.5%, and (b) incorrectly speculating that "there are likely to be of order 50 stars along the southern horizon that have comparable probabilities," making it likely that at least one such was seen. In fact, using Schaefer's atmosphere and probability function (equation 1) for the first three quadrants of Right Ascension, Gamma Arae has a post-extinction magnitude $\mu = 6.7$, giving an observation probability of .0008, or about 20 times less likely[6] than the number he quoted; and if we take "comparable probabilities" to mean any probability between half and twice that of γ Arae, there are only 13 stars (in declinations below $-45°$ in the first three quadrants) that qualify for this 1-in-1400 chance. Thus γ Arae alone is sufficient to reject Schaefer's atmosphere and-or probability function for Hipparchos, at the 99% confidence level.

B3 But Gamma Arae is not the only star that gives us problems; similar problems can be found with most of the far southern stars in Hipparchos' *Commentary*. Using a cutoff declination of $-45°$ at epoch -140.0, and the identifications of K.Manitius' edition (‡2 fn 8), there are 13 stars mentioned in the *Commentary* that appear in this region of the sky.

[4]This must be some kind of record. *JHA* has now devoted 3 lead articles and over 100 pages during the past 15 years, largely attempting to refute just one-half of one paper, Rawlins 1982. (The latest try totally ignores the other half of that same paper.) If the conclusions of Rawlins 1982 were wrong, this might be justified; but since those conclusions are in fact completely correct, the honor of being lead author in *JHA* is becoming something of an embarrassment, at least when writing about Ptolemy.

[5] The latitude of the *Commentary* can be firmly fixed, because the work contains several hundred positions of stars described according to the degree of the ecliptic that rises or sets simultaneously with the rising or setting of the star. It is not disputed that these horizon-based phenomena rigidly tie the work to a particular horizon, i.e., a particular latitude — in this case 36° North, the latitude of Rhodes.

[6] Declination of γ Arae at *Commentary* epoch -140 is $-50°.494$; true altitude = $3°.106$, apparent altitude = $3°.331$, which (by Schaefer's atmosphere) gives 14.2 Rayleigh airmasses Z (at .1066 m/Z, =1.51 m); 10.2 ozone Z (at 0.031 m/Z, =.32 m) and 16.6 aerosol Z (at 0.0924 m/Z, = 1.53 m) for a total extinction of 1.51 + .32 + 1.53 = 3.36, which combines with γ Arae's pre-extinction M of 3.31 to yield post-extinction magnitude $\mu = 6.67$. (Schaefer's airmass formulae give slightly higher numbers than those of fn 39, which just makes matters worse.) Following eq 1 below, P = .000758.

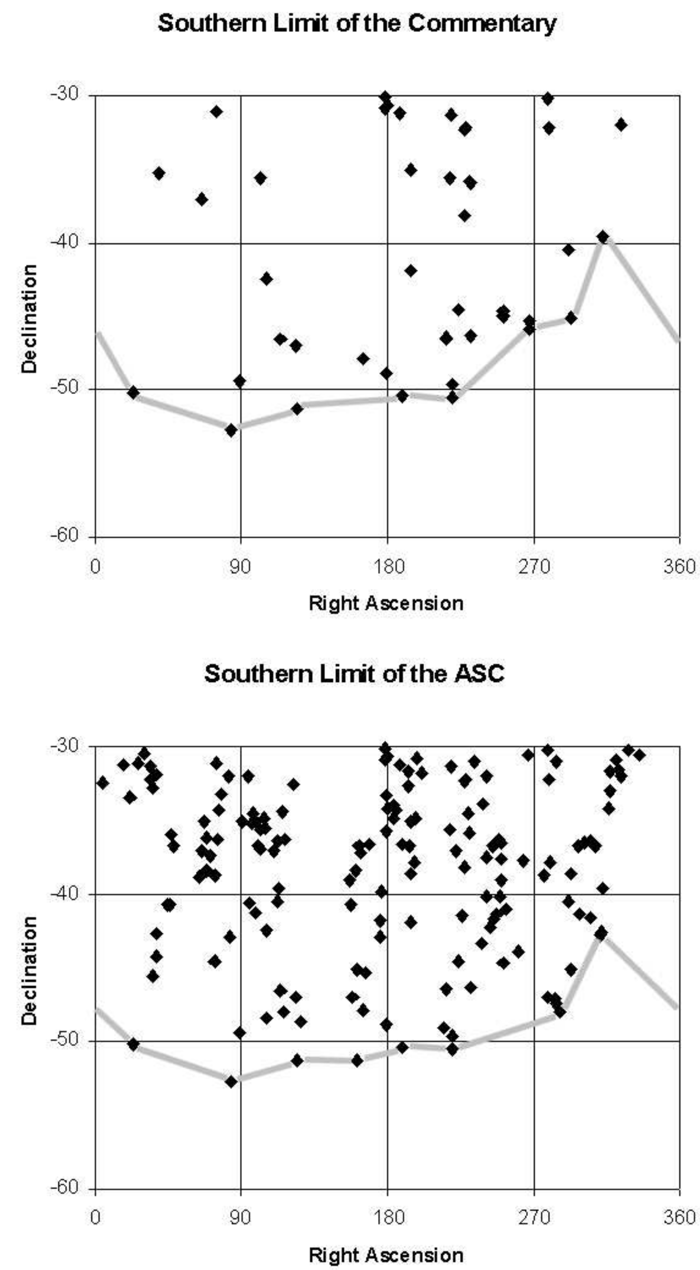

Figure 1: The southern limit of Hipparchos's *Commentary on Aratos and Eudoxos*, compared to the southern limit of the Ancient Star Catalog in Ptolemy's *Almagest*. Positions of cataloged stars are for epoch -140.

All 13 of these stars are in the first three quadrants[7] of right ascension at this epoch (the same area of the sky Schaefer finds was observed by Ptolemy in the ASC). Therefore, we will confine this analysis to just the first three quadrants of RA from now on.

B4 Ideally, we would now determine Hipparchos's personal equation or P function: that is, the probability of him observing any star based on its magnitude (after applying atmospheric extinction). However, this function is difficult to determine without a complete catalog. If a star does not appear in the *Commentary*, there are two possible reasons: (A) it may not have been visible to Hipparchos; or, (B) it was visible, but he left it out because it was not important to him at the time. Here we will make the highly conservative assumption (A) in all cases.

B5 Using an equation in the form used by Schaefer 2001, we start by determining a probability function based entirely on the *Commentary* (without reference to the ASC). For an observer at -140.0 and latitude $36°$, using all stars within the first three quadrants between declination $0°$ and $-30°$, the probability of observation for the *Commentary* is:

$$Pc = 1/(1 + e^{1.72(\mu-3.23)})$$

where μ is the post-extinction magnitude of the star. If we combine this equation with Schaefer's assumed atmospheric extinction ($k = .23$ magnitudes per airmass), and run a repeated-trial least squares test to determine the author of the *Commentary* (in the same manner that Schaefer 2001 determines the author of the ASC), we find that the first three quadrants of the *Commentary* were observed from latitude $30°$ North in 600 AD, and Hipparchos can be eliminated as the author of his own work at a 99% confidence level. Which quickly gives us an idea of how badly things can go wrong here.

B6 Clearly there is a problem, and the P function seems a likely culprit. We need a P function for Hipparchos that will increase the computed probability of observation. Such a function more nearly matches what he would have gotten had he observed an entire catalog. As a first approximation, let us use Schaefer's derived P function for the first three quadrants of the ASC (which might be Hipparchan in any case, given the overwhelming evidence that Hipparchos was in fact the ASC's observer). This equation is:

$$P = 1/(1 + e^{3.3(\mu-4.49)}) \quad (1)$$

In this equation, the constant 4.49 (called *mlim*) is the midpoint of the function (that is, the magnitude at which the probability of observation is 50%); and the constant 3.3 (called F) is a measure of the function's steepness.

B7 As a quick test of this function, we can find the probabilities of observation for these 13 stars, using Schaefer's atmosphere and P function for the first three quadrants. For epoch -140 at latitude $36°.4$ these stars and their probabilities are: β Cen, .9907; α Cen, .9899; β Cru, .9787; κ Vel, .6114; θ Ara, .0536; o Vel, .0520; τ Pup, .0248; α Car, .0213; β Ara, .0190; ϵ Ara, .0142; θ Eri, .0063; ι Car, .0023; and γ Ara, .0007. The probability that Hipparchos saw all 13 of these stars under these conditions is the product of the probabilities, or 2.6×10^{-18}, which is rejection at about the 9-sigma confidence level. In other words, the same assumptions that rejected Hipparchos as the observer of the ASC's first three quadrants at the 7-sigma confidence level (in Schaefer 2001) can also be used to reject Hipparchos as the observer of the *Commentary* at about the same level. And since we know that this result is false for the *Commentary*, we have good reason to believe that at least one of the underlying assumptions must be wrong.

[7] This fits with Schaefer's observation that there are differences in the southern sky between the first three quadrants and the fourth, although it also shows that these differences go back in time at least to Hipparchos. However, there is a much simpler explanation for these differences than the one Schaefer proposes: see §C8.

B8 Of course, the kind of probability-multiplication in §B7 is very simplistic, since it ignores other stars not observed, i.e., it does not account for the number of chances Hipparchos would have had for these observations. We can make our result formal by using a standard χ^2 test. Using equation 1 and Schaefer's atmosphere, we can derive a theoretical distribution of the numbers of stars that Hipparchos should have observed in this region[8] of the sky, and compare our theoretical distribution to the actual distribution of observed stars in the *Commentary*, and run a χ^2 test. Since our P function was derived from the ASC, which has 1025 stars, and we are applying it to the *Commentary*, which has 400, we would expect that the function will overstate the number of stars in the *Commentary* at every point in the sky. But that is not what actually happens when we look at the southern limit:

μ range	N stars	N found	Predicted N	χ^2
0 - 4.49	8	5	6.70	0.431
4.5 - 4.99	2	1	0.70	0.129
5 - 5.49	12	5	0.76	23.636
5.5 - 5.99	15	4	0.23	61.099
6 - 6.49	19	1	0.07	11.767
6.5 - 6.99	48	0	0.02	0.024
Totals	104	16	8.49	97.085

B9 The important columns to compare are N found (number of stars in the *Commentary*) against "Predicted N found" (number of stars that should be in the *Commentary*, under the assumed atmosphere and P function). As you can see, the theoretical distribution does not much look like the actual distribution; the *Commentary* contains far too many dim stars to have been observed through this atmosphere. Further, even though we would expect the P function to predict more stars than are actually in the *Commentary*, the function actually predicts *less*. Not only is χ^2 out of whack, it is out of whack in the wrong direction. In this case, the χ^2 value is a huge 97, which implies that Schaefer's atmosphere and/or P function can be ruled out for Hipparchos at a very high confidence level.[9]

B10 For any theoretical analysis such as Rawlins 1982 or Schaefer 2001 to be believable, the *Commentary* stars **must** be visible to Hipparchos, because we know that Hipparchos observed these stars. There are three ways we can do this: we can alter the atmosphere; we can alter the P function; or we can move the latitude of Hipparchos. This last strategy (moving Hipparchos) does not work, because of the constraint discussed above (fn 5). Therefore, we must either clean up the atmosphere or alter the P function; and in fact, we will need to to both. But making the *Commentary* visible to Hipparchos has obvious implications for the ASC as well.

B11 As any observer knows, the clarity of the atmosphere differs widely from night to night. This is caused primarily by the large variability in the amount of suspended aerosol[10] in the atmosphere (such as dust and air pollution). So we can improve the fit between the actual and theoretical distributions by lowering the amount of aerosol in the theoretical atmosphere until the χ^2 value minimizes. Again using equation 1, the χ^2 value for *Commentary* stars minimizes at null aerosol:

[8] Here I take the southernmost 10 degrees of the sky, between altitudes $0°$ (apparent) and $10°$ (true), assuming latitude $36°.0$ for Hipparchos. The same 10-degree swath is also used for Tycho (from Wandsbeck, §D10) and for Hevelius (from Gdansk, §D11).

[9] For 5 degrees of freedom, a χ^2 of 97 implies a probability $P = 2 \times 10^{-19}$. Substituting the *Commentary*-derived equation B5 for Schaefer's equation 1 just makes matters worse; the χ^2 becomes 157.

[10] There are three components of atmospheric extinction: Rayleigh scattering by gas molecules in the atmosphere; Mie scattering from suspended aerosols; and molecular absorbtion in the visible band from the stratospheric ozone layer. Computational procedures used in this paper can be found in fn 39.

μ range	N stars	N found	Predicted N	χ^2
0 - 4.49	17	10	13.95	1.119
4.5 - 4.99	12	4	3.34	0.131
5 - 5.49	24	2	1.99	0.000
5.5 - 5.99	28	0	0.44	0.436
6 - 6.49	54	0	0.17	0.172
6.5 - 6.99	113	0	0.07	0.066
Totals	248	16	19.95	1.925

The two distributions are now much closer, and give a χ^2 value of 1.9 at a total extinction coefficent $k = .136$ magnitudes per airmass. But this value is so low — almost no aerosol — that some might suspect that more than just the atmosphere is wrong here.

B12 Alternatively, we can minimize the χ^2 by leaving the atmosphere alone and altering the P function. Here the critical parameter for visibility is *mlim*, the post-extinction magnitude at which probability of visibiliy is 50%. The simplest procedure is to hold the F parameter (function steepness) constant, while allowing *mlim* to vary; this yields an *mlim* of 5.34 ($\chi^2 = 2.29$), nearly a magnitude deeper than equation 1. But if this P function is valid for Hipparchos, that too would invalidate the results of Schaefer 2001.

C The probability of cataloging stars

C1 There are three important differences in the analyses of Rawlins 1982 and Schaefer 2001 that are responsible for their differing conclusions. These are: the probability function, atmospheric extinction, and the selection of stars. All three are important to the method used by both Rawlins and Schaefer in determining the southern limit.

C2 Briefly, the method is this: start by assuming a given atmosphere, P function, latitude, and epoch for the observer. Then, for each star in the southern sky, find its post-extinction magnitude μ and use the P function to determine its probability of being seen. If the star is not actually observed (i.e., if it's not in the ASC), add this probability to a running total; but if it is in the catalog, subtract its probability from 1, and add the difference to the running total. After checking every star in the sky, you have a final total of probabilities for this set of assumptions, which Schaefer calls Q. Then you repeat the whole process using a different latitude and/or epoch. There will be one latitude and epoch for which the final total Q is at a minimum, and that is the indicated latitude and epoch of the catalog's observer.

C3 Schaefer begins by identifying the modern stars that appear in the ASC, by reference to Peters & Knobel (1915) and Toomer (1998). For the three stars not identified by P&K, Schafer (without saying so) follows Graßhoff 1990 in adopting 78 Cet for PK716, 73 Cet for PK717, and HR859 for PK788. But this last has a pre-extinction magnitude of 6.34, making the identification unbelievable. For better identifications of these and other questionable stars, see ‡5 later in this issue.

C4 Our first problem is to derive the P function. When fitting his function, Schaefer used the stars' raw (un-extincted) visual magnitudes V (Schaefer 2001, 15; V defined on p. 6). This is a colossal blunder which invalidates everything that follows, because the statistic Q is computed using the stars' extincted magnitudes. In other words, Schaefer starts with the implicit assumption that a star of $V = 4.49$ has a 50% probability of observation; but when he actually computes the probability of that very same 4.49 star, he adds (for an average star viewed at an altitude of 30°, through 1.5 atmospheres) .35 magnitudes of extinction to get $\mu = 4.84$, meaning that the computed probability for that allegedly 50% star becomes only 25%. In the same manner, a star with an allegedly 25% probability drops to 10% under the same conditions. Even if the star transits at the zenith, the derived probability of an allegedly 50% star becomes only 32%. In other words, Schaefer's no-extinction front-end presumption violates the statistical requirements of his back-end computational scheme.

C5 Equally inexplicable is the way Schaefer derives this function. To begin with, he derives the two key function parameters (F, function steepness; and *mlim*, function midpoint) separately. To find *mlim*, Schaefer puts all stars into magnitude bins, and then finds a χ^2 for a given magnitude distribution; then he minimizes χ^2 to get *mlim*. The F parameter is determined as a free parameter in his final probability computation. This might work in principle, but it is much simpler to derive both F and *mlim* beforehand, using a simultaneous least-squares fit. Not only does this avoid the very real problem of inadvertantly induced bias from the choice of bins, but it also reduces by 1 the number of free parameters that must be varied (and determined) in the final compuatation.

C6 But this straightforward procedure does not give the same bimodality in F and *mlim* which is one of Schaefer's primary results. This raises the question of whether the bimodality is real, or merely a computational artifact. What we see instead is more of a bipolar distribution with the deepest values in quadrant 4 (matching Schaefer's finding) and the shallowest opposite in quadrant 2, with quadrants 1 and 3 intermediate between these.

C7 If we were trying to replicate Schaefer, we would ignore extinction in this step, which gives the results below, using all stars between declinations $-10°$ to $-30°$ at Ptolemy's epoch. (This is Schaefer's sample, with the derived parameters given first, and the slightly different results based on the star identifications of ‡5 in parentheses):

1st quadrant	*mlim*=4.71 ± .03	F=5.05 ± .70	(4.72, 4.75)
2nd quadrant	*mlim*=4.38 ± .04	F=3.80 ± .49	(4.38, 3.80)
3rd quadrant	*mlim*=4.65 ± .03	F=3.87 ± .46	(4.66, 3.76)
4th quadrant	*mlim*=5.23 ± .03	F=4.93 ± .69	(5.23, 5.39)

These *mlim* values are already about .2 magnitudes deeper with this method than with Schaefer's (they would be deeper still using extinction); and although a few changes in star identification have little effect on *mlim*, they can have a large effect on F, as the function can shallow out markedly while trying to make some allegedly-seen ultra dim star appear at least somewhat more reasonable.

C8 The reason for differing values among the four quadrants certainly has more to do with the position of the galactic plane than Schaefer's explanation of multiple observers. The star-poor region around the South Galactic Pole is in the fourth quadrant, while the galactic plane cuts right through the heart of the southern sky in the second quadrant. We can confirm this by using a least-squares test to derive the values for *mlim* at varying galactic latitudes. Here I do so for the absolute value of galactic latitude, for stars at epoch -140 with declinations $> -30°$:

Absolute Galactic Latitude	*mlim*
00°-15°	4.39
15°-30°	4.63
30°-45°	4.76
45°-90°	4.75

It is clear that *mlim* values are substantially lower in the Milky Way (at galactic latitudes <30°). So it seems that the cataloger simply missed more stars in areas where stars are dense, and has taken more in areas where stars are sparse. In fact, the cataloger tells us explicitly on two occasions that he is engaging in exactly this practice: for both the Pleiades and the Coma cluster, the cataloger notes the positions only of the edges of the clusters, leaving their starry central masses to our imagination.

C9 Based on his finding of bimodality, Schaefer finds 12 constellations (out of 48) that show a "Hipparchan"-style deep limiting magnitude. It is no coincidence that 10 of the 12 (83%) are located in that half of the sky more than 30 degrees from the galactic equator.

C10 The implication: since the cataloger looks deeper where stars are sparse, then the far southern horizon should always be considered sparse, because of the effects of atmospheric extinction. This idea — that Hipparchos looks more deeply in the far southern sky — is also suggested by an interesting observation: while the *Commentary* as a whole mentions about 400 stars, or 40% of the ASC total, it contains 8 out of the 10 southernmost ASC stars, double the usual ratio. Therefore, when running our statistical test for the southern limit, we are fully justified using a P function that is based on a star-poor region[11] of the sky. (Rawlins 1982 did exactly this.)

C11 It is also logical that the cataloger would look deeper in areas of the sky that he finds useful, specifically the ecliptic. Using the same sample of stars as above, I find:

Absolute Ecliptic Latitude	*mlim*
00°-15°	4.71
15°-30°	4.65
30°-45°	4.46
45°-90°	4.42

Here the deepest part is closest to the ecliptic, as expected. Therefore, the very deepest part of the sky should be those areas of the ecliptic that are far from the galactic equator, and choosing those areas for our sample should give us the deepest possible limiting magnitude, which we need for Hipparchos to see the *Commentary* stars.

C12 We must also consider the very important question of whether a function of the form chosen by Schaefer accurately represents the true probabilities of a star being cataloged. Schaefer's function is inversely symmetrical around the 50% magnitude, by which I mean that for a typical *mlim* value of 5, the chances of cataloging a magnitude 7 star would be exactly the same as the chances of missing a magnitude 3 star. But this cannot be true, since there is one magnitude 3 star missing from the ASC (η Tauri) out of about two hundred possibilites, while there is not a single magnitude 7 star in the catalog out of several thousand possibilities. Therefore, the real probability function is slightly assymetric from the 50% magnitude, and we can account for this by adding an exponent to the P function. We will find this exponent as an additional independent variable in our least squares fit.

C13 To recap, in order to derive a correct P function for Hipparchos, we will: 1) use post-extinction magnitudes; 2) determine F, *mlim*, and the exponent simultaneously using a 3-dimensional least-squares fit; and 3) sample only areas of the sky far from the galactic equator and near the ecliptic. Our sample will be all stars more than 60° from the galactic equator and less than 30° from the ecliptic, above declination $-30°$, for latitude 36° at epoch -140. This sample comprises 355 stars of $V < 6.9$, of which 58 are in the ASC.[12] Using this sample and an extinction coefficient of .182 (justification for this in fn 18), I find:

$$P = 1/(1 + e^{1.69(\mu-6.53)})^{10} \tag{2}$$

It turns out that the exponent is only weakly recovered because there is a large family of nearly identical curves for which the sum of squares is not much different. The absolute least-squares value occurs at very high exponents,[13] but the uncertainty in the exponent also grows unacceptably large at these values. Holding the exponent to 10 results in an easier computation with little change in the fitness of the function. Equation 2 yields a 50% probability of observation at $\mu = 4.97$, nearly the value we would have gotten without the exponent ($\mu = 4.93$).

[11] This will also allow us to use a slightly thicker atmosphere than otherwise.

[12] Of the 355, twenty-nine are in the *Commentary*. But as we have seen above in fn 9, our task of making the *Commentary* visible will be much easier if we use a P function derived from the ASC. Some may object that, if Ptolemy observed the ASC, then we would expect his P function to differ from that of Hipparchos. But since the overwhelming weight of evidence (e.g. figure 1) points to Hipparchos as the ASC's observer, we are entitled to provisionally assume this as being true. If, as Schaefer asserts, the debate on the astrometric evidence has come to a standstill, it is only because Ptolemy's defenders have completely surrendered on that front. The resoundingly conclusive astrometric evidences of Newton 1977, Rawlins 1982, Graßhoff 1990, Rawlins 1994, and now Duke 2002 have inspired no worthwhile counter-arguments from any Ptolemy apologist, significantly including Schaefer himself.

D Atmospheric extinction

D1 Rawlins 1982 supposed a very clear atmosphere on the best nights of observing ($k = .15$ magnitudes per airmass) while Schaefer puts the atmosphere considerably more opaque on the best nights ($k = .23$ magnitudes per airmass). This is important, since southern stars are all observed at low altitudes (that is, through a lot of air and dust), and small changes in opacity have large effects on visibility when observing near the horizon. A more opaque atmosphere moves the catalog's observer southward (putting the southern stars higher in the sky to compensate for atmospheric extinction; see §H5.)

D2 But it is easy to show that Hipparchos (and other pre-industrial astronomers) observed through a very clear atmosphere. By using data in the *Commentary*, we can derive the clarity of Hipparchos' atmosphere by examining the southern limit of this work, whose latitude (36° North) and epoch (ca. 140 BC) are known; and we can apply the same procedures to the naked-eye catalogs of Tycho Brahe (1601) and Johannes Hevelius (1660). We will employ two independent methods to do this.

D3 Our first method will apply atmospheric extinction to the stars in the *Commentary*, and check for differences in brightness based on their position in the sky. Suppose, for example, that we assume a too-thick atmosphere. In that case, the cataloged stars in the very southern part of the sky will be extincted far too much; they will have post-extinction magnitudes greater, on average, than than those farther north. But it is silly to expect that an astronomer would see very dim stars only near the horizon, while ignoring them elsewhere. There should be no difference in post-extinction magnitudes based on location[14] in the sky, and we can adjust the aerosol fraction until this condition is met. This will give us the aerosol fraction (and therefore, the extinction coefficient) under which the catalog was observed.

D4 We test for this condition by computing the correlation coefficient r between μ, the post-extinction magnitude, and X_a, the number of aerosol airmasses through which a star is viewed (see fn 39). For a given star, the number of airmasses does not change with the aerosol extinction coefficient (which is a function solely of the star's altitude), but μ does, quite significantly for low stars. So X_a is a good way to weight each star's datum by the amount of aerosol information it contains. If stars are randomly distributed in the sky, the correlation r between these variables should be zero, indicating no relationship between post-extinction magnitude and position in the sky. In practice, however, there may be slight biases at various latitudes and epochs because the actual stars at southern limit for a given observer may be distributed non-randomly in magnitude. For example, Canopus is the second-brightest star in the sky, and it is right at the southern limit for Hipparchos. Its presence in the *Commentary* (and in the ASC) drives the expected value of r slightly negative, because it is viewed through very many airmasses. I tested for this effect by creating 100 pseudo-catalogs of stars for the latitudes and epochs of several naked-eye astronomers,[15] at various values of aerosol extintion k_a, and deriving the mean correlation coefficient for each k_a. The result (fig 2) shows that the expected value of r is indeed close

[13] This formally confirms that the P function is inversely assymetrical, since only an exponent of 1 results in a symmetric function.

[14] This test was first suggested in Rawlins 1993 (*DIO* 3 §L10).

[15] I confined these catalogs to apparent altitudes < 30 degrees, because seelction of stars higher than that is not due to atmospheric effects.

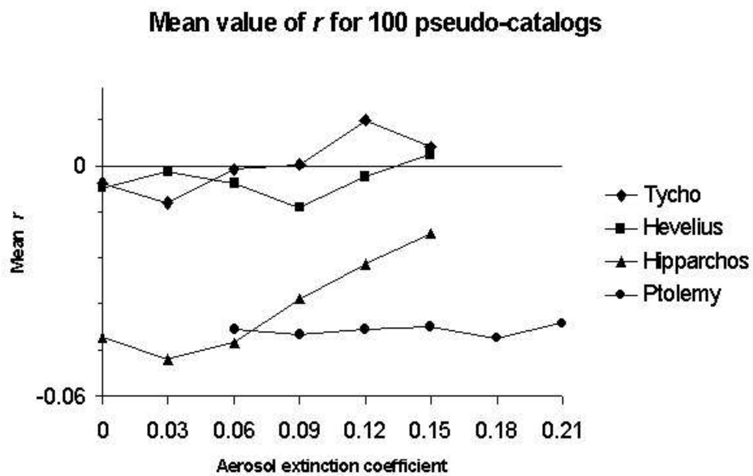

Figure 2: Expected correlation between X_a and V under various assumptions of extinction, for various observers.

to zero for Tycho and Hevelius, and slightly negative for Hipparchos and Ptolemy.

D5 For the *Commentary*, the correlation between airmass Z and mu matched the expected value of $-.045$ at an aerosol extinction coefficient of $k_{\rm a} = 0.012 \pm 0.01$, which implies a total extinction coefficient of .145 magnitudes per airmass. I repeated the same test with other naked-eye catalogs, and with the ASC[16] under two different assumptions of authorship; the results are in Table 1. All the pre-industrial catalogs imply a consistently low $k_{\rm a}$, except for the ASC under the Ptolemaic assumption. Note particularly that the ASC under the Hipparchan assumption gives a similar result to that we obtained from the *Commentary*, while under the Ptolemaic assumption the ASC has an aerosol fraction many times larger[17] than the *Commentary*.

Observer, catalog	$k_{\rm a}$	k
Hipparchos *Commentary*	0.012	0.145
Tycho catalog	0.029	0.161
Hevelius catalog	0.026	0.159
ASC, Hipparchos assumption	0.027	0.160
ASC, Ptolemy assumption	0.212	0.345

Table 1: Derived values of aerosol and total extinction $k_{\rm a}$ and k, using method 1.

[16]For Tycho, I assumed the latitude of his observatory at Hven, and eliminated four Centaurus stars likely observed from Wandsbeck (see *DIO* 2.1 ‡4 §G2, also *DIO* 3 §M5 (D1001-1004) and fnn 95&156.)

[17]Even if we confine ourselves to observations outside of the ASC, there is evidence that Ptolemy, too, observed under virtually aerosol-free skies: see Pickering 2002 below at ‡5 fn 8.

D6 Our second method for determining the extinction coefficient of the *Commentary* will use the P function we obtained above[18] in §C13.

D7 We proceed as before in §B3, this time using equation 2, (with the same sample of *Commentary* stars) and varying the aerosol component of the atmosphere to minimize the χ^2 value. The minimum value occurs at $k = .182$ magnitudes per airmass:

μ range	N stars	N found	Predicted N	χ^2
0 - 4.49	10	6	9.38	1.217
4.5 - 4.99	8	4	4.44	0.043
5 - 5.49	18	5	5.71	0.088
5.5 - 5.99	18	1	1.62	0.236
6 - 6.49	32	0	0.24	0.242
6.5 - 6.99	67	0	0.02	0.021
Totals	153	16	21.41	1.847

The probability associated with this $\chi^2 = 1.8$ is 87%, far out of the rejection region. Keeping this best-fit atmosphere ($k = .182$) but substituting equation 1 gives a χ^2 of 22.7, allowing us to reject equation 1 for Hipparchos at the 3-sigma confidence level. On the other hand, the best-fit P function (equation 2) combined with Schaefer's atmosphere (k=.23) gives a χ^2 of 97, which allows us to reject Schaefer's atmosphere for Hipparchos at a huge confidence level ($P = 2 \times 10^{-19}$). So only a combination of very clear atmosphere and deep P function will allow Hipparchos to observe the *Commentary*,as we know he did.

D8 Recall that equation 2 (like equation 1) was derived from the ASC, with 1025 stars, and is being applied to the *Commentary* with 400. When χ^2 minimizes, the predicted and actual distributions are close; but we should really expect that the number of stars predicted by the ASC-derived function to be much greater than the actual number seen in the *Commentary*. In other words, the severely conservative assumption A (see §B4) implies that the derived extinction coefficient of $k = .182$ is an upper limit only; so from this we can reject the $k = .23$ of Schaefer 2001, but we cannot reject the $k = .15$ of Rawlins 1982.

D9 In addition, we note that a very clear atmosphere was not uncommon in the pre-industrial era. For example, from his home in Knidos ($36^\circ\ 40'$ North), Eudoxos (ca. 360 BC) observed Canopus, which at that time and place was transiting the meridian at an altitude of less than 1 degree. Under Schaefer's atmosphere, the star would have had a post-extinction magnitude of 7, making observation impossible. Under a $k = .18$ atmosphere ($k_{\rm a} = .04$), its magnitude would have been a reasonable 4.7.

D10 When we look at the southernmost stars in Tycho Brahe's naked eye catalog,[19] we find that under Schaefer's atmosphere, a number of them would lie beyond the conventional 6-magnitude naked eye limit, even assuming that they were seen from his southernmost observatory at Wandsbeck. When I repeat the above χ^2 test for the southernmost stars in Tycho's catalog, I find that he was observing through an even clearer atmosphere than

[18] There is a possible objection at this stage for circular reasoning from using this function. Recall that we needed a value of $k = .182$ to determine the P function, yet now we will use the P function to determine k. This situation is easily solved by an iterative process, similar to solving M in Kepler's equation: we start by assuming some value of k (I started with $k = .2$, but any value will do) to determine P function. Then, we use the P function to derive the extinction coefficient k from our χ^2 test; then use the derived k to re-derive the P function, and so on. In practice, it only takes about two or three iterations until the value for k converges.

[19]Rawlins (1993) *DIO* 3. The epoch of the catalog as published is 1601, but I have used 1590.0 as the epoch of observation for computational purposes. I have also conservatively assumed that Tycho observed the entire catalog from Wandsbeck (latitude 53.567 N), even though probably only a handful of stars were taken from there, rather than from his observatory at Hven (56.907 N). Using the northerly Hven location would have made Tycho's atmosphere even clearer than the result presented here.

Hipparchos ($k = .158$). Oddly, in Schaefer's analysis of Tycho's catalog, he omitted the southernmost star in that catalog (2 Cen). He may have had good reason for doing so,[20] but it seems odd that he failed even to note the *fact* of this omission. Because 2 Cen is so dim (V=4.19), it is likely that including even this single critical datum would have thrown his derived latitude for Tycho into a cocked hat.

D11 A comparably clear atmosphere is found when we examine the naked eye catalog of Johannes Hevelius (Baily 1843). Although the epoch of his catalog is 1660, after the invention of the telescope, Hevelius preferred the naked eye for astrometric work, and published a complete catalog on the basis of his naked eye observations alone. The same χ^2 test for the southernmost stars on Hevelius's catalog gives a total extinction coefficient $k = .170$, similar to other pre-industrial observers.

D12 For the record, Tycho's southernmost star (2 Cen, $V = 4.19$) transits at $2^\circ.0$ apparent; Hevelius' southernmost star (ϵ Sgr, $V = 1.81$) transits at $1^\circ.5$; Hipparchos' southernmost star, in both the *Commentary* and ASC (Canopus, $V = -0.72$) transits at $1^\circ.3$; and Eudoxos' southernmost star (Canopus) transits at $0^\circ.9$. Compare these values with the southernmost star in the ASC under the Ptolemaic theory (Acrux,[21] $V = 1.28$): $6^\circ.1$. Thus Schaefer's claim that a Hipparchan observation of Canopus would be "unreasonable in light of Tycho's limit" (Schaefer 2001, 28) is doubly ridiculous: first because we know that Hipparchos did in fact see Canopus — it's right there in the *Commentary*; and second, given the dimness of 2 Cen, Tycho's extinction limit is actually *lower* than Hipparchos would need to see Canopus. It would be far more accurate to say that Ptolemy's missed observation of (to take just the most obvious example) ϵ Car ($V = 1.86$) at altitude $4^\circ.6$ is unreasonable in light of Tycho's limit, and in light of Hevelius's limit: neither of those astronomers missed a star that bright anywhere in the sky, including the bottom 5 degrees.

D13 The dimmest low star in the catalog of Hevelius is υ_1 Eridani, with a pre-extinction magnitude of 4.51; with an extinction coefficient of .23, it would appear at a magnitude of 6.8, making observation impossible even for the legendary visual acuity of the Gdansk brewer. In order for this star to be within the standard 6 magnitude post-extinction limit, the extinction coefficient for Hevelius, on one night at least, must have been $k \leq .153$.

E How clear can it get?

E1 Schaefer 2001 characterizes as "ludicrous" (p. 21) and "absurd" (p. 2) any claim that an extinction coefficient as low as .15 could occur at a sea-level site. Well, Barrow, Alaska is at sea level, and NOAA has been collecting aerosol data there since 1977. The ten-year average aerosol optical depth (AOD — see fn 22) for Barrow in June is about .0022; which gives an aerosol extinction coefficient of .0024, and a total extinction coefficient[22] of $k < .14$ — and that's not the best nights, that's an average for the whole month. July, August and September are nearly as good, and all easily absurd in Schaefer's estimation.

[20] In a phone conversation, Schaefer cited Rawlins 1992 (*DIO* 2.1) as justification, claiming the star was faked. But it seems clear [*ibid* §C7] that only one of the two coordinates was faked, and the other was actually observed [presumably at Wandsbek: see *ibid* §C8].

[21] Or, under the identifications proposed below at ‡5§C, λ Cen transits at $5^\circ.7$.

[22] For summary data, see John A. Ogren, "Enhanced Aerosol Measurements at NOAA's Baseline Observatory at Barrow, Alaska" at http://www.cmdl.noaa.gov/aero/pubs/abs/ogren/ARM97_BRW/ARM97_brw_abstract.html; or do your own processing on the raw data at http://wwwsrv.cmdl.noaa.gov/info/ftpdata. Ogren's figure shows total scattering (sigma sp) < 1 Mm^{-1}, and total absorbtion (sigma ap) < 0.1 Mm^{-1}, for a total aerosol extinction of < 1.1 Mm^{-1}; normalizing to a 2 km scale height gives Aerosol Optical Depth = .0022; dividing by the constant .921 converts AOD into the astronomical extinction coefficient for aerosol, k_a. To get total extinction, add in the components for Rayleigh scattering of .102 (Frölich & Shaw 1980) and ozone absorbtion of .03, yielding $k = .134$ magnitudes per airmass.

E2 And Barrow is not alone; in fact, it's not even the best site I could find. The same NOAA program has also been collecting AOD data at Samoa for the same period, and the AODs there are even smaller. After removing the anomalous data for the months following the El Chichon and Pinatubo eruptions, Samoa's *year-round average* AOD is .01, and about half of all months have a mean AOD of *zero*.

E3 How do Barrow and Samoa get such clear skies at sea level? The most likely answer: they are very far away from sources of industrial pollution (including agriculture, a significant contributor of dust). By contrast, the eastern Mediterranean, from which Schaefer takes his data, is downwind from western Europe, one of the world's most extensive sources of air pollution. Figure 3 is the NOAA satellite AOD composite for two weeks in 2001: February 8-15 and May 2-10. Because of the way the data are gathered, aerosol optical depth can only be recovered over oceans; land areas are black, and ocean regions of very low aerosols (AOD $< .033$) are gray. I took these weekly images at random, but note that it's not that unusual for ocean areas remote from industrial pollutants to go a whole week under "ludicrous" average conditions. In this image, gray accounts for 2.5% of the oceanic area in February, and 6.7% of the oceanic area in May.[23]

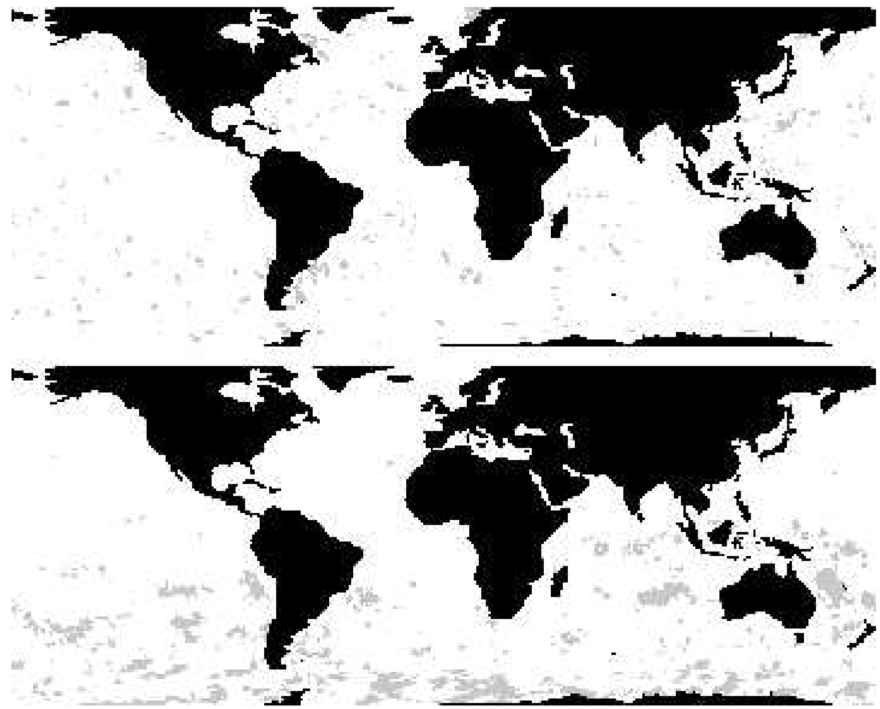

Figure 3: Oceanic surface areas with weekly average aerosol optical depth $\leq .033$ (gray), for the weeks of February 8-15, 2001 (top) and May 2-10, 2001 (bottom).

[23] Figure 3 data from http://psbsgi1.nesdis.noaa.gov:8080/PSB/EPS/Aerosol/Aerosol.html, and then click on the link for the latest "Aerosol Optical Depth Weekly Composite Color Image." The image changes every week. In the original images, AOD is shown on a color scale; I have processed the images for publication here.

E4 Another aspect of the problem that has been insufficiently considered (not just by Schaefer, but by any astronomer) is the way the atmosphere changes between day and night. Nearly all of Schaefer's observations of aerosol were taken by atmospheric scientists. There are a number of standard methods to do this in the atmospheric community, such as pyroheliometer and solar photometer, which depend on sunlight to work. This means that nearly all of Schaefer's measurements were taken during the daytime. But the nighttime atmosphere is different from the daytime atmosphere, especially when it comes to aerosols. In the daytime, the sun warms the surface of the earth, and this causes formation of a turbulent convection layer that reaches to a height of typically .2 to 5 km. In this diurnal boundary layer, aerosols from the surface are trapped and thoroughly mixed. But just before sunset, the energy balance on the earth's surface reverses, as the incoming solar energy is no longer greater than the energy loss from surface radiation. This shuts off convection and causes the diurnal boundary layer to quickly collapse; it is replaced by a nocturnal boundary layer of only about 20 to 500 meters in thickness.[24] A residual diurnal layer is still present for several hours after sunset, and can still be detected. It is as yet an open question how much the collapse of the diurnal layer to a tenth of its former height also causes the aerosols in that layer to drop to the ground; I know of no studies that have investigated the differing optical properties of either the residual boundary layer or the nocturnal boundary layer. But it is possible, perhaps even likely, that one may not have to get particularly high above the surrounding terrain to get low-aerosol seeing at night: just a couple of hundred meters might do.[25] This means that on a cliff above the ocean, one might get such a view on almost any night. There are a number of such cliffs on Rhodes, including Cape Prasonessi, from which there is evidence Hipparchos did in fact observe far southern stars.[26] There is also the possibility that Hipparchos may have observed from Mount Attabyrion, the highest point on the island; at 1215 meters, it is higher than any of the Mediterranean locations Schaefer cites. In ancient times there was a shrine to Zeus on the top of the mountain, that was still[27] frequented by visitors as late as the 1st century BC. The modern observatory at Siding Spring, Australia, at a slightly lower altitude, has recorded an average $k = .160$, with the best nights even clearer.[28]

F Ancient Data

F1 So Schaefer's 27,294 measurements, all taken in the 20th century (62% in urban areas, and apparently 99% during daytime) don't necessarily have much to do with the nighttime conditions in pre-industrial, pre-urban 140 BC. That's why it's better to derive the extinction coefficient for ancient times from ancient data, instead of using modern data as a guess. And I credit Schaefer for trying to do this, using some sparse ancient data from the prepublication of Pickering 1999.

F2 The Pickering 1999 referenced by Schaefer 2001 was a *DIO* preprint that did not actually appear in *DIO* 9.3 as intended. Its data and text are incorporated and expanded here as §F. Given the very few data actually in the preprint, it might seem odd that Schaefer chose to ignore some of them. The data he omits (those for acronychal risings) are exactly those data that argue strongly against a long-held belief of Schaefer's on how the ancients defined the heliacal rising of a planet.

F3 According to Ptolemy, a heliacal rising is the first day on which a planet or bright star can be seen to rise on the horizon after solar conjunction (*Almagest* 13.7) In fact, not only does Ptolemy state explicitly that this occurs on the horizon, he also draws a diagram in the *Almagest* showing the planet on the horizon; and then he does spherical trig on the diagram, during which he uses the said horizon line as a great circle on the sphere. All of which seems quite explicit.

[24] Arya 1988, 2.

[25] And perhaps even less: see http://www.bts.gov/itt/urban/14-4a-1.html for one scientist's measurement of a nocturnal boundary layer only 18 meters thick.

[26] Rawlins 1994, *DIO* 4.2

[27] Diodorus Sicilus 5.59; see Oldfather (1939) 3:258-9.

[28] Sung & Bessell 2000, 246. In their figure, the median seems to be about .15, and number of observations hover near the zero-aerosol level.

F4 But that's not what some (including Schaefer) believe. The alternative is that a heliacal rising occurs on the first day that the star or planet is visible *at any altitude* before dawn after the solar conjunction. This moves the planet several degrees up from the horizon. By moving the planet up off the horizon, you can add in a whole lot more atmospheric extinction and still get the same *arcus visionis* as stated by Ptolemy and others. Of course, you also get the ancient values of *arcus visionis* if you use a clearer atmospere and put the heliacal rising on the horizon, as stated by Ptolemy. Thus, Schaefer's analysis of the heliacal risings to determine ancient values for k is also, in a very esoteric manner, based on modern measurements. When taken at face value (i.e., on the horizon), the heliacal rising data support a value of k in the range of .14 to .16, right in line with Rawlins.

F5 These ancient data can be found primarily in Ptolemy's reports of what he calls "phases" of the planets, as explained in Book 13.7 of the *Almagest*. Here Ptolemy describes[29] the limits of visibility of the planets under the most difficult conditions possible: right on the horizon, and during twilight. Ptolemy says that the critical parameter for visibility is the *arcus visionis (AV)*, or the angle in degrees of the Sun below the horizon (which turns out to be correct); and he provides this angle for each of the planets near solar conjunction. Ptolemy computes these phases for the latitude of Phoenicia, implying that these observations came mostly from there.

F6 In his shorter work *Planetary Hypotheses*, Ptolemy gives the *AV* for each planet again,[30] although the values here differ somewhat from those of the *Almagest*. Ptolemy also includes the values for a first magnitude star[31] on the ecliptic. In the cases of Venus and Mercury, Ptolemy provides values for superior conjunction and inferior conjunction (although these two values are the same for Mercury). These are found in our table 2.

F7 Ptolemy's minor work *Phaseis* contains in Book I the values for both first and second magnitude stars; in the only surviving fragment[32] of Book I, a first magnitude star has an *AV* of 12 degrees, and a second magnitude star is 15 degrees.

F8 Table 2 also contains the *arcus visionis* of acronycal risings the three outer planets (Mars, Jupiter and Saturn). At many times of the year, it is possible to see a planet or bright star when it rises at night; but as the Sun moves through the celestial sphere, you will eventually come to a certain day on which the twilight is so bright when the planet rises, that your first glimpse of the planet is no longer on the horizon, but some small distance above it. The date on which this occurs is called the "acronychal" rising of the planet, and it always occurs just before the time of solar opposition. For a few days around opposition, you cannot see the planet rise or set, because twilight intervenes in both cases; but after a few days, you can then begin to see the planet set. The date on which the first setting can be observed is called the "cosmical" setting of the planet (also often called acronychal setting.) For the outer planets near opposition, Ptolemy says that the *AV* is about half that for near conjunction.

F9 Going even farther back to about 400 BC, the Greek physician Hippocrates[33] divided the year into medical seasons, noting in passing the dates of the acronycal rising of Arcturus (59 days after winter solstice) and the acronychal setting of the Pleiades (44 days before

[29] Toomer 1998, 639-640.

[30] Goldstein 1967, 9.

[31] Hugh Thurston has already suggested that ancient skies were clearer than today on the basis of this datum: Thurston 1994, 173.

[32] Morelon 1981, 4.

[33] Hippocrates *De victu* 3.68. The chapter can be found in Jones 4. My thanks to Robert H. van Gent for bringing the reference to my attention.

winter solstice). From these, we can compute the angle of the Sun below the horizon for an observer at Kos in the fifth century BC at the times of these phenomena.[34]

Source / Object	**Arcus Visionis**	**Elongation**	**Magnitude**
Almagest			
Mercury	10	12	−1.63
Venus	5	6	−3.94
Mars	11.5	13.8	1.02
Jupiter	10	12	−2.06
Saturn	11	13.2	−0.01
Planetary Hypotheses			
Mercury, superior conj.	12	14.4	−1.71
Mercury, inferior conj.	12	14.4	−1.79
Venus, superior conj.	5	6	−3.94
Venus, inferior conj.	7	8.4	−2.95
Mars	14.5	17.4	0.99
Jupiter	9	10.8	−2.06
Saturn	13	15.6	−0.01
Aldebaran	15	18	0.87
Mars, opposition	7.25	172.8	−2.93
Jupiter, opposition	4.5	175.5	−2.94
Saturn, opposition	6.5	173.5	−0.48
Phaseis			
Aldebaran	12	14.4	0.87
Antares	15	18	1.06

Table 2: Summary of ancient recordations of heliacal and acronychal risings.

F10 Our first step in analyzing these data is to compute the magnitudes of the various planets at these near-conjunction conditions. For convenience and consistency, we assume that the elongation of the planet is $1.2\times AV$ when near conjunction, and $180° - AV$ near opposition. All magnitudes are computed[35] at planet perihelion. Saturn is computed[36] at maximum ring-tilt (which was quite near perihelion in ancient times, and still is today.) The *Almagest* values for Venus and Mercury are computed for superior conjunction. The first magnitude star on the ecliptic mentioned by Ptolemy in a generic fashion, is assumed to be Aldebaran, the brightest star in the zodiac; and the second magnitude star is assumed to be Antares, the brightest star that is listed as second magnitude in the *Almagest*. We give these values in table 2.

[34] Hippocrates (460-377 BC) lived at Kos, but traveled widely. For purposes of computation, I chose four years early in his career (443-440 BC) and four years late in his career (403-400 BC). I computed the date of solstice for each year, added 59 days and computed the altitude of the Sun at the apparent rise of Arcturus. Since this day represented the first day of invisibility, I repeated the computation for the previous day, the last day of visibility. Taking the average of all 16 observations gives the threshold for visibility in terms AV: 10.95 degrees. The computation for the Pleiades was similar, except that I subtracted 44 days from the solstice, and I computed positions on the basis of 17 Tauri, the first bright Pleiad to set at this latitude. For the Pleiades, the threshold was 16.06 degrees below the horizon. These values are increased by half a degree each when computing on the basis of observing at a zenith distance of 89°.5 instead of 90°.

[35] According to the algorithms of Duffet-Smith 1988, for all planets except Saturn.

[36] Using the BASIC program of Olson 1995.

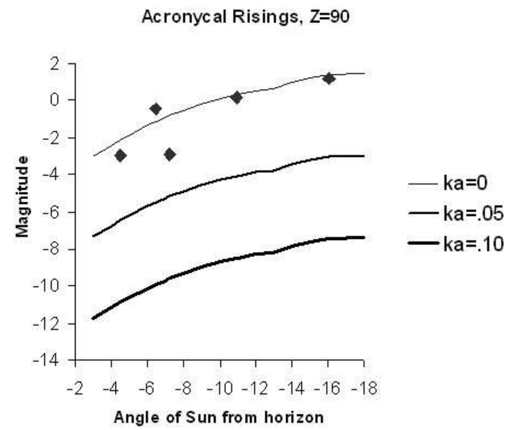

Figure 4: Visibility limits for acronychal risings, for a true horizon (zenith distance $Z = 90°$). Curves are modern computations, diamonds are ancient observations.

	$Z = 89°.5$	$Z = 90°$	Magnitude
Pleiades	0.148	0.134	1.2
Arcturus	0.149	0.134	0.16
Mars	0.180	0.157	−2.93
Jupiter	0.184	0.142	−2.94
Saturn	0.134	0.125	−0.48
Mean	0.159	0.138	
Std. Deviation	0.022	0.012	

Table 3: Implied k for ancient acronychal risings, for two horizon types.

F11 We must realize that (unlike the heliacal rising and setting) there is *no point of maximum visibility* for the planet rising acronychally; the visibility of the planet increases steadily as it rises, because the effects of atmospheric extinction and sky brightness work in tandem to make it so. Therefore, it simply does not make sense to speak of an acronycal rising or a cosmical setting unless the star or planet *is actually visible on the horizon in twilight*. When Hippocrates says that the acronychal rising of Arcturus occurs 59 days after the winter solstice, there must be some obvious difference between the rising of Arcturus on day 58 compared to day 59; and the only possible difference can be than on day 58, Arcturus can be *seen to rise* (on the horizon), while on day 59, Arcturus cannot be seen until it has already risen (above the horizon). This conforms to the ancient definition[37] of acronychal rising, and it is the only ready distinction that can be made by an observer without instrumentation, such as Hippocrates. This implies that in ancient times a planet or bright star could actually be seen on the horizon as a matter of routine — and this in turn suggests a clearer sky than some might expect.

[37] In the *Almagest* 8.4, Ptolemy uses the term "evening visible later rising," meaning that the rising of the planet or star is visible, and occurs after sunset.

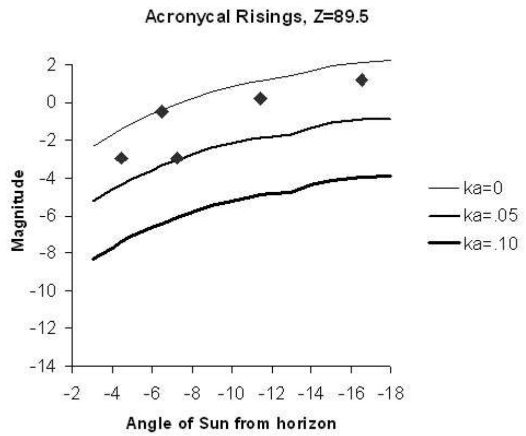

Figure 5: Visibility limits for acronychal risings, for a cluttered horizon (Z =89°.5). Curves are modern computations, diamonds are ancient observations.

F12 These ancient visibility limit data can be compared against modern computations of visibility limits under the same conditions of twilight sky and planet brightness on the horizon. Here we use the algorithms provided in Schaefer 1998, with a few enhancements.

F13 The standard visibility threshold function is that of Hecht 1947, which is a two domain (day-night) least-squares fit through the data of Knoll 1946. The Hecht function has a sharp cusp right in the twilight area that we are concerned with; the Knoll data shows a cusp, but it is not as sharp as Hecht's function makes it, nor quite in the place that Hecht's function puts it. A better least-squares fit through Knoll's data can be done with a three-domain function (day-twilight-night). The improved function fits Knoll's data about three times better[38] than Hecht, and the improvement is greatest in the twilight region that we are most concerned with here. I find the threshold T (in footcandles) for a point source against a given background brightness B in nanoLamberts, when $\log B > 6.74$:

$$T = 2.725 \times 10^{-8}(1 + \sqrt{1.114 \cdot 10^{-7} B})^2$$

When $6.74 > \log B > 2.63$:

$$\log T = .0684(K \log B)^2 - .256(K \log B) - 8.44$$

And when $\log B < 2.63$:

$$\log T = .0828(K \log B)^2 + .194(K \log B) - 9.73$$

Here the constant $K = .4343$; and we should note that the function gives incorrect results at a background darker than 0.1 nL, which does not occur outdoors.

F14 In figure 4, I have computed[39] the acronychal rising of Mars, Jupiter, Saturn, and Arcturus, and the cosmical setting of the Pleiades, according to data given by Ptolemy and

[38] Although using the Hecht function would not produce different conclusions than those presented here, for the obvious reason that both functions are fit to the same data.

[39] My procedure for refraction, for sea level, 1013 mB and 15 C: determine star's true altitude at transit from $H = 90° - \phi + \delta$ (where ϕ is the observer's latitude and δ is the star's declination); and compute paramter $v = H + (9.23/(H + 4.59))$ degrees. Refraction in arcsec is then $r = 58.7 * \cos v / \sin v$, and apparent altitude $h = H + r$. This form of refraction equation is taken from Rawlins 1982, but the

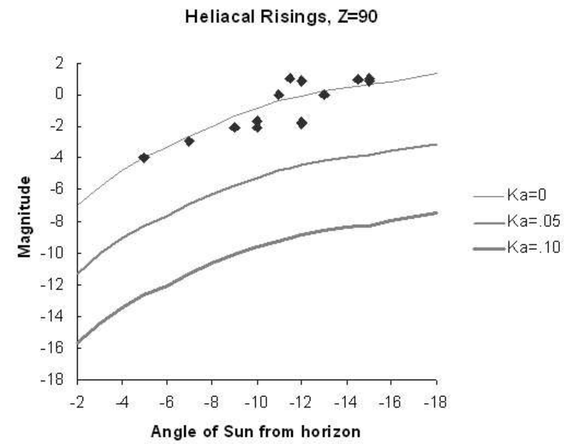

Figure 6: Visibility limits for heliacal risings, for a true horizon (Z =90°). Curves are modern computations, diamonds are ancient observations.

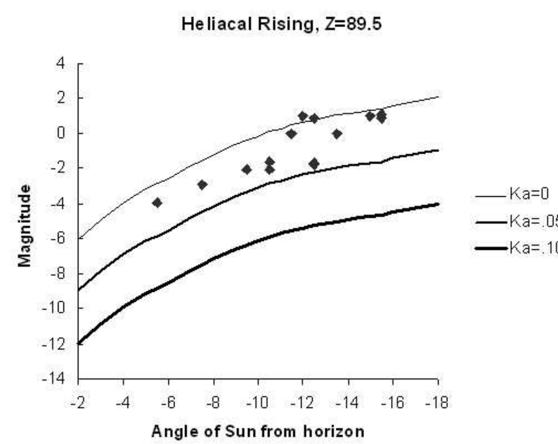

Figure 7: Visibility limits for heliacal risings, for a cluttered horizon (Z =89°.5). Curves are modern computations, diamonds are ancient observations.

Hippocrates; and at the same time I have plotted the theoretical visibility limits[40] for three different aerosol levels in the atmosphere. The lowest thick line is the value computed for an aerosol extincion coefficient $k_a = .10$ magnitudes per airmass; the narrower lines are for $k_a = .05$ and $k_a = 0$, respectively. In figure 4 I assume that the observer was watching the rising or setting against a perfect sea horizon, with a zenith distance $Z = 90$ degrees. Under these conditions, the best fit atmosphere is $k_a = .006 \pm .012$ and total $k = .138$ magnitudes per airmass.

F15 It is possible that ancient observers did not always have a perfect sea horizon; if instead these observations were made on a slightly cluttered land horizon, the apparent horizon would likely be some small angle above the true horizon; therefore, in figure 5 I have re-computed for the condition of zenith distance $Z = 89^\circ.5$ degrees. Under these conditions, the best fit atmosphere is $k_a = .027 \pm .022$, and $k = .159$.

F16 I have plotted visibility limits for heliacal risings in figures 6 and 7 and table 4 in a similar way. The most discordant datum is that for Mars from the *Almagest*. It is likely that this value (an *AV* of 11.5 degrees) is a scribal error, since the *Planetary Hypotheses* gives the *AV* of Mars as 14.5 degrees, and the numerals 1 and 4 are easily confused in ancient Greek (see ‡5§B later in this issue.)

F17 Three important facts are now apparent. First, the $Z = 90^\circ$ values (for both acronychal and heliacal risings) cluster strongly around the Rayleigh-plus-ozone value for clear air, which is not required by the observations themselves.

F18 Second, the values for k from heliacal rising are virtually identical to those obtained from acronychal rising under the same conditions (both for a perfect horizon $Z = 90^\circ$, and for a cluttered horizon $Z = 89^\circ.5$). Third, the scatter of the data is significantly lower under the ancient definition of on-the-horizon visibility than under Schaefer's modern definition, by 3 to 7 times. Ptolemy tells us that the *Almagest* observations, at least, were all taken around the summer solstice, in part because the air at that time of year is "thin and clear". We should therefore expect that the derived values for k should be (a) low; and (b) somewhat consistent with each other. The ancient definition of heliacal rising meets both of these criteria better than Schaefer's modern definition.

F19 In addition to ancient attestations of astronomical visibility, there is at least one useful ancient record of surface visibility — and it comes from Rhodes itself, the home of Hipparchos. The Greek historian Apollodorus,[41] a contemporary of Hipparchos (but writing of a time around the Trojan War), stated that from the top of Mount Attabyrion (at 1215 m, the highest point on Rhodes), the island of Crete could be seen. The same story, with the same attestation of Crete's visibility from Mt. Attabyrion, can also be found in the works of the Roman historian Diodorus Sicilus[42] (1st cent. BC). The distance from Mt. Attabyrion to Mt. Modi[43] on Crete is 196 km; then, using the relation[44] of Koschmieder 1926 for surface visibility range, the extinction coefficent must have been $k \leq .150$ in order to see Crete from Mt. Attabyrion. This number is right in line with the values we obtained from other ancient sources.

F20 To summarize, every pre-industrial source that I have been able to find, when taken at face value, implies a much clearer ancient atmosphere than adopted by Schaefer 2001. Using a half dozen different techniques and data sources, the results are all quite consistent with each other, and consistent with the idea of a low value of aerosol extinction. In all

constants have been refined by a least-squares fit to results derived from the onion-skin method found in Schaefer 1989 for sea level at 15° C and 60% relative humidity; although I modified Schaefer's program to use double precision throughout, and to use 10 times the number of atmospheric layers (each .1 times the thickness). That program is in turn based on the physical theory of Garfinkel 1967. The refractive index for Garfinkel's theory is determined for the center of the visual range (550 nm) and the stated atmospheric conditions from the Starlink algorithms of Rutherford Laboratories (http://star-www.rl.ac.uk/star/docs/sun67.htx/sun67.html). The equation presented here fits Garfinkel's theory about three times better than the equation of Schaefer 1998.

My procedure for determining Rayleigh (molecular atmosphere) airmass: after determining apparent altitude h in degrees (see above), $X_r = 1/\sin(h + 244/(165 + 47 * h^{1.1}))$. For aerosol airmass (at 2 km scale height), $X_a = 1/\sin(h + 20/(31 + 32 * h^{1.1}))$. This form of airmass equation is taken from Rawlins 1992, but the constants have been refined by a least squares fit to the results of the same onion-skin method described above. These equations are just as compact as those found in Schaefer 1998, but they fit the Garfinkel theory about ten times better; and the fit is improved most near the horizon, the area we are most concerned with in this paper. For ozone airmass, Schaefer's equation is fully adequate: $X_o = (1 - (\sin(z)/(1 + (20/6378)))^2)^{-.5}$, where $z = 90^\circ - h$.

[40] For an observer with a Snellen ratio of 1 (i.e., 20-20 vision), using a temperature of 15° C, 40% relative humidity, at sea level, latitude 33°, moonless sky, and a year near minimum solar activity.

[41] Apollodorus *Library* 3.2, can be found at Hard 1997 p.98 among other places.

Planet	Schaefer	$Z = 89^\circ.5$	$Z = 90^\circ$
Plan. Hyp.			
Mercury - inf	0.370	0.174	0.151
Mercury - sup	0.380	0.176	0.152
Venus - sup	0.170	0.151	0.132
Venus - inf	0.210	0.157	0.136
Mars	0.250	0.138	0.127
Jupiter	0.250	0.161	0.140
Saturn	0.290	0.150	0.135
Almagest			
Mercury	0.260	0.162	0.141
Venus	0.170	0.151	0.132
Mars	0.170	0.126	0.118
Jupiter	0.300	0.169	0.146
Saturn	0.210	0.141	0.127
Phaseis			
Aldebaran	0.190	0.132	0.121
Antares	0.270	0.138	0.127
Mean	0.249	0.151	0.134
Std. Deviation	0.069	0.015	0.010

Table 4: Implied k under various definitions of Heliacal Rising

[42] Diodorus 5.59 can be found in Oldfather (1939) 3:258-9.

[43] Mt. Modi (830 m), at 35°08′30″N, 26°07′45″E, is the sizable Cretan massif nearest to Rhodes. Mt. Attabyrion is located at 36°12′N, 27°52′E. Distance (196.3 km) is computed using the GRS 1980 ellipsoid. Since both mountains are high, atmospheric clarity, rather than curvature of the earth, is the only barrier to their intervisibility. The island of Karpathos nearly intervenes, but the high hills of Crete are significantly (and obviously) to the right and farther away than Karpathos. Any suggestion that Karpathos was mistaken for Crete falls to obvious rebuttals: first, under such a scenario there is no island that could be mistaken for Karpathos; second, the observer (Althaemenes) was a native of Crete who founded the temple of Zeus Attabyros on the peak specifically because he could see his home from there — making mistaken identity most unlikely.

[44] For visibility range V, extinction per unit distance B_{ext} is found by $B_{ext} = K/V$, where K is the Koschmieder constant (with 3.92 being the usual value for the limit of human ability). At a range of 196.3 km, $B_{ext} = 2 \times 10^{-5}$ m^{-1} and of this, 1.15×10^{-5} is Rayleigh (assuming $k_R = .1023$ and a scale height of 8.2 km; the constant .921 is applied to convert between astronomical units [mag/airmass] and physical units [attenuation/meter]). This leaves 8.5×10^{-6} m^{-1} for aerosol extinction, which becomes $k_a = .018$ at a generous 2 km scale height. Adding standard Rayleigh and ozone gives a total astronomical extinction of $k \leq .150$ magnitudes per airmass. Using more relaxed observational parameters, Johnson 1981 derived a Koschmeider constant of 3.0, which gives a total extinction of $k \leq .140$ magnitudes per airmass for ancient Rhodes.

cases, we have derived $k_\mathrm{a} < .05$, and in many cases quite a bit less; all are significantly less the value $k_\mathrm{a} = .1$ used in Schaefer 2001:

- minimizing χ^2 for Hipparchos (§B11), Tycho (§D10), and Hevelius (§D11) indicates $.004 < k_\mathrm{a} < .048$;
- correlation between airmass and μ for Hipparchos, Tycho and Hevelius (§D) indicates $.013 \leq k_\mathrm{a} \leq .029$;
- acronychal rising data from Hippocrates and Ptolemy (Table 3) indicate $.006 \leq k_\mathrm{a} \leq .027$;
- heliacal rising data from Ptolemy (Table 4) indicate $.002 \leq k_\mathrm{a} \leq .019$;
- surface visibilty at ancient Rhodes (§F19) indicates $k_\mathrm{a} \leq .018$; and
- observation of low, dim stars by Ptolemy (below at ‡5 fn 8) indicates $k_\mathrm{a} \leq .010$.

The implication is that two centuries of industrial and agricultural activity have left the atmosphere (especially in Europe) much dirtier now than it was in ancient times.

G Recovering the Catalog's Epoch

G1 Because the latitude we derive for the observer of the ASC is closely related to the extinction coefficient that we adopt, the foregoing explains why Hipparchos cannot be eliminated as the observer of the ASC's first three quadrants because of his latitude. But what about the observer's epoch? The answer here requires a major digression.

G2 Back in 1998, I first took a look at the southern limit as an exercise to see if I could confirm and refine the results of Rawlins 1982. The procedure I adopted was quite similar to that eventually adopted by Schaefer 2001. Although I had no problem replicating Rawlins' latitude, I found that I was deriving an epoch for the ASC in the early middle ages, way too far forward in time even for Ptolemy. Recognizing that this indicated a problem with my procedure, but not willing to spend the considerable time needed to find a hidden flaw in such a large and complex algorithm, I dropped the whole thing and moved on to other interests. Then it turned out that Schaefer also derived an epoch way too far forward for the first three quadrants, and (more interestingly) an epoch way too far backward when considering the fourth quadrant only. When testing his procedure against the catalog of Tycho Brahe, Schaefer again derived an epoch centuries too far forward. All the clues were there, and I considered the problem anew.

G3 The position of the South Celestial Pole on the celestial sphere moves as precession advances. In the time of Hipparchos and Ptolemy, the SCP was located in the constellation Hydrus and was moving away from the Phoenix/Fornax/Sculptor region and toward the Crux/Centaurus region. (At any given time the SCP is moving toward 12h RA and away from 0h RA as it circles the South Ecliptic Pole, which always lies along 6h RA.) And as the SCP moves, it carries with it a zone of invisibility (for northern hemisphere observers) centered on it. So we can call the Crux region the "leading edge" of the invisible zone, and the Phoenix region the "trailing edge."

G4 This means that the zone of invisibility is moving away from a star-poor region and into a star-dense region along the Milky Way; and this creates a dynamic imbalance of critical importance to the statistical method employed by both Rawlins and Schaefer in determining the observer's epoch.

G5 Imagine that there are only two stars in the sky, of equal brightness, and neither one is in the catalog. Place one at the "leading edge" of the zone of invisibility, and one at the "trailing edge." The statistical procedure "wants" the zone to cover both unseen stars, and is drawn toward both stars equally; so the function will minimize at the epoch when both stars have equal visibility. A good analogy is to imagine that the zone of invisibility is "attracted" by uncataloged stars (and "repelled" by cataloged stars.) Therefore, uncataloged stars at the leading edge will draw the derived epoch forward in time, while uncataloged stars at the trailing edge will draw the derived epoch backward in time. The reverse is true for cataloged stars, but there are far more uncataloged stars than cataloged ones, so their effect is very much predominant.

G6 In the real sky, as the zone of invisibility advances in time, its leading edge (in the Crux region) is attracted by a whole lot of uncataloged stars along the Milky Way, while its trailing edge[45] (in the Phoenix region) is attracted by only a few. So unless we take suitable precautions, the zone will be drawn too far forward[46] in epoch by the presence of the many uncataloged stars in the Crux region of the Milky Way. This too-forward epoch is eventually stopped by the covering of a cataloged star; but even this final brake will be dampened if the assumed latitude of the observer is too low.

G7 Rawlins 1982 did in fact take a precaution to avoid this problem: he ignored all the dim stars. This tends to equalize the number of cataloged and uncataloged stars, so that the "attraction" and "repulsion" effects of the cataloged and uncataloged stars are more nearly equal. (Although to be fair, Rawlins was unaware of this problem; it seems instead that he employed this procedure as a matter of simplicity.)

G8 Schaefer's procedure not only fails to do this, it makes matters worse by dividing the analysis into quadrants, lumping the first three quadrants of RA together, and keeping the fourth quadrant separate. The fourth quadrant contains half of the trailing edge, and we recall that the uncataloged stars at the trailing edge act as a "brake" on the forward advance of the derived epoch. With half of his brakes gone, Schaefer's analysis of the first three quadrants was even more strongly attracted to an epoch too far forward in time.

G9 Meanwhile, the fourth quadrant (when considered alone) has no leading edge. The uncataloged stars at the trailing edge draw the epoch of the 4th quadrant too far backward in time, since they are not balanced by uncataloged stars at the missing leading edge. If we split the sky this way, we would expect that such a procedure would falsely indicate the epoch of first three quadrants too late, and of the fourth quadrant too early. This is exactly what happened in Schaefer 2001.

H Fun with fake data

H1 Schaefer 2001 claims that the results obtained are "robust", meaning that they don't change under differing input assumptions. There is an easy way to test a complex procedure such as this, and it does not involve tweaking the inputs to see how much the output changes; instead, we can give the *entire process* a dataset of known origin, and measure directly how well the process finds the correct results. For example, given any P function, atmosphere, epoch, and latitude, it is easy to generate a pseudo-catalog that might have been taken by a naked-eye observer under those conditions. Having such a catalog, does the statistical procedure recover the correct epoch and latitude of the observer?

H2 I created four such pseudo-catalogs for 0 AD and latitude 36°, using an extinction coefficient of $k = .18$ and using equation 2 as a P function. Knowing the correct value of k and the correct P function in advance, and using all quadrants as input, the procedure did rather well in recovering the latitude, getting the correct result (within 1 degree) 3 out of 4 times. It was less successful in recovering the epoch, getting the correct result (within a century) only 1 out of 4 times, and going a century too far forward in the other cases.

H3 But when I tried to recover the latitude and epoch using only the first three quadrants of the catalog, the process went badly awry, for reasons discussed above at §G8. The latitude was recovered correctly only one out of four times, and the epoch was not recovered correctly

[45] In August 1999, Schaefer tested his procedure by viewing the sky for one hour while on vacation in Bermuda. The hour chosen by Schaefer for his test (around 3 AM local time) insured that the transiting part of the sky was centered around 0 hours RA, where there is no leading or trailing edge. Such a small test in this restricted region of the sky would be incapable of detecting the effect described here — and in fact, it didn't.

[46] This is exactly what happened Schaefer 2001, not only for the ASC, but also for Tycho's catalog, for which he derived an epoch of 2000 AD ± 500.

at all, with all results being at least three centuries too far forward. Since the difference between Hipparchos and Ptolemy is three centuries, the de-coupling of the quadrants *alone* creates a larger error than the size of the effect we are trying to measure.

H4 Equally bad results were obtained when I used a different P function in recovery than I used in creating the catalog. For example, when I substituted equation 1 in the recovery phase and de-coupled the quadrants, the statistical algorithm not only failed to recover the correct latitude and epoch in all cases, it also *rejected* the correct latitude and epoch at a statistically significant level ($\sigma > 2$) in three out of four cases. It is worth mentioning that the results obtained were both too far forward in time, and too low in latitude in all cases. Therefore, we should expect that the results obtained by Schaefer 2001 were also too low in latitude and too far forward in time than the actual observer.

H5 As one might expect, changing the assumed atmospheric extinction has a significant effect on the latitude recovered. I found that an increase in k of .1 will lower the derived latitude by between 4.5 and 5 degrees. This means that the difference between the value of k used by Rawlins 1982 (.15) and Schaefer 2001 (.23) would alone account for most of the latitude difference between Ptolemy and Hipparchos (P function accounts for the rest).

H6 When recovering the epoch and latitude using the correct P function and extinction coefficient, using a magnitude limit had little effect on the results. But when using an incorrect P function or incorrect atmosphere, I found that a magnitude limit of between 3.5 and 4.5 tended to reduce the error of the derived result. There are two reasons for this. First, as suggested above, this is because such a magnitude limit tends to equalize the number of cataloged and uncataloged stars. The second reason can be demonstrated when we look at the final statistic Q which we are trying to minimize. When examining all stars, Q will typically minimize around 400 or so. In such a case, we can reject any latitude/epoch combination with $Q \geq 404$ at a significant (2-sigma) confidence level. But why are we allowed, statistically, to eliminate that Q value of 400 from each total? Because we *assume* that minimum Q of 400 represents "noise" that infects the "signal" we are trying to detect; and we further assume that this noise is perfectly random, Gaussian noise. In effect, we are trying to detect a teacup's worth of signal atop a skyscraper of noise. But the assumption of random noise can only be true if stars are randomly placed in the sky, and they are not: the Milky Way insures that. The benefit of using a magnitude limit is that by throwing out the dim stars, the remaining stars are more nearly random in their distribution across the sky.

H7 Overall, the pseudodata study reveals that the recovery of latitude and epoch from this statistical method is a very delicate balancing act that can easily go wrong for a number of reasons. In many ways, the procedure is chaotic, i.e., there is a sensitive dependence upon initial conditions — those conditions being the chosen atmosphere, P function, and magnitude limit. It is quite possible, when several of these factors work together, that the statistical procedure will reject the correct value at a statistically significant level. When, as in the case of Schaefer 2001, we combine an incorrect P function, an overly opaque atmosphere, and decouple the quadrants (all at the same time), an incorrect result is almost impossible to avoid.

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