Vol. 12 **2002 Sept** ISSN 1041-5440

DIO

The International Journal of Scientific History

The Ancient Star Catalog

Novel Evidence at the Southern Limit
(still) points to Hipparchan authorship

Instruments & Coordinate Systems

New Star Identifications

Table of Contents

Page:

Instructions for authors

See our requirements on the inside back cover. Contributors should send (expendable photocopies of) papers to one of the following *DIO* referees — and then inquire of him by phone in 40 days:

Robert Headland [polar research & exploration], Scott Polar Research Institute, University of Cambridge, Lensfield Road, Cambridge, England CB2 1ER.

Charles Kowal [celestial discovery, asteroids], Johns Hopkins University Applied Physics Laboratory, Johns Hopkins Road, Laurel, MD 20707.

Keith Pickering [navigation, exploration, computers, photography, science ethics], **DIO**, Box 999, Watertown, MN 55388.

E. Myles Standish [positional & dynamical astronomy], Jet Propulsion Laboratory 301-150, Cal Tech, 4800 Oak Grove Drive, Pasadena, CA 91109-8099.

F. Richard Stephenson [ancient eclipses, ΔT secular behavior], Department of Physics, University of Durham, Durham DH1 3LE, UK; tel (44) 191-374-2153.

Hugh Thurston [early astronomy, pure math, WW2 cryptography, skepticism], University of British Columbia, Math Department, 121-1984 Mathematics Rd, Vancouver, B.C., Canada V6T 1Z2.

Christopher B. F. Walker [Mesopotamian astronomy], Department of Western Asiatic Antiquities, British Museum, Great Russell Street, London WC1B 3DG, UK.

Phone numbers: Walker 44-207-323-8382, Thurston 604-531-8716, Stephenson 44-191-334-3413, Standish 818-354-3959, Pickering 612-955-3179, Kowal 443-778-6000, Headland 44-1223-336540.

***DIO* Note:**

With this issue of *DIO*, we have — with way too much effort* — inaugurated a long-awaited upgrade from our creaky old LaTeX 1 system (running under MS-DOS) to the much more useable LaTeX2ε (running under Linux or Windows). This allows us greater flexibility, a wider variety of fonts in multiple languages, and insures a stable typesetting system (and operating system) for the foreseeable future. Our thanks go to those authors who have been waiting so patiently.

* [Publisher's Note: Said effort has been largely that of Editor Keith Pickering.]

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

by KEITH A. PICKERING[1]

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.

[1] Editor, *DIO*, Box 999, Watertown, MN 55388.

[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.

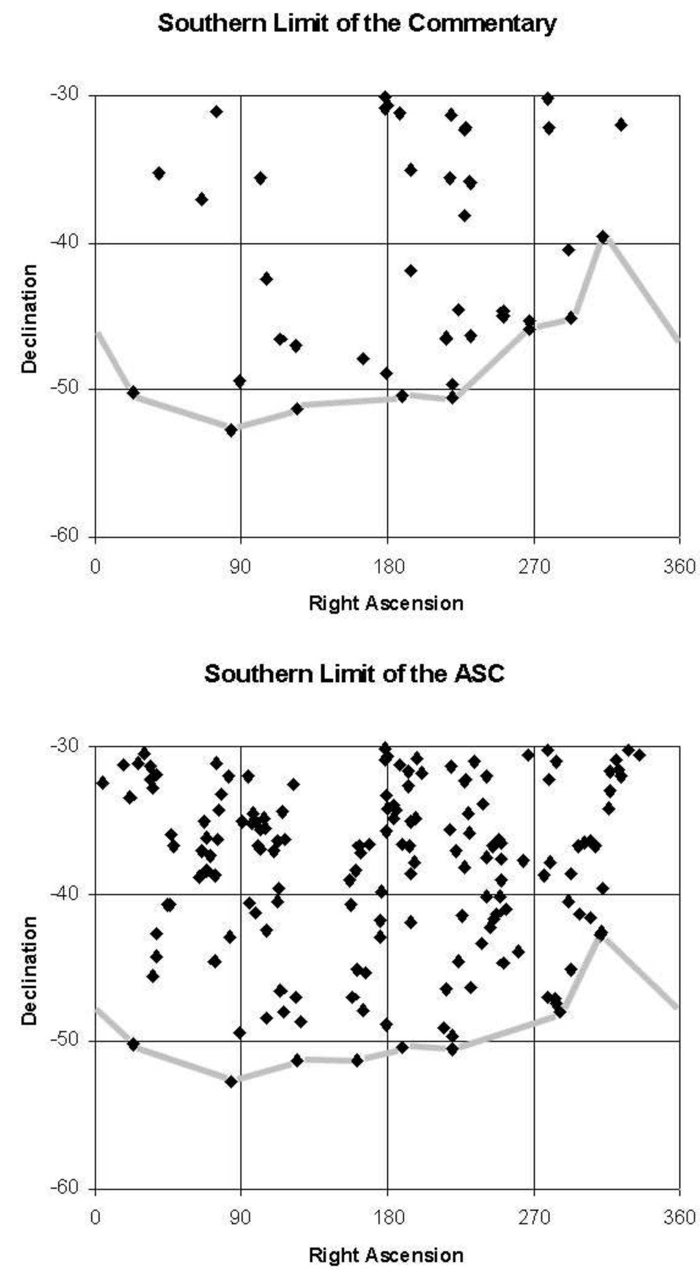

Figure 1: The southern limit of Hipparchos's *Commentary on Aratos and Eudoxos*, compared to the southern limit of the Ancient Star Catalog in Ptolemy's *Almagest*. Positions of cataloged stars are for epoch -140.

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:

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

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

[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.

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 ($\mu = 4.93$).

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.

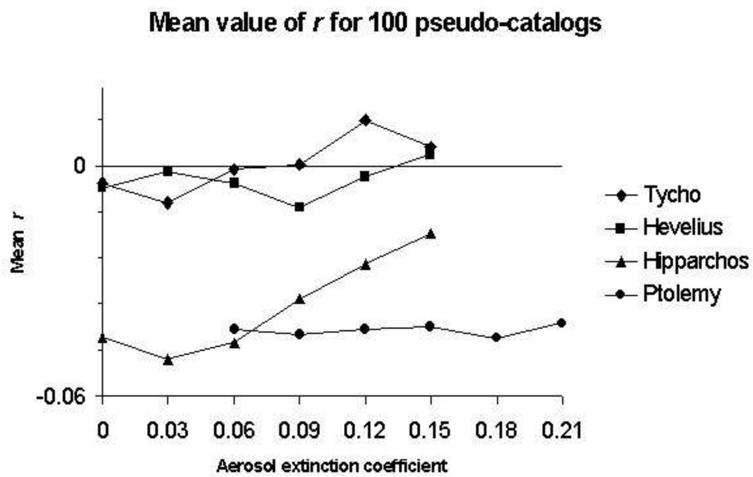

Figure 2: Expected correlation between X_a and V under various assumptions of extinction, for various observers.

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 ($36° \, 40'$ 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]

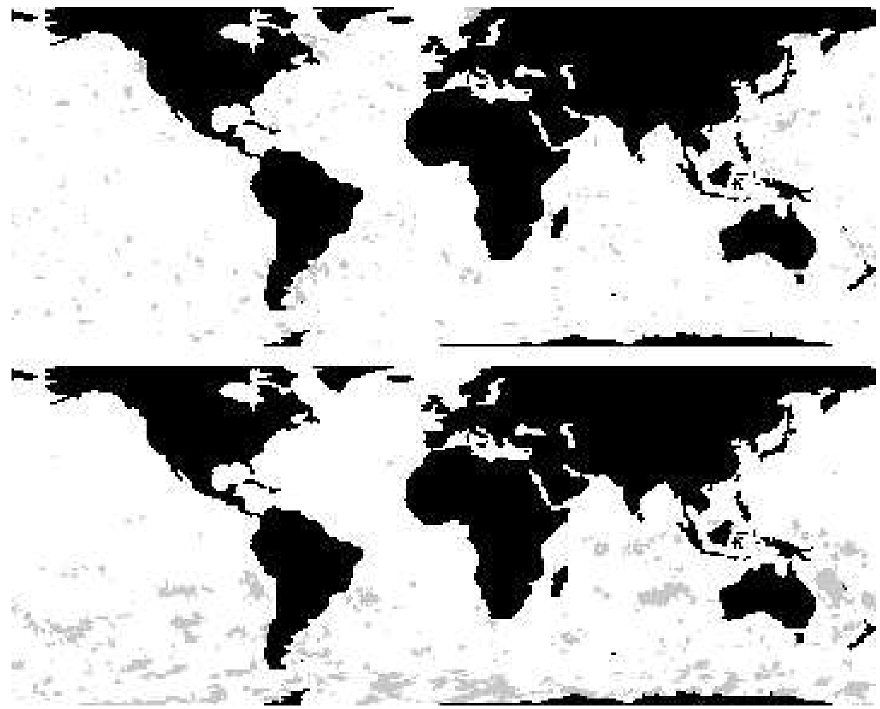

Figure 3: Oceanic surface areas with weekly average aerosol optical depth $\leq .033$ (gray), for the weeks of February 8-15, 2001 (top) and May 2-10, 2001 (bottom).

[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.

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

[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.

[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.

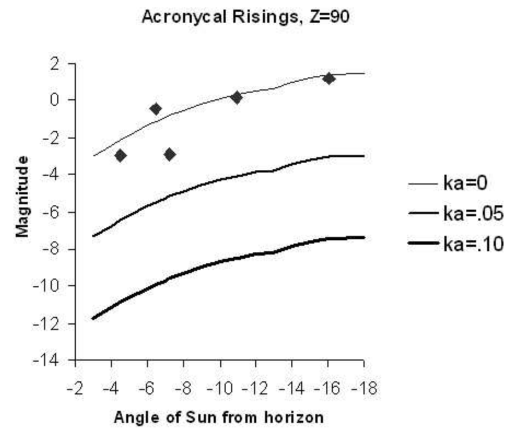

Figure 4: Visibility limits for acronychal risings, for a true horizon (zenith distance $Z = 90°$). Curves are modern computations, diamonds are ancient observations.

	$Z = 89°.5$	$Z = 90°$	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.

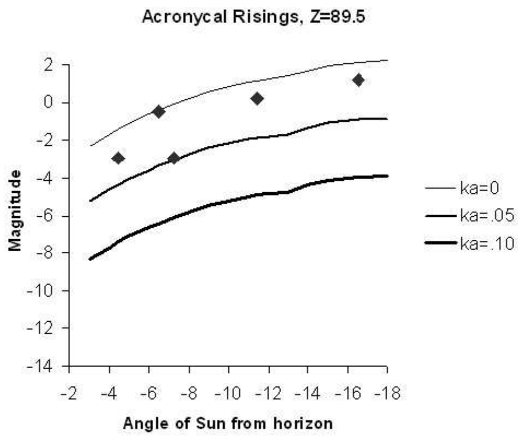

Figure 5: Visibility limits for acronychal risings, for a cluttered horizon (Z =89°.5). Curves are modern computations, diamonds are ancient observations.

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

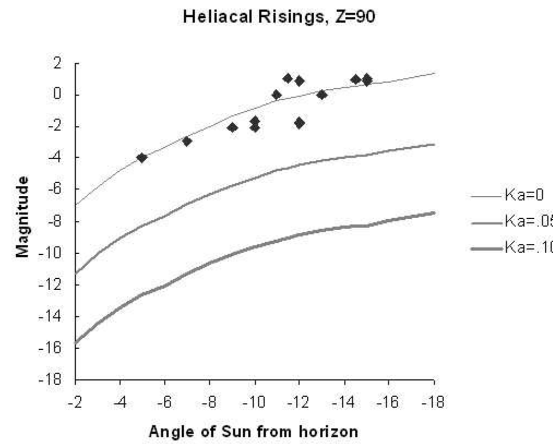

Figure 6: Visibility limits for heliacal risings, for a true horizon (Z =90°). Curves are modern computations, diamonds are ancient observations.

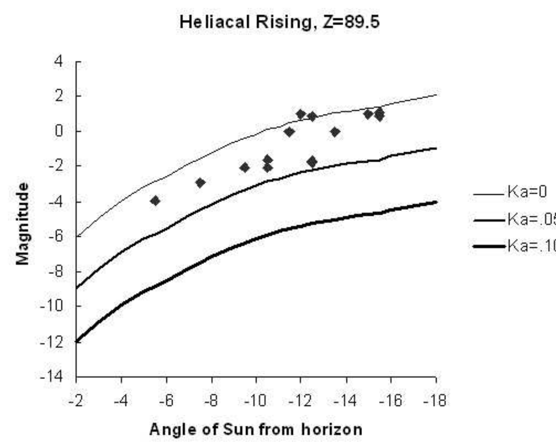

Figure 7: Visibility limits for heliacal risings, for a cluttered horizon (Z =89°.5). Curves are modern computations, diamonds are ancient observations.

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 $k_\mathrm{a} = .10$ magnitudes per airmass; the narrower lines are for $k_\mathrm{a} = .05$ and $k_\mathrm{a} = 0$, 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 $k_\mathrm{a} = .006 \pm .012$ 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 $k_\mathrm{a} = .027 \pm .022$, 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°.5**	$Z = $**90°**
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 $B_\mathrm{ext} = K/V$, 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, $B_\mathrm{ext} = 2 \times 10^{-5}\ \mathrm{m}^{-1}$ and of this, 1.15×10^{-5} is Rayleigh (assuming $k_\mathrm{R} = .1023$ 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 $8.5 \times 10^{-6}\ \mathrm{m}^{-1}$ for aerosol extinction, which becomes $k_\mathrm{a} = .018$ 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 $k_a < .05$, and in many cases quite a bit less; all are significantly less the value $k_a = .1$ used in Schaefer 2001:

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

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.

References

Arya, S. P. (1988). *Introduction to Micrometeorology*, Academic Press.

Baily, Francis (1843). The Catalogues of Ptolemy, Ulugh Begh, Tycho Brahe, Halley, Hevelius. *Mem. RAS* 13.

Duffet-Smith, Peter (1988). *Practical Astronomy with your Calculator, Third Edition.* Cambridge University Press.

Duke, Dennis (2002). Associations between the ancient star catalogues, *Archive for History of Exact Sciences*, 56, 435-450.

Evans, James (1987). On the Origin of the Ptolemaic Star Catalogue: Part 2. *JHA* 18, 233-278.

Frölich, G., & Shaw, Glenn E. (1980). New determination of Rayleigh scattering in the terrestrial atmosphere. *Applied Optics* 19:11, 1773-1776.

Garfinkel, B. (1967). Astronomical refraction in a polytropic atmosphere. (*Astronomical Journal* 72, 235.)

Goldstein, B. (1967). An Arabic Version of Ptolemy's Planetary Hypotheses. *Transactions of the American Philosophical Society* 57, no. 4.

Graßhoff, Gerd (1990). *The History of Ptolemy's Star Catalogue.* Springer-Verlag.

Hard, Robin (1997). *Apollodorus: The Library of Greek Mythology.* Oxford Univ Press.

Hecht, Selig (1947). *Journal of the Optical Society of America*, 37:1,59.

Hipparchos. In Arati et Eudoxi phaenomena commentariorium [see below at ‡3 fn 2].

Hoffleit, E.D., & Warren Jr., W.H. (1991) *Yale Bright Star Catalog*, 5th ed. Yale University Observatory. Available at http://adc.gsfc.nasa.gov/adc-cgi/cat.pl?/catalogs/5/5050/ .

Johnson, R. W. (1981). Daytime visibility and nephelometer measurements related to its determination. *Atmos. Environ.* 15, 1935-1845.

Jones, H. L. (1917) *The geography of Strabo.* G.P. Putnam's Sons.

Jones, W.H.S. (1967). Hippocrates. Harvard University Press, 1962-67.

Knoll, H.A., R. Tousey, & E.O. Hulbert (1946). Visual Thresholds of Steady Point Sources of Light in Fields of Brightness from Dark to Daylight. *Journal of the Optical Society of America* 36:8, 480-482.

Koschmieder, H. (1926). Theorie der horizontalen Sichtweite, *Beit. Phys. Atmos.* 12, 33-55.

Morelon, R. (1981). Fragment arabe du premier livre du 'Phaseis' de Ptolémée. *J. Hist. Arabic Sci.* 5 (1-2), 3-22.

Oldfather, C. H. (1939). *Diodorus of Sicily.* Harvard University Press.

Olson, Donald W., Russell L. Doescher, and Jonathan Gallmeier (1995). The Rings of Saturn. *Sky & Telescope* 92:5 (May), 92-95.

Peters, C.H.F. & E.B. Knobel (1915). *Ptolemy's catalogue of stars.* The Carnegie Institution of Washington.

Pickering, Keith A. (2002). [See below here at ‡5.]

Rawlins, Dennis (1982). An Investigation of the Ancient Star Catalog. *PASP* 94, 359.

Rawlins, Dennis (1992). Tycho catalog's completion was faked. *DIO* 2.1 35-50.

Rawlins, Dennis (1993). Tycho's 1004-star Catalog. *DIO* 3.

Schaefer, Bradley E. (1989). Sky & Telescope, March 1989

Schaefer, Bradley E. (1998). To the Visual Limits. *Sky & Telescope* 95:5 (May), 57-60.

Schaefer, Bradley E. (2001). The latitude of the observer of the *Almagest* star catalogue. *JHA* 32:1-42.

Sung, Hwankyung & Bessell, Michael. S. (2000). Standard Stars: CCD Photometry, Transformations and Comparisons *PASA* 17:3 244-254. Available at http://www.atnf.csiro.au/pasa/17_3/sung/paper.pdf

Toomer, G.J. (1998). *Ptolemy's Almagest*. Princeton University Press.

Thurston, Hugh (1994). *Early Astronomy*. Springer.

‡2 On the Clarity of Visibility Tests

by DENNIS DUKE[1]

The *Almagest*[2] star catalog (ASC) has for centuries invited speculation about who actually compiled it. Of its many curious features, the fact that the catalog contains no stars which are visible in Alexandria but not visible in Rhodes suggested to Delambre[3] that perhaps the catalog was actually compiled by Hipparchus, who is known to have lived in Rhodes (at about 36° north latitude), and not by Ptolemy, the author of the *Almagest*, who is known to have lived in Alexandria (at about 31° north latitude).

In 1982 Rawlins[4] constructed a model that, subject to its assumptions, provides a quantitative test of how well the catalog's southern limit tells us the latitude of the observer. Rawlins' application of the model produced a clear signal in favor of an observer at the latitude of Rhodes. In 2001 Schaefer[5] used Rawlins' basic model but with an updated set of technical inputs to reach a substantially different conclusion, basically favoring an observer at the latitude of Alexandria for at least three quadrants of the sky. It is the purpose of this paper to carefully examine exactly how the model works, and how conclusive are the results of either Rawlins' or Schaefer's analysis.

Here is how the model works: we assume as input all the stars in the sky that are visible to the naked eye, and a catalog, in this case the ASC, that contains some subset of these stars. For each such star, we compute its apparent magnitude m and a probability of visual detection $P_{\rm det}$, which is a function of m. Then the probability that the ith star is included in the catalog is $P_{\rm i} = P_{\rm det}$, while the probability that a given star is not included is $1 - P_{\rm det}$. The product L of the $P_{\rm i}$ for every star is the likelihood that the catalog was assembled subject to our assumptions.

The details of the calculation include the computation of the apparent magnitude m and the probability of detection P_{det}. The apparent magnitude is determined by adjusting the tabulated visual magnitude V for atmospheric extinction of the star's visible light. Briefly, we assume that a star is actually observed at its meridian culmination height $h = 90° - \varphi + \delta$, where ϕ is the latitude of the observer and δ is the declination of the star. The epoch T of the observer also matters, as the star's declination is affected by precession. The height h determines the depth X of the Earth's atmosphere that the star's light traverses, and given an extinction coefficient k, the apparent magnitude is given by $m = V + kX$. In practice,[6] the depth X is usually split into components for Rayleigh scattering, ozone absorption, and aerosol scattering, each with its own extinction coefficient. Given m, the calculation proceeds with the computation of $P_{\rm det}$. In general, we expect $P_{\rm det}$ to be near unity for bright stars and near zero for very dim stars. Rawlins used a piecewise monotonic function for $P_{\rm det}$ while Schaefer used a specific functional form $P_{\rm det} = 1/(1 + e^{F(m-m_0)})$, which introduces two parameters F and m_0.

In order to compute the likelihood L we must know values for the parameters ϕ, T, and k, and in Schaefer's version of the model, also F and m_0. We use a computer program to vary the parameters until the likelihood L is maximized, or equivalently, until the negative log-likelihood $S = -2 \ln L$, is minimized. Thus the model assigns penalty points (values of S) to an observer who either includes in his catalog a dim star or omits a bright star. Complete details are given in the papers of Rawlins and Schaefer.

First, we summarize Rawlins' analysis. He chose for the input sample of stars not all visible stars in the sky, but instead a subset of 30 southern stars that are in a sparsely populated area of the sky. Of these, 16 are included in the ASC, 14 are not. Assuming essentially zero scattering by atmospheric aerosols, Rawlins found $S = 14.4$ for Hipparchus' latitude and epoch and $S = 75$ for Ptolemy's latitude and epoch. The differences in S tell us that in this analysis Hipparchus is indicated with about a 7.8-sigma significance level. Schaefer pointed out that Rawlins' result depends critically on both his sample of selected stars and on his assumed value of k.

Next, we summarize Schaefer's analysis. He chose for the input sample of stars essentially all stars in the *Bright Star Catalog,*[7] combining close neighbors that would be visually indistinguishable. Schaefer also assumed a minimum value for aerosol scattering based upon the best visibility conditions at sea level today in the areas around Rhodes and Alexandria. For three quadrants of the sky (right ascensions in the range $0° < \alpha < 270°$, and declinations less than $-10°$) he found $S = 667.4$ for Hipparchus and $S = 615.5$ for Ptolemy. For the fourth quadrant in right ascension he found $S = 176.2$ for Hipparchus and $S = 182.7$ for Ptolemy. The differences in S tell us that for the first three quadrants Ptolemy is indicated with about a 7-sigma significance level, while for the fourth quadrant Hipparchus is indicated with about a 2.5-sigma significance level. Schaefer also found that his results are very robust to a multitude of reasonable variations of his input assumptions, as long as aerosol scattering stays above a minimum level.

I have independently repeated the calculations of both Rawlins and Schaefer, and have confirmed that both sets of calculations are technically correct: if you use their input assumptions, you do get their result. Further, I have used the generally more complicated model of Schaefer, which also allows variation in the $P_{\rm det}$ function, to analyze Rawlins' selected subset of 30 stars, and I again get substantially the same result as Rawlins originally published.

So what should we conclude from these analyses? If either is to be believed, we must have confidence in the input assumptions. I would like to point out in particular the following three assumptions:

- a star is included in the catalog based *exclusively* on the probability that a star of its apparent magnitude is *visible* at a specific latitude. This makes no allowance for the possibility that an observer might include stars reported to him from other, perhaps more southerly, locations, or that the observer might work harder to include stars at lower altitudes. This also does not take into account that each star in the catalog was not only *seen*, but its position was also *measured*. Anyone who has ever tried it will know that the latter is much harder than the former.
- when analyzing a fixed area in the sky, the model assumes that every star in that area was observed at the *same* latitude. In order to find a composite catalog the analyst must carefully search different areas of the sky to see if different latitudes are indicated. This is exactly what Schaefer did, to find his three quadrants for Ptolemy and one quadrant for Hipparchus solutions. But if the catalog is truly composite, as many catalogs are, with multiple observers at multiple latitudes, the model cannot reveal that fact.
- the test does not use any *other* information we might have about a particular star that might shed light on who observed that star.

To illustrate the impact of these assumptions, let us consider the case of Canopus (α Car and BN892 in the ASC). In 130 BC Canopus culminated at about 1.3° at $\varphi = 36°$. Its visual magnitude was -0.72 (presumably the same as today) but its apparent magnitude in Rhodes was about 5. In Alexandria in 137 AD Canopus culminated at over 6° and its apparent magnitude was about 1.4. So the likelihood for Ptolemy is much larger than for

[1] Florida State University; dduke@scri.fsu.edu
[2] *Ptolemy's Almagest*, transl. by G. J. Toomer (London, 1984).
[3] J. B. B. Delambre, *Histoire del'astronomie ancienne* (2 vols, Paris, 1817), ii. 261-4.
[4] D. Rawlins, "An investigation of the ancient star catalog", *Publications of the Astronomical Society of the Pacific*, xciv (1982), 359-73.
[5] B. Schaefer, "The latitude of the observer of the Almagest star catalogue, *Journal for the History of Astronomy*, xxxii (2001), 1-42.
[6] B. Schaefer, "Astronomy and the limits of vision", *Vistas in Astronomy*, xxxvi (1993), 311-61.

[7] D. Hoffleit, *The bright star catalog* (New Haven, 1997).

Hipparchus. Yet we know for certain than Hipparchus *did* in fact include rising and setting information for Canopus in his *Commentary to Aratus*,[8] and Vogt[9] was able to use these data to deduce the coordinates that Hipparchus must have had for Canopus. Further, the data that Hipparchus reported imply that his coordinates for Canopus contained rather large errors of about 5°, and amazingly enough, we find those same large errors *repeated*[10] in the star coordinates for Canopus that appear in the ASC (see Table 1 and Figure 1).

Thus we have a case where the maximum likelihood test tells us that Ptolemy is favored over Hipparchus as the observer of Canopus, while we have additional information that is *not* used by the test that tells us exactly the opposite.

Name	BSC	Baily Number	Type	Commentary Error	Almagest Error
θGem	2540	426	1	4.06	4.04
	2540	426	2	3.03	3.24
ιCan	3474	455	1	−5.72	−3.04
	3474	455	2	−3.17	−3.61
βSgr	7337	592	3	−7.34	−5.74
	7337	592	4	−4.92	−3.94
θEri	897	805	1	−2.54	−2.61
	897	805	2	−2.91	−3.42
	897	805	3	5.75	7.06
	897	805	4	6.76	8.28
αCar	2326	892	3	5.11	4.69
	2326	892	4	5.03	5.25
πHya	5287	918	1	3.48	3.07
	5287	918	2	3.65	3.45
	5287	918	3	−6.52	−7.52
	5287	918	4	−3.75	−4.39
αCen	5459	969	1	4.73	4.74
	5459	969	2	6.79	6.33
θAra	6743	992	1	−1.62	−2.96
	6743	992	2	−2.53	−3.53
γAra	6462	995	3	−7.89	−8.80
	6462	995	4	−5.91	−5.84
βAra	6461	996	3	−12.72	−8.69
	6461	996	4	−9.01	−5.55
ζAra	6285	997	1	−1.30	−1.15
	6285	997	2	−1.05	−1.37

Table 1: The stars common to both the *Commentary* and the *Almagest* that either have large shared errors or which play a role in the visibility test.

In order to see whether this is a harmless special case or a more general problem, let's take a close look at how the difference in S values of about 52 actually arises in Schaefer's analysis of the first three quadrants — the area in the sky that provides the strongest pro-Ptolemy result. When I repeat the analysis using my input star catalog (which differs in

[8] Hipparchos, *In Arati et Eudoxi phaenomena commentariorium*, ed. and transl. by K. Manitius (Leipzig, 1894).

[9] H. Vogt, "Versuch einer Wiederstellung von Hipparchs Fixsternverzeichnis", *Astronomische Nachtrichten*, ccxxiv (1925), cols 2-54.

[10] G. Graßhoff, *The history of Ptolemy's star catalogue* (New York, 1990).

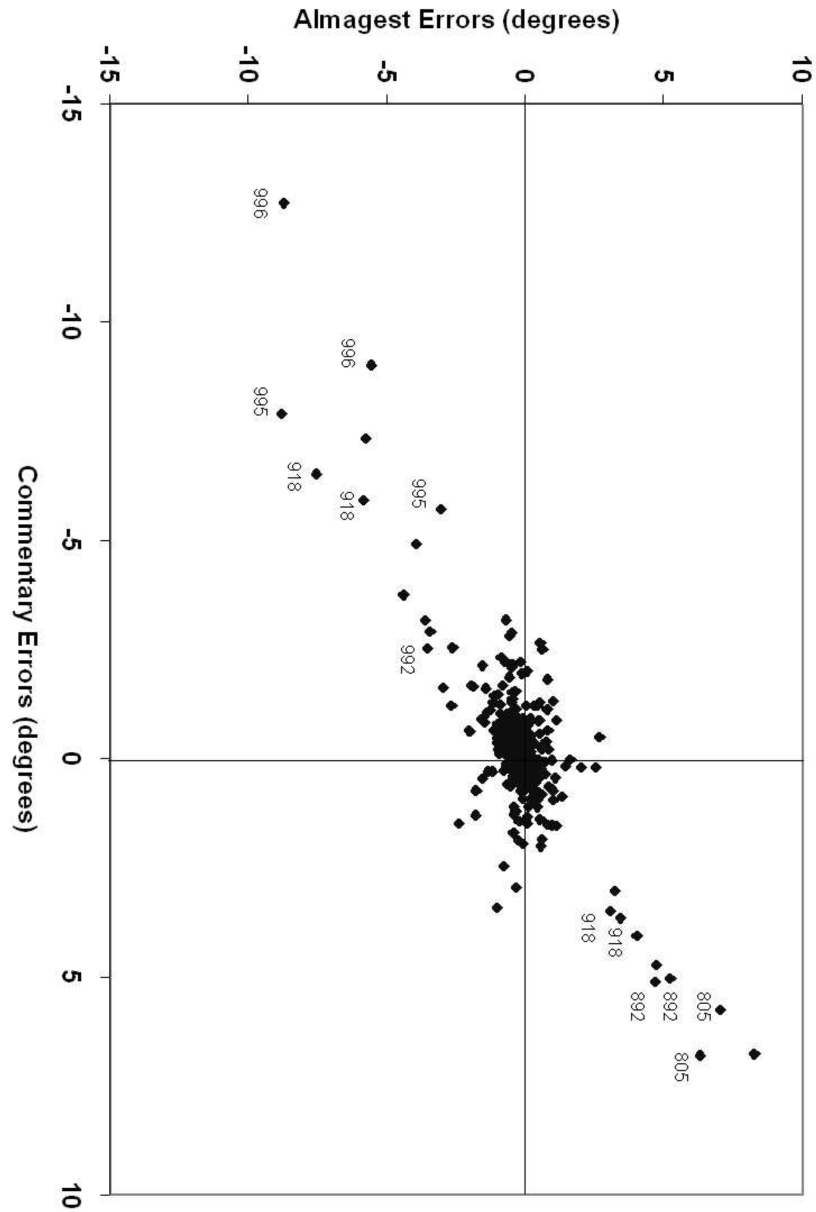

Figure 1: A scatter plot showing the correlation of the *Commentary* and *Almagest* errors for phenomena of types 1-4. Those stars with large shared errors that are discussed in the text are marked with their Baily number (column 3 in Table 1).

details from Schaefer's), approximately the same parameter assumptions, and my computer program, I find a difference in S values of about 54, so we know we are both in general agreement (and other more detailed comparisons confirm this completely).

Consider first those stars in the sky that *do not* appear in the ASC. For Hipparchus and Ptolemy, these stars contribute to S about 278 and 269, so the difference of 9 is a 3-sigma effect in favor of Ptolemy. Not negligible, but a small part of the overall difference of 54. Therefore, we see that *most* of the pro-Ptolemy signal is coming from stars that were actually in the catalog, not from stars that were omitted.

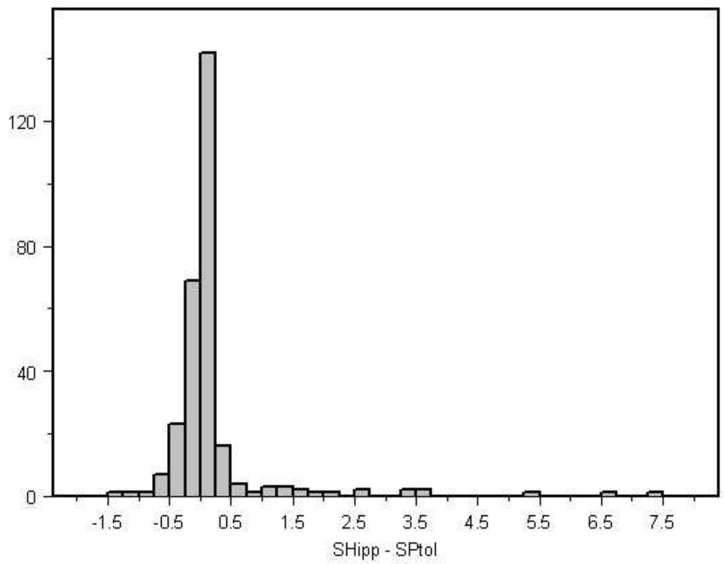

Figure 2: The distribution of S differences for the stars that are in the ASC.

If we look at the differences in S values for the 284 stars in this part of the sky that are also *included* in the ASC, on a star-by-star basis, we get the histogram shown in Figure 2. We notice that this histogram is nearly symmetric about zero, except for a tail[11] of stars at positive S. Indeed, we notice that if we compute the sum of the S values for all stars except the 13 with the largest positive S values, i.e. those that favor Ptolemy most, then that sum is very nearly zero. This means that a very large part (46 out of 54) of the pro-Ptolemy signal in this test is in fact arising from 13 specific ASC stars. These stars are listed in Table 2.

[11] The reader might wonder whether this tail is peculiar to the case at hand, or a general feature that should be expected. Monte Carlo simulation confirms the second possibility. Indeed, I have generated hundreds of synthetic star catalogs by extracting with probability $P_{\rm det}$ stars from the Bright Star Catalog. When these synthetic catalogs are analyzed, distributions very similar to that shown in Figure 1 always result. Indeed, it is fairly obvious that when the model indicates a southern observer, the reason will always be that the northern observer was penalized for including too many dim, low altitude stars. Conversely, when a northern observer is indicated, it will be because the southern observer *omitted* too many bright stars.

Name	**Baily #**	$S_{\rm Hipp}$	$S_{\rm Ptol}$	$S_{\rm Hipp} - S_{\rm Ptol}$
1195	803	3.47	1.76	1.71
1143	804	6.94	4.28	2.66
θEri*	805	5.44	0.19	5.26
χCar	884	4.63	1.07	3.57
oVel	885	2.75	0.96	1.79
V344 Car	887	14.80	8.09	6.71
N Vel	889	3.23	0.54	2.69
τPup	893	3.60	0.27	3.34
θAra*	992	2.73	1.07	1.65
εAra	994	4.88	2.82	2.06
γAra*	995	9.74	2.34	7.40
βAra*	996	4.09	0.43	3.66
ζAra*	997	4.06	0.74	3.32

Table 2: A number in column 1 gives the star's ID in the *Bright Star Catalog*. The number in column 2 gives the star's ID in the *Almagest* star catalog. $S_{\rm Hipp}$ and $S_{\rm Ptol}$ are the contributions of that star to the log-likelihood assuming Hipparchus and Ptolemy as the observer, respectively. The stars marked with * have large errors shared by the *Commentary* and the ASC, and hence we can be fairly certain that Ptolemy copied them from Hipparchus.

Of the 13 stars, 5 of them, BN805, 992, 995, 996, and 997[12] *also* appear in Hipparchus' *Commentary to Aratus*, and like Canopus, each[13] has large errors common to the *Commentary* and the ASC (see Table 1 and Figure 1). We can therefore be pretty certain that these five stars, which are contributing a total of 20 to S, are in fact, like Canopus, giving us contradictory signals: a pro-Ptolemy signal from the [Schaefer] visibility test, but a pro-Hipparchus signal from the coordinate errors. (Remember, the only information the visibility model takes from the ASC is whether or not a star is included — the actual coordinates and magnitudes listed in the ASC are not used in any way.)

How deep does this problem reach? Without further independent analysis, we can only speculate, but the following line of thought is not unreasonable: let us consider whether the other eight stars in our signal might have been also copied. We know that BN805 (θ Eri) was copied, which at least suggests that BN803 and BN804, nearby neighbors in Eridanus, are also good candidates for copying. We know that 4 Ara stars, BN992, 995, 996, and 997 were copied, which suggests that BN994, also in Ara, might also be copied. That leaves BN884, 885, 887, 889, and 893, all in Argo Navis. Now we know that Ptolemy copied at least two stars from Argo Navis: 892 (Canopus) and 918 (π Hya), but these stars did not make our list of 13 'critical' stars. Still, it might be taken to suggest that Ptolemy copied others from Argo, further weakening the case against Hipparchus. In fact, a simple model analysis[14] of the size of the correlations between the *Commentary* and *Almagest* errors suggests that a large fraction, even up to 100%, of stars common to the *Commentary* and the *Almagest* were copied, so these speculations are far from groundless. All in all, then, we have either direct or circumstantial evidence that a very large part of the pro-Ptolemy

[12] Manitius and Graßhoff identified the first star to rise in Ara as ε Ara (BN994), but the surrounding textual and astronomical evidence in the *Commentary* establishes beyond any reasonable doubt that the correct identification is ζ Ara (BN997).

[13] G. Graßhoff, *op. cit.* (ref. 10), 331-34.

[14] D. Duke, "Associations between the ancient star catalogues", *Archive for History of Exact Sciences*, 56 (2002) 435-450; D. Duke, "The Depth of Association between the Ancient Star Catalogues," *Journal for the History of Astronomy* (forthcoming [has since appeared: *JHA*, 34 (2003) 227-230]).

signal issued by the visibility test is, in fact, contradicted by the coordinate error data.

How should we resolve this dilemma? One way out was recently offered by Schaefer,[15] who points out that we need merely assume that Ptolemy did everything he claims, i.e. look at the sky and measure the positions of the stars, but then perhaps compares his results with old records he had from Hipparchus and for some reason included Hipparchus' coordinates for a subset of the stars instead of his own measurements in the ASC. This scenario thus uses in a crucial way the model assumption that the only issue being tested, and hence the only conclusion that can follow, is whether a given star was *observed* at a particular latitude. It would be interesting to try and further test this scenario, but I don't presently know how to do that.

Another option is to incorporate into the maximum likelihood calculation the *a priori* knowledge that *some* stars were definitely observed and measured by Hipparchus and copied by Ptolemy. For those stars it makes little sense to blindly apply the basic model assumption that every star is included in the catalog with probability P_{det}. Indeed, for those stars the statistically sound procedure would be to say that P_{det} is simply unity for Hipparchus and zero for Ptolemy (or perhaps use a gaussian probability distribution sharply peaked at the parameters implied by Hipparchus as the observer). In that case, however, the likelihood

$$L = \prod_{i=1}^{N} P_i$$

will obviously be sharply maximized for Hipparchus, *no matter what* the contributions of the other stars (unless someone can find a star that is *known* to be measured by Ptolemy and not by Hipparchus — so far, not a single such star is known). The reader might complain, correctly, that this makes the whole question default to Hipparchus, but the real reason this happens is the model assumption that *all* the stars with a fixed region of the sky were measured at the *same* latitude. So in fact, the default is built into the model.

It appears to me that we must ask which conclusion do we trust the most, which in turn means which set of underlying assumptions is most likely to be true in *this specific case*. I know of no reason to mistrust the evidence from the large shared errors, but we must admit that only five of the crucial 13 stars are virtually certain to be of Hipparchan origin. The evidence for the remaining eight is, strictly speaking, circumstantial and statistical. On the other hand, the discussion above makes it clear that the fundamental assumptions that underlie the visibility test may not be nearly so solid, at least in the case at hand. Certainly the simplest resolution is that the visibility test, as implemented, just doesn't work for the ASC. It would be interesting if someone could find an objective way to distinguish these options.

ACKNOWLEDGEMENT

I am especially grateful to Bradley Schaefer for patiently answering dozens of questions from me during the months I was learning the details of the model and writing and testing the computer programs that implement it.

[15] B. Schaefer, "The Great Ptolemy-Hipparchus Dispute", *Sky & Telescope*, 103 (February 2002), 38-44.

‡3 The Measurement Method of the Almagest Stars

by DENNIS DUKE[1]

I suggest that the correct standard model of early Greek stellar astronomy is:

- Someone, perhaps Hipparchus, measured a fairly complete star catalog in *equatorial* coordinates.
- That catalog was the basis for the results presented in Hipparchus' *Commentary to Aratus*.[2]
- Analog computation was used to convert most of the catalog to *ecliptical* coordinates.
- It is this converted catalog, with longitudes shifted by $2°40'$, that we have received through Ptolemy and the *Almagest*.

The supporting argument in brief is:

The star coordinates in Hipparchus' *Commentary to Aratus* are clearly equatorial right ascensions and declinations.[3] Although we have no surviving hint *how* those coordinates were measured, or even *who* measured them, it is reasonable to assume that the coordinates were measured in the same way they were presented: equatorial coordinates. Ecliptical stellar coordinates are conspicuous in their absence.

The correlations between the errors in the *Almagest* data and the *Commentary* data show that those two data sets are associated in some way. This is substantiated by[4]

- several stars with large common errors in each data set,
- detailed statistical analysis of the error correlations between the two sets of data, and
- similar systematic errors in the two data sets.

These facts are most easily reconciled by assuming a catalog in equatorial coordinates that was used to calculate the *Commentary* data, and was eventually used in substantial part for the *Almagest* catalog. Strictly speaking, this catalog need not originate from Hipparchus.

The star coordinates in the *Almagest* are ecliptical longitudes and latitudes, which are clearly the most convenient form for any astronomer in the era Hipparchus-Ptolemy, whose primary interest would likely be lunar and planetary positions. Ptolemy claims that he measured the star coordinates with a zodiacal armillary sphere, but several analyses show that his claim must be largely not true, and that he must have copied most if not all the coordinates from some other catalog,[5] adjusting the longitudes to account for precession.

We now invoke Newton's fractional ending observation to conclude that the catalog that Ptolemy copied *from* was, at the time he did the copying, also in ecliptical coordinates, but with excesses of $00'$ endings in both longitude and latitude. This implies that the catalog Ptolemy copied from was either the result of

- direct measurements in ecliptical coordinates, or
- conversion from equatorial coordinates by some method that resulted in excesses at $00'$ endings in longitude and latitude.

[1] Florida State University; dduke@scri.fsu.edu

[2] Hipparchus, *Commentary on the Phenomena of Aratus and Eudoxus*, trans. Roger T. Macfarlane (private communication). Until this is published, the interested reader must use Hipparchus, *In Arati et Eudoxi phaenomena commentariorium*, ed. and trans. by K. Manitius (Leipzig, 1894), which has an edited Greek text and an accompanying German translation.

[3] D. Duke, "Hipparchus' Coordinate System", *Archive for History of Exact Sciences* 56 (2002) 427-433.

[4] G. Graßhoff, *The history of Ptolemy's star catalogue* (New York, 1990); D. Duke, "Associations between the ancient star catalogues", *Archive for History of Exact Sciences*, 56 (2002) 435-450; D. Duke, "The Depth of Association between the Ancient Star Catalogues", *Journal for the History of Astronomy* (forthcoming [see fn 14]).

[5] J. B. J. Delambre, *Histoire de l'astronomie ancienne* (2 vols, Paris, 1817), ii. 261-4; R. R. Newton, *The crime of Claudius Ptolemy*, (Baltimore, 1977); D. Rawlins, "An investigation of the ancient star catalog", *Publications of the Astronomical Society of the Pacific*, xciv (1982), 359-73; *ibid.*, DIO 1.1 (1991), 62-63; *ibid.*, DIO 2.3 (1992) 102-113; G. Graßhoff, *op. cit.* (ref. 4).

However, direct measurement of the ecliptical position of each star would give coordinate errors that were statistically uncorrelated with the equatorial coordinate errors mentioned above, and so is hard to reconcile with the clear and strong common heritage of the *Commentary* and the *Almagest* data sets. This suggests, therefore, that the most likely scenario is that someone converted the original equatorial coordinates to ecliptical using some form of analog computation. Hipparchus using a celestial globe is an obvious candidate.[6]

SUPPORTING DISCUSSION

The conclusion that Hipparchus used equatorial coordinates is based on the following observations:[7]

- in the *Commentary* Hipparchus actually quotes the positions of numerous stars directly in right ascension or declination (or more often its complement, polar distance),
- polar longitudes are not directly measurable, since the measurement of any longitude is always with respect to some other previously measured longitude, and there is no way to measure one polar longitude with respect to another polar longitude.
- polar longitudes are in fact *never* quoted directly for a single star in the *Commentary,* and
- since Hipparchus did not measure the rising, setting, and culmination numbers directly in the sky, he must have *computed* the numbers somehow, using some *other* set of numbers as input to the calculation. Hipparchus gives an explicit example, and that example uses right ascension and declination as the initial input data.

The statistical evidence that the rising/setting phenomena data in the *Commentary* and the *Almagest* coordinates share a common heritage is substantial. Figure ‡3 shows cases of stars with large and similar errors in both data sets. It is unlikely that independent observations of all these stars would result in essentially the *same* large errors. Omitting the outlier cases and analyzing the correlations between the smaller errors in the *Commentary* and the *Almagest* also shows that the data sets most likely have a common heritage. The correlations are quantitatively understood by means of a simple model: the *Almagest* errors are ε_i, where ε has mean zero and variance σ_A^2, while the *Commentary* errors are $\varepsilon_i + \eta_i$, and these have mean zero and variance σ_C^2. The ε and η errors are completely uncorrelated, while the added η errors account for the empirical fact that the variance σ_C^2 in the *Commentary* errors is larger than the variance σ_A^2 in the *Almagest* errors. A simple extension of the model allows an estimate of the fraction of stars copied by Ptolemy and concludes that the fraction is large, and not inconsistent with unity. Finally, it is possible to estimate the *systematic* errors in the *Commentary* phenomena data, and they show a clear similarity to the systematic errors observed in the *Almagest* coordinates. Although the comparison of the *Almagest* and *Commentary* statistical errors is limited to the 134 stars common to both catalogs, the clear association between the *systematic* errors implies that the association is more broadly based, since the systematic errors are relatively smooth, few-parameter, collective effects that permeate the entire data sets in both the *Commentary* and the *Almagest* catalog. These observations taken together thus strongly suggest that the *Commentary* data and the *Almagest* coordinates share, at least in large part, a common heritage. In the case of the *Commentary* we also know, as discussed above, that the heritage comes from a catalog expressed in equatorial coordinates.

Are the positions of the stars included in the *Almagest* catalog consistent with measurement with an armillary? Comparing the number of stars catalogued with the number

[6] R. Nadal and J.-P. Brunet, "Le Commentaire d'Hipparque I. La sphère mobile", *Archive for History of Exact Sciences*, 29 (1984), 201-36 and "Le Commentaire d'Hipparque II. Position de 78 étoiles", *Archive for History of Exact Sciences*, 40 (1989), 305-54. And, of course, Ptolemy tells us explicitly in *Almagest* VII.1 that Hipparchus had a globe.

[7] D. Duke, *op. cit.* (ref. 3) gives complete details.

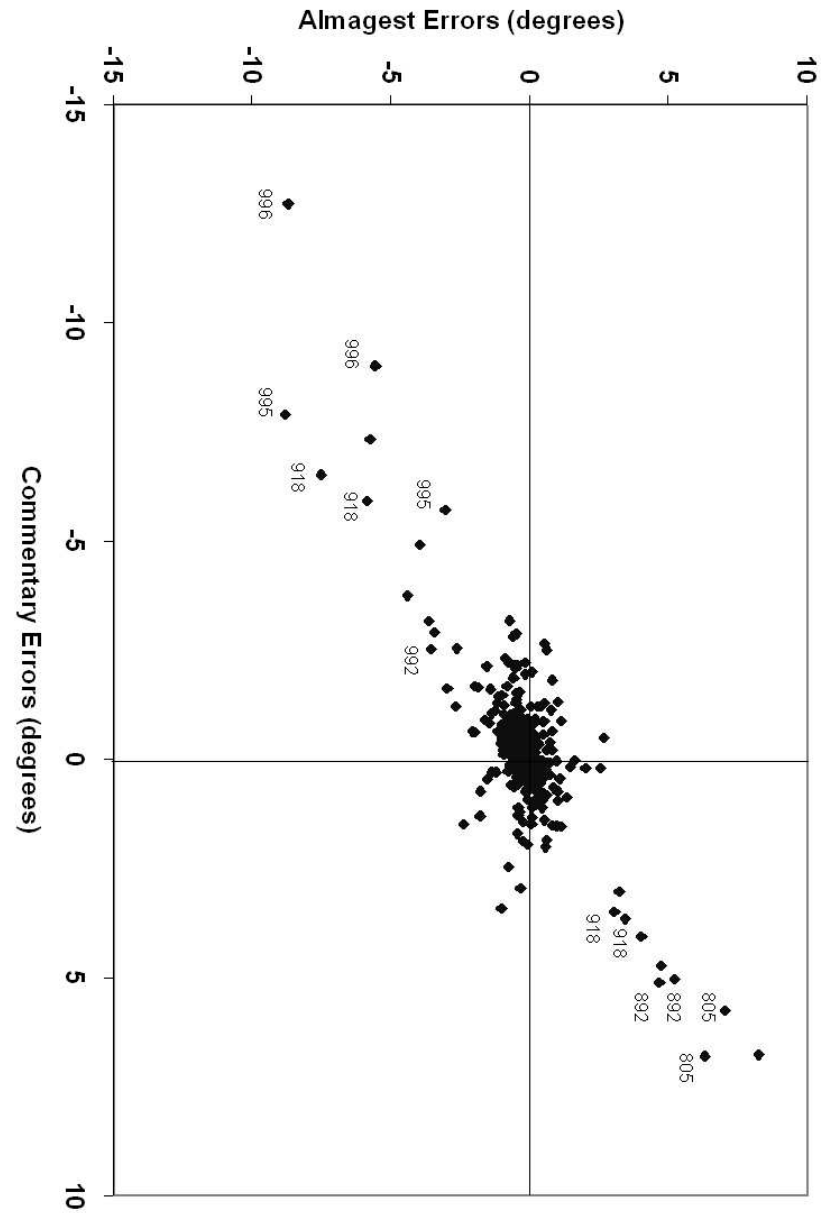

Figure 1: A scatterplot showing the correlation of the *Commentary* and *Almagest* errors for phenomena of types 1-4. Stars with large shared errors are marked with their Baily number (the number of the star in the *Almagest* catalog).

easily seen in the sky (i.e. those with visual magnitude less than 5) reveals that the cataloger included 25 of the 28 stars (89%) within 15° of the ecliptic pole, and 12 of the 20 stars (60%) within 15° of the equatorial pole. Near the center he included 77 of 86 stars (90%) within 3° of the ecliptic, and 39 of 59 stars (66%) within 3° of the equator. Overall, he included 442 of the 730 stars (61%) north of the ecliptic and 444 of the 744 stars (60%) north of the equator, corresponding to a catalog limiting magnitude[8] of just under $V = 5$. Thus the star densities near the equator and its pole are consistent with the overall density of inclusion, while the densities near the ecliptic and its pole are substantially elevated.

However, when using an armillary sphere, either zodiacal or equatorial, it is particularly difficult to accurately measure stars near either the pole or the equator of the system.[9] The statistical error[10] distributions of the *Almagest* coordinates are shown in Figs.‡3-‡3, and they do not reveal any anomalous behaviors near either equator or pole. The fact that the star positions, especially the latitudes near the ecliptic or equator and the longitudes near either pole, are relatively well measured is hard to understand if the measurer used an armillary of any sort.

On the other hand, measuring the star positions in equatorial coordinates does not require an armillary. Indeed, one plausible scenario is that the declinations were determined by measuring the altitude (or zenith distance) of the stars at meridian transit, while the right ascensions could be determined by measuring the distance of the star from the standard star-clock star positions that Hipparchus noted in Book 3 of the *Commentary*.[11] Indeed, there is a much older (*ca.* 700 BC at the latest) Babylonian tradition of *ziqpu* star-clock observations,[12] so it would not be surprising that Hipparchus might have used a similar strategy. In any event, such measurement methods offer an essentially unobstructed view of the ecliptic, the equator, and both associated poles, and thus are much easier to reconcile with the selection of catalogued stars than the idea that an armillary sphere was used for the measurements.

The systematic errors in right ascension and declination are shown in Figures ‡3&‡3,

---

[8] D. Rawlins, *op. cit.* (ref. 5); B. Schaefer, "The latitude of the observer of the Almagest star catalogue, *Journal for the history of astronomy*, 32 (2001), 1-42.

[9] Primarily because the rings themselves obstruct the view of a star near either the pole or the equator of the instrument.

[10] I use the method of Dambis-Efremov to estimate these errors. See A. K. Dambis and Yu. N. Efremov, "Dating Ptolemy's star catalogue through proper motions: the Hipparchan epoch", *Journal for the history of astronomy*, 31 (2000), 115-134. See also D. Duke, "Dating the almagest star catalogue using proper motions: a reconsideration", *Journal for the history of astronomy*, 33 (2002) 45-55, which explains in detail how to separate the statistical and systematic errors.

[11] One way that Hipparchus might have used is to construct a V-shaped instrument with two pieces of wood, perhaps a meter long, with a string across the top of the V, perhaps marked with equal increments of 1/15$^{\text{th}}$ the length of the chord. He would adjust the length of the chord so that the angle is 15°, something he definitely knew how to do. Then, *assuming he has his star-clock table at hand*, he waits until a star transits, and keeping his instrument level, measures the distance, or number of $1/15^{th}$ increments, to the nearest star-clock star. If the target star is near the equator, he is done. If it is at some non-negligible distance from the equator, he would have to correct for what we call the $\cos\delta$ factor, but we know from the *Commentary* he knew how to do that, too. Using the chord as described is equivalent to linear interpolation in a table of chords, but he might have figured out how to do better. For all we know, some reasoning like this *led* him to the table of chords. How did he get the star-clock table? He only needs one star to start, then he can use the above procedure to bootstrap his way around the equator. Presumably he can get that one star from an observation during a lunar eclipse. Certainly his solar theory was adequate to get the accuracy we know he eventually published in the *Commentary* star-clock lists. Or perhaps he used the moon and his lunar theory, which was probably accurate enough near a full moon.

[12] J. Schaumberger, *Zeitschrift fur Assyriologie,* 50 (1942), 42; B. L. van der Waerden, *Science Awakening II: the Birth of Astronomy* (1974) 77-79; D. Pingree and C. Walker, "A Babylonian Star-Catalogue: BM 78161", *A Scientific Humanist: Studies in Memory of Abraham Sachs* (1988), 313-322.

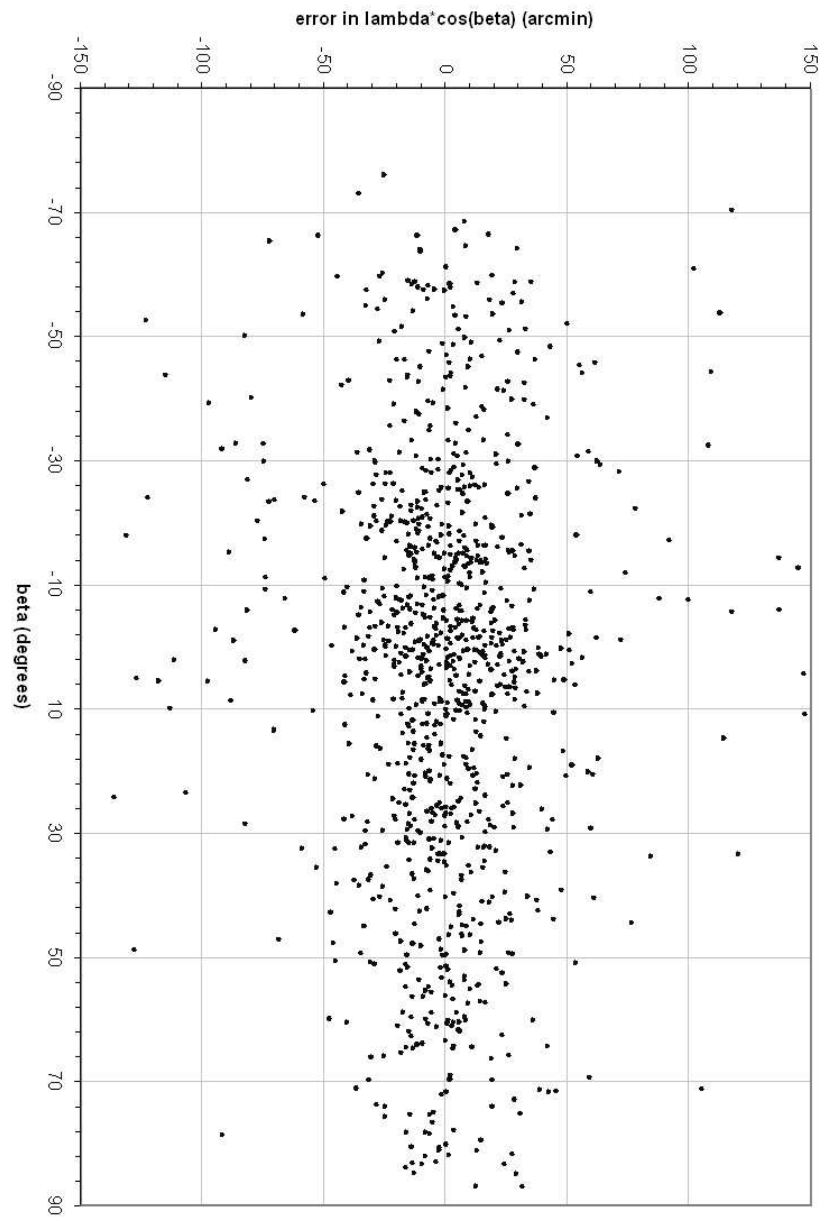

Figure 2: The statistical errors in longitude (reduced to great-circle measure) of the 1,028 *Almagest* star-positions.

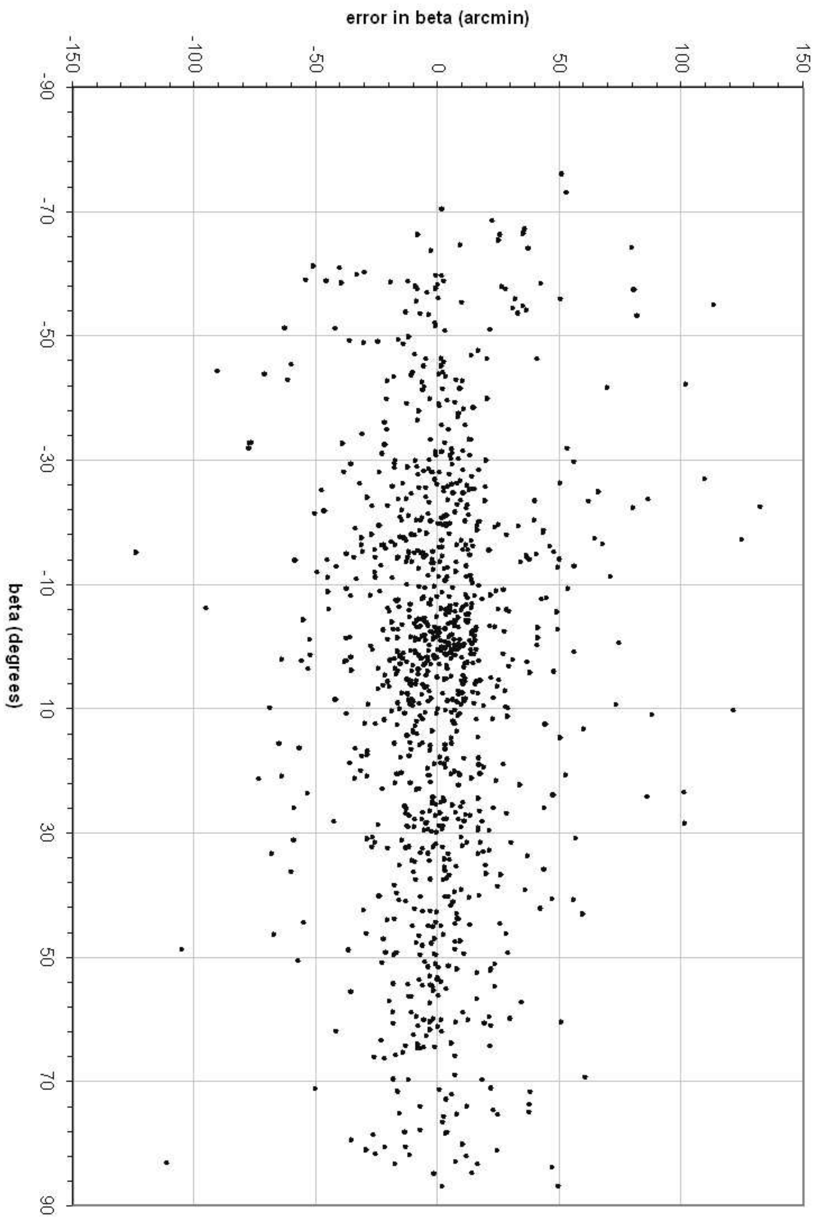

Figure 3: The statistical errors in latitude of the 1,028 *Almagest* stars.

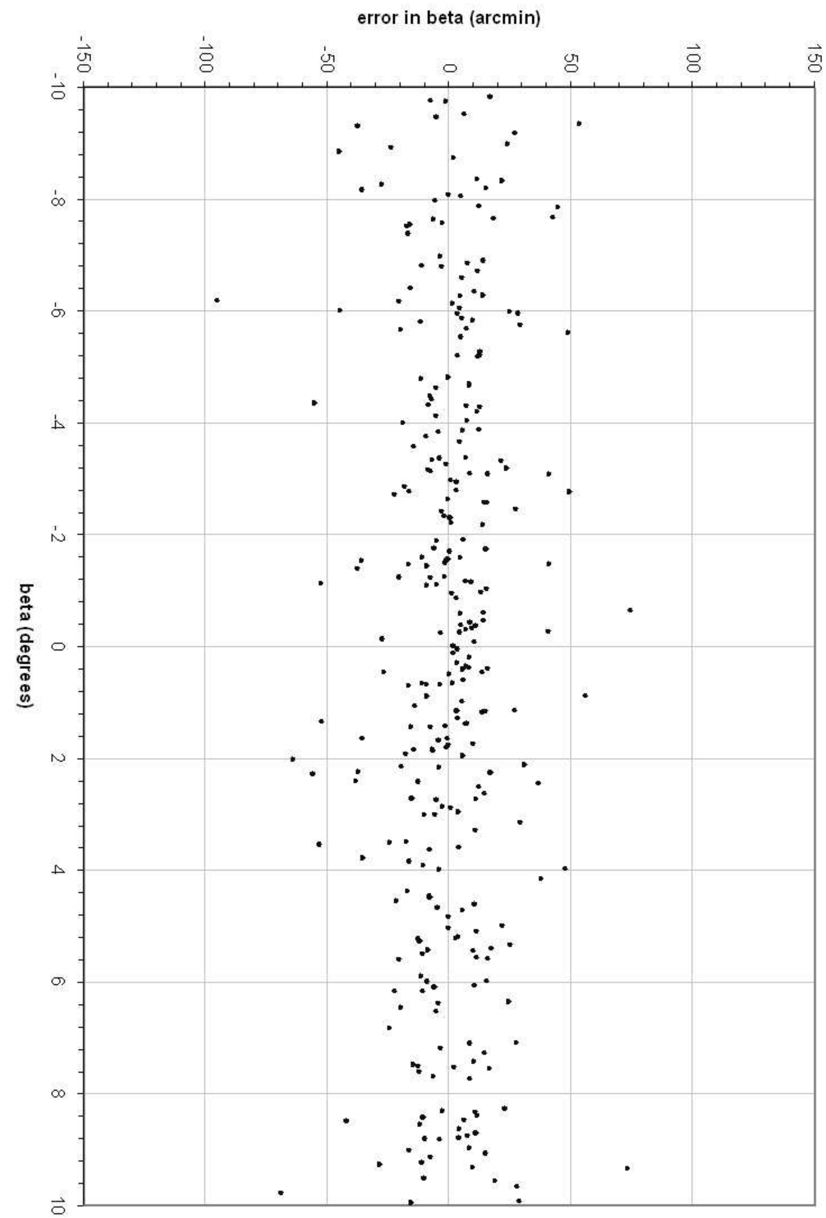

Figure 4: As in Figure ‡3 but looking close to the ecliptic.

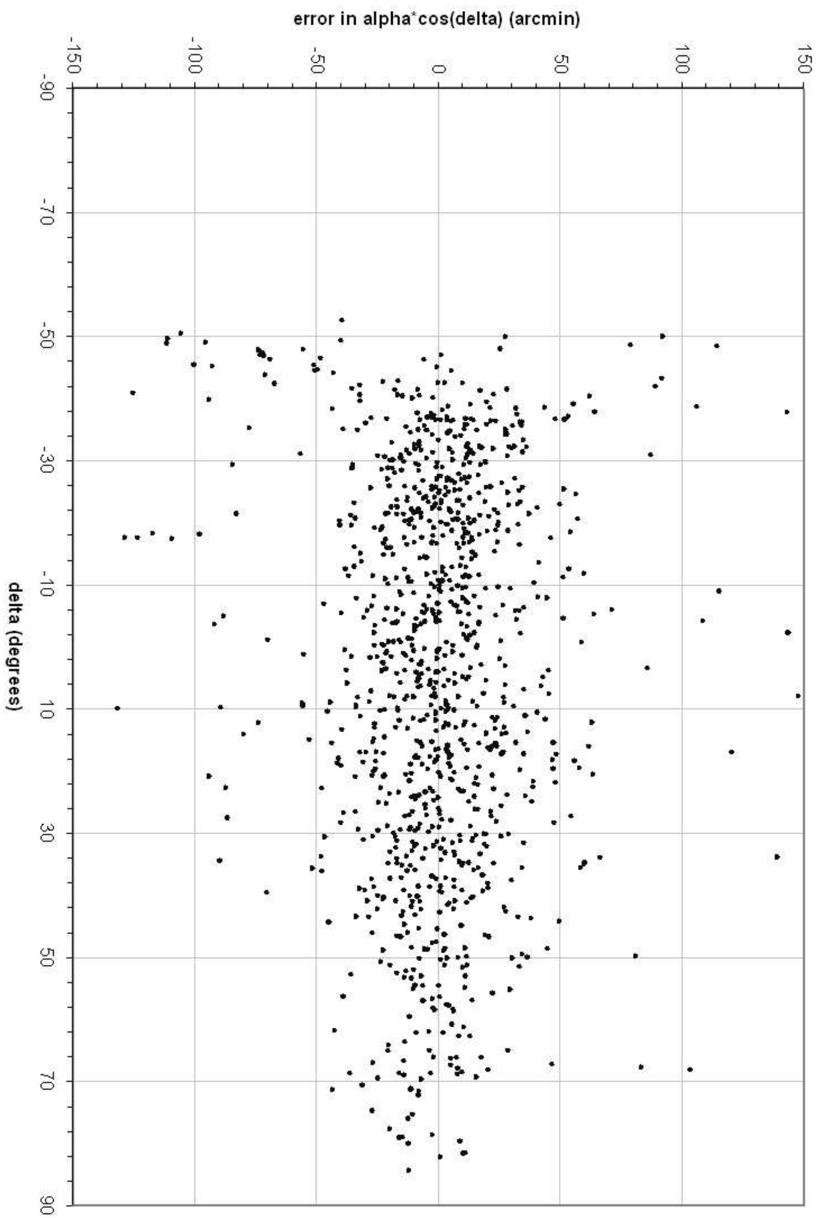

Figure 5: The statistical errors in right ascension (reduced to the great circle) of the 1,028 *Almagest* stars.

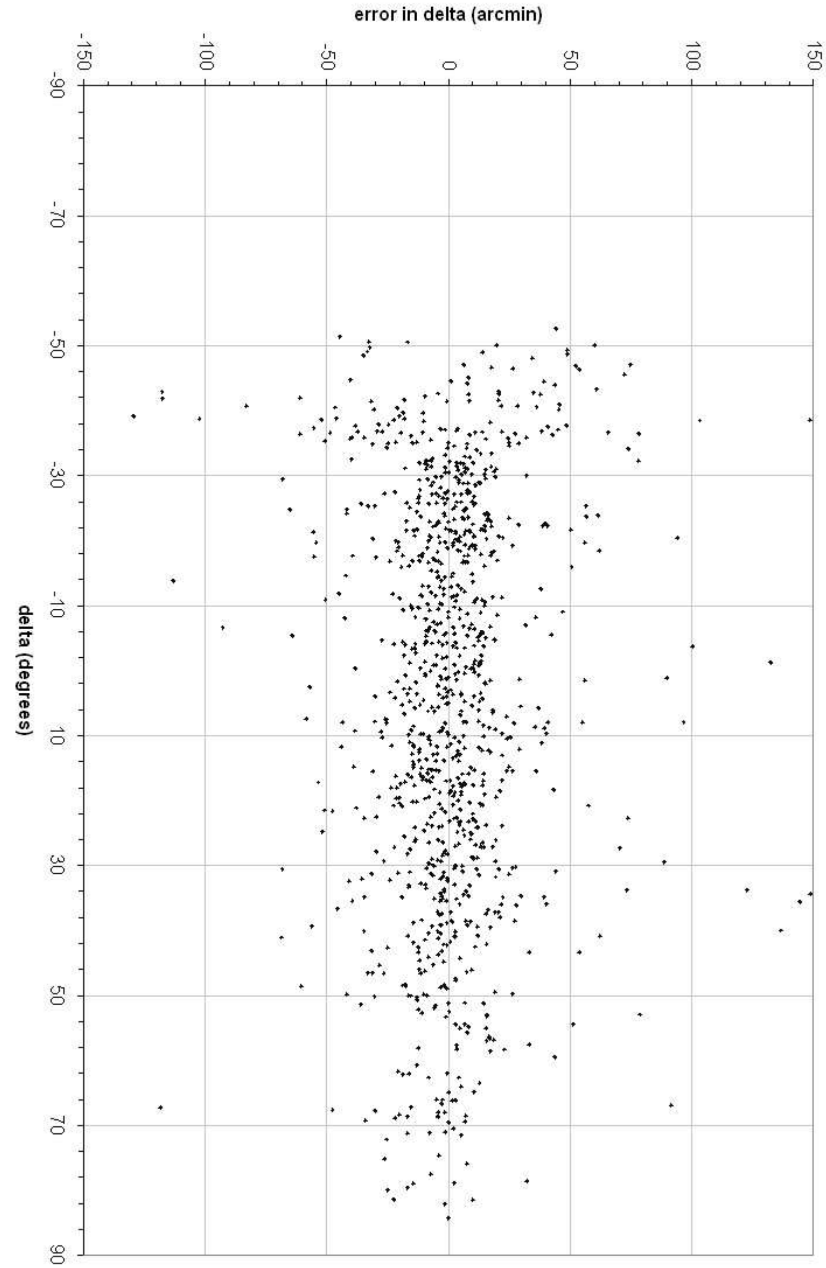

Figure 6: The statistical errors in declination of the 1,028 *Almagest* stars.

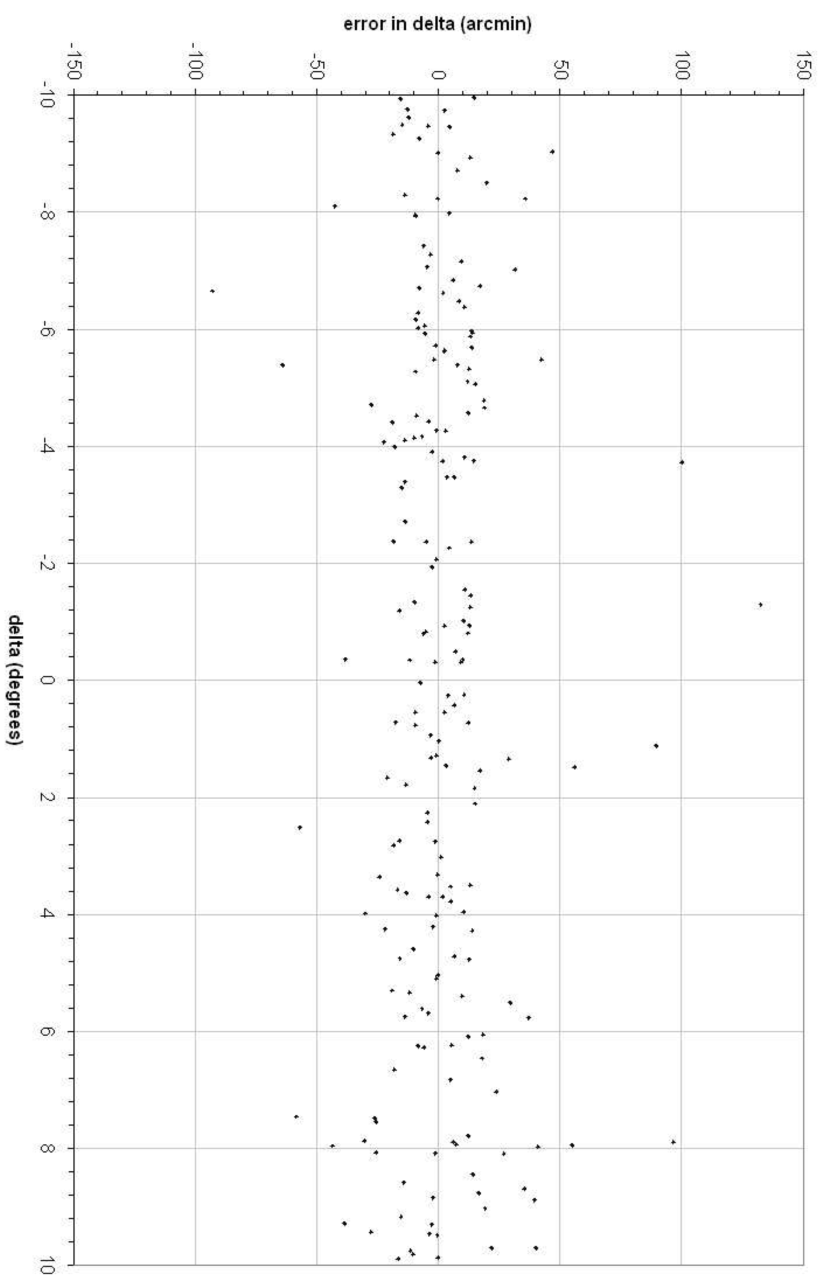

Figure 7: As in Figure ‡3 but looking close to the equator.

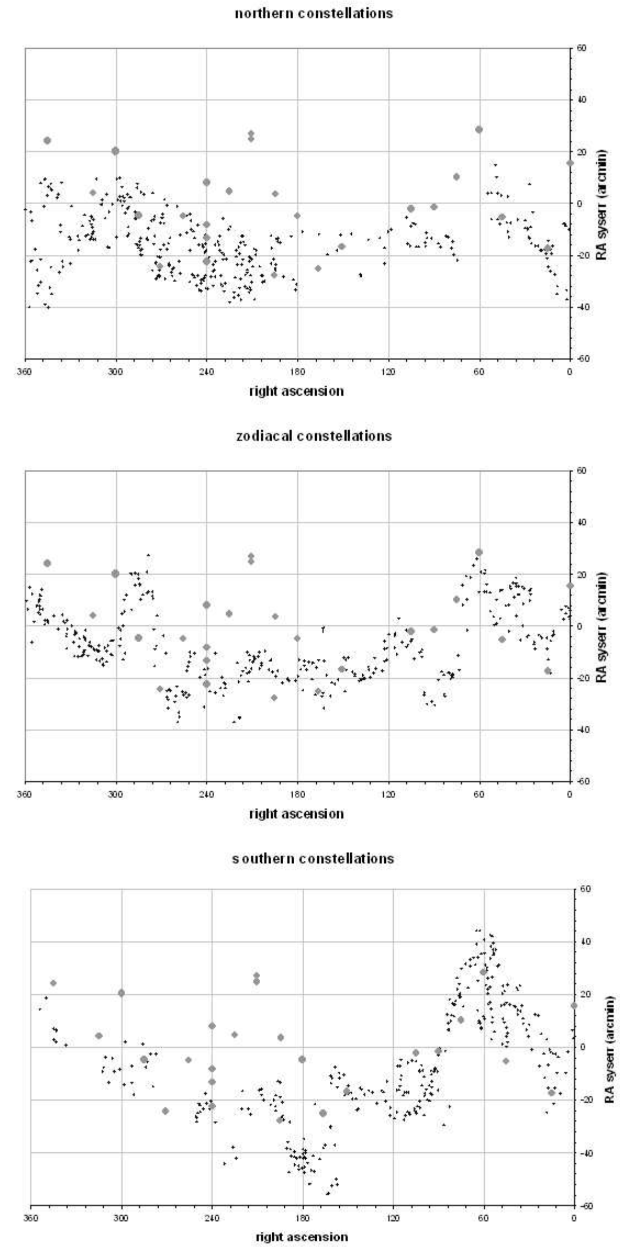

Figure 8: The systematic errors in right ascension, weighted by $\cos\delta$. The larger light circles are the errors in right ascension for the Hipparchan clock-stars with visual magnitude brighter than 4, which might have been used as reference stars to measure the right ascensions of target stars.

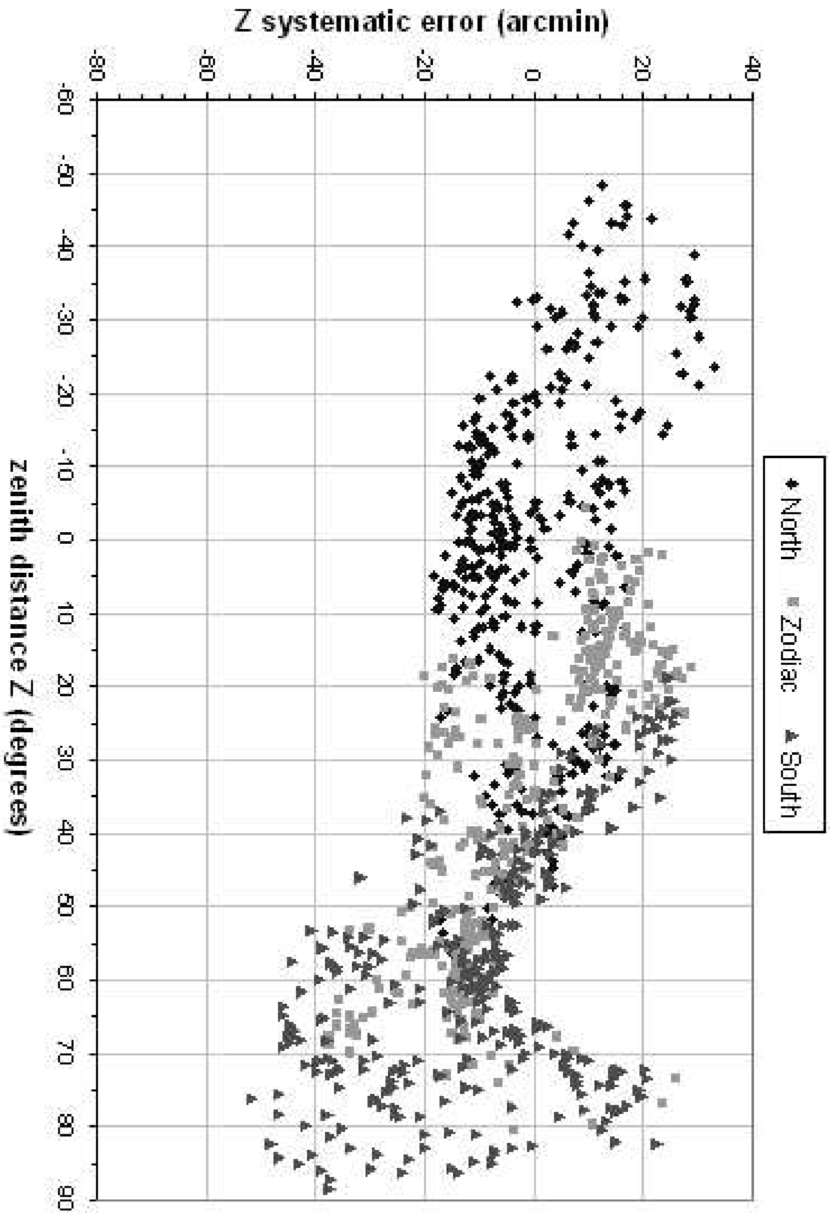

Figure 9: Systematic errors in zenith distance z, where $z = \phi - \delta$. The stars in the northern, zodiacal, and southern constellations are shown separately.

separated by the northern, zodiacal, and southern constellations as grouped by Ptolemy. It is possible, of course, that some other grouping would reveal more interesting information. A comparison of the systematic errors in right ascension of the *Almagest* star positions with the errors in Hipparchus' star-clock positions is shown in Figure ‡3. If the declinations were determined by measuring the zenith distance z at meridian transit using the relationship $\delta = \varphi - z$, where φ is the geographical latitude of the observer, then it is possible that analysis of the data in Figure ‡3, perhaps along the lines suggested by Rawlins,[13] will yield interesting information.

Newton's analysis of the distribution of fractional endings suggests that someone added $n°40'$, with n an integer, to each ecliptical longitude. Thus if the original ecliptical longitude endings had excesses at $00'$ then the *Almagest* longitude endings would have excesses at $40'$, as Newton indeed observed to be the case. One option is that Ptolemy had a catalog that Hipparchus had himself converted to ecliptical coordinates. Another option is that someone did the conversion from equatorial to ecliptic at a later date, perhaps even Ptolemy himself. The sheer quantity of computation would be a good reason to resort to analog computation, no matter who did it. In any event, Ptolemy tells us directly that[14]

> "one has a ready means of identifying those stars which are described differently [by others]; this can be done immediately simply by comparing the recorded positions."

thereby implying that he was not the first to use ecliptical coordinates in a star catalogue.[15]

Table 1 gives the distribution of fractional endings for several groupings of stars. In preparing the table I have subtracted $2°40'$ from the *Almagest* longitude for each star. If the original longitudes were binned like the latitudes, i.e. in bins of $00'$, $10'$, $15'$, $20'$, $30'$, $40'$, $45'$, and $50'$, then the subtraction will unfortunately not recover the original ending distributions, since the original cases of $15'$ and $45'$ cannot occur in the reverse process. This adds a layer of complexity to the analysis of each case. Newton suggested that Ptolemy rounded the $15' + 40' = 55'$ cases to $00'$ and the $45' + 40' = 25'$ cases to $20'$. If so, when we reverse the process the $00' - 40' = 20'$ cases and the $20' - 40' = 40'$ cases will show elevated populations, since some of their members should really be in the nearby $15'$ and $45'$ bins. This should be kept in mind when inspecting the distributions in Table 1.

On the other hand, under the scenario suggested in this paper the declinations and right ascensions were measured with two different instruments, and so it is not obvious that we should expect the same binning of observed values in each case. It is also possible, of course, that the equatorial to ecliptical conversion was a mixture of processes. The fact that the excess of $00'$ and $30'$ endings in latitude occurs for northern and zodiacal stars but not for southern stars,[16] whose endings are consistent with random distribution, was the basis for Rawlins' conclusion[17] that the southern stars were measured in equatorial coordinates by Hipparchus and then transformed to ecliptical using trigonometry. That may well be the case, and is worthy of further investigation. What is important, though, for the scenario suggested in this paper to be true, is that peaks at $00'$ endings appear in longitude and latitude after the conversion process. This would definitely not be the case if the conversion was done exclusively using trigonometry, so it is essential that some form of analog conversion was used, at least for most of the catalog.

---

[13] D. Rawlins, *DIO* 4.1 (1994) 33-47.

[14] *Ptolemy's Almagest*, trans. by G. J. Toomer (London, 1984), p. 340.

[15] And further, since he says the comparison may be done 'immediately' and 'simply', Ptolemy is perhaps also telling us that other star catalogues in ecliptical coordinates were readily available, both for his readers and for himself (Noel Swerdlow, private communication, 2001).

[16] M. Shevchenko, An analysis of errors in the star catalogues of Ptolemy and Ulugh Beg", *Journal for the history of astronomy*, 21 (1990), 187-201.

[17] D. Rawlins, *op. cit.* (ref. fn 13).

	Longitudes							Latitudes								
endings	00	10	20	30	40	50	N	00	10	15	20	30	40	45	50	N
All Stars																
N	96	45	62	61	68	29	361	110	30	34	38	76	38	10	25	361
Z	94	24	82	52	58	35	345	66	31	28	33	81	50	21	40	350
S	51	32	79	65	58	32	317	50	44	33	40	53	40	17	40	317
total	241	101	223	178	184	96	1023	226	105	95	111	210	128	48	105	1028
Constellated Stars																
N	87	41	54	56	66	28	332	101	24	32	35	72	34	9	25	332
Z	83	23	63	39	50	29	287	60	25	22	23	65	38	20	37	290
S	51	30	70	62	55	30	298	47	42	31	38	50	37	16	37	298
total	221	94	187	157	171	87	917	208	91	85	96	187	109	45	99	920
Unformed Stars																
N	9	4	8	5	2	1	29	9	6	2	3	4	4	1	0	29
Z	11	1	19	13	8	6	58	6	6	6	10	16	12	1	3	60
S	0	2	9	3	3	2	19	3	2	2	2	3	3	1	3	19
total	20	7	36	21	13	9	106	18	14	10	15	23	19	3	6	108
Bins of Declination																
45° to 90°	33	10	18	22	21	11	115	28	4	16	14	24	15	2	12	115
15° to 45°	68	28	50	47	54	24	271	79	27	25	33	55	29	9	15	272
−15° to 15°	66	21	74	38	58	33	290	56	33	26	26	64	41	15	33	294
−45° to −15°	70	41	73	64	47	27	322	61	35	26	32	66	40	19	43	322
−90° to −45°	4	1	8	7	4	1	25	2	6	2	6	1	3	3	2	25
total	241	101	223	178	184	96	1023	226	105	95	111	210	128	48	105	1028
Bins of Right Ascension																
0° to 90°	64	31	70	41	48	26	280	64	26	29	29	56	30	14	33	281
90° to 180°	27	9	51	51	45	24	207	35	28	23	28	38	30	7	21	210
180° to 270°	81	30	52	43	47	21	274	55	29	26	29	64	32	9	30	274
270° to 360°	69	31	50	43	44	25	262	72	22	17	25	52	36	18	21	263
total	241	101	223	178	184	96	1023	226	105	95	111	210	128	48	105	1028

Table 1: The number of fractional endings in longitude and latitude for various groupings of stars. The longitudes have $2°40'$ subtracted from their *Almagest* values. N, Z, and S in the first column denote the groups of northern, zodiacal, and southern constellations as defined in the *Almagest*. The five *Almagest* stars with longitudes ending in $15'$ are omitted from the longitude half of the table.

The scenario suggested in this paper differs from previous interpretations in various ways:

• Some previous interpretations of Hipparchus' catalog are that if he had one at all, it was expressed in a mixed system of non-orthogonal coordinates: declinations and polar longitudes.[18]

• Some authors thought that the analysis of Vogt[19] provided conclusive proof that the *Almagest* coordinates are original to Ptolemy, at least in large part.[20]

• Some authors have suggested that one way to understand the structure of the *Almagest* catalog is to assume certain reference stars were used to measure the ecliptical coordinates on a constellation-by-constellation basis.[21]

• Some authors have suggested that Hipparchus measured his catalog of star coordinates directly in ecliptical longitude and latitude, probably using a zodiacal armillary sphere.[22] A partial exception, mentioned above, is the suggestion of Rawlins[23] that the southern stars were measured in equatorial coordinates.

Hopefully it will be fairly straightforward to find or cite additional evidence that either strengthens or refutes the suggested model. The following list of questions, while no doubt incomplete, represents issues that would likely benefit from additional thoughtful consideration:

• When, where, and how were the original equatorial measurements made? Also interesting, but perhaps hard to answer, is whether it was Hipparchus or someone else who made the measurements.

• Can one identify any *Almagest* catalog stars that were likely *not* measured in equatorial coordinates? How many independent sources of coordinates do we find in the *Almagest* catalog?

• What was the precision of the coordinates quoted in the original equatorial catalog? And related, how did the $10'$ bin sizes in the *Almagest* arise?

• When and how was the transformation from equatorial to ecliptical coordinates accomplished?

Was a zodiacal armillary ever used by any ancient astronomer? The scenario suggested in this paper certainly does not require that either Hipparchus or Ptolemy ever used one for measuring star positions. It is quite possible, though, that one was used for measuring elongations near the zodiac between stars, planets, and the Moon. We have numerous records of an Arabic tradition of the zodiacal armillary,[24] probably inspired by Ptolemy's description in the *Almagest*, but we have no surviving records of any substantial set of measurements made with one before the time of Ulugh Beg (ca. 1437). Applying the

[18] See, for example, O. Neugebauer, *A history of ancient mathematical astronomy*, (3 vols., Berlin, 1975), p. 277-80; G. J. Toomer, *Hipparchus*, Dictionary of Scientific Biography 15 (1978), p. 217; J. Evans, *The History and Practice of Ancient Astronomy* (New York, 1998), p. 103; G. Graßhoff, "Normal star observations in late Babylonian astronomical diaries", *Ancient astronomy and Celestial Divination* (1999), ed. N. Swerdlow, p 127 and footnote 23.

[19] H. Vogt, "Versuch einer Wiederstellung von Hipparchs Fixsternverzeichnis", *Astronomische Nachrichten*, 224 (1925), cols 2-54.

[20] O. Neugebauer, *op. cit.* (ref. 18), p. 280-4; G. J. Toomer, *op. cit.* (ref. 18), p. 217; N. M. Swerdlow, "The enigma of Ptolemy's catalogue of stars", *Journal for the history of astronomy*, 23 (1992), 173-183; J. Evans, "The Ptolemaic star catalogue", *Journal for the History of Astronomy*, 23 (1992), 64-68.

[21] J. Evans, "On the origins of the Ptolemaic star catalogue", *Journal for the History of Astronomy*, 18 (1987), 155-172, 233-278; M. Shevchenko, *op. cit.* (ref. 16); J. Włodarczyk, "Notes on the compilation of Ptolemy's catalogue of stars", *Journal for the History of Astronomy*, 21 (1990), 283-95.

[22] R. R. Newton, *op. cit.* (ref. 5) 255-6; D. Rawlins, *op. cit.* (ref. 13); K. Pickering, *DIO* 9.1 (1999), 26-29.

[23] D. Rawlins, *op. cit.* (ref. 13).

[24] Aydin Sayili, *The Observatory in Islam* (1960).

fractional ending test to Ulugh Beg's catalog seems to indicate that his data were measured in ecliptical coordinates,[25] but we also know that he had many other instruments to use, and we have little information about how any of his measurements were made. We know that Tycho Brahe built one but found it so difficult to use that he quickly abandoned using it.[26] It is possible that Ulugh Beg's catalog might provide a useful test case for further investigation of some of the issues raised in this paper.

ACKNOWLEDGEMENTS

I am grateful to Alexander Jones, Bradley Schaefer, and Hugh Thurston for helpful comments on drafts of this paper.

[25] James Evans, *op. cit.* (ref. 21).
[26] Victor Thoren, *The Lord of Uraniborg: a Biography of Tycho Brahe*, (1991), p. 165.

‡4 The instruments used by Hipparchos

by KEITH A.PICKERING

A A revealing gap

A1 More than 2000 years after its compilation, it is now possible to determine with some confidence the kinds of instruments that were used to observe the stars of the Ancient Star Catalog, and in which parts of the sky the various instruments were employed. The fact that there were multiple instruments, and that the ASC was not, as stated by Ptolemy, observed with a single ecliptical astrolabe, does more than provide yet-another proof that the catalog was observed by Hipparchos (for we have more than enough of those already); it also allows us a glimpse into the hitherto unknown workings of astronomy and instrument manufacture as they were practiced at a critical point of ancient Greek science.

A2 The *Almagest* divides the ASC into three sections, for the northern, zodiacal, and southern parts of the sky. Looking at the northern sky, figure 1 plots the absolute errors in longitude for each northern star in the ASC, according to its actual longitude at the epoch of the catalog (which we will take to be -128.0). In looking at the plot, note particularly that there is an odd gap in the plotted stars at about 70° ecliptic longitude. Note also that there is a similar gap at about 250° ecliptic longitude, exactly 180° away. This gap can be more easily seen if we overlay the second half of the longitudes (180-360) on top of the first half, as we have done in figure 2. Note particularly that the longitude errors increase in absolute magnitude as we get close to the gap. For purposes of comparison, figure 3 plots the absolute errors in right ascension by right ascension: the gap disappears.

A3 This gap is significant because it shows us, first, that the northern sky was observed primarily with a single instrument; second, that the instrument was an ecliptic astrolabe; and third, that the astrolabe was of a somewhat different design from the description given by Ptolemy, and indeed different from any previously known to have existed.

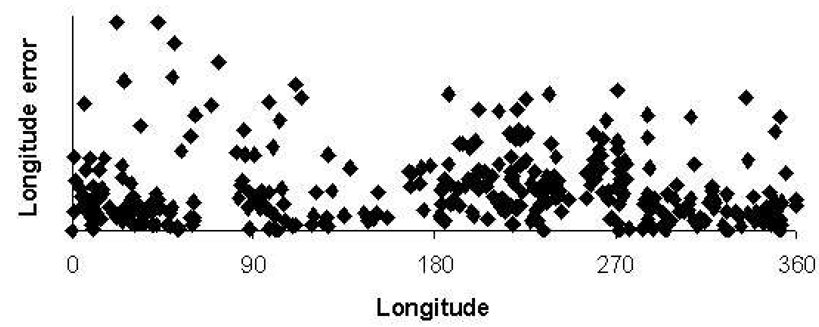

Figure 1: Errors in longitude by longitude, for northern stars in the ASC. Note vertical gaps in the data.

B The astrolabe

B1 The armillary astrolabe used in ancient Greek astronomy is, at first glance, a bewildering maze of nested rings, fitted closely inside each other, that rotate in complex

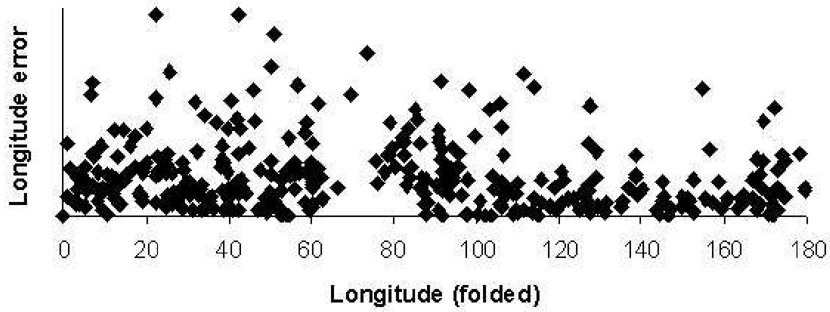

Figure 2: Errors in longitude by longitude, with longitudes overlain. Note vertical gap in the data.

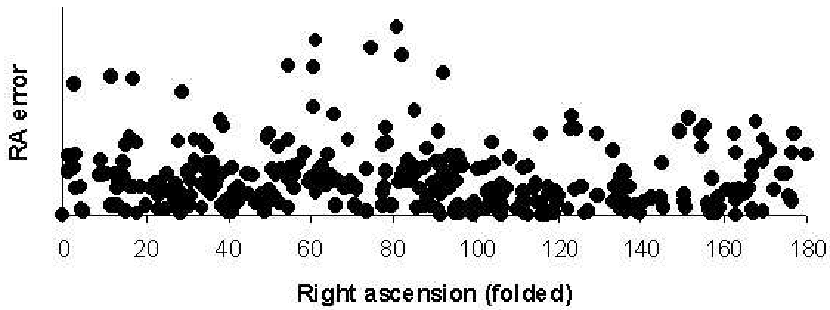

Figure 3: Errors in right ascension by right ascension, for northern stars in the ASC. Note lack of vertical gaps in the data.

ways. Let's look at the way an armillary astrolabe is contructed, from the inside out. The innermost ring (Ring 1) contains a pair of sighting holes or pinnules, diametrically opposite each other, through which the star is sighted. Immediately surrounding Ring 1 is Ring 2, whose inside diameter is fractionally larger than the outside diameter of Ring 1. Ring 1 is constrained so that it rotates inside Ring 2, in the same plane, their edges just touching. Ring 2 has a scale of degrees on its edge, indicating the rotational position of the pinnules on Ring 1. (See figure 4.) If we wished, we could mount Ring 2 on the meridian, and then use the Ring 1&2 assembly as a transit instrument. To do this, we would have to orient Ring 2 so that it points north-south, and so that its zero-degree points on the scale were horizontal, and 90-degree points were vertical.

B2 But to make the Ring 1&2 assembly more useful, we will mount it differently. We construct an outer ring, Ring 3, set vertically so that the whole Ring 1&2 assembly can pivot within it, around a vertical axis. We run axle pins from the 90-degree poles of Ring 2 into the inner edge of Ring 3; so now Ring 1&2 can rotate to any azimuth. To determine the azimuth at which Ring 1&2 is pointing, we add Ring 4, which is fixed horizontally and at right angles to Ring 3. Ring 4 carries another scale of degrees, indicating the rotational position of Ring 2. Rings 3&4 now form a cage, within which Ring 2 rotates freely in

azimuth, while Ring 1 rotates freely in altitude within Ring 2. (See figure 4). The instrument can now be used as a theodolite, since we can determine the altitude and azimuth of any star with it. We will call this arrangement the 4-ring instrument.

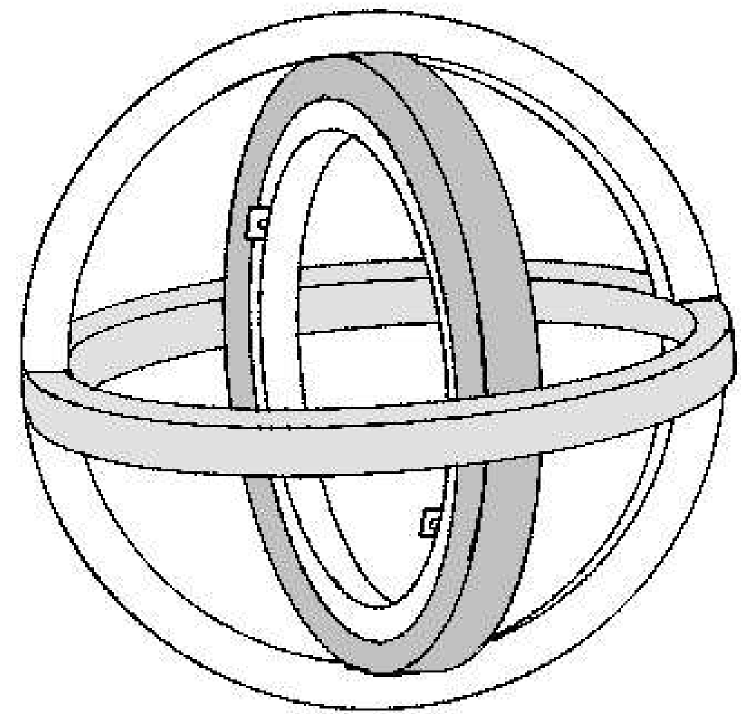

Figure 4: The four-ring instrument. Ring 1 (innermost white) carries pinnules through which the star is sighted. Ring 2 (dark gray) has a scale of degrees. Ring 3 (outer white) holds the polar axis. Ring 4 (light gray) contains the second scale of degrees. When Ring 4 is horizontal, the instrument can be used as a theodolite; when mounted to rotate with the sky, it is an astrolabe.

B3 The 4-ring instrument is capable of pointing to almost any point in the celestial sphere, making it quite useful. In fact, there is only one fly in the ointment to this whole arrangement: at certain rotational positions, Ring 2 becomes so closely aligned with Ring 3 that a star cannot be seen through the pinnules, because Ring 3 gets in the way. There are two such rotational positions, exactly 180° apart. For the same reason, it is impossible to observe very near to the horizon, because Ring 4 gets in the way.[1]

B4 A larger issue with the 4-ring instrument is one of orientation. With Ring 4 oriented horizontally, it makes a fine theodolite, but horizon-based coordinates are of limited utility in astronomy, because the sky moves as the earth rotates. It is much better to mount the

[1] Areas near the axis are also blocked by both Ring 3 and Ring 2.

4-ring instrument so that it rotates too, following the sky.

B5 Recall that the purpose of Ring 3 is entirely structural: it holds the axis around which Rings 1&2 rotate. So the most obvious arrangement is to simply extend that axis, and orient the axis toward the celestial pole. Then the entire 4-ring instrument could be rotated along with the sky. The astrolabe, if mounted this way, would read equatorial coordinates directly, because Ring 4 would be permanently aligned with the celestial equator. All that would be needed would be a way to align the instrument in right ascension.

B6 Although equatorial coordinates are used extensively today, in ancient times ecliptical coordinates were more widely used. So in practice, what was really needed was a way to mount the 4-ring instrument so that: (a) it could rotate with the sky; and (b) Ring 4 would be aligned with the ecliptic instead of the celestial equator. And in the *Almagest* V.1, Ptolemy describes how this was done: a second axis was drilled in Ring 3 (this would have been $23°51'$ from the first). Then the 4-ring instrument was mounted so that the second axis was pointed to the celestial pole. The entire instrument could then rotate (around the polar axis) to follow the sky; while the coordinate readings from Ring 2 and Ring 4 are stuck in a different coordinate frame, tilted in exactly the same manner as the ecliptic is tilted to the equator. And there we have it: the ecliptic armillary astrobale, nearly the same as described by Ptolemy in the *Almagest*.[2]

B7 Except for one big thing. When we drilled the polar axis in Ring 3, at that moment we permanently fixed the ecliptic longitude of Ring 3 along the 90°-270° solstitial colure. This is the great circle in the sky through which both the ecliptic poles and celestial poles run, and now this colure must also run through Ring 3 too, since both instrumental axes run through Ring 3. Now we know that Ring 3 will get in the way of some observations, so if we build an astrolabe this way — as described by Ptolemy — we should expect there to be a gap in observed stars at 90° and 270° ecliptic longitude. As we have seen, there is a longitudinal gap, but it is not at 90°-270°; it is at 70°-250°. This means that the astrolabe which Hipparchos actually used to observe the ASC was built in a somewhat different manner than the one described by Ptolemy in the *Almagest*.

B8 Instead of drilling a second set of axis holes in Ring 3, Hipparchos (or his instrument maker) must have used separate bearing journals to hold the polar axis. There would be two such journals clamped or affixed to opposite sides of Ring 3 at the celestial poles (see figure 5). Since the solstitial colure (which defines 90-270 ecliptic longitude) must contain both axes, the colure no longer contains Ring 3; rather, it is offset by some amount. In the instrument actually used by Hipparchos, this amount was about 20 degrees. This arrangement has a structural advantage, because it avoids putting another set of holes in Ring 3, which has already been weakened by the holes for the ecliptic axis.

B9 If he had used more than one astrolabe for observing the Northern sky, Hipparchos could have arranged to have the journals on astrolabe #2 mounted on the opposite sides of Ring 3 than the arrangement on astrolabe #1; so the blind spot of astrolabe #2 would be at 110-290, and the blind spot of one instrument could be covered by the other. Therefore it is apparent that large parts of the northern sky were observed with a single instrument — or nearly so.

B10 You recall that there is another blind spot, along Ring 4. This ring falls right on the ecliptic, so we might expect to see a gap in the data here, too, just as we found in the longitudes. In the northern sky, only one constellation (Ophiuchus) dips all the way down to the ecliptic; but there is no such gap along the ecliptic in Ophiuchus. In fact, there is no such gap among the stars of the zodiacal constellations, either.

[2] For purposes of simplicity, we have left out Ring 5, which is used only as an aid to orientation. Ring 6, which is fixed to the earth as a strutctural support for the whole instrument, holds the second axis, which points toward the North Celestial Pole.

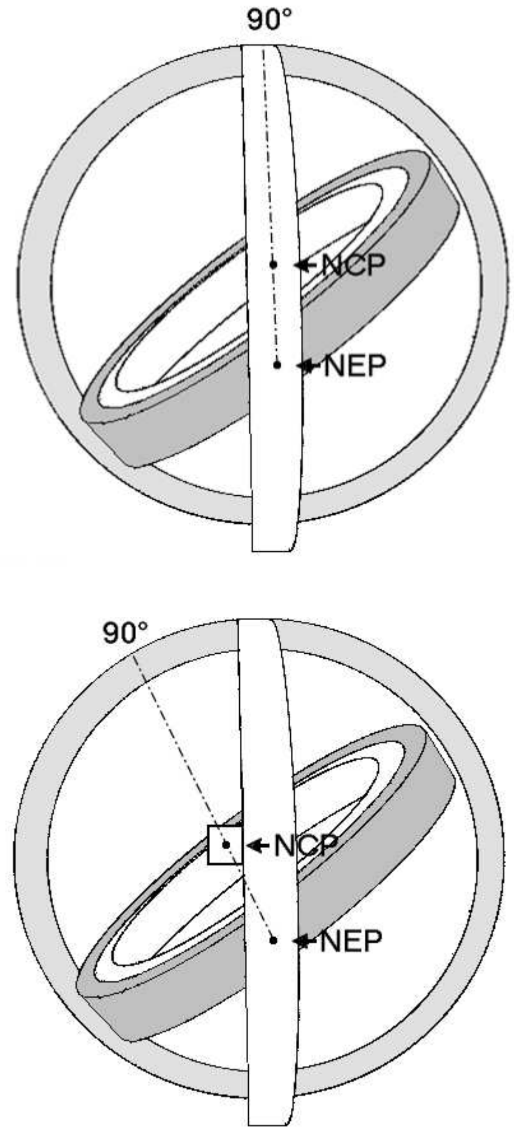

Figure 5: The four-ring instrument viewed from above the poles. Ring 2 (dark gray) pivots around the North Ecliptic Pole (NEP), while the entire 4-ring instrument rotates around the North Celestial Pole (NCP) to follow the sky. (The NCP axis is affixed to an outer Ring 6, which is not shown in the diagram.) According to the *Almagest*, the NCP axis is drilled in Ring 3 (top); in the astrolabe used by Hipparchos, the NCP axis is carried on a separate bearing journal (bottom). The 90-270 solstitial colure is the great circle joining the two axes — along Ring 3 (top) or offset from it (bottom).

B11 So there must have been a different instrument or a different technique (or both) for observing right at the ecliptic. One possibility is a second set of pinnules.[3] The primary pinnules would be mounted on Ring 1 at diametrically opposite positions, as already described; while the second set would be mounted above these, a little more than one ring-width away. Thus, the sightline through the first set would be exactly parallel to the sightline through the second set. When the first set was too close to the ecliptic to observe, the second set would still be able to see over the top of Ring 4.

B12 I have been unable to find similar gaps in either the Zodiac or the South sections of the ASC. This implies that the instrument used in the North was different than the one(s) used in other parts of the sky.

C Gap Characteristics

C1 Are there bright stars in the gap that Hipparchos usually would have taken, or is the reason for the lack of cataloged stars simply that there are no bright stars in this region of the sky? In other words, is the gap real? As it turns out, there are only five stars in the Northern sky brighter than magnitude 3.9 that Hipparchos left out of the catalog: χ UMa, α Lac, 46 LMi, 109 Her, and α Sct. Two of these five (109 Her and α Sct) are in the gap. Since the gap represents only about 5% of the sky, this is clearly a significant number.

C2 The gap is caused by the physical presence of Ring 3, which has a constant physical width. But the longitudinal width of Ring 3 increases toward the ecliptic pole, because the lines of longitude converge there. We can determine the relative thickness of Ring 3 by close examination of the edges of the gap. In figure 6, I have plotted the region near the gap in latitude and "folded" longitude, along with lines indicating the position that the gap would have if Ring 3 was centered at 69.5 - 249.5 and had a width of 3.7 degrees. These parameters fit the actual gap quite well (although smaller widths cannot be excluded).

C3 Similarly, in figure 7, I have plotted all stars of magnitude 4.5 or brighter that are missing from the catalog, with the same gap limits. Note particularly that there are no missing stars this bright above latitude 75. This is a good indication of the polar limits of the astrolabe, and shows the region in which a different instrument was probably used. This also explains why there are a couple of holdout stars present in the gap: the holdouts are both at very high latitudes.

C4 The edges of the gap are between 4 and 5 degrees apart at the ecliptic. The exact edges depend on how far north one chooses to assume was observed with this single instrument. The gap is actually two adjacent gaps: one in which the lower pinnule is blocked by Ring 3, and one in which the upper pinnule is blocked. Therefore, the 5-degree width of the gap implies that the physical width of Ring 3 was between 2 and $2^{\circ}.5$ degrees. The exact center of the gap is a bit tricky to pin down, but it seems to be very close to $69^{\circ}.5$ of ecliptic longitude.

C5 Further, between these two adjacent gaps, at the very center, there is a very narrow "gap within the gap," where a star lying at that precise longitude should be visible. This is because, when Ring 2 is *exactly* aligned with Ring 3, neither pinnule on Ring 1 is blocked by Ring 3: the line of sight passes along the edge of Ring 3 just as if it were a wide extension of Ring 2. As it turns out, there is in fact a cataloged star lying almost exactly at the center of the gap: ϵ UMi. But since it also lies at a very high latitude, there is no guarantee that it was observed through the "gap within the gap," rather than in the same manner as other stars near the ecliptic pole.

C6 A ring about 2 degrees wide is rather narrow, structurally speaking, which in turn places limits on the material used to construct the astrolabe. For example, if Ring 3 was 50 cm in diameter, it could be no more than about 1 cm (perhaps less) in width. I tend to doubt that a wooden instrument of this narrow aspect ratio could be stiff enough against the weight it must support to be very accurate; bronze seems a more likely material.

[3] Ptolemy does not mention a second set of pinnules in the *Almagest*, but given the foregoing, he cannot be taken as a wholly reliable source on the construction of astrolabes.

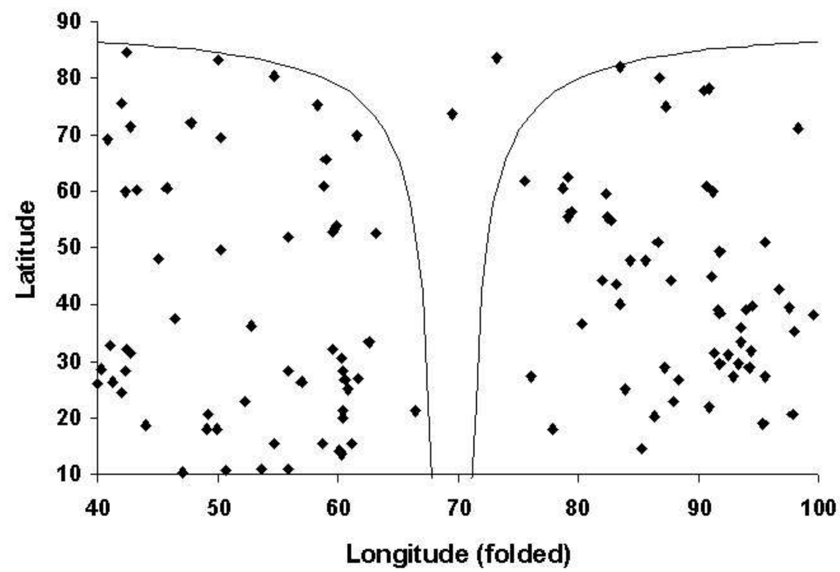

Figure 6: Close view of the gap in cataloged stars. The lines show the limits of a gap 3.7 degrees wide centered at 69.5 - 249.5 degrees longitude.

D Epoch of the Northern Catalog

D1 The single-instrument hypothesis implies that the northern sky was observed all at once, before the instrument had time to become worn or damaged; in other words, a matter of months or a few years, rather than decades. Careful analysis will allow us to determine the epoch of this northern observational effort.

D2 After subtracting Ptolemy's $2^{\circ}\ {}^{2}/_{3}$ false precessional constant, we can reconstruct the actual longitudes of these stars as observed by Hipparchos. Due to precession, stars advance from west to east parallel to the ecliptic, maintaining their same ecliptic latitudes, but increasing their ecliptic longitudes at a rate of about $83'$ per century. So, as a first cut, we can simply take these reconstructed Hipparchan longitudes and assume that they were (on average) correct as measured, then find the epoch at which such an assumption would be true. For the northern stars, this works out to -157 ± 59 years.

D3 There is a problem with this procedure, however, because the longitudes observed by Hipparchos were not actually correct, on average. There is a systematic error which we must account for. The longitude of the stars is determined ultimately by reference to the Sun. The Sun is observed just before sunset, on a day just after new Moon. The longitude of the Sun is known from theory, and the difference between the Sun and Moon gives the Moon's longitude; then, after sunset of the same day, the difference between the Moon and a fundamental star is observed, to give the longitude of the fundamental star; and finally, the longitudes of individual stars are observed by their difference from the fundamental star. But each of these steps requires the astrolabe to be briefly clamped in position while the

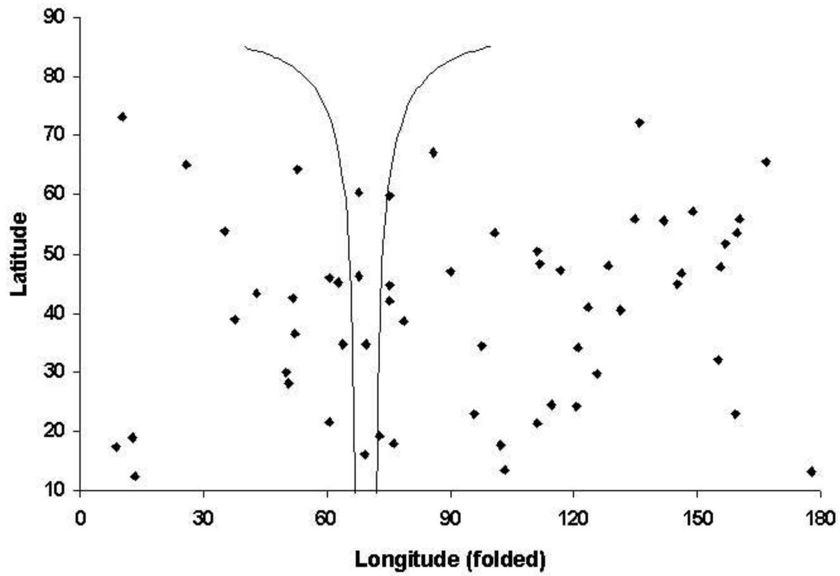

Figure 7: Stars brighter than magnitude 4.5 missing from the northern part of the ASC. Note that none are missing above latitude 75°, possibly indicating that a different kind of instrument was used in this small region of the sky.

measurements are being made; and these successive clampings tend to push the longitudes lower than true, because the earth rotates during these brief intervals. In other words, there is a systematic error in rotation of the astrolabe around the equatorial axis.

D4 Rawlins 1982 has shown that misrotation of the astrolabe with respect to the real sky will make itself known by the presence of a cosine error wave in the observed latitudes. Further, the amplitude of this cosine error wave is proportional to the amount of astrolabe misrotation. And in fact there is just such an error in the latitudes of the northern stars. This error wave has an amplitude of 10.6 $\pm$1.8 arcmin, implying that the astrolabe was systematically misrotated by 24.2 $\pm$4.2 arcmin. It took precession 29.2 years to move a star that far in longitude, meaning that the actual epoch of observation for the northern stars was -128 $\pm$59 years. This is very nearly the epoch implied by Ptolemy's precessional constant.

References

Rawlins, Dennis (1982). An Investigation of the Ancient Star Catalog. *PASP* 94, 359.
Toomer, G.J. (1998). *Ptolemy's Almagest*. Princeton University Press.

‡5 A Re-identification of some entries in the Ancient Star Catalog

by KEITH A.PICKERING

A Introduction

A1 The realization that the Ancient Star Catalog (ASC) is in fact a precessed version of the earlier catalog of Hipparchos leads research in some fruitful directions. It has already been shown[1] that some entries in the ASC were originally observed using equatorial coordinates; and it has been shown[2] that at least some entries in Hipparchos's *Commentary on Aratus and Eudoxus* were originally observed using ecliptical coordinates; and we also know[3] that there is a strong correlation between positional errors in the *Commentary* and errors in the ASC. The emerging picture tends to support Graßhoff's supposition that the *Commentary* and the ASC were both derived from a common "proto-catalog" of observations, but this proto-catalog was observed with various instruments, recorded in various co-ordinates, and perhaps also observed from various locations at various times.

A2 This realization allows us to broaden our perspective when identifying certain stars in the ASC which have had troublesome identifications in the past. The number of possible errors that might have been encountered between the recordation of a datum at the time of observation, and the centuries-later recordation in extant manuscripts, has grown larger, and so has the range of likely possibilites to explain such errors. In particular, the possibility that stars may have been observed and originally recorded in equatorial[4] coordinates (rather than the ecliptical coordinates of the ASC as written) expands the range of likely scribal errors.

B Common errors

B1 Ancient Greek was written in uncial (single case) characters, and numbers were written using letters, in the following fashion:

A	1	Z	7
B	2	H	8
Γ	3	θ	9
Δ	4	ι	10
ϵ	5	κ	20
ς	6	Λ	30

So, for example, 32 would be written: ΛB. Fractions are written using reciprocal integers and their sums, indicated by appending the integer with a prime symbol ('). Thus, 1/2 is B', 1/6 is ς', and 3/4 is B'Δ'. In addition, there were a variety of special symbols in use for common fractions, especially 1/2, 1/3, and 2/3, whose usage varied among times and places.

[1] Rawlins 1994, Duke 2002 (*DIO* 12 ‡3 in this issue).
[2] Pickering 1999.
[3] Graßhoff 1990.
[4] Obviously, evidence of such equatorial observation strikes yet another blow against the theory that Ptolemy observed the ASC. Ptolemy claimed not only that he observed all the stars himself, he also claimed to have done so with an ecliptic astrolabe — an instrument that records only ecliptical coordinates.

B2 The most common scribal error is mistaking "A" (1) for "Δ" (4), or vice-versa. Mistakes between "ϵ" (5) for "θ" (9) is also common, as are instances of dropped (or inadvertantly added) ' signs. Peters & Knobel (1915) have already corrected the most obvious such occurrances.

B3 In the discussion below, we use the standard astronomical symbols β for ecliptic latitude, λ for ecliptic longitude, δ for declination, and α for right ascension. We assume throughout that the longitudes appearing in the *Almagest* are precessed from original Hipparchan coordinates by adding $2^2/_3$ degrees. Further, where appropriate, we also may assume that the Hipparchan ecliptic coordinates were in turn derived (via spherical trig) from earlier coordinates in the equatorial reference frame. The ASC star numbers prefixed "PK" are those originally of Baily, and adopted by Peters & Knobel,[5] indicating the number of the star in the Ancient Star Catalog. Star numbers prefixed "HR" are Harvard Revised numbers used in the *Yale Bright Star Catalog*, 5th edition. I have taken the star identifications of Baily, Pierce, and Schjellerup from Peters & Knobel (P&K).

C Star Identifications

C1 PK18: Commonly thought to be ϕ UMa, based on verbal description and longitude; but this may be a hybrid with χ UMa, using its latitude (41) which was very early on misread for 44.

C2 PK40, PK41, & PK42: These three *informata* (unformed stars, i.e., not forming part of the "picture" of the constellation) in Ursa Major have caused a lot of head-scratching, because although PK40 is fairly near 10 LMi, there is nothing much near the cataloged positions of PK41 and PK42, especially considering that the systematic error in this part of the sky is south or southeast. Our interest is piqued by the observation that these three stars lie nearly on the same line; and that this line would be on Hipparchos's western horizon as these stars are setting. In other words, these stars have nearly the same Phenomenon 4, and this Phenomenon is compellingly integral: both PK41 and PK42 set with degree 137 of the ecliptic, while PK40 sets with degree 135.5. This value, when combined with the Hipparchan Phenomenon 5 (polar longitude), would be enough to determine the star's position, after conversion to ecliptical coordinates. A simple scribal error in this process could account for the misplacment of all three stars: the polar longitudes of these would be written as 16, $11^2/_3$, and $10^1/_2$ degrees of Libra respectively, all of which start (in ancient Greek) with the letter ι. If this small letter had been inadvertantly added (perhaps as part of a column divider), just prior to conversion to ecliptical coordinates, all three stars would (after removing the erroneous ι) slide northwest ten degrees along the western setting horizon line, and become placed nicely near HR3579, HR3508, and HR3422.

C3 PK98: 48χ Boo (HR5676), agreeing with Baily and Schjellerup, is four times closer to the cataloged position than than η CrB, given by P&K and Toomer. The easterly systematic error in this part of the sky is not hugely compelling for these dimmer stars; nearby PK102 being a good counterexample.

C4 PK191: NGC869, the western half of the double cluster in Perseus. For error analysis purposes, I use a bright star in the center (HD14134) for its position.

C5 PK233: 4ω Aur (HR1592) is demanded by the descriptive position, agreeing with Baily and Pierce. This better than 14 Aur given by P&K and Toomer, which is not "over the left foot" as described. The identification helps us to sort out the variations in coordinates by using $\beta = 16$ (in the Greek tradition) and agreeing with Toomer on $\lambda = 50^2/_3$ (which is a Hipparchan 48).

[5] I adopt this prefix not to slight Baily, whose work I admire, but because the work of Peters & Knobel deserves recognition as unmatched in the field, and because "B" seems too short and cryptic a prefix.

C6 PK251: 39o Oph (HR6424/5), as suggested by Rawlins 1992 (*DIO* 2.1 ‡4 §C5). Hipparchos' original $\lambda = 21^1/_2$ was misread as $24^1/_2$ by Ptolemy, who added $2^2/_3$ getting $27^1/_6$ as seen in the *Almagest*. The negative sign of the latitude was also dropped along the way.

C7 PK371: 63 Ari (HR1015) is not only brighter than Toomer's τ Ari, it is also much closer to the cataloged position.

C8 PK405: Based on relative position, should be 41 Tau (HR1268), not 44 Tau as given by other sources. The other three stars in this quadrilateral are all in error to the southeast by 20 to 60 arcmin. But 44 Tau would be in error to the west, while 41 Tau is in error to the south. It is also .3 mag brighter than 44 Tau.

C9 PK410: 17 Tau (HR1142), agreeing with Manitius, fits both the descriptive and numerical positions better than Merope, as given by P&K, Baily, and Toomer.

C10 PK417, 418: The brightest candidates fitting the descriptive positions are 119 Tau (HR1845) and 126 Tau (HR1989), respectively, although all identifications are unfirm. The numerical position of PK417 is badly wrong in both coordinates. Based on the frequency of integer longitudes, all of the Taurus *informata* may be Ptolemy's observations, not Hipparchos'. Another possibility is that Hipparchos may have precessed early (and therefore, more likely inaccurate) observations by $1/3$ degree to the later epoch of his catalog; Ptolemy's addition of $2^\circ\ ^2/_3$ would then restore the integer fractions. In this context, the error in PK417 can be mostly explained if, in converting from equatorial coordinates to ecliptical, Hipparchos inadvertently used the star's polar longitude (55°) instead of its right ascension ($52^\circ.5$). The remainder of the position error is about 1° too high in declination.

C11 PK432: 63 Gem (HR2846), agreeing with Manitius. The largest part of the position error is a missing negative sign in the latitude, which we restore. P&K and Toomer give 58 Gem, but at visual magnitude $V = 6.17$, this is most unlikely.

C12 PK448: ζ Cnc is OK (agreeing with all other sources). The error in longitude is probably a slip in spherical trig, since the given position (88° Hipparchan epoch) is two degrees west of the solstical colure, while the actual star was very nearly two degrees east of the solstical colure.

C13 PK457: β Cnc is correct, agreeing with other sources. The three-degree error in position is due to a scribal error in zenith distance. The star was observed equatorially: the observed zenith distance of $21^1/_6$ was misread as $24^1/_6$, and combined with a correct polar longitude to arrive at the reported position. This error is possible only from the latitude of Rhodes City ($36^\circ\ 24'$).

C14 PK458: The descriptive position ("above the *joint* of the claw", i.e., the part of the claw closest to the body) demands 62o Cnc (HR3561), agreeing with P&K, not π Cnc as given by Toomer, Baily, Schjellerup, Pierce, and Manitius. 62 Cnc is also brighter, especially when combined with nearby 63 Cnc. We adopt Peters' $\lambda = 15^2/_3$ as the original, which is entirely reasonable despite Toomer's doubts: this is the most logical starting point from which all textual variants can be simple transcription errors.

C15 PK482: 81 Leo (HR4408), agreeing with Toomer, is fine here. Most of the longitude error is easily accounted for: Hipparchos writes $14^2/_3$, Ptolemy misreads as $11^2/_3$, then adds $2^2/_3$ to get $14^1/_3$ as given in the *Almagest*.

C16 PK504: P&K, Toomer, Baily, and Pierce all give 46 Vir (HR4925) at $V = 5.99$; but 44 Vir (HR4921) at $V = 5.80$ is more likely seen, and the position is slightly better too.

C17 PK512-515 (Vir 16-19): The "quadrilateral in the left thigh" of Virgo, which under the previous identification (shared by P&K, Manitius, and Toomer) is not a quadrilateral at all. There is a quadrilateral in the sky, however, formed by 74 Vir, 80 Vir, 82 Vir, and 76 Vir (HR numbers 5095, 5111, 5150, and 5100); but the positions and descriptions have become corrupt. The latitude of dim PK513, given as $^1/_6$ in Toomer, has an Arabic tradition of 6 which we adopt; at some early time, the original 6 was incorrectly copied as 1/6 by a scribe. (This is still in error by more than a degree, but given the dimness of the star, the error is not unreasonable). But that would have made PK513 not the northernmost of the lead pair, as described, but the southernmost. Therefore, the same scribe or a later one "corrected" the text by switching the north-south descriptions of PK512 and PK513, while leaving the magnitudes alone. Finally, the latitude of PK515, given as -3 in Toomer, has an Arabic tradition of $-1/3$ which we also adopt, and the quadrilateral is complete.

C18 PK541-542: P&K and Toomer give HR5810 for PK542 at $V = 5.82$; since the *Almagest* magnitude is 4, this seems unlikely. Better is κ Lib (HR5838, $V = 4.75$) for PK541, agreeing with P&K, and then for PK542, 42 Lib (HR5824) at $V = 4.95$. The error in position of PK542 is just a 1-for-4 scribal slip in the latitude ($-1\,^1/_2$ becomes $-4\,^1/_2$), as confirmed by the descriptive position.

C19 PK567: Graßhoff gives the open cluster M7 (NGC6475), called "Ptolemy's cluster" for this reason; but at about 3 degrees away from the cataloged position, this is most unlikely. Much better is HR6630, agreeing with P&K, Manitius, and Toomer, which is much closer in position and brighter. The "nebulous" magnitude is due to adjacent NGC 6441, a dim globular cluster. Assigning PK567 to M7 makes HR6630 one of the brightest stars in the sky not in the catalog.

C20 PK586: Toomer and Manitius give 57 Sgr, apparently on the basis of magnitude alone (Ptolemy gives 6, while 57 Sgr is $V = 5.90$ by modern measurement). But 56 Sgr (HR7515), agreeing with P&K, is much better in position, and at $V = 4.88$ is more likely to be seen. The one-magnitude brightness error is not unusual.

C21 PK595: Toomer gives $\kappa_1 + \kappa_2$ Sgr, apparently a misprint for $\theta_1 + \theta_2$ Sgr (HR7623 and HR7624).

C22 PK657: Toomer has ψ_3 Aqr, but brighter ψ_2 Aqr (HR8858) is more likely to have been taken, and is also much better in position. The slight error in magnitude is unimportant.

C23 PK658: Toomer has HR8598, which is awful. In spite of the longitude error, brighter, fits the descriptive position better, and has the correct latitude. There are two possibilities for the longitude error. First: Hipparchos' original longitude was $15^2/_3$, which is about right for his epoch. This was misread by Ptolemy (or an earlier scribe) as $19^2/_3$ in the common theta-for-epsilon slip; Ptolemy added $2^2/_3$ degrees to this, getting $22^1/_3$, written in Greek κBΓ', which was misread (or miswritten) as κB'Γ', or $20^5/_6$ as recorded. Second: Hipparchos' original longitude was $15^1/_2$, to which Ptolemy added $2^2/_3$, getting $18^1/_6$. Then, shortly afterward, Ptolemy inadvertantly added $2^2/_3$ a second time, getting $20^5/_6$ as recorded.

C24 PK699-700: P&K's and Toomer's identifications of 68 Psc and 67 Psc are unconvincing due to the extreme dimmness of 67 Psc ($V = 6.08$). Better fits for visibility and the descriptive positions are σ Psc and 68 Psc. The error in PK699 (about three degrees) can be explained if, in conversion from equatorial coordinates, the computer mistook a zenith distance of $16^2/_3$ for a declination of $16^2/_3$. Of course, this only makes sense for an observer at the latitude of Hipparchos.

C25 PK707: An inconvenient orphan. The descriptive position demands $81\psi_3$ Psc, but there is no obvious explanation for the 3 degree longitude error.

C26 PK728-PK731: Star PK729 is a repeat of PK728 (both are ϕ_2 Ceti); and PK731 is a repeat of PK730 (both are ϕ_1 Ceti). Each repeat has the same magnitude as the previous entry, and each is 1 degree south in latitude and 1/3 degree west in longitude from the previous entry. This is almost directly south in declination by 1 degree, implying that the positions were converted from equatorial coordinates. (In each case the first postion shares the error common to other stars in this part of the sky, while second position is more accurate.) Alternate identifications are too dim and too misplaced to be convincing. Note that the *Almagest* description of this asterism as a "quadrilateral" indicates that the author of the description was working from a list of stellar positions, and was a different person from the actual observer of these stars — since no such quadrilateral exists in the sky. This implies that Ptolemy may be the author of the descriptive positions, in at least some cases. There are a number of scenarios that can account for the double entry. The stars may simply have been re-observed equatorially and re-computed at a later time. For example, ϕ_2 Cet may have been originally observed at $\alpha = 345^1/_2$, $\delta = -21^1/_6$, and converted to ecliptical coordinates. This would produce the value for PK728. At the same time, ϕ_1 Cet was observed at $\alpha = 343^2/_3$, $\delta = -21^1/_6$ and converted the same way to produce PK730. Then at some later time, the stars were re-observed (more accurately) in zenith distance, producing declinations of $-22^1/_6$ for both stars. Using the same right ascensions, Hipparchos recomputes and arrives at the positions given for PK729 and PK731. Similar multiple observations are common in Hipparchos' *Commentary*, his only surviving work; a clerical error put both positions in the catalog. Yet another possibility: they may have been observed once equatorially, then converted incorrectly to ecliptic coordinates due to a confusion between ordinal and cardinal numbers. E.g., ϕ_2 Cet was recorded as being at the 58th degree of the zenith. The computer subtracts 58 from the latitude $35^5/_6$, getting a declination of $-22^1/_6$; but since the first degree of the zenith is the same as $Z = 0°$, the computer should have subtracted $35^5/_6 - 57 = -21^1/_6$. A recomputation gave Hipparchos the correct coordinates, but both numbers ended up in the catalog.

C27 PK787, PK788: These are ρ_2 Eri (HR917) and η Eri (HR874). The magnitudes of PK787 and PK788 have been reversed, causing a number of unconvincing identifications; e.g. P&K give HR859 for PK788, but at $V = 6.31$ this is hard to accept.

C28 PK802, 803, 804: Best fit for position are HR1214, HR1195 and HR1143, agreeing with P&K. The large latitude error in PK804 may be a trig slip, since 2crd $52°\ 34'$ (which rounds to the latitude given in the *Almagest*) is 95 18$'$ in the ancient system of chords of a circle with a radius of 60. Meanwhile 2crd $55°$ (the actual latitude) is 99 18$'$. The 5 and 9 digits are easily confused in Greek.

C29 PK859: This star is described in the *Commentary* as the triple star under the tail of the dog (Canis Major); while in the *Almagest* it becomes the northern of the two stars in the stern-keel of Argo (the southern of which is π Pup). This firmly identifies PK859 as a combination of HR2819, HR2823, and HR2834, of which the latter is the brightest and closest to the *Almagest* position.

C30 PK870: Toomer has HR3439 at $V = 5.21$. Based on the cataloged magnitude (< 4) and possible scribal errors, most likely is HR3591 at $V = 4.46$. The position error is then a A-for-Δ slip in the latitude ($-51^1/_2$ should be $-54^1/_2$), and-or an ϵ-for-θ slip in Ptolemy's longitude ($125^2/_3$ should be $129^2/_3$, which is Hipparchos' 127). The remaining error puts the cataloged position northwest of the star, matching the errors of PK871 and PK872.

C31 PK882: Toomer has HR3055 at $V = 4.11$; from both magnitude and position, much better is HR2998 at $V = 5.05$ (since the *Almagest* magnitude here is 6).

C32 PK887: P&K and Toomer both give f Car (HR3498), which at $V = 4.50$ is far too dim for a star described as second magnitude. Better is ι Car (HR3699, $V = 2.21$), which is the only second-magnitude star in the region unaccounted for, and which also matches both the descriptive position and the latitude quite well. The huge thirteen-degree error in longitude (five degrees along the great circle) can be explained if Hipparchos mis-recorded the longitude interval by one step.[6] (The astrolabe was graduated in step intervals of fifteen degrees.)

C33 PK905: α Hya is of course correct, as given by all others. But the latitude error proposed by P&K and endorsed by Toomer has no textual support, and the alleged scribal error (23 read as 20 $^1/_2$) is weak. The error is actually due to a dropped minus sign in declination prior to conversion to ecliptical coordinates (see PK920 below for another example of this in Hydra.) The star was accurately observed with a declination of -1 and a polar longitude of 113.5 (or a right ascension of 115.5). After dropping the minus sign in declination, and using the Hipparchan obliquity of $23°\ 51'$, the position converts to $\lambda = 117\,^1/_3$, $\beta = 20^1/_2$ after ancient rounding. Then adding Ptolemy's $2^2/_3$ to the

[6] My thanks to Dennis Rawlins for this suggestion.

longitude, we have exactly the position given in the *Almagest*.

The descriptive position claims that PK905 is "close" to PK904, but this is only true for their cataloged positions, not their positions in the sky. This is another indication that in some cases the descriptive positions were written by a person working from the cataloged list, not the actual observer (see §C26 above for another example).

C34 PK920: Based on the given magnitude (3) and descriptive position, this must be λ Hya (HR3994), with a mistaken plus-for-minus in declination prior to conversion to ecliptic coordinates. Other stars suggested by Toomer (ϵ Sex) and P&K (α Sex) are far too dim and misplaced to be convincing. Without this identification, λ Hya would easily be the brightest star in Hydra missing from the catalog. A similar error is given above at §C33.

C35 PK962 is ϵ Cen, which would be missing otherwise under the proposal below. The magnitude is a poor fit, but the position is much better than the alternative HR5172.

C36 PK963-969. The hind legs of Centaurus, today mostly part of the constellation Crux, the Southern Cross. This area of the sky is a mess, with all stars having large positional errors, and all identifications uncertain. Standard practice has been to assign the right hind leg (PK965 and PK966) to γ Cru and β Cru, which means the left hind leg (PK 967 and PK968) becomes δ Cru and Acrux (α Cru). This puts all stars east or northeast of their cataloged positions by a huge 3 to 5 degrees.

I was intrigued by the description of PK968 as being "on the frog of the hoof" (i.e., on the underside of the hoof) rather than the more straightforward "on the hoof"; this is the only place in the *Almagest* where this term is used. My interest was heightened even further by the only other description of this part of the sky in the *Almagest*, in the delineation of the Milky Way at VII.2, where Ptolemy mentions "the stars on the hock"[7] of this leg — a clear distinction from the frog, for two reasons: first, because the "star" on the frog is singular, while the "stars" on the hock are plural; and second, because the frog is on the bottom of the hoof, while the hock is just above the hoof, between the hoof and the ankle.

Therefore I propose that PK968, the frog of the hoof, is really λ Cen, and the "stars on the hock" are formed by the corona of 5th magnitude stars[8] HR4511, HR4499, HR4487, and HR4475 — a unique feature not present in any other celestial equine leg. (Acrux has no visible stars above it to form a hock.) Then PK967, the knee-bend of that leg, becomes $o_1{+}o_2$ Cen (HR4441 + 4442), whose combined magnitude of 4.39 fits just fine. This in turn means that the right hind leg becomes Acrux (the hoof) for PK966 and δ Cru (the knee-bend) for PK965. This proposal greatly reduces the positional errors for all four stars.

Bright γ Cru and β Cru are not left out, however; I assign them to PK963 and PK964 respectively, described as the two stars under the belly. The magnitudes of these two fit well, although the positional errors are quite bad; however, the standard identifications of ϵ Cen and HR5141 are not much better. In this context, it's interesting to note that the cataloged position of PK964 rises (at Rhodes) at the same time as β Cru (i.e., it has the same Hipparchan Phenomena 1 and 2), and its setting phenomena (Hipparchan Phenomena 3 and 4) are off by almost exactly 10 degrees. So this may be a scribal slip just before a spherical trig conversion.

This means that λ Cen (at $V = 3.12$) becomes the southernmost star in the catalog at Ptolemy's epoch ($-53°07'$, compared to $-52°51'$ for Acrux). At Hipparchos' epoch, Canopus remains the southernmost. Star PK968 has the southernmost cataloged position at either epoch.

C37 PK971 must be ϵ Cru (HR4700) under the above proposal. The positional error is not hugely different from other stars in the region, and less than the standard μ Cru.

[7] Toomer 400.

[8] Some might oppose this identification on the grounds that these stars would have post-extinction magnitudes of 6.95, 6.77, 6.92, and 6.81 under Schaefer's atmosphere (see ‡1 fn 6) at the latitude and epoch of Ptolemy; so this identification implies that even Ptolemy observed under an atmosphere of $k_a \leq .01$. But I have no better explanation for what these "stars on the hock" might be.

C38 PK982-983: P&K and Toomer give ρ Lup and ι Lup. I prefer ι Lup (HR5354) and HR5364. The descriptive and numerical positions are both better, although the magnitudes are worse; they may have been reversed.

C39 PK987-988: We follow P&K, not Toomer, as χ Lup (HR5883) and ξ_1 Lup (HR5925) here. Most positions in this part of the sky are displaced to the west and a bit north, which makes these identifications preferable.

C40 PK1017: P&K and Toomer give ζ PsA (HR8570), extremely dim at $V = 6.43$; much better is HR8563 at $V = 5.94$, which is also slightly closer in position.

D The Unique Mistake of ϕ Ceti: A Datum Recovered

D1 This pair of inadvertant repeats (cf. above at §C26) gives us a unique opportunity to determine the original coordinate system used by Hipparchos and the way positions were converted. We would like to know two things: first, the obliquity that Hipparchos used when doing coordinate conversions;[9] and second, whether the original east-west coordinate was measured in Right Ascension or polar longitude. For this analysis, I make these assumptions: that the stars are indeed repeats; that the original east-west equatorial coordinates were the same for each pair; and that the original declinations for each pair differed by exactly one degree.

D2 We would like to find the original equatorial coordinates for each star, rounded according to ancient rounding rules. Normally this is not difficult, since ancient rounding is fairly loose. In this case, however, we have the rounded results of two different computations with the same east-west coordinate, which tightens the fit somewhat.

D3 For example, suppose that Hipparchos used an obliquity of $23°\ 40'$ and measured RA (instead of polar longitudes) as the east-west coordinate. Looking at PK728, if we back-compute the equatorial coordinates, we see the original rounded coordinates must have been close to $\delta = -21°10'$, $\alpha = 345°30'$. But when we forward-convert these into the ecliptical frame (following the computations we suppose for Hipparchos, including rounding the final result according to ancient rules), the result becomes $\beta = -13°45'$, $\lambda = 338°20'$. The longitude is fine, but the latitude differs from that of the *Almagest*, which is $-13°40'$. Tweaking the starting declination up to -21 results in $\beta = -13°30'$, skipping right over the desired result. So we know that this combination of obliquity and Right Ascension does not work.

D4 In practice such exclusions are rare, because one is usually able to find a combination that computes correctly by tweaking the starting coordinates a bit. But with the addition of a second conversion for the same star, any tweaking of the input coordinates becomes less likely to succeed, because the same tweak must be simultaneously successful for both conversions of that star.

Obliquity $23°40'$ RA: Conversion for PK728 fails. PL: Conversion for PK728 fails.
Obliquity $23°51'$ RA: Conversion for PK728 fails. PL: All conversions work.
Obliquity $23°55'$ RA: All conversions work. PL: The conversion for PK728 fails at $\delta = -21°1/4$, while the alternative ($-21°1/6$) fails for PK729.

D5 There are only two possibilites: either Hipparchos used $23°55'$ as his obliquity, combined with RA as the east-west coordinate; or, he used $23°51'$ as the obliquity, and polar longitudes as the east-west coordinate. The latter combination has better textual support in both elements, and is therefore much preferred.

D6 Although all conversions work under these parameters, the conversion for PK731 appears to fail at first, giving $\lambda = 335\ 1/4$ and not the expected 335 1/3; but this is deceiving, because of Ptolemy's "slide & hide" procedure: any Hipparchan longitude ending with 1/4 was rounded up an extra 5 arcmin, to avoid disallowed fractions in the *Almagest*. Thus, 335

[9] There are three possibilities: $23°40'$ (*DIO* 1.2), $23°51'$ (*Almagest*),and $23°55'$ (Rawlins 1982, Rawlins 1994).

1/4 is perfectly acceptable, and indeed this becomes the first (and so far only) example of a lost Hipparchan 1/4-degree fractional longitude being recovered.

E The Strange Case of Pi Hydrae

E1 The odd case of π Hydrae (PK918) has been noted by others (e.g., Graßhoff 1990), who have pointed out that not only does this star have a huge error — over five degrees — but also that the same error appears in this star's position in the *Commentary*, proof positive that the ASC coordinates were taken from Hipparchos and not observed independently.
E2 But until now, there has been no compelling explanation for the five-degree error. The mystery is cleared up when we realized that other stars in Hydra (PK901, PK920) were observed equatorially, then converted to ecliptical coordinates. It then becomes clear that almost the entire error in the position of π Hya is in declination. Converting back to the original equatorial coordinates (after subtracting Ptolemy's $2°2/3$ precession), the Hipparchan equatorial coordinates would have been $\delta = -20.5$, $\alpha = 182°.5$. The actual declination of π Hya was very nearly $-15°.5$ at Hipparchos' epoch. So the error is a simple scribal slip: the written number ιε (15) was misread as κ (20) due to a malformed or missing cross-stroke on the ϵ.
E3 Astoundingly, Ptolemy may have observed this star himself, and then *thrown away his own correct observation* in favor of Hipparchos' huge error! In the *Almagest* VII.1, Ptolemy records[10] that π Hya is on a straight line with α Lib and β Lib. This observation is true for the actual star; but it is *not* true for the erroneous position of π Hya as recorded in the ASC. Just prior to this, Ptolemy claims that he had observed this alignment himself, and that it had not been recorded by any previous astronomer.[11] Of course, there is no evidence that Ptolemy's alignment observation also included a position measurement.

F Stars Observed Equatorially

F1 It is clear that a number of stars, especially in the south, were observed with equatorial instruments, and had their coordinates transformed into ecliptical coordinates for the catalog. The following cases have good evidence for this process: β Cnc (PK457), σ Psc (PK699), ϕ_2 Cet (PK728/9), ϕ_1 Cet (PK730/1), α Hya (PK901), π Hya (PK918), and λ Hya (PK920).

References

Duke, Dennis (2002). The Measurement Method of the Almagest Stars. *DIO* 12 ‡3, 35-50.
Graßhoff, Gerd (1990). *The History of Ptolemy's Star Catalogue.* Springer-Verlag.
Hoffleit, E.D., & Warren Jr., W.H. (1991) *Yale Bright Star Catalog*, 5th ed. Yale University Observatory. Available at http://adc.gsfc.nasa.gov/adc-cgi/cat.pl?/catalogs/5/5050/ .
Peters, C.H.F. & Knobel, E.B. (1915). *Ptolemy's catalogue of stars.* The Carnegie Institution of Washington.
Pickering, Keith A. (1999). Evidence of an Ecliptical Coordinate Basis in the Commentary of Hipparchos. *DIO* 9.1, 26-29.
Rawlins, Dennis (1982). An Investigation of the Ancient Star Catalog. *PASP* 94, 359.
Rawlins, Dennis (1994). Hipparchos' Observatories Located. *DIO* 4.1 ‡4, 33-47.
Toomer, G.J. (1998). *Ptolemy's Almagest*. Princeton University Press.

[10] Toomer 326.
[11] Toomer 325.

DIO

DIO, The International Journal of Scientific History [www.dioi.org] & its occasional humorous adjunct *Journal for Hysterical Astronomy* are published by:

DIO, Box 19935, Baltimore, MD 21211-0935, USA.
Telephone (answering machine always on): 410-889-1414.
[Email: dioi@mail.com.] Research & university libraries may request permanent free

subscription to *DIO*.
Complete sets of back issues also available at no charge.

Each issue of *DIO, The International Journal of Scientific History* will be printed on paper which is certified acid-free. The ink isn't.

Editor: Keith Pickering, *DIO*, Box 999, Watertown, MN 55388.
Publisher: Dennis Rawlins (DR), address above.

DIO is primarily a journal of scientific history & principle. However, high scholarship and-or original analytical writing (not necessarily scientific or historical), from any quarter or faction, will be gladly considered for publication. Each author has final editorial say over his own article. If refereeing occurs, the usual handsome-journal anonymity will not, unless in reverse. No page charges. Each author receives 50 free offprints.

The circumstance that most *DIO* articles are written by scholars of international repute need not discourage other potential authors, since one of *DIO*'s purposes is the discovery & launching of fresh scholarly talent. Except for equity & charity reply-space material, submissions will be evaluated without regard to the writer's status or identity. We welcome papers which are too original, intelligent, and-or blunt for certain handsome journals. (Dissent & controversy are *per se* obviously no bar to consideration for *DIO* publication; but, please: spare us the creationist-level junk. I.e., non-establishment cranks need not apply.)

Other journals may reprint excerpts (edited or no) from any issue of *DIO* to date, whether for enlightenment or criticism or both. Indeed, excepting *DIO* vols.3&5, other journals may entirely republish *DIO* articles (preferably after open, nonanonymous refereeing). No condition is set except this single one: *DIO*'s name, address, URL, and phone number are to be printed adjacent to the published material and all comments thereon (then *or later*), along with the additional information that said commentary may well be (and, regarding comments on DR output, will certainly be) first replied to — if reply occurs at all — in *DIO*'s pages, not the quoting journal's.

DIO invites communication of readers' comments, analyses, attacks, and-or advice. (And we urge informants to snitch on lurking typos: they get rubbed out at reprinting-time.)

Written contributions are especially encouraged for the columns: Unpublished Letters, Referees Refereed, and regular Correspondence. Contributor-anonymity granted on request. Deftly or daftly crafted reports, on apt candidates for recognition in our occasional satirical *J. for Hysterical Astronomy* will of course also be considered for publication.

Free spirits will presumably be pleased (and certain archons will not be surprised) to learn that: at *DIO*, there is not the slightest fixed standard for writing style.

 ISSN #1041-5440 This printing: 2015\8\11.

A Fresh Science-History Journal: Cost-Free to Major Libraries

Telephone 410-889-1414

DIO

dioi@mail.com

DIO — The International Journal of Scientific History.

Deeply funded. Mail costs fully covered. No page charges. Offprints free.

• Since 1991 inception, has gone without fee to leading scholars & libraries.

• Contributors include world authorities in their respective fields, experts at, e.g., Johns Hopkins University, Cal Tech, Cambridge University, University of London.

• Publisher & journal cited (1996 May 9) in *New York Times* p.1 analysis of his discovery of data exploding Richard Byrd's 1926 North Pole fraud. [*DIO* vol.4.] Full report co-published by University of Cambridge (2000) and *DIO* [vol.10], triggering *History Channel* 2000&2001 recognition of Amundsen's double pole-priority. New photographic proof ending Mt.McKinley fake [*DIO* vol.7]: cited basis of 1998/11/26 *New York Times* p.1 announcement. *Nature* 2000/11/16 cover article pyramid-orientation theory: *DIO*-corrected-recomputed, *Nature* 2001/8/16. Vindicating DR longtime Neptune-affair charges of planet-theft and file-theft: *Scientific American* 2004 December credits *DIO* [vols.2-9]. *DIO*-opposites mentality explored: *NYTimes* Science 2009/9/8 [nytimes.com/tierneylab].

• Journal is published primarily for universities' and scientific institutions' collections; among subscribers by request are libraries at: US Naval Observatory, Cal Tech, Cornell, Johns Hopkins, Oxford & Cambridge, Royal Astronomical Society, British Museum, Royal Observatory (Scotland), the Russian State Library, the International Centre for Theoretical Physics (Trieste), and the universities of Chicago, Toronto, London, Munich, Göttingen, Copenhagen, Stockholm, Tartu, Amsterdam, Liège, Ljubljana, Bologna, Canterbury (NZ).

• New findings on ancient heliocentrists, pre-Hipparchos precession, Mayan eclipse math, Columbus' landfall, Comet Halley apparitions, Peary's fictional Crocker Land.

• Entire *DIO* vol.3 devoted to 1$^{\text{st}}$ critical edition of Tycho's legendary 1004-star catalog.

• Investigations of science hoaxes of the -1^{st}, $+2^{\text{nd}}$, 16$^{\text{th}}$, 19$^{\text{th}}$, and 20$^{\text{th}}$ centuries.

Paul Forman (History of Physics, Smithsonian Institution): "*DIO* is delightful!"

E. Myles Standish (prime creator of the solar, lunar, & planetary ephemerides for the pre-eminent annual *Astronomical Almanac* of the US Naval Observatory & Royal Greenwich Observatory; recent Chair of American Astronomical Society's Division on Dynamical Astronomy): "a truly intriguing forum, dealing with a variety of subjects, presented often with [its] unique brand of humor, but always with strict adherence to a rigid code of scientific ethics. . . . [and] without pre-conceived biases . . . . [an] ambitious and valuable journal."

B. L. van der Waerden (world-renowned University of Zürich mathematician), on *DIO*'s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: "*marvellous*." (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)

Rob't Headland (Scott Polar Research Institute, Cambridge University): Byrd's 1926 latitude-exaggeration has long been suspected, but *DIO*'s 1996 find "has clinched it."

Hugh Thurston (MA, PhD mathematics, Cambridge University; author of highly acclaimed *Early Astronomy*, Springer-Verlag 1994): "*DIO* is fascinating. With . . . mathematical competence, . . . judicious historical perspective, [&] inductive ingenuity, . . . [*DIO*] has solved . . . problems in early astronomy that have resisted attack for centuries . . . ."

Annals of Science (1996 July), reviewing *DIO* vol.3 (Tycho star catalog): "a thorough work . . . . extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed . . . . excellent investigation".

British Society for the History of Mathematics (*Newsletter* 1993 Spring): "fearless . . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth."