DIO
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Sostratos’ Lighthouse Ploy
EarthRadius 40800 Stades
Aristarchos’ Vast Universe
Science’s EoPrometheus
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News Notes
From the International Herald Tribune 2008/1/12-13 p.1 obit for Edwin Hillary, 1953/5/29 co-conqueror of Mt.Everest: “In the annals of great heroic exploits, the conquest of Mt.Everest by Hillary and [Tenzing] Norgay ranks with the first trek to the South Pole by Roald Amundsen in 1911 and the first solo nonstop trans-Atlantic flight by Charles Lindbergh in 1927.” In the era B.D. (Before DIO) this would instead surely have read: Peary-N, Pole & Lindbergh-Atlantic. Popular history takes far too long to reach accurate equilibrium. And all-too-often never does. But we may savour justice as it blossoms.

[Note added 2008 Dec.] DIO’s newest winners of its $1000 R.R.Newton Award for Scientific History are S.Albers & G.Graßhoff, honoring their originality and fruitfulness.

Steve Albers was first to propose (Sky & Telescope 1979 March) the ingenious notion of investigating the ms records of earlier-era astronomers who had searched for satellites of any known planet at times when it had been near conjunction with then-unknown planets — in order to find out whether the latter had been accidentally recorded as possible satellites. Albers’ nomination for this DIO prize was due to the R.R.Newton Award Committee’s Charles Kowal, who (with Stillman Drake) had in 1980 taken up Albers’ suggestion and made the sensational discovery of Galileo’s 1612-1613 observations of planet Neptune. The remarkable 1980 committee will be told by Kowal for the first time in DIO volume 15. (Before the committee existed, the DR-selected winner of the first RRN Award was Kowal himself, for this very discovery.)

Gerhard Graßhoff’s 1986 University of Hamburg thesis (Springer Verlag 1990) was based upon a hypothesis of a very successful experiment (which no one [including R.Newton & DR] had thought of) to detect mass-statistical correlations between the hundreds of star-positions in Hipparchos’ Commentary & Ptolemy’s Almagest star catalog. This was a crucial contribution to eventual conversion of what had seemed a needlessly ever-undead controversy into a genuinely dead one — a valued rarity in cemental-cult-infested academe.

1 [Note added 2009.] An 1165 AD report (Proc. Brit. Acad. 19/277-292 [1933] pp.280&282-283) has the Pharos-flame-replacement mosque’s base 31+15+4 = 50 fathoms high, or (contra PBA conversion) 300ft. (Six ft = 1 fathom = stretched hands’ tip-to-tip span, one of the least infirm ancient measuring units.) The most detailed eyewitness Pharaoh image we have (late 1st & 2nd century AD Alexandria) shows like proportions: see inset in Fig.1. Of oldest few extant Pharos-height reports (Thiersch 1909 p.66 & PBA), most are in the range 300-to-306 units. For oldest of all, see InductionQuake at p.12 within.

2 When in 1999 the body of 1924 Everest-challenger George Mallory was found 2000 ft below Everest’s summit, the question arose: was he going up? — or coming down, after attaining the top? Hillary responded by noting that no conquest should count unless the conquerer returned to base. Hmmmm. And just where would that leave Brit ultra-polarhero Rob’t Scott?

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§1 Eratosthenes’ Too-Big Earth & Too-Tiny Universe

Eratosthenes-Pharos Science Behind Alexandria-Aswan Myth
Lighthouse Flame Height Exactly 300 Feet = Half Stade
Ultimate Geocentrist’s Sun Smaller Than the Earth
High-Precision Ancient Science Doubly Verified

Dedicated to the Memory of Our Irreplaceable Friend HUGH THURSTON
1922/3/28-2006/10/29

A Big-Science Dawn: Sostratos’ Pharos, Precise Earth-Measurer?

A1 Over 22 centuries after Eratosthenes’ legendary Earth-measure, newly-mined ancient sources finally permit arrival at the non-astronomical truth behind the most famous of ancient geographical tales, the long-suspect myth of his 600-mile-travel to compare (§A4[a]&D3) the Sun’s noon altitude at Alexandria vs Aswan. The actual method instead used hometown measures of the height & night-visibility-distance of the Alexandria Lighthouse designed by Ptolemy II’s architect Sostratos, which explains the result being too high by a factor of 6/5 (eq.28), just the error (§B3) expected from air’s bending of horizontal sealevel light. (This paper was revised in 2013 & 2017 for Sostratos’ recognition and for DIO 20 §1 fn 2.)

A2 Rawlins 1982N (p.217 & n.26) discussed two easy stay-at-home methods which would account for the overlargeness of Eratosthenes’ Earth-size, one being: measure how far over the sea a known-height lighthouse is visible at night. (Near-attestation at §A4[c].) But neither DR nor anyone else noted the coincidence that the tallest lighthouse in the world debuted right at Eratosthenes’ time & place, the 2nd century BC Alexandria (§D5) — the “Pharos” (Greek for “lighthouse”), 2nd most durable of the ancient 7 Wonders of the World, surviving for 1 1/2 millennia. until ultimately falling to earthquakes and their aftershocks.

A3 With this glimmer of where we’re headed, we now plunge into solving the entire Eratosthenes Earth-measure mystery: method, place, all his data (terrestrial and celestial), and each’s respective atmospherically-induced error: (All these errors would be appreciably weaker for great heights’ thinner air: fn 1.)
Cancelling $r^2$ from each side and dropping relatively trivial $h^2$, we have the naïve Airless Lighthouse Equation which ancients would have used to determine Earth-radius $r$:

$$r = \frac{v^2}{2h} \quad (2)$$

**B3** But to find the Real Lighthouse Equation (based on Earth-with-atmosphere) at sealevel, one must account for horizontal atmospheric refraction, which stretches $v$ artificially by the square root of $6/5$ since horizontal light is bent with curvature equal to $1/6$ of the Earth’s curvature (S.Newcomb 1906 pp.198-203) so $v^2$ in eq.2 is augmented by factor $6/5$, producing an Earth-radius high by $20\%$. (Curvature is defined as inverse of radius.)

To return the problem to the straight-ray Pythagorean math behind eq.2 requires undoing the effect of the ray’s curvature. Ancients may have suspected atmospheric refraction (fn 56), but no evidence for quantitative corrections exist until Tycho (c.1600 AD). Since the radius-estimate an ancient scientist would compute (via good Pharos-Method data) would be high by factor 1.2, the Real-Earth Lighthouse Equation is (using eq.2):

$$R = r/1.2 = \frac{v^2}{2.4h} \quad (3)$$

— from which one can get an accurate estimate of the Earth’s real radius $R$, instead of the $20\%$-exaggerated $r$ one would get from the ancients’ refraction-innocent eq.2.

**B4** Rawlins 1979 applied very similar elementary straight-ray math & diagram to the §A4[d] Sunset Method of Earth-measure. (Though that method’s resulting Earth-radius is low by factor 5/6, from air-refraction.) The pre-refraction-correction math of the §A4[b] Mountain Method (result high by 6/5, like the §A4[c] Pharos Method) is much the same.1

**B5** Application of the Pharos Method would have been particularly simple because the shore along the Alexandria region is straight enough that one would not need to bother with ships: $v$ could’ve been found by simply wheel-odometering the distance along the shore (checking by triangulation) until the Pharos light was no longer visible. The Pharos’ height $h$ was knowable via trig or by measuring ropes hung from flame, to successive sections, to sea; though, as suggested below ([11]), the exact height was probably already known.

**B6** K.Pickering notes that on the nearly-linear coast just west of Alexandria, at distance c.20 nmi, the Pharos (slightly off said coast) is seen over the sea at azimuth c.40°. In this direction, the $R$ corresponding to the sea’s real curvature can be shown to be 6371 km $= 3440$ nautical (geographical) miles $= 3959$ statute mi $\approx 4400$ stades, so we take this as the effective value of $R$ in the discussions below, where we use the standard 185m Greek stade (embedded in all our fits, which thereby confirm conventional opinion [§J1] on the stade).

**C Pharos’ Approximate Height**

**C1** Josephus *War* 4.613 says the flame of the Pharos was visible to ships for 300 stades (obviously a round figure for $v$), which would by eq.3 make it the world’s then-tallest building (exceeding the Great Pyramid); yet it was never so described. Solution to Josephus’ datum: the crow’s-nests of tall ancient ships were roughly 1/4 of the Pharos’ height, meaning (eq.3) that approximately 1/3 of Josephus’ 300 stades was due to ship-height; so $v \approx 200$ stades is a adequate rough estimate for the Pharos’ visibility-distance $v$ at sealevel.

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1 While seeking an explanation of Eratosthenes’ result, DR has in recent years been inexplicably distracted by the §A4[b] Mountain Method. (Thurston 2002S p.66 evidenced better memory and sense.) Yet it is obviously inferior (to the §A4[c] Pharos Method): it involves measuring a small angle — and the 1% precision of agreement with Eratosthenes’ actual Earth-radius would require 1’ measuring accuracy under difficult seeing conditions. (Also, the great height required to get an angle large enough to render observer-error negligible would lead to weakening of refraction due to decreased atmospheric density-gradient, yet the error in $C_{QK}$ is closely [§3] consistent with virtually full-strength sealevel refraction.) Advantageously, the Pharos Method does not even get involved with angles at all, and the requisite relative precision is attained with ease. Note: the Mountain Method would lead to two-significant-digit results; the Pharos Method, three. So the very fact that Eratosthenes expressed his Earth-radius to three (eq.13) provides yet another indication that it was based on the Pharos Method.
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C2 Thus eq.3 gives us a pretty good idea of the Lighthouse’s height \(h_L\):
\[ h_L = \frac{v^2}{2 AR} = \frac{20^2}{(2.4 \cdot 34400)} \approx 0.48 \text{ stade} \approx 1/2 \text{ stade} \approx 90m \tag{4} \]

D1 Eusebius, Bishop of Caesarea-Palestine, is most remembered for leaving us his invaluable Ecclesiastical History of the Christian church at its time of triumph.

D2 We will henceforth also owe him for the long cast-aside, here vindicated clue relayed in his Preparatorio Evangelica, which unlocks the full truth behind the most enduring of ancient geographical legends, Eratosthenes’ measurement of the Earth. The key data (Eusebius PE 15.53):² Eratosthenes had the Moon 780000 stades distant; and the Sun, 4080000 stades. We formally list these two Eratosthenes distances:

\[ M_E = 780000 \text{ stades} \tag{5} \]
\[ S_E = 4080000 \text{ stades} \tag{6} \]

D3 The traditional Eratosthenes Earth-circumference \(C_E\) is based upon the famous §A4[a] Kro “experiment” (Kleomedes 1.10): Summer Solstice Apparent Noon Sun’s zenith distance (90° minus altitude \(h\)) was 1/50 of a circle at Alexandria but null at Aswan-Elephantine (very near Tropic of Cancer) where legend had vertical sunshine reaching well-bottom (though see Rawlins 1985G p.258) — 2 cities 5000 stades apart in latitude. (NB: Kleomedes 1.10 doesn’t say that the 5000 stade distance was measured, merely calling it a “premis”.) So:

\[ C_E = 50 \cdot 5000 \text{ stades} = 250000 \text{ stades} \tag{7} \]

If one checks this vs the Bishop Eusebius-reported solar distance \(S_E\), we find ratio \(p_{BK}\):

\[ p_{BK} = \frac{2\pi S_E}{C_E} \approx 103 \tag{8} \]

much too unround a number, given ancient convention (±2 fn 37) of using powers of 10 for loosely-determined distances. (This habit is the earliest historical evidence for use of order-of-magnitude [ordmag] estimation of that which is too uncertain for more exact gauging. In this tradition, Poseidionos made the solar distance 10000 Earth-radii: ² [F2 eq.15.] If we instead adopt the Eratosthenes circumference \(C = 252000 \text{ stades} \) (which he’d presumably [vs fn 6] adjusted slightly for geographical convenience to a round ratio of 700 stades per great circle degree: Strabo 2.5.7), a fresh check instead produces ratio \(p_{BG}\):

\[ p_{BG} = \frac{2\pi S_E}{C_G} \approx 102 \tag{9} \]

but this is also unacceptably non-round.

D4 However, years ago, DR analysed the Nile Map which Strabo 17.1.2 attributes to Eratosthenes, and showed (Rawlins 1982N p.212) that the underlying measure was

\[ C_N = 256000 \text{ stades} \tag{10} \]

[Noted also at Rawlins 1985G p.259 & Thurston 2002S p.66.] When we check this vs Eusebius’s \(S_E = 4080000 \text{ stades} \) (eq.6), the Sun-Earth-radius ratio \(p_{BN}\) provides a pleasant shock, as we begin our realization that \(C_N\) unleashes the long-dormant Eusebius data-treasure of eqs.5&6:

\[ p_{BN} = \frac{2\pi S_E}{C_N} \approx 100.1 \tag{11} \]

²See www.tertullian.org/fathers/eusebius_pe15_book15.htm or H.Diels Doxographi Graeci Berlin 1879 pp.362-363. Eq.6’s \(S_E\) is so startlingly small (entailing a Sun smaller than Earth: eq.16) that Heath 1913 p.340 just can’t believe it. Such inertia has prevented entertainment of the hypothesis (§F3) that pol’s-poL Eratosthenes found it advantageous (& healthy: ² [fn 69]) to be a geocentrist’s-geocentrist.

D5 This is a hit that carries us right into the heart of the Earth-measure mystery. The obvious conclusion from eqs.6&11 is that Eratosthenes had the Sun’s distance equal to 100 Earth-radii, so

\[ S_E = 100R_E \tag{12} \]
\[ r_E = 40800 \text{ stades} \tag{13} \]

the only 3-significant-digit Eratosthenes figure for the Earth’s size directly based on empirical data. (Compare eq.13 to eq.7.) All pre-Pharos C were 1-significant-digit-rough:

\[ 400000 \text{ stades} \text{ to } 3000000 \text{ stades} \text{ (Dikaiarchos c.300 BC). Yet §[11] after the Pharos’ debut, we find ordmag 100 times greater precision in 3-significant-digit eq.13.}

E Eratosthenes’ Moon

F1 While placing the Sun 100 Earth-radii distant, far short of Aristarchos’ solar distance, Eratosthenes nonetheless adopted the farcical lunar distance of pseudo-Aristarchos,

³ 19 Earth-radii (Heath 1913 pp.339 & 350; but see §[C5], as eq.13 verifies:

\[ M_E = 19R_E = 775200 \text{ stades} \tag{14} \]

which matches eq.5. Eusebius’ report. (The match is far better than that figured at Heath 1913 p.340, where \(2\pi/19\) is divided into the hitherto-conventional Eratosthenes \(C = 252000 \text{ stades} \), yielding about 760000 stades.)

E2 But if we try recovering the lunar distance from the Nile Map \(C_N\) (eq.10):

\[ 19C_N/2\pi = 774130 \text{ stades} \approx 770000 \text{ stades} \tag{15} \]

we find that it does not check with eq.5.

E3 Comparison of eq.15 to eq.14 begins a linchpin realization: Eratosthenes’ root-measurement was Earth-radius, not Earth-circumference. The historical import of this revelation will become evident below (§G2).

F Eratosthenes’ Sun

F1 Remarkably, Eratosthenes had the Moon’s distance almost 1/5 of the Sun’s — which goes counter to easy visual checks, since if his 19:100 ratio were true, half-Moons would occur with the Moon more than 10² from quadrature. (Arcsin 0.19 ± 11°.)
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F2 This bizarrity seems less likely to be the result of observation than of patch-work synthesis: melding two distances from two distinct sources, regardless of compatibility. A possible trigger: the Sun’s size shrank for ascientific reasons (royally-oy Eratosthenes was a fave of the Ptolemies’ theocratic Serapic regime: Rawlins 1982G p.265), the Sun’s greater size having been a likely spark to the proscribed heliocentrist heresy.

F3 From Eratosthenes’ 100 Earth-radii solar distance (eq.12), we see that the Earth’s angular semi-diameter as seen from the Sun would be $180^\circ/100\pi = 0.573$, while the semi-diameter of the Sun (seen from the same 100 Earth-radius distance) was pretty accurately estimated ($\S$[C1]) to be $0.25$. Therefore, the implicit solar size $s$ in Earth-volumes is:

$$s = \left(0.25/0.573\right)^3 \approx 1/12$$

Eratosthenes was pretending that the Sun was 12 times smaller than the Earth. Such cosmology doubtless delighted (and offered justifying comfort to) gov’t-catering geocentrist priests, whose anti-progressive view of the universe dominated the world by force for millennia, until modern times. This discovery widens our basis for appreciating how Eratosthenes climbed to academic eminence in Ptolemaic Alexandria, promoting a cozy universe trillions of times smaller than that already proposed by Aristarchos of Samos. (See \S[2 fn 33 & \S[H1].)

G Eratosthenes’ Earth

G1 The Nile Map’s Earth-size is now confirmed by congruence (eqs.5-14) with Eusebius’ numbers, so we ask how well the map’s underlying $C_N$ (eq.10) generates the radius:

$$C_N/2\pi = \frac{256000}{2\pi} \div 40700 \text{ stades} \neq r_E$$

— no match. But the reverse process does create a match to eq.10. Starting from eq.13:

$$2\pi r_E = 2\pi \cdot 40800 \text{ stades} \div 256000 \text{ stades} = C_N$$

This contrast (eq.17 vs eq.18) confirms the \S[E3] finding, so that we now have double-evidence that Eratosthenes’ radius generated his circumference $C_N$, not the reverse.

G2 What is the significance of this priority? Simple: it kills the legend that Eratosthenes got the size of the Earth by the famous Kleo Method (based on measuring the distance from Alexandria to Aswan: \S[A4][a]), because that method’s math (eq.7) produces circumference. By contrast, the Pharos Method (\S[A4][c]) directly yields the Earth’s radius: eq.2. Thus, the clear implication of the radius’ computational priority is that the Pharos Method (not the Kleo Method) was that actually used by Eratosthenes or his source to find the Earth’s size. (The Kleo Method’s untenability will be independently confirmed below: \S[K2 & fn 7].)

H Inventing the “Experiment”

H1 As noted at Rawlins 1982N n.10, Eratosthenes was possibly unsure of whether the Mediterranean Sea’s curvature matched the world’s. If so then (ibid p.216) he may have unwittingly based his 5000 stade supposed-meridian (Alexandria-to-Aswan) & his $C_K$ ultimately upon use by another scholar (see, e.g., \S[I] of the very method he questioned. It is also possible that he knew where the basic measurement came from and himself concocted the famous “experiment” as a useful illustration even though it was actually founded upon a rounding of $C_N$ (eq.10), as titulary noted by Rawlins 1982N — and while doing so found that a round distance of 5000 stades would nearly dovetail $r_E$ with his (defective: Rawlins 1982G n.19) gnomon observation of the solstitial Sun’s culmination zenith distance, $7^\circ 12^\prime 1/2$ (ibid n.20 & Table 3), the rounding of which to $7^\circ 15^\prime$ or $7^\circ 12^\prime 1/2$ became the purported basis of his ultimately canonical $C_K = 250000$ stades.\footnote{Note Sun-shrinker Eratosthenes’ Scylla-Charybdis narrowings: bringing the Sun near enough to make it smaller than Earth, while putting the Moon not too close to the Sun (thereby inflating $\S$ eq.4’s $\gamma$) but not too close to the Earth, since that would entail huge daily parallactic retrogrades. (A contended Macrobius passage has Eratosthenes’ Sun 27 times Earth’s size: I.Kidd 1988 p.454. Did Macrobius invert the ratio? If the math of $\S[F3]$ used smaller solar $sd$ (Heath 1913 p.312-314), perhaps also rounding $\pi$ to 3, then the computed Earth/Sun radii-ratio could be $\approx 3$, the cube of which is 27.)}

H2 Instead of walking 5000 stades or 500 nautical miles (nmi), the actual Earth-measurer walked merely (eq.4) c.200 stades or 20 nmi. Eratosthenes’ “experiment” was just an inadop theoretical exercise whose $C$ was swiped from Sostratos’ prior outdoor Pharos scheme, a grab-exposed by its preservation of the lighthouse-method’s 20% systematic error from unremoved atm refraction (vs 0% for Eratosthenes’ alleged method) which is thus indicated as uncounted in Sostratos’ era. Had he known of (corrective) eq.3, he would have found:

$$R = r_E/1.2 = 40800 \text{ stades}/1.2 = 34000 \text{ stades}$$

close to the truth (\S[B6]), 34400 stades. For naïve eq.2, perfect data would’ve given (\S[B3])

$$r = 1.2 \cdot 34400 \text{ stades} = 41300 \text{ stades}$$

The discrepancy with eq.13 is merely 1%, on the order of naturally occurring variations in eq.20’s 1.2 factor. So the ancient mystery of Eratosthenes’ $C$ has a solution.

I Pharos’ Height: Chosen for Sostratos’ Public Science Experiment?

I1 We next launch a speculative (‘til eq.24) attempt at finding the Pharos’ exact $h$. (The following reconstruction of precise $v$ originated subsequent to $C_S$’s rough estimate of it.) The Pharos was a pioneering, literally-superlateral civic-science project. So: was its height $h$ a proud world-lighthouse-record round number of Greek feet? (Greek foot $\div 12^\prime 17$ English.) We already have evidence (\S[C2]) that $h_L \approx 1/2$ stade, so was the Lighthouse deliberately constructed to be 300 Greek feet high, the flame exactly (vs eq.4’s roughly) 1/2 stade above sealevel? — thereby DISAPPEARING eq.2’s denominator (a streamlining possible only because Sostratos has it-in-stades), as eq.2’s $r = \sqrt{v^2}/2h_L$ becomes simply:

$$r = \sqrt{v^2}$$

So anyone could find the Earth’s radius $r$ in stades, just by pacing $v$ in stades and squaring it. The massive metal ring in Alexandria’s Square Stoa was a public-science equinox-detector (Alm 3.1), so could the sailor-beacon Pharos have doubled as a huge round-Earth-measure public-demo science experiment (as the Empire State Building originally doubled as a dirigible-dock)? Was such a neat idea planned (c.270 BC, the Museum’s apogee: \S[T fn 33]) by Pharos-builder Sostratos & fellow scientists, who thus should (\S[A2]) have found $r = 40800$ stades (eq.24) before Eratosthenes? Our speculation isn’t disconfirmed if 40800 is consistent with the square of a 3-digit integral $v$: there is only a 25% a priori probability that the 1/2-stade-Pharos-height theory will meet this condition. If Sostratos’ $r_E$ were, say, 40600 or 40700 or 40900 stades, our eq.21 speculation would collapse. But, rooting $r_E$:

$$\sqrt{40800} = 201.99$$

\footnote{Once the 5000 stades baseline led (eq.7) to $C_K = 250000$ stades, it is possible that the question of parallax was raised. Parallax correction for an Alexandria S.Solstice culminating Sun at 1000 would shave 1% off the zenith distance and thus add 1% to the circumference, yielding c.252500 stades (or (rounding low) 252000 stades (700 stades per degree) which offers an alternate explanation (vs \S[D3]) for the origin of that famous value. If $7^\circ 12^\prime 1/2$ was not rounded to $7^\circ 15^\prime$, then $C = (5000 \text{ stades}) \cdot 360^\circ/7^\circ 12^\prime 1/2 = 249711 \text{ stades. Adding 1% yields 252208 \text{ stades}}$.}
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Le., the 1/2-stade-high-Pharos theory survives. So, using it, we’ll compute out a determination of \( r \) on the assumption that Eratosthenes’ measured (§B5) sealevel Pharos-visiblity distance \( v \) was

\[
v = 202 \text{ stades}
\]

(Not far from the crude ¶C1 estimate used in eq.4.)

I2  When these values are substituted into eq.2 (or eq.21), the result is:

\[
r_E = \frac{v^2}{2h_L} = (202 \text{ stades})^2/(2 \cdot 1/2 \text{ stade}) = 40804 \text{ stades} = 40800 \text{ stades}
\]

which neatly matches the Sostratos-Eratosthenes radius (eq.13).

I3  To illustrate the accuracy of the work behind Sostratos-Eratosthenes’ value, we check via eq.3, using the real Earth-radius \( R = 34400 \text{ stades} \) of §B6, and (somewhat over-ideally taking the equation’s 1.2 factor as exact) find that a perfect Pharos Experiment for a 1/2-stade Lighthouse would have measured \( v = 203 \) stades. Not only does this (compared to eq.23) evidence the care of the Greek scientists who performed the necessary measurements, but it also reminds us that (because \( v \) is squared in eqs.2&28) the relative error in the ancient experimenters’ resultant \( r \) is about double that of \( v \), so that their finding an Earth-radius 19% high (vs 20% high expected) shows experimental error of not 1% but roughly half that.

NB. This point is independent of the 1/2-stade Pharos theory, and applies also to the Sunset Method (§A4[d]), whose resulting \( C_P \) (eqs.26&28) likewise depends upon the square of the crucial measurement. (Inversesquare of time-interval between sunsets in that instance. See Rawlins 1979.) In any case, since the 1.2 factor is not rigidly precise, the proper conclusion is that the two widely adopted ancient Earth-measures, Eratosthenes’ (\( r_E = 40800 \text{ stades} \); eq:13) and Poseidonios’ (\( C_P = 180000 \text{ stades} \); eq:26), are so close (eq.28) to the values expected from the Pharos and Sunset experiments, respectively, that we can regard both tiny discrepancies as within experimental noise (¶H2).

I4  So the matches for both famous ancient Earth-size values provide as precise a validation as one could reasonably require, for the sea-horizon-refraction theory of the values’ origins. They are thus a spectacular refutation of & rebuke to the ubiquitous modern cult that has misled generations of young scholars into accepting the fantasy that ancient science was empirunpecule: see, e.g., ¶J2 §§A1, A6, B3, & especially the priceless gem at ¶2 fn 20.

J  Playing-Accordion with the Stade

J1  There has been a long tradition of attempting to force agreement of the Eratosthenes and Poseidonios values with each other and with reality by arguing for whatever stade-size would make-E&P-right. But it is encouraging to report that this sort of manipulation is no longer taken seriously by most specialists. Dicks, Neugebauer, Berggren, & Jones never fell for it. [Engels 1985 mashes it.]

Amusing details of testimony-twisting (used to carry out such programmes) are exposed at Rawlins 1982N App.B and Rawlins 1996C fn 47.

J2  Eqs.24-28’s matches gut not only the credibility of stade-juggling-for-Eratosthenes but even (¶3 fn 13) the very need for it. [Note added 2013. Despite the good sense of top scholars, eminent forums&books & [Wikipedia] are the prime promoters of such folly, while popular sources (Webster’s & Baedeker) correctly adopt the 185m stade.]

J3  Lack of serious instability in the Hellenistic stade is also detectable from Ptolemy’s geographical evolution. In the 18th century, Pascal Gossellin 1790 noted that the macro-geographical longitude errors of Ptolemy’s Geography (GD) showed exaggerations of 30%-40%. Rawlins 1985G p.264 used least-squares analyses to find the mean exaggeration (factor 1.36 ± 0.04) and explained this as the result of switching Earth-sizes.

J4  In the Almajest Ptolemy was under Hipparchus’ influence, so he presumably adopted his C which was (Strabo 2.5.34) Eratosthenes’ \( C_E \) (§D3). When Ptolemy switched (¶3 fn 13 & ¶L3) to \( C_P \) for his later GD, he obviously used travellers’ east-west distance-estimates more than astronomically based longitudes and thus (in order to switch

his great-circle scale from 700 stades/degree to 500 stades/degree) had to stretch degree-longitude-differences between cities. So the Almajest longitude-degree distance from Rome to Babylon was increased by over 30% (¶3 fn 13), nearly the ratio of the prime Earth-sizes, plain evidence that the stade was a constant in the midst of geographical transformation.

K  How Atmospheric Refraction Fruitfully Explains BOTH Standard Ancient Earth-Size Estimates’ Precise Errors

K1  As noted at ¶A4 & ¶B4, atmospheric refraction makes the ¶A4[d] Sunset Method of Earth-measure (Rawlins 1979) give a result low by factor 5/6. Since the actual circumference of the Earth is virtually by definition 21600 nautical miles (a nmi is now defined as exactly 1852m, nearly identical to \( \pi \) of great-circle measure on the Earth’s globe), then given that a stade (185m) is almost exactly 1/10 of a nmi, we know the Earth’s real circumference is:

\[
C_0 = 2160000 \text{ stades}
\]

(600 stades/degree). The Poseidonios value (Strabo 2.2.2) of the Earth’s circumference (which could appear only after the 2nd century BC advent of sph trig: Rawlins 1979) was

\[
C_P = 180000 \text{ stades}
\]

(500 stades/degree), which agrees exactly with the ¶A4[d]-predicted Sunset Method’s –17% error; and we have doubly found (eqs.10&18) Eratosthenes’ empirical circumference

\[
C_{E0} = 2560000 \text{ stades}
\]

(711 stades/degree), the +19% error of which is almost perfectly consistent with the ¶A4[c]-predicted Pharos Method’s +20% error.

K2  While the Kleo Method (eq.7) should lead to a nearly correct circumference-estimate (for the method’s near-north solar altitudes, refraction would be trivial), the two actual standard ancient values for the Earth’s circumference are 6/5 high and 5/6 low, thus eliminating the Kleo Method right off the top — which backs up our earlier elimination of it through a different approach (¶G2). When we check ratios of theory and testimony, we find virtually exact hits on the horizontal-light-ray atmospheric-refraction-hypothesis’ 6/5 factor, for the sources of both attested standard C:

\[
C_N/C_0 = 256000/216000 = 5.93/5 \quad C_E/C_P = 216000/180000 = 6.00/5
\]

which shows how dramatically successful the refraction theory has proven — an ideal example of a fruitful theory, it uses the same mechanism (horizontal atmospheric refraction) and the same stade (standard 185m) to near-perfectly explain both of the only two widely adopted ancient Earth-size estimates. (NB: Rawlins 1996C fn 47.) Oddly, the spat attending the Aswan-Alex tale’s reality: Since the Nile is far from straight, how would one reliably measure the length of a path (really c.10% less than 500 nnm) which could not have been direct without highly arduous and dangerous travel over desert? Also, Eratosthenes placed (Kleomedes 1.10) Aswan due south of Alexandria (see also Rawlins 1982N), though travel straight from Alexandria to Aswan would have to knowingly steered 20° east of south to hit Aswan. Finally: if the Kleo Method were actually carried out (across awful Egyptian terrain) over a N-S straight line, it would get an accurate result. (More than 1000 before the experiment was actually done [elsewhere], successfully.) [Did an ordnag 1000-stade Nile-parallel version occur c.300 BC? See DJ 20 1 n.2.]

* For those who cannot immediately see why the two methods yield such different results (one over 40% higher than the other!): see DJ 20 3 ¶8A, where extreme examples easily illustrate why one method leads to a too-high result and the other to a too-low result. (The Mountain Method is examined there instead of the Pharos Method, but the atmosphere’s effect on each is similar for low mid-height.) That is, if Earth’s sealevel atmosphere-density gradient were high enough, horizontal Pharos-light-rays’ curvature could be the same as Earth’s, so (for null extinction) the Pharos would be visible no matter how far away one receded, and this infinite \( v \) would (by eq.2) make computed \( r = \infty \) a flat Earth. For the same dense atmosphere, the Sunset Method would yield \( r = 0 \) (DIO loc cit; Rawlins 1979 eq.13).

---

\( \text{Note added 2013. Despite the good sense of } \)

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\( \)
K3 But given the cascade of startling new matches above [& at this page’s end], little doubt can remain that the unattested Pharos & Sunset Methods underlay the only 2 standard ancient Earth-sizes, \( C_p \) & \( C_p \), resp. Which tells us what has often been shown in these pages (see, e.g., fn 9, §2 fn 38, §3 §A3): much of high ancient science has been lost & so is only recoverable by reconstruction, a finding unsurprising to most of us, yet which nonetheless eludes induction-challenged chauvinists who whenever convenient will (DIO 11.1.3 p.3 & §2 fn 7) pretend that they cannot accept anything without extant textual explanation.

K4 But even more important than such details is the implicit general message contained in the foregoing precise vindication of the atmospheric theory that coherently explains the 2 ancient Earth-measures, the fact that both agree with the theory to one percent (§13 & eq.28) overturns the long-established delusion (§14; §2 §A1, fnn 20&31) that the Greeks were mere theorists with little interest in or capacity for empirical science. DIO has been contending otherwise since its 1991 inception, arguing that this “blanket libel of ancient scientists” (DIO 1.1 §1 fn 24) is false — and obviously so, to those possessing a genuine acquaintance with the way scientists think and work. We hope that the present paper will help diffuse a more appreciable view of the priorities, ingenuity, and perfectionism of those ancient Greek pioneers who laid the baserock-beginnings of high-precision science.

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InductionQuake AfterShock
This paper was 1st posted and referees alerted on 2008/3/12. But on 2008/3/15, DR happened upon the obscure sole extant ancient estimate of the Pharos’ height \( h \): 306 fathoms (Steph.Byz 735a [1825 ed. 3:1251]; Strabo [H.Jones] 8:246), taller than any building ever. Unless Greek feet were meant. If so, \( h \) is within 2% of our eq.21, and \( v = 204 \text{ stades} \). But it’s suggestive that 306 & 40800 are both unround by factor 1.02. Did a later scholar try estimating \( h \) by putting \( r = (252000 \text{ stades} )/2\pi \approx 40000 \text{ stades} \) (Neugebauer 1975 p.654) and \( v = 202 \text{ stades} \) (eq.23) into eq.2 to find \( h = 0.51 \text{ stades} \approx 306 \text{ ft} \)? Regardless, after years of exaggerations, we now have doubt evidence for a conservative estimate:

\[
\text{Pharos' flame's height } h_1 = 93\text{m} \pm 1\text{m}
\]

\[ ^9 \text{ Such achievements as eclipse-cycle determination (§2 §F9) of all three of the Greek lunar periods (to a precision of one part in ordmag at least a million) might've triggered parallel enlightenment.} \]

‡2 Aristarchos Unbound: Ancient Vision
The Hellenistic Heliocentrists’ Colossal Universe-Scale Historians’ Colossal Inversion of Great & Phony Ancients History-of-Astronomy and the Moon in Retrograde!

I am restless. I am athirst for faraway things.
My soul goes out in a longing to touch the skirt of the dim distance.
O Great Beyond, O the keen call of thy flute!
I forget, I ever forget, that I have no wings to fly, 1 that I am bound in this spot evermore. 2

Summary
Genuine ancient astronomers made repeated use of the fact that the human eye’s vision-discrimen limit is ordmag 1/10000 of a radian. Use of this key empirical figure is connectable (§F9) to all 3 of the huge astronomical scales attributed to the school of Aristarchos of Samos, the 1st certain public heliocentrist visionary. Evidence also suggests Poseidonios’ sympathy with (and enhancement of) this same vast heliocentric worldview (§F2), which entailed a universe a trillion times larger than the geocentrists’. 3

A Mufa Vision
A1 Today, it’s widely supposed that the astronomy of Aristarchos of Samos 4 (c.280 BC) was mostly theoretical; i.e., he is viewed within the constraints established by the fabulously logical reasoning of modern history-of-astronomy (hist.astron) on Greek science. For example, Neugebauer 1975 (pp.643) presumes that all the work attributed to Aristarchos has “little to do with practical astronomy”. The famous “Aristarchos Experiment” based its ratio of the distances of the Sun&Moon upon the half-Moon’s occurring 3° sunward of.

1 Likewise, the historian of things ancient has no temporal wings to fly into the past. He can experience bygone times only in his imagination. Rising from an evidential ground, he soars above it only by the strength of his inductive skills.

2 From the Indian poet R.Tagore. This particular poem inspired Viennese composer Alexander von Zemlinsky to his most dramatic musical success: the first song of his 1923 Lyric Sympony Op.18. It should be stated explicitly that DR shares none of the mysticism of either artist. And I note that Dionysos the Renegade (c.300 BC), for whom I suggest (DIO 1.1 §1 fn 23) Aristarchos named the 365\(3/4\)th Dionysos calendar, based his philosophy ultimately upon hedonism. (Another part of the same Tagore poem contains the famous phrase, “stranger in a strange land”, now perhaps best known as an R.Heinlein sci title. The phrase is not original with either Tagore or Heinlein. It is from Exodus 2:22 & 18.3. It also appears in Twain’s 1870 satire, “Goldsmith’s Friend Abroad Again”.)

3 [Note added 2011: Trillion-factor based on cubing result of fn 72’s concluding ordmag-rounded calculation. (Without rounding: said factor will be an ordmag less.)] Rawlins 1985K proposes that the highly accurate Venus & Mars mean motion tables (major improvements to Aristarchos’ tables), underlying the Almajest 9.3 tables of those 2 planets, were originally designed for epoch Kleeplata 1 (51/95). Chronologically, this is consistent with Poseidonios being among the promulgators of the original tables, whether or not based on his own work.

4 Unlike most writers on ancient science, I use the Greek ending “os” (instead of the Roman ending “us”) for Hellenistic individuals’ names. (E.g., Hipparchos instead of Hipparchus. Of course, other DIO authors are free to spell as they wish in their own articles.) The particular situation that caused me to do this was the question: if scholars are so casual about endings that they unblinkingly refer to “Aristarchos of Samos”, then: is it equally OK to use “Aristarchos of Samus”? (Given Aristarchos’ revolutionary contributions, we note in passing that Samos was historically notorious for rebelliousness.)
of quadrature (eq. 4 below); but hist.astron-don Neugebauer 1975 (pp.642-643, quoted by Van Helden 1985 pp.66&167 n.8) claims that this is “a purely fictitious number” (part of a purely mathematical exercise?), and that the data of a supposed lone extant Aristarchos ms, “On Sizes & Distances” — which DR ascribes to an otherwise unknown soon-after indoor mathematical pedant pseudo-Aristarchos — “are not anything but arithmetically convenient parameters [§3C], chosen without consideration for observational facts which would inevitably lead to unhandy numerical details.” (One might as well straight-out call Aristarchos an idiot. Such pontifications by the ever-intolerantly arrogant Neugebauer-cult — formerly known here as the Mufa — themselves ignore the crucial significance of a glaringly “unhandy detail”, the demonstrable falsity of the longtime attribution to Aristarchos of solar distances — grossly overblown unempirical 25” solar diameter. It is not a JHA-scored modern novitiate, but no other than the immortal Archimedes, who says [and see additional confirmation at fn 33] that the real Aristarchos got-it-right. §C1 item [a,].) Similarly, on 1984/6/28, O.Gingerich astonished a small Zürich gathering (including van der Waerden, myself, my wife Barbara, and others), by supposing aloud that Aristarchos’ heliocentricity was not really a full-fledged theory: perhaps he’d merely broached the idea one day while chatting with another scientist.

A2 See OG’s similar 1996/8 remarks (12° after the Zürich meeting) at Gingerich 1996 — projecting his own bizarre Aristarchos-demoting fantasy onto Hugh Thurston, who has informed me, in further astonishment (plus DIO 6/3 §[H1]) that the JHA’s old habit of careless mentalism (Rawlins 1991W §[B1&2], DIO 2.1 ddag 3 §[C9]), that that this is naturally just Gingerich’s imagination at work. Art Levine’s satire comes to life yet again in the unique JHA11 What follows will suggest that these Neugebauer-Mufa appraisals are as correct & perceptive as ever. (See also fn 70.)

A3 But I must call a brief interlude at this point, in order that the reader not miss the weird inversion going on here in §A1&A2, the Neugebauer-overall-ancient-astronomy-conception’s perversion-pinnacle: *rebel&heliocentrist-pioneer Aristarchos was a non-observing fabricator, while go-along-geocentrist&data-faker* Ptolemy was antiquity’s fraudulence. (See the typically entertaining *JHA* editorial statement cited here at fn 17 & fn 64 [and specially placed on-line by DIO at www.dioi.org/JHA.htm#ghgs].) And note its debts to O.Neugebauer & O.Gingerich’s “fn 20.” Evans 1992 p.68 still takes the pseudo-A’s 2” solar diameter so unlikely that this author of Oxford Univ Press’ *History and Practice of Ancient Astronomy* draws overcartain — not to mention conclusions — about the evolution of ancient astronomy during its two most productive centuries. (The usual for culists who think great ancient astronomy only flowered with the faker Ptolemy.) See also fn 16.

If heliocentricity alone is held not to prove that Aristarchos had a planetary theory, we may ask what Ptolemy meant by (Heath 1913 p.304) heliocentricity “saving the phenomena?”. If we merely consider Earth & Sun, heliocentricity causes no simplification of theory — but (§A5) the elimination of epicycles does accomplish this. For years, such an obvious point was implicitly understood by able historians. But, with modern poLarchons’ advent, acceptance of (or merely grasping) even elementary ideas has come to require awesome mental struggle.

[See DIO 8 §[J 11, which relays Levine’s spoof of his own WashMonthly’s penchant for projection, chuckling that fellow writers reading WM accounts of their output “find themselves espousing ideas they’ve never even heard of, much less agree with.”]

Ptolemy’s fraudulent tendencies did not end at mere fabrication of data. He had also a proclivity for suppressing all mention of inconvenient facts. E.g., when he pretended (Almajest 3.1) that the solstices of Aristarchos & Hipparchos were consistent with the Hipparchos PH solar theory (Rawlins 1991W §K10) adopted for the *Almajest*, he suppressed (DIO 1.1 §[A5]) the time of each of these 2 solstices and no other, of the common of equinox-solstice data provided therewith — thereby hiding the fact that each disagreed with said theory. (Each by the same amount: minus √3°.) Likewise, to prevent heliocentrist heresy from sulllying his readers’ minds, Ptolemy at Almajest 9.1 discusses the question of whether Mercury and Venus circuit points appear above or below the Sun — but not the possibility (already entertained by Aristarchos and Theon of Smyrna among others) that these planets’ orbital center was virtually at the Sun. (Similarly, when dispensing with theories that the Earth moves or spins, Almajest 1.7 doesn’t mention heliocentrism.)

[See, e.g., the bizarre attempt at Neugebauer 1992 p.284 (shamelessly followed by, e.g., Evans 1992 and Evans 1998 pp.273-274 & n.32 and even by Dambis & Efremov 2000 p.133 [which was refereed by Evans]) that Ptolemy was a better observer than Hipparchos. Oblivious to the 2 mens’ relative errors, random & systematic: Rawlins 1999 §[E3-E4]. This particular hyper-inversion (started by Vogt 1925) is based merely upon the fact that semi-popular Hipparchos Comm commonly uses roundings which are much more crude than those in the Catalog or those in Hipparchos’ declinations (Almajest 7.5). Furthermore, these apologia utterly and entertainingly conflict with those emitted by Huber (DIO 2.1 §[JH]), Swerdlov 1989, Graßhoff 1990, & Gingerich 2002, who contend that Ptolemy’s greatness in data-reportage was shown not at all by his alleged observations’ superior accuracy but rather through the inner planetary projection by which he either fudged his inferior observations or replaced them by forgeries from theory! Question: Does an intellectually healthy and open community leave itself open to tommy-smart sophistry by getting into such pretzel-thought?

[Despite Rawlins’ 1992K §[J1, Gingerich 1992K p.105 nonetheless persists in stating that there was an “absence of proof” of heliocentricity even as late as the 16th century. This though Gingerich 1992K (earlier on the same page) notes that the outer planets’ motion exhibited a peculiarly as cohesive as the inner planet oddity cited at Rawlins 1991P §B1. (Uncited by Gingerich 1992K. Naturally.]

But distances are never computed in pseudo-A’s “Sizes & Distances”. (See Neugebauer 1975 pp.636, 639, & 643. Also Rawlins 1991W fn 220. Scrupulous and able mathematical analyses of this work are available by Heath 1913 data and Berggren & Sidoli 2007.) Perhaps realization of the contra-outdoor-sky results (§C1) of such calculations stopped pseudo-A from continuing his ms.
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§G2 item [c], Rawlins 1987, & Rawlins 1991P) Yet one looks in vain for mention13 of it in classic Muffia output, including Neugebauer 1975 & Van Helden 1985. Centrist historians have long insisted that Greek epemherides did not exist until at least Hipparchos’ time. By contrast, DR suggests that it was the onset of planetary tables in Greek science, possibly even as early as 4th century BC, which caused the conversion of intelligent scientists to heliocentrism, since planetary tables inevitably exhibited with rigid fidelity — elements of the “solar” motion in each and every planet’s model. (See Rawlins 1987 pp.237-238.)

A6 We find (as at Neugebauer 1975 pp.643 & 646) not a hint of the source of Aristarchos’ 10000 AU distance to the fixed stars (eq. 14), namely, the invisibility of stellar parallax for a heliocentric Earth-motion (§B2). This is obvious to any scientist worth the name. (Most underestimate the point immediately.) It is implied in the ancient work, the “Sand-Reckoner” (Archimedes p.222). The point is regarded as too obvious for elaboration by, e.g., van der Waerden 1963 (p.203). (By contrast, Neugebauer 1975 p.643 says that the 10000 AU radius Aristarchan universe reported by Archimedes p.232 has “as little to do with practical astronomy” as Aristarchos’ Experiment: eq. 4.14. B.Rawlins wonders if selling putative Babylonian originality and genius has led Mufosi into denigrating Greek empirical work occurring before the central Babylonian astronomical texts’ era.) And this realization is (along with §A5) another point which is absolutely critical to understanding Aristarchos’ vision, as well as representing the crux of the two-millennium-long (!) heliocentrist-vs-geocentrist debate — the greatest controversy in the history of astronomy, ranking with the (far briefer) natural-selection fight as one of the focal points of the rise of science and rationalism. (I.e., the Muffia’s obsessive pretense, that geocentrist astrologers were brilliant, is glorifying the side that suppressed the actual great scientists of their time. Even the Roman church isn’t trying to cast those popes & cardinals as templates supposed Galileo as the actual top intellec[ts] of the medieval helio-vs-geocentrist dispute. So, in the field of outrageous historical-revision-apologia, the Muffia outdoes even the master.)

A7 The claim that Hipparchos, “improved” heliocentrist Aristarchos’ measure of the universe is particularly curious, since Hipparchos and other geocentrists probably put the stars at roughly Ptolemy’s distance (ordmag 10 AU), vs. Aristarchos’ ordmag 10000 AU. (See §E5. Actual distance of Proxima Centauri = 270000 AU.) In brief, Muffiosis15 regard it as just a meaningless coincidence that heliocentrists proposed the biggest ancient universe. This achievement, of the finest ancient scientists, is passed off as just primitive, perhaps numerical guesswork — even while the worthless & demonstrably (§F7) false numerological speculations of a succession of geocentists and-or astrologers (see tables of Van Helden 1985 pp.27, 30, 32) are palmed off on the modern scholarly community as the best science available in antiquity,16 without even referencing dissenting literature.

A8 How could such a mix of innocence and prejudice (e.g., in fn 14) adorn a standard (gov’t funded) history-of-astronomy survey volume, written by historian (& sometime JHA Adv Editor) A.Van Helden? The answer is found in the ancient astronomy archives he depended on. Van Helden 1985 p.vii (see also p.168 n.2): “In the course of this project I incurred many debts. . . . A Research Fellowship from the [NEH] . . . . For the mean value of the [planets’] orbit [this] I have relied heavily on the researches of [Neugebauerian cups] Bernard Goldstein [also sometime NEH beneficiary] and Noel Swerdlow.” (Van Helden 1985 was published by Swerdlow’s University of Chicago.)

B The Cohesive Myriad Factor

B1 Just after midnight of 1992/1/25-26, DR happened to ask himself the following question: since eq.45 of Rawlins 1991W explained17 “Aristarchos’ Experiment” by presuming that Aristarchos had regarded the angular-discrimination limit of man’s vision to be about

\[
\mu = \frac{1}{10000} \text{ of a radian}
\]

(1) then (for null visible stellar parallax), shouldn’t his distance \(r_\phi\) to the stars be 10000 Astronomical Units? After noting this in my diary, I consulted the “Sand-Reckoner” (Archimedes p.232) and found that it reports that Aristarchos’ universe had a limiting radius which was indeed 10000 times bigger18 than an AU.

13 The cause of this imposition (and presumably of the who-cares-who-was-right-or-brave-or-ethical-or-original idee-fixe of the modern ancient-astronomy establishment: fn 67) is simply that the number of extant ancient texts created by competent scientists is tiny compared to the lot of supernumerous pseudo-science that survives. Thus, realistic grantmanship virtually forces a coherent pretense that the latter is respectable scientific material, requiring decades of well-funded research. (See [H4; also Rawlins 1984A pp.984-986 & Rawlins 1991W fn 266].) [Fortunately, some professional historians’ evaluation of Ptolemy has lately been less defensive and more realistic. See esp. Alex Jones’ analyses.]

14 For the Moon’s horns must deviate 1/5000 of a radian from the middle of the terminator (\(\phi\)). The arcsin of this divided by 1/10000 of a radian, the value underlying (eq. 33) of the ratio of this to Aristarchos’ lunar semi-diameter (1/4: eq. 3) equals \(2^3\pi^{38} ≈ 3\). (Rawlins 1991W §R9’s analyses used 0.4 instead of 1/10000 of a radian, yielding \(2^5\pi^{37}\) by the same equation.) Note that DR has not arbitrarily conjured-up \(\mu ≈ 0.4\) for the purposes of this paper: Rawlins 1982G (p.263, in a quite different context) noted that the mean angular separation of the retina’s foveal cones is 0.4°-0.5°. (The arcsin of 0.4/15° is \(3^\circ 26^\prime\) ≈ 3°.) I found by experiment long ago that the eye’s primitive visual limit is about 1/3°. (The arcsec of this divided by 1/4° is 2°33’ ≈ 3°.) Aristarchos presumably performed just such an experiment to arrive at his value for \(\mu\). These estimates agree closely with Dawes’ limit (consistent with diiffusive Airy disk) for a human eye’s pupil-size, and all (1991W) \(\mu ≈ \frac{1}{10000}\) of a radian, the value underlying (eq. 32) all Aristarchan celestial scales. [Note added 2010: Was 87° computed from a null experiment? See www.dio.org/cot.htm#nxm.]

15 The “Sand-Reckoner” development is found in Archimedes (pp.221f) or Neugebauer 1975 (pp.643-647). Aristarchos would (as also Poseidonios: Heath 1913 p.348) likely call 10000 AU a lower not upper limit, but Archimedes prefers the latter (to count sand-grains). The same factor-of-2 ambiguity, which we encountered in a previous paper (Rawlins 1991W §§R9-R11), also exists here (Archimedes p.222 & Neugebauer 1975 p.646). Realizing that the full stellar parallax baseline was really only 2 AU (§E4), we see that, by an alternate interpretation here throughout, we could found Aristarchos’ universe scale upon the limit of human vision being 1/5000 (not 1/10000) of a radian. Against this is not only fn 17 but also the obvious preferrability of whole ordmags — so obvious from Archimedes’ “Sand-Reckoner” (which also notes that, at the myriad-mark of 10000, the Greek numerical notation starts repeating itself). On the other hand, if Aristarchos’ development employed more exact ratios than powers of 10, these figures might have been rounded to the nearest ordmag by Archimedes. The evidence is not certain, but I lean to believing that the original use of 10000 in eq. 13 was Aristarchos’.
Thus, I realized at a stroke that all the famous Aristarchos astronomical scale measures could turn out to be consistent with the *very same empirical base*, namely, the limit of human vision was experimentally realized by Aristarchos to be about 1/10000 of an arcmin. (And this is about right for raw human vision: see fn 17.)

NB: It is *asserted* that Aristarchos investigated optical science. (Thomas 1939&41 2:3.)

It may seem remarkable that no one previously noticed this. But such an astonishing oversight is, in fact, precisely what one would expect of the history of ancient astronomy community as now constituted, since the enterprise is primarily into detailing-repeating the contents of ancient sources (and other safe-predictable sabbatical-length projects), and “original” research largely involves relating source A to source B — with but very occasional success at inducing the science behind either A or B. (Muffia disability here is seasoned with naked contempt for non-Muaffa scholars who try.) Such work is more apt to encyclopedist-bibliographers, than to thinking scholars. (Few Muaffia capos are scientists. They naïvely presume that some mathematics background will suffice to protect them from misperceiving ancient methods; but: this presumption is just one more Muaffia misperception. The idea that practical experience in relating empirical data to theory might be of use in doing history of science would seem to be self-evident. Not to Muaffios.)

### C Moon & Historians in Retrograde

#### C1 For roughly 2 millennia, since Eratosthenes (§1 fn 3) and Pappos (Rawlins 1991W fn 220), the allegedly Aristarchos work, “On the Sizes & Distances of the Sun & Moon”,

19 E.g., Van Helden 1985 p.7 on Aristarchos’ Experiment: “his method proved to be impractical. Even if he would have tried to measure his numerical data accurately, he would have found that determining the exact moment of dichotomy [half-Moon] and then measuring the angular separation of the two luminaries is a hopeless task.” Mere echo of Neugebauer’s equally indoor ignorance: fn 5.

20 Since a hallmark of the Neugebauer sales-cult is its consistent confusion of superstitious ravings (e.g., §3A&K7) with genuine science, one can readily understand how this clique got into the habit of overlooking the very idea of attempting to relate real science to ancient texts. See, e.g., Gingerich 1976’s hyperdiagnostic-alibi-quotes defending Ptolemy (taken from Neugebauer 1975 pp.107-108), e.g., “It makes no sense to praise or condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure”. (Compare such added archonal naive to the realities of §F9 and §1 fns.) This astonishing bit of mis-hegemonization (definitivelyvalorized at §1 §§E&K4 and fn 9) was dished up to excuse Ptolemy’s Almagest 5.4 analysis, a fudgepot so incredible that even genial centrist W.Hartner calls it a “fairly-tale” (Hartner 1960 p.26). O.Gingerich’s promotion of ON’s rationalization appeared in the American Association for the Advancement of Science’s main organ, *Science*. And it reflects official editorial policy at OG’s extremely handsome *Journal for the History of Astronomy* (see fn 6). It would be pleasant, even if naively visionary, to imagine that DR might someday induce an astronomy-historian to attempt an experiment in empathy: imagining that he is the resurrected shade of a genuine ancient astronomer. In life, this scientist had spent decades [a] scrupulously testing (against observed data) various competing theories, and [b] empirically refining orbital elements & other astronomical quantities. He now returns to find 20th century archons slaying or ignoring this honest labor, instead preferring astrologers’ lazy fake-observations & other plagiarisms, maybe ripoffs of the shade’s own original genuine work. Just the sort of appreciation scientists pour out their lives for. (See fn 67 & Rawlins 1993D §B3.)

21 Among numerous instances (Neugebauer 1975 p.655 n.1): “The famous paper by Hultsch [1897] on ‘Poseidonius über die Grösse und Entfernung der Sonne’ is a collection of implausible hypotheses which are not worth discussing.” However, I urge non-Muaffios to not emulate such arrogance and to instead appreciate that even illmanered bigots can make genuine contributions, which should be treated strictly on their merits.

22 There is also an implicit notion that avoiding offending archons will protect one from misadventure. Perhaps, but the level of scholarship resulting from such artificiality has been a contributing factor in judgement-degeneration that has cursed modern history of ancient astronomy.

23 The failure of prior historians, to face the outlandish absurdities of the pseudo-Aristarchos ms, is a mystery. (None has previously realized that it entailed a retrograde Moon, despite our broad hints [fn 25] on earlier inside covers.) See, e.g., Heath 13 p.350, Neugebauer 1975 pp.634-643 (which came nearest to fully realizing the ms’ folly — but then attacked Aristarchos instead of the ms’ attribution); also Neugebauer 1992 p.68.

24 “Sand-Reckoner” p.223. With respect to the strange controversy (Rawlins 1991W fn 53) as to whether Aristarchos (also Timochares & Aristyllus) used degrees: note that the various empirical magnitudes surely connected to Aristarchos are all easy fractions or multiples of degrees: 1°/2 (solar diameter), 3° (half-Moon vs quadrature), & 10°/23 or 32°/3 (suns remaining: Rawlins 2002A eq.6).

25 The “Upcoming” lists (inside-cover) of *DIO* 2.2 & *DIO* 2.3 published warnings of this bomb well over a decade ago (1992): “Histisci accepts, as genuine, famous ancient treatise putting Moon into retrograde!” The *JHA*-H.A.D. crowd never picked up on the clue. Is anyone surprised?

26 See the equally-ironic comments at *DIO*-JHA 1.2 fn 284. The Neugebauer 1957 p.196 passage (there compared to p.206) was first brought to DR’s attention by the late R.Newton.

27 In this handsome photo, the Moon is seen in its rising aspect (obvious to an outdoor astronomer) low behind the Sphinx, which is looking at the camera. But the Sphinx faces eastward.
C3 Let us see how the deliciously zany retrograding consequence ($\S 11\{g\}$) comes about. Pseudo-Aristarchos’ implicit mean lunar distance is (eq.5) $r_M = 20\degree.10$ (where 1$\degree$ = 1 Earth-radius). But it is well-known that the Moon’s sidereal period is & was 27$^d.32$ (mean sidereal motion 0$^o$.549/hr) or 27.4 sidereal days. So an observer on the Earth’s Equator, watching the Moon (with mean distance & motion), transiting in the zenith, must therefore be travelling 27.4/20.10 = 1.36 times faster than the Moon, which will thus appear to be moving in reverse at about 0$^o$.2/hr — the peak-speed of a (diurnal-synodic) retrograde loop (similar to the annual-synodic retrograde loops familiar to planet-watchers).30

C4 Recall another serious problem with the pseudo-A work. We will define $\gamma$ as the half-Moon’s angular distance from quadrature. Rawlins 1991P $\S 1$ suggested 31 that the famous Aristarchos value

\[
\gamma_A = 3^o = \arcsin(r_M/r_S) = \arcsin(1/19) \tag{4}
\]

was an upper bound, not a precise figure. (The notation: $r_M =$ the Moon’s distance, and $r_S =$ the Sun’s distance.) Even allowing this, 32 Rawlins 1991W fn 272 showed that as merely

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an upper bound, said 3$^o$ figure depends upon visual discernment of ordmag 1/10000 of a radian — c.1$^/$3, very near the limit of human ocular discernment. (I am of course taking it for granted that the fineness of human vision has not changed significantly since 280 BC.)

C5 We have seen earlier from Eusebius ($\S 1\{e\}$) that Eratosthenes placed the Moon at a distance of 19 Earth-radii, a figure presumably gotten from pseudo-Aristarchos. (Unless universe-shrinking Eratosthenes was himself pseudo-A. The document’s curiosities [e.g., $\S 1\{f\}$ cannot be traced back beyond Eratosthenes].) And this is the figure computed from pseudo-A’s propositions 11&17 at Heath 1913 pp.338-339. Yet Heath bases this upon averaging depressingly crude brackets associated with needlessly pedantic geometric proofs. By contrast, an exact computation (e.g., Neugebauer 1975 p.637) finds 20 Earth-radii instead of 19:

\[
r_M = \frac{1 + \sin \gamma_A}{1 + \sqrt{3}/2} \sin \theta_0 = 20^\circ.10 \tag{5}
\]

using pseudo-A’s false data ($\S 8\{e\}$ & $\S 2\{f\}$): shadow-Moon ratio $\sqrt{3}/2$ and solar semi-diameter $\theta_0 = 1'$. Question: if you wished to find 1/$\sin 1'$ or (virtually the same) the distance/size ratio for something subtending 1$'$, wouldn’t you just figure that the circumference is 2$\pi$ times the distance and $1'$ is 1/360 of that, so distance/semi-diameter = $360/2\pi = 57.3$? (The pseudo-A brackets instead can only put the number somewhere between 45 & 60! It’s hard to accept that Aristarchos was this limited.) Is there a more reasonable explanation for why a very simple computation which should have produced 20 instead got 19? [Our next speculation parallels known Hipparchan researches: Alm 5.11.] Try this: since DIO has for years pointed out ($\S 4\{c\}$) that $\gamma = 3'$ is probably an upper bound (not an exact figure), why not explore the obvious consequence of this assumption, namely, that Aristarchos (not knowing where $\gamma$ was in the range 0$'$ to 3$'$) simply made it null for solar distance $r_S \approx \infty (= 0')$. In that case, eq.5 becomes:

\[
r_M = \frac{1 + \sin 0'}{(1 + \sqrt{3}/2) \sin \theta_0} \pm 19^\circ.100 \tag{6}
\]

(More efficiently: $r_M \leq 60\pi \leq 19.1$.) So, Eusebius’ verification that a lunar distance of 19$'$ was an accepted figure turns out to lend potential if as-yet-speculative support to the common-sense DIO theory that eq.4’s $\gamma = 3'$ was indeed ($\S 4\{c\}$) an upper bound for Aristarchos, showing his openness to the possibility that the universe was many times larger than that implied by taking the 3$'$ figure as exact.

that the early 3$^{rd}$ century BC was the transition period when newly-invented trig was widely but not universally used by mathematicians. Or, Aristarchos may simply have opined that geometric clothing for his demonstration would enhance its academic impact.) [b] The implied visual precision would be impossible, anyway. The range (18 to 20) corresponds to $\gamma$ equaling 3$^\circ \pm 0.5'$.15 — which in terms of visual discrimination corresponds to half (fn 17) of 1$'$ (lunar semi-diameter) times a 0$'$, or barely 1$'$, clearly not visible. Rawlins 1991P $\S 1$ regarded 3$'$ as an upper bound. No other empirical interpretation makes sense. And we now find here that this seemingly speculative interpretation has led straight into realization of its consistency with Aristarchos’ other cosmic-measure work: $\S 1\{b\}.$

33 Has it been previously noted that Aristarchos’ near-contemporary Archimedes (probably a few years older and near and earlier years brighter than Eratosthenes) reports none of the follies of pseudo-Aristarchos? (Which perhaps sandwiches the time of pseudo-A’s origin into the 2$^{nd}$ half of the 3$^{rd}$ century BC.) The nearest he comes is in referring to Aristarchos’ Sun/Moon distance-ratio as being between 18 & 20, a mere confusion (identified elsewhere: fn 32) of geometric method with precision. But Archimedes doesn’t repeat any of the key giveaway screwups of pseudo-Aristarchos: 2$'$-wide Sun (indeed, he contradicts it), lunar distance 19$'$, Earth-shadow/Moon ratio $= 2$. Note also the clash between Archimedes-Aristarchos (eq.15) and pseudo-Aristarchos (Heath 1913 pp.339 & 350) on $r_S$: 10000$'$ vs 360$'$, respectively. Woe to Aristarchos’ works more welcome in Archimedes’ Syracuse than in Eratosthenes’ Alexandria (then of less-Greek rulership, and fiscally strained from funding wars, e.g., Pyrrhos’ $\S 1\{f\}$ See $\S 1\{f\}$. (What Alexandria instrumental star data survive from the 100$'$ after Aristyllos, 260 BC?)


29 The pseudo-Aristarchos Moon, at mean geocentric distance 20$^\circ.10$, will travel 20.1 times farther per Earth-circuit—than will an observer on the terrestrial Earth. But this circuit will take 27.4 times longer to perform. Thus, as noted above, the mean geocentric speed of the equatorial observer must be 27.4/20.1 = 1.36 times greater. When the Moon is in the equatorial observer’s zenith, he is only 19$'$ distant from pseudo-A’s Moon, so the Moon’s relative hourly angular “topocentric” or observer-centered motion is (20.10 27.4/20.10 1) times the mean geocentric hourly lunar motion (0$^o$.549) or: 0$^o$.2. (Obliquity’s cos = 92%, ignorable for rough mean-situation: [a] when the Moon is on or off the equator, its motion is not parallel to the terrestrial observer’s (i.e., pseudo-A’s epicycles); [b] when the Moon’s geocentric motion is parallel to the Equator, the Moon is not on the Equator.)

30 Maximum apparent retro-motion would always occur around lunar transit (which is one reason why $\S 2\{c\}$ calls National Geographic’s faked rising-Moon photo irrelevant to the present discussion), analogously to an outer planet’s motion near opposition. This entire effect may sound as if it is purely theoretical, whereas there is in fact a readily-discernable slowdown of topocentric lunar angular speed when the actual (not ancient-theoretical) Moon is high. I.e., there is a retrograde tendency, due to the Earth’s spin; but in reality this superposed parallactic motion’s speed is — due to the Moon being about 60$'$ (not 20$'$) away from the Earth’s center — not fast enough to overcome the Moon’s own sidereal motion. For the real overhead equatorial Moon at mean distance & mean sidereal speed, the equatorial observer will be traveling only 27.460.27 times the Moon’s sidereal speed, so the Moon’s average topocentric 0$^o$.56/hr speed is slowed to a relative angular speed of about 0$^o$.3/hr. (When the Moon is near the equatorial nadir, this relative speed would be seen — if it were visible — to be 0$^o$.8/hr. Over time, the speed must of course average out to the mean lunar geocentric sidereal speed: 0$^o$.549/hr.) This generally-neglected effect (which I have frequently observed firsthand — and without optical aid — during temperate-latitude high Moon-star appulses) could easily have been measured by the ancients, to yield a useful estimate ($\S 1\{c\}$) of the Moon’s distance. Yet another reason for the incredibility of the wildly false values for $r_M$ entailed by pseudo-Aristarchos. Without, that is, both the emendations here suggested (in $\theta$ & $\nu$), which lead to the reasonable values found in eq. 11.

31 A weird variant of DR’s upper-bound approach (to explaining Aristarchos’ 3$'$) appears in Evans 1998 p.72. (With no citation of Rawlins 1991P.) Though Evans speaks of “least perceptible” inequality in crescent and gibbous portions of the month (without asking how the $\gamma_A = 3'$ boundary between these portions is determined! — a difficulty which throws us right back into the mire of the very problem allegedly being solved), he says Aristarchos “simply made up the value” — faithfully converting a physical argument (“perception”) into the orthodox Neugebauerism cited above at $\S 1\{a\}$.

32 As early as Archimedes’ (p.223), Aristarchos was cited as claiming that the Sun/Moon distance ratio is between 18 & 20 (prop.7). At first glance, it might seem that this bracket reflects data-precision. Hardly. [a] The range indicated is purely mathematical (not empirical). (See Heath 1913 pp.376-381. The math is a geometric approach to a problem more accurately done by either simple circle-math [like that of $\S 5\{c\}$] or by trig, which could suggest that trig did not yet exist c.280 BC. For contrary evidence c.275 BC, see Rawlins 1985G p.261 & fn 9. The two evidences together may indicate
In addition to the flock of pseudo-A difficulties cited above (§C1 & fn 32), Rawlins 1991W §R10 also revealed a hitherto-unnoticed internal contradiction in the pseudo-A work: the explicit (and false) statement that 1/3960 of a right angle is too small to be visually discerned (Heath 1913 p.370, Neugebauer 1975 p.640). However, 1/3960 of a right angle is 4 times bigger than 1/10000 of a radian. So, this pseudo-A statement washes out the entire visual basis (fn 17) of Aristarchos’ Experiment!

The foregoing shows (in overkill proportions) that the pseudo-A treatise is not to be accepted as the output of a competent astronomer. One may assume either: [a] Aristarchos was a fool (fn 34), or [b] the work is not by him. I prefer option [b]. However, more important than the author’s identity, is the astronomy behind pseudo-A.

Having thus already (§C1a: "μετοπιν") cleared up pseudo-Aristarchos’s most obvious absurdity (eq.2: 1° lunisolar semi-diam = θ), we check another highly suspect pseudo-A statement, namely, that, at the Moon’s distance, the pseudo-Aristarchos ratio v_p of the Earth’s umbra (shadow-width) to the lunar angular-diameter is just 2. (Computing with accurate v is crucial for finding the lunar distance: eq.11.) But this v (eq.10) cause central eclipses’ Entirety (Partiality + Totality) to be 3 times longer than Totality. Letting p stand for the Entirety/Totality ratio, we have pseudo-A’s p_p = 3 (eq.10). But it is well known that an eclipse’s maximum possible Entirety is instead just under 4, while maximum possible Totality is slightly more than 1 3/4 — that is, roughly 2½ — creating an Entire/Tot ratio p of barely 2 (far short of Ent/Tot = 3). For the mean distance situation, the actual shadow/Moon ratio v is 2.7 (corresponding to Ent/Tot ratio p = 2 1/6; fn 35). And we know that Hipparchos used v = 2.5 (Almagest 4.9), while Ptolemy used v = 2.6 (Almagest 5.14).

So, how could an observing astronomer set v = 2?! The basis for estimating v of eclipse records. (And Aristarchos may have researched and drawn wisdom from such records more than any other Greek of his day: DIO 11.1 §1.) The simplest method would be to use central eclipses (Earth-shadow & Moon concentric at mid-eclipse): those for which the lunar path virtually bisects the shadow. By averaging a few empirical duration data from such central events, one may (eq.7) compute v from the ratio p by the ratio of the time of an entire umbral eclipse to time of Totality (for central eclipses), which is (crudely) 4 2/3 ≈ 2, a figure that reveals (via eq.7) v to be much nearer 3 than 2. Even aside from Aristarchos’ access to centuries of Babylonian eclipse records, he could have observed first-hand the 21-digit eclipse of 286/5/20 (p = 2 1/5); and-or the 19-digit eclipse of 279/6/30 (p = 2 1/4), which occurred just a few days after his famous S. Solstice observation. Such easy observations would make it clear that v was nowhere near 2. One possible cause of pseudo-A’s wacky v = 2 is amateurish confusion: pseudo-A carelessly took ρ (something about in-shadow, wasn’t it . . . ?) to be v. (We already know from §§A1 & C1 how easily confused pseudo-A was.) Keep in mind: the Entire/Totality ratio p is an easy raw-empirical number, while v is derivative. Another possible explanation of the pseudo-Aristarchos v-vs-p foulup arises quite naturally from an examination of the neat inter-relationship between v and ρ:

\[ v = \frac{\rho + 1}{\rho - 1} \quad \rho = \frac{v + 1}{v - 1} \quad (7) \]

Eq. 7 is a special case (where constant a = 1) of what I’ll call the “Reversible Fractional Function” (RFF):

\[ y = R(x) = \frac{x + a}{(x - 1)} \quad (8) \]

It is not immediately obvious that the deceptively simple expression R(x) brings out the fun in function — by the following cute property:

If \( y = R(x) \), then \( x = R(y) \).

Had the real Aristarchos genuinely believed \( v = 2 \), he must have realized that this correlated (again via eq.7) to \( \rho = 3 \) — which was plainly false, as anyone of the slightest experience with eclipse records would know. But we recall (§C8) that actual ρ just 35 exceeds 2, and no lunar eclipse datum is easier to find. Thus, it is not credible that Aristarchos would opt for \( p = 3 \) a value nearly five times as far from the truth as that which I will here suggest was actually his original, namely, a rounding of the crude ρ = 4 2/3 ratio noted in §C8 as too plain to miss, that is: \( \rho_p = 2 \). And this entails (via eq.7) a comparatively better value for the shadow-moon ratio \( v_A \), so we can be pretty sure Aristarchos used:

\[ \rho_A = 2 \quad v_A = 3 \quad (9) \]

Note that, if we accept pseudo-Aristarchos, eq.9’s roughly valid values became reversed into ridiculous falsity:

\[ v_p = 2 \quad \rho_p = 3 \quad (10) \]

Thus, in brief, inspired by our §C1 revelations of pseudo-A’s unreliability, I am suggesting (§C8-C10) that pseudo-A, through sloppiness or enslavement by symmetry (of the eq. 8 RFFunction), either:

[a] misunderstood a reference to ρ (commonly known to be about 2) as a reference to \( v \), or
[b] simply confused Aristarchos’ \( \rho_A = 2 \) & \( v_A = 3 \) with each other! (Easy mix-up for an amateur, since, as eqs. 7&9-10 have revealed: when either of the two variables equals 3, the other equals 2. Note also cycloidal-shadow confusion at fn 34.) Let us now explore the consequences of this simple (though speculative) hypothesis. (11)

We substitute eqs. 3 & 9 into the usual eclipse diagram equation \( r_M = \frac{1 + \sin \gamma_A}{(1 + v_A) \sin \theta_A} \approx 60° \) for \( \gamma_A = 3° \) (eq.4) or \( \gamma_A = 0° \) (eq.6), respectively. Both \( r_M \) are correct within c.5%. (Moon’s actual mean distance: 60°. 27. It should be kept in mind that \( r_M \approx 60° \) might already have been independently realized [roughly] by measuring: [a] the slowing of the Moon’s motion near transit, as described here at fn 30; or, [b] rising-vs-setting parallax, as hinted at in §C1 [c].) It is by no means improbable that \( r_M \) was known to within a few Earth-radii in 280 BC — after all, it depends critically (in eq. 11) only upon \( v \) (or \( \rho \)) and \( \theta \); and both of these are easy to find accurately enough for that purpose. (Keep in mind that Aristarchos knew the Moon’s period to a precision that certainly doesn’t sound like a mere “theoretical” math-pedant: §F9 vs. §A1, fn 20, & fn 34.) In fact, the idea that Aristarchos knew the Moon’s period to a precision that certainly doesn’t sound like a mere “theoretical” math-pedant: §F9 vs. §A1, fn 20, & fn 34.) In fact, the idea that Aristarchos knew the Moon’s period to a precision that certainly doesn’t sound like a mere “theoretical” math-pedant: §F9 vs. §A1, fn 20, & fn 34.) In fact, the idea that Aristarchos knew the Moon’s period to a precision that certainly doesn’t sound like a mere “theoretical” math-pedant: §F9 vs. §A1, fn 20, & fn 34.) In fact, the idea that Aristarchos knew the Moon’s period to a precision that certainly doesn’t sound like a mere “theoretical” math-pedant: §F9 vs. §A1, fn 20, & fn 34.)

D Solar System Scale

D1 We next find what the foregoing implies for solar distance \( r_s \). From eqs. 4 & 11:

\[ r_s = r_M \sin \gamma_A \approx 60° \sin 3° = 1146° \approx 1000° \quad (12) \]

In reality, mean \( \rho \approx 2 1/6 \), as one will find from a glance through an eclipse canon or by substituting \( v = 2.7 \) (§C8) into eq. 7.

Almajest 5.15 or Rawlins 1991W eq.27. This equation depends upon setting the solar & lunar semi-diameters equal to a common \( \theta \).
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The obvious large uncertainty in γ justifies rounding37 1146′ to 1000′.

Such a step could have triggered the later tradition — discovered at Hipparchan eqs.23&24 of Rawlins 1991W — of dividing38 the AU into units of thousandths: 1 AU = 1000′.

D2 About 900 AD, Al-Battani’s solar work, explicitly building upon the remains of Greek solar theory, exhibited precisely rs = 1146′ (and failed to supply coherent justification for the choice: fn 39). This suggests (though it hardly proves)39 that 1146′ had become a standard value in some Greek traditions.

D3 Previous attempts to deduce Aristarchos’ rs (from eq. 11) led to values such as 384′ (Heath 1913 p.339 or Neugebauer 1975 p.637 eq.20, computing exactly) and, quadruple that, 1536′ (Rawlins 1991W [§Q5]). (The first value was based on unaltered pseudo-Aristarchos; the Rawlins 1991W rs value was based upon only 1 of the 2 emendations to pseudo-A, adopted there & nowhere else. However, neither of these 2 values is directly attested. Thus, given Al-Battani’s use (§D2) of 1146′ (eq. 12), we may conclude that: [a] the value 1146′ is the preferred choice (of those discussed here) for Aristarchos’ early rs (see also fn 37), thus [b] our 2 emendations (eqs. 3&9) are not-disconfirmed.

E Aristarchos & the Seagost: Expanding the Universe a Trillion Times

E1 The irony is that Aristarchos’ famous Experiment was far inferior40 to his greatest heliocentrist scale-contribution. As remarked here at §B1, Aristarchos thought out the implications of heliocentricity to their astonishing and historic conclusion: the absence of

37 The hypothetical rounding of rs = 1146′ (to 1000′) would produce a slight inconsistency in eq. 12, but (for rs = 60′) would yet imply rs = 3′26′′ ≈ 3′. Note that 1146′ is much nearer 1000′ than any previous scholar’s estimate of Aristarchos’ value for rs: §D3. From fn 18 or eq. 13, we see that Aristarchos ultimately may have ordmag-rounded rs/1000 to 1000. In any case, Rawlins 1991W eqs.23&24 prove that he (at least initially) and-or later followers rounded 1146′ to the nearest ordmag, 1000′, or divided the AU into a thousand milli-AU: 1000′. Whether or not these ancients’ micro-measure was Earth-radii, the 1991 analysis shows that their macro-measure was heliocentrically AU-based.

38 Whatever its origin, this standardization does not imply perfectly consistent identification of 1′ with 1′, though such an equation may well have had at least passing popularity. It seems that, during the 3rd century BC, rs was initially (from Aristarchos’ Experiment) set at ordmag 1000′; and then later (due to failure to observe planetary diurnal parallax, as noted here at §F), heliocentrist astronomers (contra geocentrists; §F5) enhanced rs an ordmag, up to 1000′— the same Archimedian myriad ratio also adopted for rs/rs at eqs. 13 & 14.

39 It is always possible that the values broached above (rs = 60′ & rs = 1146′) actually came from a completely different source than here suggested. Swerdlow 1969 has made a persuasive argument that Hipparchos’ rs = 490′ was based on an adopted solar parallax of the rounded value 7′. Similarly, if an ancient had adopted a rounded solar parallax of 3′, he would (as independently noted at Van Helden 1985 p.31) deduce rs = 180′60′/3′(π) = 1146′ (a figure later used by Al-Battani: §D2 & fn 57) — and he could then, from a rearranged version of eq. 12, arrive (backwardly & shakily) at rs = 60′. On the other hand, it might be that, if Hipparchos concluded for rs ≈ 490′ (Swerdlow 1969), he did so (as he did so much else, e.g., Rawlins 2002A fn 14, 16, 17) following Aristarchos’ lead, which in this case would probably mean building upon γ rather than solar parallax. If he adopted rs = 60′ from Aristarchos (eq. 11), and believed he had measured γ to be 7′, then he would revise eq. 11 as rs = 60′/sin 7′ = 490′ ≈ 490′. If Hipparchos inadvertently stuck by an early value [Rawlins 1991W §R1] rs = 77′ [itself based on γ = 3′] and then shifted to γ = 9′, he might have incoherently computed rs = 77′/sin 9′ = 492′ ≈ 490′. For Hipparchos’ rs = 77′, see, e.g., Swerdlow 1969 p.289.) Van Helden 1985 p.167 n.8 supplies similar speculations.

40 The intimate relation of Aristarchos’ Experiment to heliocentricity is seldom mentioned in modern textbooks (perhaps due to the ironic geocentric-preference preference noted at fn 72), though obvious from the Experiment’s large implied solar volume: Rawlins 1991P §C3. That the Experiment & heliocentricity are due to the same scientist is thus implicitly regarded as merely a coincidence!
were, say, 1000 AU distant and \( \alpha^1 \) Cap much\(^{48} \) more remote, then, the 2 stars’ relative positions in April vs. October would correspond to baseline 2 AU (see fn 18) — and thus: a total eclipsal parallactic swing of about 2.3438/1000 or 7'. As noted above, the ecliptical component of the 5' gap (between the 2 stars comprising Giedi) was 3'.7 in antiquity. But our hypothesis (1000 AU stellar distance for \( \alpha^2 \) Cap) entails 3'.4 of ecliptical parallax — which thus predicts the unmissable spectacle of \( \alpha^2 \) Cap oscillating semi-annually, from eclipsal near-conjunction (October) with \( \alpha^1 \) Cap, to being (April) distant by an angle equal to c.1/2 the lunar semi-diameter! Obviously, no such effect was observed — and careful ocular monitoring of Giedi and similar star-pairs would have produced an ample reservoir of new results. For heliocentrists, said null-parallax would rule out the premise that the stars were merely 1000 AU distant\(^{39} \) — and thus supplied the empirical basis underlying ancient heliocentrists’ "scientific" (not "theoretical") conclusion for eq. 13: stars without annual parallax had to be at least another ordmag distant, namely, 10000 AU.

**E5** But we need not speculate on the existence of such observations, since it is obvious from *Almajest* 7.1 (c.160 AD) that, indeed, the ancients had carefully measured lineups and relative positions between stars. And the same source is clear that no such stellar shifts had ever been observed — which is why (until Halley) the stars’ relative positions were regarded as ‘‘fixed’’.\(^{51} \) So the logical conclusion for heliocentric visionaries\(^{52} \) would be that the stars were roughly 10000 AU distant (or more), as already expressed in eq. 13.

### F Later Heliocentric Improvements

**F1** There is a hint (Archimedes p.222, Neugebauer 1975 p.646 eq.11) that Aristarchos, ultimately promoted a provocative distance-limit symmetry (\( \mu_T = \text{Earth radius} \)):

\[
s_{\text{3}}/s_{\text{1}} = s_{\text{3}}/s_{\text{T}} = 10000
\]

This would, if true, represent an abandonment of eq. 12. Regardless of our speculations as to whether Aristarchos himself shifted from eq. 12 to eq. 14 (Archimedes suggests otherwise),\(^{53} \) we know (§F2 & eq. 14) that astronomers did so shortly thereafter.

**F2** Kleomedes 2.1 reports (Heath 1913 p.348, Neugebauer 1975 p.565). I Kidd 1988 p.445) that Poseidonios (1ST century BC) considered the possibility that the Sun was at (least fn 18) 10000' distant.\(^{54} \) This is already given in eq. 14, namely:

\[
s_{\text{3}} = 10000^\circ
\]

\(^{48} \) Apparently dimmer \( \alpha^1 \) Cap is (fn 44) roughly 6 times more distant than \( \alpha^2 \) Cap.

\(^{49} \) To attain to an appropriate perspective on vying ancients’ relative intelligence, recall from §A7: [a] Geocentrists were claiming the stars were ordmag 10 AU distant, e.g., Van Helden 1985 pp.27f. [b] The real distance of Proxima Cen, nearest extra-Solar system star, is ordmag 100,000 AU: §A7.

\(^{50} \) See, e.g., §A1 & fn 20.

\(^{51} \) *Almajest* 7.1: because the stars “maintain the formations [of their constellations] unchanged and their distances from each other the same, we are right to call them ‘‘fixed’’. I believe that most previous historians have examined this statement entirely with respect to proper motion, but have ignored the parallax question which was of at least equal interest to ancient heliocentrist observers. Geocentrists such as Hipparchos & Ptolemy, who supplied most of our links to serious ancient astronomy, do not relay discussions of star-shifts in this dangerous parallactic connection.

\(^{52} \) Neugebauer 1975 p.657: Pliny, much-grumbled at nonutility of seeking universe’s scale.

\(^{53} \) Archimedes (‘‘Sand-Reckoner’’ p.233) connects Aristarchos to eq. 12, not eq. 15. See fn 32. Note: eq.14 is based on Aristarchos’ denial of the visibility of both solar & stellar parallax, expressed for the latter case by his analogy that stars’ huge distances render Earth’s orbit punctal by comparison.

\(^{54} \) Heath 1913 p.348 supposes that the 10000' figure (for which no sensible Poseidonios evidence survives) is based on Archimedes’ ‘‘Sand-Reckoner’’ exercise. But this speculation was lodged before 1/10000 of a radian was found (§C4 or Rawlins 1991W fn 272) to underlie Aristarchos’ Experiment – with the attached suggestion that it was ancient scientists’ recognized \( \mu \) (eq. 1). The further suggestion is that Archimedes’ allegedly pure-math exercise actually prevails prevailing heliocentrist opinion, in its double use (eq. 14) of 10000 as the key scale ratio of the system. Note that Archimedes speaks of 10000 as an upper limit for both ratios of eq. 14; but Poseidonios does not do so. He instead goes on (§F2) to propose that stars’ sizes can exceed the Sun’s. (A similar statement regarding brightness would be more indicative. After all, even Ptolemy taught that stars were nearly as big as the Sun: fn 27., Heath 1913 p.266.) This slight alteration may reflect post-Archimedes refinements (e.g., larger terrestrial baseline) for the planet-star occultation observations discussed at §F7F.

\(^{55} \) See the precious puzzlement of Toomer 1984 (p.257 n.66 emph added): “There is no point in estimating the relative volumes of the bodies, but it was evidently traditional in Greek astronomy.” The incomprenhension here (by the very scholar whom Mufa satellite P.Huber calls “the expert” on the *Almajest*, PH’s emphasis) beautifully typifies the Mufa’s uncanny non-intution regarding what real ancient scientists were about.

\(^{56} \) Poseidonios taught several conflicting schemes: Neugebauer 1975 p.565. One of his values, \( \mu = 1625^\circ \), is more consistent with 1536^\circ (§D3) than with 1146^\circ (idem). An accurate ancient Earth-circumference is implicit in one of Poseidonios’ schemes: 600 stades/degree (Neugebauer 1975 p.565 n.3; or, with p.655 eq.11: 625 st/deg). Yet his math at Neugebauer 1975 p.656 eq.20 presumes 700 st/deg; and Poseidonios is known from Strabo 2.2.2 to have promoted 500 st/deg. Note another 600 st/degree suggestion in Pliny: Neugebauer 1975 p.654. If some ancients (e.g., Ptolemy) assumed a relatively (so 1 nmi = 10 stades wasn’t an accident?), had they — possibly suspecting refraction — averaged standard Cs, 25,000 stades & 18,000 stades (§1 §D3&I3) to find accurate \( C = 216000 \) stades?

\(^{57} \) See fn 39 & Swerdlow 1968 pp.92-94. I offer a novel speculative explanation of Al-Battani’s contradictions: [a] He or a predecessor computed the Moon’s distance for solar distance = (eq. 14) to provide that stars’ sizes can exceed the Sun’s. (A similar statement regarding brightness would be more indicative. After all, even Ptolemy taught that stars were nearly as big as the Sun: fn 27., Heath 1913 p.266.) This slight alteration may reflect post-Archimedes refinements (e.g., larger terrestrial baseline) for the planet-star occultation observations discussed at §F7F.

\(^{58} \) Poseidonios taught several conflicting schemes: Neugebauer 1975 p.565. One of his values, \( \mu = 1625^\circ \), is more consistent with 1536^\circ (§D3) than with 1146^\circ (idem). An accurate ancient Earth-circumference is implicit in one of Poseidonios’ schemes: 600 stades/degree (Neugebauer 1975 p.565 n.3; or, with p.655 eq.11: 625 st/deg). Yet his math at Neugebauer 1975 p.656 eq.20 presumes 700 st/deg; and Poseidonios is known from Strabo 2.2.2 to have promoted 500 st/deg. Note another 600 st/degree suggestion in Pliny: Neugebauer 1975 p.654. If some ancients (so 1 nmi = 10 stades wasn’t an accident?), had they — possibly suspecting refraction — averaged standard Cs, 25,000 stades & 18,000 stades (§1 §D3&I3) to find accurate \( C = 216000 \) stades?

\(^{59} \) See fn 39 & Swerdlow 1968 pp.92-94. I offer a novel speculative explanation of Al-Battani’s contradictions: [a] He or a predecessor computed the Moon’s distance for solar distance = (eq. 14) to provide that stars’ sizes can exceed the Sun’s. (A similar statement regarding brightness would be more indicative. After all, even Ptolemy taught that stars were nearly as big as the Sun: fn 27., Heath 1913 p.266.) This slight alteration may reflect post-Archimedes refinements (e.g., larger terrestrial baseline) for the planet-star occultation observations discussed at §F7F.
We will next show that the superiority of Poseidonios’ conception was probably based on observation, not “naive” guesswork (Neugebauer 1975 pp.655-656). For solar distance 1146° (eq. 12), the Sun’s diurnal parallax is 3°. Now, when Mars reaches a station and is roughly near perihelion, it can be less than 0.5 AU from the Earth — which means that a 3° solar parallax corresponds to about 6° of Mars parallax. At Alexandria’s latitude, 31°N, while Mars is visible during the night, an observer will be transported well over 1 Earth radius (transversely to the Earth-Mars vector) by the Earth’s axial rotation. So, for

\[ r_S = 1146° \]

Mars ought to show ordmag 10° of diurnal parallactic shift in one night — an angle easily detectable by eye (comparable to the lunar semi-diameter). Meanwhile (as could have been noted by a transit observer like Timocharis), Mars’ apparent geocentric longitude will vary by merely about half an arcmin over the 48° period around the station (1° before/after). Such stations must have frequently occurred near enough to stars that the invisibility of the predicted parallactic shift was repeatedly verified.

There is another planet-star method which requires (not the neat timing of hitting on a station but) a wide geographical range of observations. When Venus is near inferior conjunction, it can be less than 0.3 AU from the Earth. (About 1/3 of an AU at stations.) I.e., Venus’ diurnal parallax can be more than triple the Sun’s. But for 3° solar parallax (AN), Venus’ greatest diurnal parallax should be as high as about 10°. If Venus passed near a star, then one need only compare observations taken, say, at Meroë (latitude \( L = 17° \)), vs. ones taken, say, at Byzantium (\( L = 41° \)). The north-south angular distance between planet & star at conjunction should differ by about 5° — simply detected by the naked eye.

I propose that our fragmentary record (AN) of ancient planet-star occultations is part of Aristarchos’ systematic empirical testing — which eventually converted heliocentrics, c.270 BC (sometime between Aristarchos’ Experiment & the more famous “Before & after”). Such stations (eq. 12) to \( r_S = 10000° \) (eq. 15). (Such observations, in proving solar remoteness, also proved solar hugeness and thus supported heliocentricity: §F2 & Rawlins 1991P (C3.)

Summing up the evidential situation: we have examined all 3 of the surviving astronomical scales connectable to ancient heliocentrics (eqs. 4, 15, & 13); and we have found that each of the 3 is founded on exactly the same empirical base: eq. 1, namely, the correct assumption that the limit of human vision is about \( \mu = 1/10000 \) of a radian. This precious coincidence lends more crediblity to the empirical-base theory proposed here, than most current astronomy-historian archons will ever admit. However, these archons’ own standard myth of the Greeks as mere navel-contemplating theorists has here been revealed as just that: a myth — based upon (implicitly) treating surviving documentation of ancient work as a representative sample. And the slightest common-sense consideration of the long process of filtration of ancient materials (before they reached us) will warn a freshman historian against such naivete. (Which is spoofed at Dio 1.2 §11 §3. See also Dio 9.1 §3 fn 8.) Since I expect the old view to persist regardless, I merely urge loyalists to offer a coherent hypothesis explaining how allegedly indoor Greek “theorists” came into possession of the sidereal year and the periods of the Moon (sodic, anomalistic, draconitic), Mars & probably Venus) which are accurate to 1 part in ordmag a million or better. (See Rawlins 1984A p.984, Rawlins 1985K, Rawlins 1985G §5, Rawlins 1991H fn 1, Dio 11.1-2, Dio 13.1, www.dioi.org/hrh.htm.) DR evidently was the 1st to publish these startling facts, since the Mufa had willfully overlooked this remarkable achievement. After all, the Mufa has decreed in Science that accuracy is irrelevant to ancient astronomy.

Since the JHA 1980/6 editorial policy statement cited elsewhere here (fn 64) calls it “a mortal sin to judge the present solely in the light of the present”, I offer the observation that, by this unexceptionable JHA criterion, it would be mortally-sinful if a modern academic cult projected onto ancient scholars its own creative sterility, technical ignorance, and conscienceless amorality. This patently fantastic example is of course purely a DR fabrication, innocently concocted, like Ptolemy’s fakes, entirely “for pedagogic purposes” — to borrow the brilliant phraseology of Gingerich 1976.

G The Force of Reason and the Force of Prise

G1 We recall O’Gingerich’s suggestion (AN) that Aristarchos’ contributions were minor and off-the-top-of-the-head. Thus, Aristarchos’ demotion may be rationalized in the same fashion as the Mufa’s downgrading of the works of creative moderns of whom it disapproves. Gingerich 1985A (p.41): “For better or worse, scientific credit goes generally not so much for the originality of the concept as for the persuasiveness of diurnal parallactic shift in one night — to borrow the brilliant phraseology of Gingerich 1976.

The most obvious problems with these typically anti-revolutionary OG comments (on 2 brave revolutionaries):

[a] To suggest that we slight Aristarchos, merely because attacks on his heresy and on his intellectual freedom succeeded in virtually burying his work — despite his high ancient reputation (Rawlins 1991W §Q1) & achievements — is effectively to endorse dictatorial bullying & idea-imprisonment. I cannot begin to imagine why the Mufa would sympathize with and effectively endorse such behavior.

[b] Must we follow Neugebauer&OG in letting the brilliance, boldness, & vindication of Aristarchos be lost in the celeb-spotlight both men shine instead on astrologer-quack

62The values for the sidereal year and the synodic month — generally known as the “System B Babylonian month” — are good to about 2 parts in ten million, and DR has traced both to Aristarchos (Rawlins 1991H fn 1, Rawlins 1999, Rawlins 2002A). The earliest cuneiform record of the “Babylonian” month is decades after Aristarchos.

63See also Gingerich 1976 & even valuable Grasshoff 1990’s pp.215-216, excusing Ptolemy’s fudgings to agree with predecessors’ theories. Should a field’s leaders become automatic prominent apologists for the most notorious intellectual thief in the history of astronomy?

64See also excessing of discovery-misattribution in OG’s JHA 11.2:145; 1980/6 (statement by Lord H & OG). One senses just how upset the JHA Editorial gets at plagiarism.


66Besides the present findings, see e.g., Rawlins 1991P fn 1 and Rawlins 1991W §N7 & eqs.22-24.

67If we were asked to point to the single feature that most clearly separates scientists from centrist historians in this area of scholarship, it would be this: history of astronomy has become (fn 6, 20, & 64) so knee-jerk anti-judgemental regarding its subjects (though not its turf-competitors) that it has lost the fact that vindication-by-future-experimentation is not anachron-twisted mis-history but rather is: [i] what scientists dream of, & [ii] the standard test of scientific theories’ truth or falsity. To trace how hist.astron scholars have even so divorced from these realities (of the very field they purport to chronicle) is a job I recommend for an enterprising young archaeologist of strong stomach & disfunctional nose. (Is it coincidental that Hist.sci was the womb from which the “paradigm” alibi for inferior science was born? Whether symptom or cause: an unfortunate backward step for modern Hist.sci may have been its archon T.Kuhn’s launching of the buzzword “paradigm”. When I was involved in anti-occultist efforts years back, I found that, while virtually no productive scientists have any use for the word “paradigm”, it was a fave with explo cultists who longed to obscure and alibi the failures & fakeries of astrologers & other pseudoscientists.)

58Here, Venus has a higher diurnal parallax than Mars, but the method fails for Venus since it rises/sets so soon ere/after Sun’s rise/set when stationary. By contrast, stationary Mars stays up most of the night.

59 Aalmajest 9.1 taught that planetary diurnal parallax was invisible. (See Rawlins 1991P §F3.) But Swerdlow 1968 correctly notes (p.102) that planetary diurnal parallax “is too large to be ignored” (ordmag 1° for Mercury, in Ptolemy’s system) — even though Ptolemy continued to insist (p.103) that such parallax cannot be measured! Ptolemy later admitted (PlanHyp 1.2.5, B.Goldstein 1967 p.9) that Mercury, Venus, & Mars must show some diurnal parallax, according to his solar distance; but he does not claim he ever observed such — or even tried to.

60 Harter 1980 p.12 points out that, by Ptolemy’s scheme, even larger diurnal parallaxes should be exhibited by Venus & especially Mercury. See fn 59.

61 Ptolemy eventually acknowledged that nontrivial diurnal planetary parallax was implied by his system. See fn 59, and the useful discussion & distinction at Taub 1993 p.167.
H Heroes & Zeros

H1 Since most great work is the tip of a pyramidal anonymiseberg, it is risky (and usually unjust) to single out one figure as The Greatest, in any field. However — despite Cleanthes’ worst efforts at grounding him — Aristarchos’ winged mentality soared beyond his terrestrial confines of physical gravity and academic bigotry. And he still glimmers, through the haze of our indistinct record, as the ancient astronomer who perceived, proved, & published the realization that the universe’s volume is ordmag a trillion (10^{12}) times larger than hitherto understood, which reveals to him to have done even more for our spatial perspective than what 19th century geology & biology did for our temporal vision. His achievement, among the most extraordinary in the history of human thought, merits more than its fate to now: a mere (largely-uncomprehending) footnote in science history.

H2 The brains (and their retinae & retinas), which accomplished this feat, are now dust in the ground — still far from the sky they explored and first comprehended. That dust is even more irrecoverable than the exact details of their original manuscripts, also long gone to dust. But their great discoveries shine on.

H3 For now, this light is darkened and distorted by the turbid, twisted medium of certain modern cultists. (Who do not even appreciate the link between Aristarchos’ work and his vast vision: DIO 4.2.29 [K13].) Sadly, “for the indefinite future” (DIO 1.2.3B3), intelligent scholars must press on [& calibrate for] the warp created by our grant-begging era, when [a] survival priorities swamp concern for truth, and [b] power-first business scholars’ intellectual depth establishes the limit of (public) scholarly debate & consensus.

H4 The modern ironic reality: Aristarchos’ greatness is still being submerged — more than 2000 years after his views’ persecution! — largely because (fn 16) grant-raising via Ptolemy’s fatter extant corpus is more profitable. To put it crudely: there are, numerically, more Ptolemy texts to write theses about. (The advantage this gives to the pretense that geocentrist were genii is, of course, DUE TO two millennia of systematic suppression & banning of heliocentrism by Cleanthes, Ptolemy, the Roman church, etc.) This primitive factor is especially critical when too many of the scholars dominating a field are comparably primitive technically, and so are all too often incapable of going beyond what ancient texts explain in terms simple enough for literal mentalities to follow. So, I conclude by suggesting that, in future, our evaluations of scientific heroes be guided not by pre-packaging & ([§2 item [b]]) hype-suprelatives imposed from the (political) heights, by the Cleantheatic ideal-killers of our own era — but instead by simple considerations of evidence, logic, & decency, mingled with grateful appreciation for the longago adventurous minds who bequeathed us a heritage of high genius and courage, which stands for the best in humanity.

Epilog

Because of some (hopefully ever-more-anachronistically) strong critiques in the foregoing, one should understand that it (and other already-published papers on the same subject) evolved over more than 15 years (p.47) into a “roundly opposed” treatise that had to conflict with the research. But that scholastic influence has waned, while among its prime present legacies are G.Toomer’s scrupulous Alm edition, and Toomer’s protégé, the brilliant and creative classicist, Alex Jones, of New York University’s hugely endowed new Institute for the Study of the Ancient World.

Sadly, the Mufa’s former mal-influence has been somewhat replaced by the Gingerich-pawn Historical Astronomy Division (of the unsupervising AAS), whose members’ dissent-resembles those described in the newest exposé of sororities. (See Alexandra Robbins Pledged NYC 2004 for their dominatrices & shunnings, e.g., p.128.) Even at its worst, the Mufa at least displayed scholarly dedication. By contrast, much of the ancient astronomy scholarship promulgated by the HAD (using the credulous “science press” whenever possible) is just embarrassingly amateure. (See, e.g., www.dioi.org/ccc.htm.)

Meantime, however, Heath 1913 at least encourages, thanks to Robert Halleux, Dennis Dierra, Margaret Ressiter, and Hugh Thurston (among others), the history of science community (which was never comfortable with the Mufa’s arrogance) and DIO have come to appreciate each other, a process which culminated with the contributions to Isis (History of Science Society) by Thurston and DR in 2002-2003. We here thank all those who helped effect this productive amicability, which most of us thought might never come to pass in our lifetimes.

Boxing: Jake and professionally-punchy Rocky leave the gym together, and Jake points up into the sky and asks: “Hey, Rocky, what’s that bright thing — the Sun or the Moon?” Rocky: “Aaaah . . . Aaaah . . . Awww, Jake, how would I know? I don’t live in this neighborhood.”
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A2 Aubrey Diller was (1983/3/6 letter to DR) the 1st scholar to point out the 360-site total and to suggest its deliberateness. 3

A3 The longest-day M (in hours) at a site is a sph trig function of latitude L (in degrees) and the Earth’s obliquity $\epsilon$ (also in degrees), by an equation known at least since the 2nd century BC (Hipparchos [DIO 5 & DIO 16 §3]) — a remarkable historical revelation, primarily owed to the mathematical investigation of Aubrey Diller 1934. [Readers not into sph trig may now skip from here to §B] The equation for computing each klima (§A1) attested for the 2nd century AD at Almajest 2.3:

$$\cos(15M/2) = -\tan L \tan \epsilon$$

(1)

(whence obliquity $\epsilon$ was usually taken to be 23°5/6 or [the discovery of Diller 1934] 23°2/3).

A4 Why different data-format for GD 2-7 vs GD 8? Two potential answers:

[1] Books 2-7, like the Important Cities part of Ptolemy’s HanTabl, are in the form of Marinos’ manual or map, presumably after his (though see §C1) systematic tectonic massalation (GD §8A—GD §8E) to force macro-geographical accord (through eq.1) 400 to 600 rows of the above-hypothesized (§A1) network-grid-basis, which had been severely pro-corrected by roundings by (§D1&D5) in tables long used by astrologers. Remarks at, e.g., GD 1.18 suggest that, like (following?) astrologer Hipparchos, Marinos clumped (§D4) cities under parallels. Also, Marinos gave primacy (GD 1.20.3 & 24.3; and below at §6) to Hipparchos’ 36° parallel (arc $\theta$-k-,$\lambda$ in Fig.1 [p.50]) through the east-Mediterranean island of Rhodos, suggesting both an astrological-tradition connexion and even the possibility that Marinos’ table of rounded-longest-day parallels (for at least the Mediterranean-region) was a hand-me-down from Hipparchos, whose main observatory was located on Rhodos (D149), probably just north of the town of Lindos. (See Rawlin 1949L §F [pp.42-45]).

[2] The data of Book 8 are not for a map — but are in precisely (§D2) the hour-based form for astrologers’ convenient use in computing a horoscope for a site other than Alexandria (D149), which was obviously the standard meridian for astronomical & astrological ephemeres in the Hellenistic world. 4 So GD 8 could have been called the Handiest Tables — perfectly set up for astrologers’ convenience. [Some versions of the Handy Tables operate likewise: Neugebauer 1975 p.938 n.9.] Listing cities by longest-day superficially appears odd & cumbersome, and it gave no special aid when using data for maps. (To the contrary: §D3 [b].) However, astrological tables of the outdoor-invisible “Ascendant”

3See Aubrey Diller 1984’s scrupulously-wrought establishment of the text of the entire contents of Book 8 at www.dioi.org/gad.htm. The total of his-site-lists is 359. Nobbe’s total is 358. But Nobbe omits Tarentum and Sousaleos, while Diller semi-omits Limyra. (Though, see end of this fn.) Merging the lists, we have exactly 360 sites in 26 sections, corresponding to GD 2-7’s 26 maps. Sections: 10 of Europe (118 GD 8 sites), 4 of Africa (52 GD 8 sites), 12 of Asia (190 GD 8 sites).

I propose scholars’ agreement upon a conventional numbering of all 360, based upon the sequence of Diller’s XZ Codices, dovetailing with the UNK Codices (to cover sites either skipped), which follows Diller’s desire to give primacy to the former. We use prefix D, to number every GD 8 site, so that “D $x$” refers to the xth site. Adding to Nobbe’s edition of GD 8: Tarentum (GD 3.1.12.8.8.8) as site D53, Sousaleos (GD 3.3.4.8.9) as site D63. (Note that we are dovetailing these two sites into Nobbe in passages that [exceptionally] already list more than one site — which may help explain these two oversights.) To Diller’s version, we add Limyra (GD 5.3.6.8.17.25) as site D193, Diller XZ Codices Asia-Map 1 site #22—#22a, “Myra”, whose coordinates are identical to Nobbe’s “Limyra” at GD 5.8.17.25. D193 is UNK’s item#22, whose coordinates are identical to Nobbe’s GD 5.8.17.23, “Myra” (GD 5.3.6). Note that one finds “22a” in Diller’s hand in the left margin of his p.X13, showing that he suspected the need to add this site as the final touch to perfecting his epochal document. I.e., even at age eighty-plus, his sharp eye was still missing nothing! 5

4The very choice of longest-day (instead of latitude) as GD 8’s measure of northerliness tips us off to the astrological connexion. (Hardly a stretch: recall that Ptolemy compiled the superstitious horoscope-delineation book that is still astrologers’ bible: the Tetrabithos. Note that the geographical table in his astrologer-oriented Handy Tables was at this stage still inconveniently in degrees.)

5It will help to provide an example, using the Almajest 2.8 table for Rhodos (D189) at Sidereal Time (the Right Ascension of the meridian, or Hour Angle of the Vernal Equinox) $21^h23^m36^s = 320^\circ54'54"$ (which is chosen to avoid interpolation in step 1, as will be evident):

Adding 60° or 90° gives 50°54' (the rising point on the Equator). Then, find 50°54' in the Almajest 2.8’s “Accumulated Time-Degrees” column for Rhodos (longest-day M = 14°1/2, the basis of this column’s ancient computation and arrangement): Almajest 2.8 (Toomer 1984 p.101). The value on the same row in the column “10° Intervals” is zodiacally 10° of Gemini or 10° of Sagittarius, i.e. 10° e.w. (scliptically 70°, so that is the Ascendant. The Descendant (ecliptical point that is setting) is opposite: 250° or 10° SGR 00' 10° of Sagittarius). The Midheaven (polar longitude of transiting zodiac point) is then found by linear interpolation on Toomer 1984 p.100: in the “Accumulated Time-Degrees” column, under the “Sphaera Recta” heading, we find 312°32’; 320°54’ (ST) exceeds this by 8°22’ of the 9°58’ interval corresponding to the 10° interval between 10° AQR 00’ and 10° AQR 00’ (in the column “10° Intervals”), so: add 10° (8°22’ 9°58’ = 8°24’ to 10° AQR 00’, which yields Midheaven = 18° AQR 24’ (18° 4’ of Aquarius) on the zodiac or ecliptical longitude 318°24’. The Nadir is opposite: 138°24’ or 18° LEO 24’. This establishes the 4 cardinal points of the astrological houses for the chosen place & time. Division of each quarter into 3 parts then establishes the 12 astrological houses, but said division differed between house systems. Tables of houses, presumably though not demonstrably sph-trig-based, go back at least as far as Theodosios of Bithynia’s “Houses”, 2nd century BC. Finding Ascendant & Descendant (and thus house-divisions) is the sole use most modern astrologers have for geographical latitude. (Ancients also used latitude to enter parallax tables, but such scrupulousness is rare among today’s astrologers.) Finding longitude was usually merely for additively converting (§D2 [3]) local time to ephemerides’ standard zero-meridian, presumably that of Alexandria.

6All three latitudes are correct — perhaps a notable Egyptian achievement, since the GD lists Heliopolis (the Greek name for On) at the wrong latitude (exhibiting a peculiarly Greek error: −1°4’ from asymmetric gnomon), not realizing (similarly at §K5) that it is the same place as the holy city called “On” by the Egyptians and Genesis 41:45. 5f10}Suggestively, the correct latitude is associated with the ancient Egyptian name, not the later Greek one. Details at Rawlin 1985G p.260.
Acre (Ptolemais), Tyre (§C2) & Sidon have errors of only a few miles, not quite as right-On as the Egyptian trio, but nonetheless impressive for antiquity — and highly unusual for the GD, suggesting that Marinos in Phoenicia (like Hipparchos at Rhodos) got particularly accurate latitudes from his own observations or from those of local astronomers or navigators, even while (fn 10) absorbing and relaying ordmág 1° errors for regions outside his or his associates’ direct experience. Of these 6 sites, only Memphis (D151) is listed in GD 8.

B2 The implication: those major cities not listed in GD 8 and civilized enough to afford astronomers (note §D6) show a better chance of having accurate GD 2-7 latitudes (§§J2&K11) than those which don’t.

C The Unresolved Mystery of Marinos the Phoenician

C1 Why hasn’t it been previously noted that GD Book 1’s extensive critical discussion of Mediterranean-region scholar Marinos’ data fails to provide unambiguously a single Marinos latitude in degrees for any Mediterranean city? — or, indeed, any city within the Roman Empire.5 So, though Marinos’ latitude for the extra-Empire city Okelis (§H2) (D19) shows a better chance of having accurate GD 2-7 latitudes (§J2&K11) than those which don’t.

C2 After all, how is it that an (apparently) eminent geographer from Phoenicia (a legendary naval center, where latitudes & stellar declinations would have been vital for navigating commercial vessels if nothing else) was ultimately — via his own or others’ sph trig — depending, for his latitudes, upon crudely-rounded (§D6) astrological tables? (Of longest-day data: see below at §D1.) If he was. Note (§B1) that the Marinos-of-Tyre-based GD 5.15.5k27 latitude of Tyre is just about exactly correct (to its 1°/12 precision) if founded upon observations of circumpolar stars (affected by c.2° of atmospheric refraction), a wise and theoretically parallax-free latitude-determination method which may (§B1) go back to the time of the Great Pyramid.10 Was the purpose of Marinos’ geographic naval? Or natal?11

D Astrologers’ Handiest Tables, InterRelations, Accuracy Degraded

D1 GD 8.2.1 states that the data of Book 8 were computed (via eq.1) from latitudes & longitudes. However, a detailed mathematical case has been made by Rawlins 1985G pp.260f that — though the remote-past origin of longest-day M data were obviously computed from latitudes — the highly (§§D5, K10, & L5) corrupted latitudes of major cities listed in GD Books 2-7 must have been computed (via eq.1) from conventionally over-rounded longest-day M-data (§A4) of just the sort12 we see in Book 8. Flagrant examples are apparent, e.g. Babylon (fn 16) & SE Asia (§K7). The suggestion here is that distortions in GD latitudes go back at least to Hipparchos, while the distortions in longitude probably occurred later than Hipparchos, since they involve a shift (fn 25; §1 §J4) from the Hipparchos 25200-stade Earth-circumference (fn 47) to the 180000-stade Earth-circumference which fellow-Rhodian Poseidonios seems to have switched to (Strabo 2.2.2) during the 1st century BC. (Though Taibak 1974 eruditely wonders if this switch wasn’t much later.) The Almajest was still using the larger Earth-size during the mid-2nd AD, and the earliest rock-certain attestation of the smaller value’s use is by Marinos, around the same time.13 (Columbus’ belief, that the shortest trip to China’s Kattigara [DS36] was westward not eastward, was much influenced by Marinos’ over-big Earth.)

5 See Rawlins 1985G pp.255-256, as well as Rawlins & Pickering 2001; see also DIO 13.1 [2003] (www.dioi.org/vols/wd1.pdf) [pp.2-11]. Similarly, Hipparchos knew his own latitude, but seems (§B1) to have reported weak elsewhere, e.g., placing Athens a degree south of its actual latitude (Hipparchos Comm 1.11.3&8) and Babylon 2°1/2 north (§L6) of its — both values copied (fn 10) by the GD.

10 If Marinos’ data were as corrupt as the GD’s, silence (fn 8) on Marinos’ latitudes within the Roman Empire argues for expansion by 4/3 (fn 45). Note that the latter theory (§D1) has here been limited to proposing the high likelihood that the Marinos-Ptolemy’s too low by 1/16, is shown by at least 5 considerations: [1] Ptolemy’s 4-5 expansion (130°→127°1/2) of the Rome-Babylon longitude-difference, between Alm & GD. (2) The GD’s similarly large (33%-40%) systematic over-estimate of many actual longitudes. See the least-squares test of Rawlins 1985G p.264, leading to p.265’s table of reconstructions. (The first scholar to sense that ancients had multiplied longitudes by adjustment-constants when adopting new Earth sizes, seems to have been Pascal Gosselin’s G´eographie des Grecs 1790. See his several tables exploring the hypothesis; also Rawlins 1985G n.22, which credits Gosselin & van der Waerden for this penetrating realization.) [3] DR’s neat common explanation (using the same 185 meter stade) of BOTH C-values’ errors from atm ref of horizontal light (§§J1 & A4 & K6) with 1/6 the curvature of the Earth. [4] Strabo 1.4.1 reports that the largeness of Eratosthenes’ Earth-circumference was genuinely (not illusory) high by 1/5, and Marinos’-Ptolemy’s too low by 1/16, is shown by at least 5 considerations: [1] Ptolemy’s 4-5 expansion (130°→127°1/2) of the Rome-Babylon longitude-difference, between Alm & GD. (2) The GD’s similarly large (33%-40%) systematic over-estimate of many actual longitudes. See the least-squares test of Rawlins 1985G p.264, leading to p.265’s table of reconstructions. (The first scholar to sense that ancients had multiplied longitudes by adjustment-constants when adopting new Earth sizes, seems to have been Pascal Gosselin’s G´eographie des Grecs 1790. See his several tables exploring the hypothesis; also Rawlins 1985G n.22, which credits Gosselin & van der Waerden for this penetrating realization.) [3] DR’s neat common explanation (using the same 185 meter stade) of BOTH C-values’ errors from atm ref of horizontal light (§§J1 & A4 & K6) with 1/6 the curvature of the Earth. [4] Strabo 1.4.1 reports that the largeness of Eratosthenes’ Earth-circumference was genuinely (not illusory) high by 1/5, and Marinos’-Ptolemy’s too low by 1/16, is shown by at least 5 considerations: [1] Ptolemy’s 4-5 expansion (130°→127°1/2) of the Rome-Babylon longitude-difference, between Alm & GD. (2) The GD’s similarly large (33%-40%) systematic over-estimate of many actual longitudes. See the least-squares test of Rawlins 1985G p.264, leading to p.265’s table of reconstructions. (The first scholar to sense that ancients had multiplied longitudes by adjustment-constants when adopting new Earth sizes, seems to have been Pascal Gosselin’s G´eographie des Grecs 1790. See his several tables exploring the hypothesis; also Rawlins 1985G n.22, which credits Gosselin & van der Waerden for this penetrating realization.) [3] DR’s neat common explanation (using the same 185 meter stade) of BOTH C-values’ errors from atm ref of horizontal light (§§J1 & A4 & K6) with 1/6 the curvature of the Earth. [4] Strabo 1.4.1 reports that the largeness of Eratosthenes’ Earth-circumference was genuinely (not illusory) high by 1/5, and Marinos’-Ptolemy’s too low by 1/16, is shown by at least 5 considerations: [1] Ptolemy’s 4-5 expansion (130°→127°1/2) of the Rome-Babylon longitude-difference, between Alm & GD.
grid-network of Important Cities’ latitudes (§D5) — a grid which typically misplaced geographically-key cities by ordmag a degree, grossly mislocating their latitudes, e.g., Byzantion (D87 [Istanbul]) by 2° (though, as B&D p.29 n.37 truly marvel, the false GD latitude continued to be believed at religiously non-empirical Byzantion until c.1000 AD!); Carthage (D131) by 4°, a huge error (revealed at Rawlins 1985G p.263 as due to false L) that enormously distorted maps of N.Africa (up to the Renaissance, over 1000 yrs later). Not to mention Babylon (D256) by 2° 1/2 (fn 10; Rawlins 1985G n.13) — a discrepancy which is difficult16 to reconcile with a modern historian-cult’s non-empirical insistence (fn 46) that Greece had high-astronomy debts to Babylon. DR suspects (§A4) that the latitudinal shortcomings of the prime grid-network derive primarily from astrologer Hipparchos (not Marinos or Ptolemy): see at GD 1.4.2 & (8.1.1) on Hipparchos’ listing-clumping of cities of differing latitudes under the same clima (§A3), for astrolagers’ convenient entry (§A4) into common longest-day-based tables of houses. This degenerative step typified the fateful laxity (www.dioi.org/cot.htm#twvr) which DR’s §D1 theory proposes was the prime source17 of latitude-accuracy’s corruption in GD 2-7.

D5 Rounding climates to fractions of hours (GD 8’s practice) correlates to FAR cruder precision than rounding latitudes to twelfths of degrees, which is the precision of Books 2-7’s data. Ancient longest-day tables often rounded M to the nearest 1/5. (See, e.g., Alm 2.6, Neugebauer 1975 pp.728f.) But when using eq.1 in the Mediterranean region, a longest-day error of merely 5 time/min would cause an error of nearly a full degree. And ordmag 1° is the actual (terribly crude) accuracy of the data of Books 2-7. (Example of degeneracy [SE Asia] traced in detail at [K10].) This is (along with the plethora of places whose latitudes fall conspicuously upon exact latitudes) one of the best arguments for the Rawlins 1985G theory that rounding the longest-day data (GD 8’s or its derived type) was still basis (§G3) for calculating city latitudes of GD 2-7. Note the historically vital (if paradigmist-verboten)18 lesson imparted: competent ancient geography’s heritage to us was corrupted — crippled (§G2) might be a more accurate indictment — by the societal ubiquity of a pseudo-science, astrology (§D4). But keep in mind (DIO 4.3 §15 [C3]) that Ptolemy worked for the newly-cosmopolitan, astrology-saturated Serapic religion, and doing horoscopes internationally requires (then & now) 3 manuals: astronomical tables, geographical tables, & interpretational handbook. Ptolemy’s prime works were: Almajest, GD, & Tetrabiblos.

16 It is common knowledge (§L6) that the longest-day value (GD 8.20.27) for Babylon (D256), 14°5/12, is a rounding of 14°2/5 — which is 3/5 of a day and the M basis of computation (§G2) [cf.] of the revealingly inaccurate latitude L = 35°N (GD 5.20.6), 2° 28′ (148 nautical miles) too far north.

17 There remains the question of whether Hipparchos was responsible for the fateful step of converting (via eq.1) crude tabular longest-day M values from hours to degrees of latitude L. In the light of DR’s 2007 realisation (www.dioi.org/cot.htm#twvr) of just how admirably accurate Hipparchos’ longitudes may’ve been, the odds that he was not the culprit are enhanced. Has the remarkable irony been noted that the Geographical Directory (at GD 8.1.1) itself scoffs at the common folly of clumping cities under parallels? Or that this contradicts GD 1.4.2, where Hipparchos is praised for his alleged aloeness in performing the very same clumping? Of course, the GD 8.1.1 complaint is merely that parallel-lists [like the pre-Ptolemy one of Pliny (77 AD); analysed at Rawlins 1985G p.262] waste time and space, but the statement is valuable in its suggestion of ancient currency of the very lists upon which the DR theory is founded. (Said currency could help a defense of Hipparchos as not-necessarily the unique source of the GD’s macro-errors; however, his attractive fame and his citation by both Marinos [GD 1.7.4] and Ptolemy [Almajest, passion] argue in favor of his culpability here, though see speculation above [in this fn], on his longitudes.) We needn’t speculate anyway, on the existence of lists of a few hundred key cities’ coordinates. Just such a list survives, e.g., in the Ptolemy Handy Tables, the Important Cities table of which (N.Halma 1:109f [1822]), appears closely related (§K4) to GD 8 in both quantity and sequence: 364 sites in all, with 12 not in GD 8, and 8 missing in HT. See also the Important Cities lists (fn 43) provided in E.Honigmann 1929 [pp.193f]. Vatican 1291 [493 sites] and Leidenis LXXXVIII [a comparable number of sites]. These lists’ positions are [like GD 2-7] given entirely in degrees east of Blast Isles and north of the Equator.

18 See [2 fn 67 & DIO 2.1 §13 [C10] [p.31].
D6 Suggested Solution to Two Mysteries As shown in the tables of Rawlin's 1985G p.262, GD latitude-errors for major cities are often sphi-trigonometrically consistent with the§1D1 theory. See eq.1 or Rawlin's 1985G p.261, for the relevant math. See also discussion (ibid,p.259) of a further revealing point: without the DR theory presented there & here (§C2), how could one reasonably explain two shocking oddities (which had evidently escaped the notice of previous commentators): [1] GD latitudes (as already noted) are two ordmags cruder than ancient astronomers’ latitude-accuracy. (Roughly: a degree vs an arcmin.) [2] The GD latitude errors’ large size (again: ordmag a degree) is comparable to that of its pre-expansion (fin 13&25) sources’ longitude errors — this, though: [a] The former should be 30 times smaller than the latter. (Or 41 times smaller, if eclipse-observations aren’t taken as raw-data pairs.) [b] Again, real astronomers knew their latitude to ordmag an arcmin.29

E GD 8’s Disconnect: GD a Hybrid

E1 The order of data-listing for GD 2-7 and GD 8 are similar. (And the former’s 26 locmaps correlate in designation and sequence with the latter’s.) This suggests (§D4) some sort of inter-causation or co-causation. (GD 8.2.1’s statement that GD 8’s data are from degree-lists does not say that they were those of GD 2-7, though that may be the implication-and-or the truth.)

E2 However, throughout the GD, we find repeated instances of differences in order-of-listing.30 Which argues against GD 8 being computed directly from31 GD 2-7 or vice-versa.

E3 Decades ago, Aubrey Diller pointed out to DR that the GD never mentions Book 8 — until the reader arrives there.

E4 DR has noted something similar: throughout GD 1, there is no mention of Alexandrian Ptolemy’s claimed home and his Alm’s prime meridian. By contrast, GD 1 mentions such sites as: Thule (D11), Ravenna (D56), Libyaeum (D67), Carthage (D131), Rhodes (D189), Canopus (Ptolemy’s true home), Syene (D154), Meroë (D165), Arbela (D261), Okelis (D281), Kattigara (D356), among many others. Since Ptolemy is a multiply-conflicted plagiarist (Pickering 2002A; Duke 2002C), one may ask: is it credible that

29 Rawlin’s 1982G p.263 in 17. Note that GD 1.2 shows awareness that astronomical observation is the most reliable basis of latitude-measure. This returns us to the question: if sophisticated cities knew their latitude (§B2), how did most of these data get corrupted by astrologers? Was there a long astronomical tradition (§C1) of geographical tables, which Marinos (note GD 1.17.2’s semi-connection of astrologers’ klimata to Marinos) and-or Ptolemy felt forced to assent to the flawed-important-cities latitudes of? Just as usually-equant-prefering Ptolemy may’ve forced to go along (in the Almajest) with Hipparchos’ flawed but long-pagan-sacred eccentric-model solar tables.


31 See §G1. For the consistent there, were calculations of one section’s data from the other (in one or both directions) or scrupulous attention was paid (in 25) to math-consistency between the two sections (whether at the outset or during later editors’ touchings-up) — though there are occasional incompletenesses, e.g., the longitude of Rome (D49): GD 3.1.6.1 puts Rome 36°23′ west of the Fortunate Isles, while GD 8.8.3 puts Rome 15°8′ east of Alexandria. (Itself 60°12′ east of Blest Isles by GD 4.5.9, or 4° [60°] by GD 8.15.10. See Rawlin’s 1985G n.25.) But (60°12′ — 36°23′) (15°8′) ≈ 17′12′′ < 1′5′′. Similar incomparability: Salinae (GD 3.8.7.11, 8.11.4, D79). See also §K3.

32 Noble 1.46 inserts Alexandria at the 14° klima (GD 1.23.9), but it is clear from Müller 1883&1901 (1883) 1.57, B&J p.85&111, and S&G 1:116 n.4 that this was not in the original, which (in GD 1) claimed only four klimata north of the Equator: Meroë [D165] (13°), Syene [D154] (13°1′), Rhodes [D189] (14°1′2′), Thule [D1] (20°). Selection repeated GD 7.5: B&J p.111. Note that Alexandria [D149] is mentioned at GD 7.5.13-14. allegedly (Almajest 3.1) Alexandrian Ptolemy would write a preface to his Geography which never mentions his own city, when it is the prime meridian for his astronomical works, for his earlier-announced (ibid 2.13) forthcoming geography, and for GD 8?

F Blest Isles & Identified: the Cape Verde Islands

F1 Conversely, the Blest Isles, the GD ekumene’s west bound (and GD 2-7’s implicit prime meridian), have no GD 8 entry. In GD 8, this linch-pin site is only mentioned at two places, rather in-passing: at GD 8’s prime meridian Alexandria (GD 8.15.10) and at the GD ekumene’s east bound, Thinai (GD 8.27.13), where it is noted that Thinai is 8° east of Alexandria and thus 12° east of the Blest Isles.

F2 Yet another oddity: the GD repeatedly states that the Blest Isles are the west bound of the ekumene. (Though, curiously, not at GD 7.5.2. even while soon after saying so at GD 7.5.14.) Yet the writer of GD 1 does not explicitly state that all the longitudes of 2-7 will be measured from the Blest Isles; and the Blest Isles have no entry in GD 8. Its position appears23 under Africa at GD 4.6.34. Additionally, one notes that there is not a single absolute longitude in GD 1 — every longitudinal value is given in strictly differential terms. Now, if one is writing a preface to a compendium that provides the longitude-east-of-Blest-Isles of 8000 sites, one would think that the east-of-BI part just might get mentioned somehow. Instead, GD 1 is completely non-committal regarding what will be the prime meridian of the work. And GD 2.1 (the preface launching the reader into the 8000 sites) is likewise. (If one were just grabbing — virtually unedited — a preface to another work, something like this could easily happen.)

F3 In the GD, there are (§F4) a few islands near Mauretania at about the latitude of the Canaries, which are the litherto-standard-identification of Ptolemy’s Blest Isles. (E.g., S&G 1:455 n.200, which scrupulously notes that the identification of the Blest Isles with the Canaries is uncertain.) But these islands are not GD-listed at or even very near longitude zero; nor is the center of the real Canaries longitudinally beyond the real western hump of Africa, which is where the western-most-anciently-known land obviously ought to lie.

F4 GD 4.6.33 lists some non-zero-longitude off-shore islands, incl. “Kerne” at 5°E & c.26°N, latitudinally & phonetically near the Canaries which at (actually) c.28°N, are the better part of a thousand miles north of Ptolemy’s six “Blest Isles”, listed by him (Nobbe ed. GD 4.6.34) at longitudes 0° (four) or 1° (two), at latitudes ranging from 10°1/2 to 16°1/2: about right for the Cape Verde Islands. (Actual CVI latitudes: c.75 nmi N&S of the (actual) western-most point (hump) of Africa (Dakar, Cap Vert) at latitudes that are again a convincing match for the Cape Verde Islands, which are therefore firmly identified as the Blest Isles. F5 The GD’s knowledge of the Cape Verde Islands stands as a testament to ancient explorers’ courage: they are indeed c.400 mi from Cap Vert, the mainland’s nearest point. (By contrast, eastern Canaries are barely off the NW-Africa shore.) So the island’s discoverer was himself the nearest thing to an ancient Eriksen or Columbus. Over 1000 before sailors discovered tacking, trips there were presumably extremely rare and hazardous. Possibly galley-slave rowing-power was the key to the ancients’ knowledge of the Cape Verde Islands. And perhaps they were regarded as Blest because European civilization had not yet significantly uplifted the inhabitants by the introduction of their ever-brewing wars & their ever-resultant slavery.23

23Thanks to Alex Jones for reminding DR of this.

24E.g., B&J plate 6 (c.1300 AD); same in plate 1, marked as “Fortuna insula”. Also S&G 2:838 & volumes’ inside-covers. Online at http://en.wikipedia.org/wiki/Image:PtolemyWorldMap.jpg, the same six “Fortunate” islands can be seen at the west end of Ptolemy’s world map, again at a position close to that of the Cape Verde Islands. The astonishingly persistent previous confusion presumably originated with realization that the 6th of the 6 islands listed at GD 4.6.34 is named “Kanaria Nesos”.

Ptolemy’s GEOGRAPHY 2008 March DIO 14 §3

Ptolemy’s GEOGRAPHY 2008 March DIO 14 §3
G Hours as the Route of All Evil in Ptolemy’s GD

G1 Looking at GD 1-7 and GD 8 as separate sections of the GD, one must notice that each of the two sections’ cross-citations of the other’s prime meridian is patryal at best (and could well have been from later interpolation) — so let’s keep our eye on the main point: there is no mention of the Blest Isles in the preface to GD 8, any more than there is any mention of Alexandria in the forward (GD 1) of GD 1-7. It would be hard to ask for better evidence that neither (§D1) section was the immediate direct source of the other’s totality.

G2 But let us return to the essence of the DR theory (§D1&D5, fn 12) that the data of GD 2-7 were based upon data of the type found in GD 8, and fix upon the main points regarding the source of GD 2-7’s major-site data:

[a] Whereas all latitudes were originally measured angles (method: Almajest 1.12), the inaccuracy of the latitudes in GD 2-7 show that these data had been corrupted by subjecting to crude rounding (§D5) for astrolabors’ longest-day tables in hours, before being computationally converted into the latitude-degree data that ended up in GD 2-7.

[b] All accurately-based latitudes in GD 2-7 were originally in hours,5 as noted in GD 1.4. This, because based upon comparisons of lunar-eclipse local-times.26[c] Thus we have arrived at a hitherto-unappreciated realization (obvious example at fn 16): ironically, every jot of the astronomically-determined data of the basic network of cities underlying GD 2-7’s thousands of degree-expressed positions, was at some point (during its mathematical descent from its empirical base) rendered in time-units: hours. As proposed in Rawlins 1985G.

G3 And, as a result of rounded longest-days (§D5) and Earth-scale shifting (§L3), these hour-data became the semi-competent- occultist conduit (§D1) for data-corruption which tragically destroyed a sophisticated civilization’s laboriously accumulated high-quality astronomically-based ancient geographical data.

25 Wrongly (fn 45), Ptolemy believed (GD 1.4&12-13) that eclipse-based longitudes were rare. (The method of finding longitude-differences between sites by comparing local times of simultaneously-observed eclipses, as obviously well-known. See, e.g., Strabo 1.1.12 or GD 1.4.2. Least-squares tests on ancient longitudes show that the eclipse method had been extensively used by genuine ancient scientists: Rawlins 1985G §§5&9 [pp.258-259 & 264-265].) And so he assumed that generally-accepted longitudes were primarily based upon travellers’ stade-measured distances (terrestrial) instead of eclipse-comparisons (celestial) — a crucial, disastrous error, which undid generations of competent scientists’ eclipse-based accurate longitudes-in-hours and thereby wrecked (§L3) the GD’s longitude macro-accuracy in angle. (Though not in distance: idem.) Note: said miss-step must have occurred before the hypothetical dovetailing (fn 21) of GD 2-7 and GD 8, perhaps (§D1) in the 1st century BC.

26 A number of network-cities’ GD 2-7 longitudes could have been calculated directly from GD 8 or its source, using Alexandria (D149) longitude (east-of-Blest-Isles) 60° 1/2 (GD 4.5.9) or 60° 1/5 (GD 8.15.10). Some examples:

1. London (GD 2.3.27, 8.3.6, D4), Bordeaux (2.7.8, 8.5.4, D21), Marseilles (2.10.8, 8.5.7, D26), Tarentum (Diller 1984 Codices NZ Europe-Map 6 site #5) (3.1.12, 8.8.4, D53), Brindisi (3.1.13, 8.8.4, D54), Lilybaeum (3.4.5, 8.9.4, D67), Syracuse (3.4.9, 8.9.4, D68), Kyrene (4.4.11, 8.15.7, D146), Meroë (4.8.21, 8.16.9, D165), Kyzikos (5.2.2, 8.17.8, D176), Miletos (5.2.9, 8.17.13, D181), Knidos (5.2.10, 8.17.14, D182), Rhodes (5.2.34, 8.17.21, D189) — allowing for common [Rawlins 1994L §F3] ancient rounding of 1° 8’ to 8°, Jerusalem (5.16.8, 8.20.18, D247), Persepolis (6.4.4, 8.21.13, D271).

However, these could as easily have been computed in the other direction. The majority of less grid-critical sites’ degree-coordinates couldn’t (§D1&D5) have been computed directly from those of GD 8 (at least in its present state), but could’ve gone the other way; e.g., Smyrna (5.2.7, 8.17.11, D179) & Pergamon (5.2.14, 8.17.10, D178). Given the GD as it stands, if GD 8 is contended to be the direct ancestor of GD 2-7’s longitudes, one would have to argue that the underlying network-basis was far less in number than GD 8’s 360 sites — which, if we are speaking of sites whose longitudes (vs Alexandria) had been astronomically determined, would not (in itself) be an unreasonable contention.

H Precession and Aristarchos

H1 Precession is the difference in the length of the tropical and sidereal year, caused by a gradual shift of the Earth’s axis — an ancient discovery which we can easily trace back to Aristarchos (not-so-coincidentally also the 1st astronomer to publicly announce that the Earth moved, and indeed that some of his appreciation (§D5) for the DR theory’s discovery. Both of Aristarchos’ year-lengths are provided at Rawlins 1999 §§B7 [p.33]; see also Rawlins 2002A fnn 14&16 [p.8].

H2 Precession was known to the author of GD 8.2.3.28 Thus, the GD 1.7.4 discussion seems awfully strange,29 since it here quotes the statement of Marinos of Tyre (c.140 AD: §11) that all the constellations rise&set in the tropical geographical regions — with the sole exception of UMi, which becomes ever-visible after a northward traveler passes latitude +12° 2’5, Hipparcos’ long-precessionally-obsolete NPD (North Polar Distance = declination’s complement) for α UMi. (I.e., modern “Polaris”: the brightest star in UMi, and the most northern easily-visible UMi star for us; the most southern for Hipparcos.) And α UMi’s NPD actually was 12° 27’ (Decl = 77° 33’3”) at Hipparcos’ chosen epoch, — 126.278 (126 BC Sept 24 Rhodos, Apparent Noon: Rawlins 1991H eq.28 [p.58]). Marinos further states that this parallel is 1° north of Okelis, which he mis-places (§C1) at 11° 2’5 N latitude.30 (A poor estimate, since Okelis (D281 [modern Turbah, Yemen]) is actually at 12°41’N, 43°32’E.) Yet, by Marinos’ time (§H2), α UMi’s NPD had precessed down to about 11°: in 140 AD, 10°59’, So, his statement prove he didn’t account for precession. But the most peculiar aspect of this matter is that GD 1.7.4 makes no comment at all on Marinos’ flagrant omission of precession — and this though Ptolemy is (as usual) in full critical mode (alertly questioning [GD 1.7.5] whether any of Marinos’ discussion is based upon the slightest empirical research), and though the writer of the Almajest certainly knew (Alm 7.2-3) the math of precession. Comments:

H3 There can be little doubt that the authors of GD 1.7.4 and GD 8.2.3 were not the same person.

H4 If Okelis were where Marinos placed it, α UMi’s ever-visible circle would have been south, not north of Okelis.

H5 It has been noted that, by the time of Marinos & Ptolemy, α UMi was (thanks to precession) no longer the most southern of UMi’s seven traditional stars?! — Ὠ UMi and especially 3rd magnitude γ UMi were much more so. Indeed, for the time of the GD, γ UMi was over a degree (1° 0’4 at 160 AD) more southern than α UMi. (Shouldn’t the “Greatest Astronomer of Antiquity” [12 §G2] have known this? — especially since he pretended he’d cataloged the whole sky’s stars: Almajest 7.4, I.e., the GD 1.7.4 statement on α UMi disagrees not only with the sky but with Ptolemy’s own tables.31 Similar cases at fn 45.) Thus, γ UMi had long since assumed the distinction (one interjected by Marinos, ironical-irony of being the outlier-star whose NPD determined whether a geographical region was far enough north to attain UMi-ever-visibility. (Note that GD 6.7.7 puts Okelis at latitude 12°N [and false-Okelis at 12°1’]; so, credibly, the GD’s Okelis latitude was closer to reality than to Marinos. Note also that 12° is almost exactly the theoretical

25 Note: not a single historian has yet indicated publicly that he understands this rather self-evident point. (Though some have privately.) Which gives us hope that sociology can yet attain to the predictivity of astronomy. (See §E2 Epilog [p.31].)

28 GD 8.2.2 by the arrangement of B&K or 8.2.2 in Diller 1984 (the only reliable English translations) at DIO 5.

29 Though some experts disagree: B&K p.65 n.23 & p.120 n.3.

30 This is a revised & mutilungled re-hash of an original Hipparcos estimate that Okelis was on the arcic (ever-visible) circle of α UMi?! — which would have been correct in 170 BC and OK to ordinig 0°1’ during his career.

31 Even Ptolemy’s very insufficiently precessed plagiarism of Hipparcos’ star catalog has γ UMi 9’ south of α UMi.
I Marinos Mis-Dated?

II Nowadays, it seems to be almost universally assumed (e.g., Neugebauer 1975 pp.879 & 939) that Marinos flourished very early in the 2nd century AD, sometime during Trajan’s reign, around 110 AD.33 Which is curious, since in c.160 AD (or perhaps even later: §12) Ptolemy refers to Marinos as (GD 1.6.1 emph added) “the most recent [of those] of our time” who have attempted a large geography. Now, if you were currently writing of a geographer of the mid-1950s, would you speak of him so? (GD 1.17.11 has been taken to indicate that Marinos was retired or dead by Ptolemy’s day, but the passage is hardly unambiguous on that point — and would make more sense if Marinos’ last publication was merely 5 or 10 years past.)

12 Moreover, Alex Jones points out (2007/5/23 conversation) that the forward dating of Marinos would help solve a problem first emphasized at Schnabel 1930 p.216: when did Ptolemy become aware that people lived south of the Equator? *Almajest* 2.6 says the S.Hemisphere is unexplored, though Marinos says otherwise and (§M1) the GD agrees. This implies, since the *Almajest* might have been compiled during Marcus Aurelius’ reign (Rawlins 1994A, Table 3 & fn 45 [p.45]), that Marinos’ date could be as late as c.160AD.

13 The argument adduced to date Marinos to much earlier (than Ptolemy) is that Marinos’ work took into account names of sites reflecting the changing Empire, e.g., “Trajan in Dacia (GD 3.8, 8.11.4 [roughly modern Romania]) up to c.110 — but not later in Parthia (GD 6.5, 8.21.16-18 [roughly modern Iran]) and north Africa. But how sure is such tenuous reasoning? How strongly should it rank? — in the face of:

[a] GD 1.6.1’s plain statement of Marinos’ contemporaneity, and

[b] the incredibility of the long-orthodox implicit assumption that, in a busy mercantile empire, a succession of macro-geographers (GD 1.6.1 implies plurality) suddenly ceased for 1/2 a century!

14 Moreover, why assume that Marinos adopted all the latest name-changes? Ptolemy didn’t: his preface’s criticisms complain (GD 1.17.4) that Marinos misplaced the Indian trading town Simylla (D330) and didn’t realize that the natives call it Timoula. Yet the GD’s data-listings (GD 7.1.6 & 8.26.3) both retain Marinos’ name: Simylla, not Timoula. B&J

33Likewise, 1000 nmi to the southwest of Okelis: regarding the location of the two lakes feeding the Nile, the GD astutely makes a major correction to Marinos in placing both lakes much nearer the Equator than Marinos had them. (In reality: the Equator runs through the eastern source, Lake Victoria. And the western source, lake-pair Edward & Albert, straddles the Equator.) Remarkably, the GD’s maps of Africa were still consulted by geographers in the mid-19th century, when these lakes were finally 1st reached by Englishmen. (See *J Roy Geogr Soc* 29:283, 35:1, 7, 12-14; *Proc RGS* 10:258.)

34Also fn 45. See Rawlins 1985G p.260 (On vs Heliopeis: fn 6) and p.266 & fn 6. We find similar hints of patch-workery throughout the GD, e.g., at GD 1.24.11-vs-17, as the lettering for two consecutive projection-diagrams are needlessly shuffled. (See B&J p.91 n.80.) See also another Ptolemy-compiled work, the *Almajest*, where, e.g., the mean motion tables’ Saturn—Mercury order of the planets (Alm 9.3-4) is the reverse of the Mercury—Saturn order followed in their fraudulently (Rawlins 1987 pp.236-237 item 5; Rawlins 20033 §K) alleged derivation at Alm 9.6-11.8. For more such patch-work indications, see frequently here, and at Thurston 1998A end-note 17 [p.17] & Rawlins 2002V §C [p.76].

35Indica of such patch-workery in the GD are frequently noted here, due to the inexplicably-repeated modern claim of coherent unity for each of Ptolemy’s works.

36Quite aside from the present discussion: for compelling evidence against this date, see H.Müller’s clever discovery: §17.

n.53 (p.76) note an even more revealing careless reference.36 Marinos’ Aromata latitude. So, what should be tested isn’t whether all but whether any post-Trajan geography appears in the GD.

15 Especially since it doesn’t seem that there’d likely be many changes. After all, it’s well-known that Dacia was the last solid addition to the Roman Empire. (It may not be coincidental that around this time the Roman army was becoming predominantly alien-mercenary.) Trajan’s army was of course stronger than Dacia’s. (So, we know who ended up with Dacia’s gold, some of it possibly pictured in Fig.2.) But it wasn’t stronger than that of the Parthian Empire; thus, the attempted-rape3 victim got in all the Part’n shots, and the puppet ruler whom Trajan had placed into power at the then-capital (Ctesiphon [D262], near Babylon [D256]) passed on soon after, as did Trajan (117 AD). Trajan’s adventure in Parthia having been an expensive failure, his two successors chose not to try expanding the empire. Hadrian (117-138) did not share certain current warlords’ fiscal profligacy. Similarly for Antoninus Pius (138-161 — which takes us up to the time of Ptolemy’s geographical work). These points recommend some caution before we draw conclusions on Marinos’ date from lack of the-very-latest Parthian information.

16 Next, we note that the most notorious exception to the non-expansion policy of Hadrian occurred in Palestine. In 130 AD, he visited Jerusalem and ordered its re-building. Since Hadrian’s family name was Aelius, he re-named Jerusalem: “Aelia Capitolina”. (His supervision evidently triggered a local revolt — put down in 132-134, with Hadrian sometimes on the scene.) So, does the GD reflect the change? Yes: GD 5.16.8 lists “Ierosoluma [Jerusalem], which is called Ailia Kapitolias”. And GD 8.20.18 lists “Ailia Kapitoias Ierosolomou” without further comment but obviously reflecting the same up-to-date change. Therefore, if we have indications that both the GD’s data-sections (GD 2.7 and GD 8), previously adduced to date Marinos to c.110, actually contain material from the 130s or later.

17 An example of the fruitfulness of the foregoing: Almost 2 centuries ago, H.Müller made the brilliant observation that a GD-listed N.German town “Siatoutanda”, was probably non-existent, just (another: fn 45) Ptolemy-compilation mis-read of a foreign language: Tacitus’ Latin description (Annals 4.73) of a N.German “triumvirate), [2] exhausting the incredibility of the long-orthodox implicit assumption that, in a busy mercantile empire, a succession of macro-geographers (GD 1.6.1 implies plurality) suddenly ceased for 1/2 a century!

36These situations remind one of the common modern mis-interpretation (Rawlins 2002B fn 7 [p.12]) of Almajest 3.7 to mean that no Babylonian astronomical records came through to Ptolemy prior to 747 BC, though the actual statement is rather that continuous records went back that far.

37Over 4 centuries of botheration, Parthia repelled three Roman invasions: [1] swallowing the army of Crassus (suppressor & crucier of Spartacus, and member of the 1st triumvirate), [2] exhausting emperor Trajan, and (after a temporary setback at Marcus Aurelius’ hands) [3] slaying last pagan emperor Julian the Apostle (unless he was fraged). And, yes, “parting shot” is thought to come from Parthian archers’ tactic of shooting arrows even when retreating or pseudo-retreating.

38Such an explicit update is rare in the GD’s data-body. Another such passage, even more unusually discursive, is found at GD 7.4.1, where it is stated that Taprobane (modern Sri Lanka [though known as Ceylon in Diller’s & DR’s youth]) was formerly called Simoundou but is now called Salike by the Englishmen. (See Rawlins 1985G p.260 (On vs Heliopolis: fn 6) and p.266 & fn 6. We find similar hints of patch-workery throughout the GD, e.g., at GD 1.24.11-vs-17, as the lettering for two consecutive projection-diagrams are needlessly shuffled. (See B&J p.91 n.80.) See also another Ptolemy-compiled work, the *Almajest*, where, e.g., the mean motion tables’ Saturn—Mercury order of the planets (Alm 9.3-4) is the reverse of the Mercury—Saturn order followed in their fraudulently (Rawlins 1987 pp.236-237 item 5; Rawlins 20033 §K) alleged derivation at Alm 9.6-11.8. For more such patch-work indications, see frequently here, and at Thurston 1998A end-note 17 [p.17] & Rawlins 2002V §C [p.76].

39Indica of such patch-workery in the GD are frequently noted here, due to the inexplicably-repeated modern claim of coherent unity for each of Ptolemy’s works.

40The “Siatoutanda” goof reminds one of St.Philomena, of whose “life” whole books used to be written (DR possesses a copy of one), though she never existed: “Philomena” turned out to be just an
all-too usual in the ancient-science community, Müller's novel and obviously valid discovery has been doubted on grounds so tenuous (in comparison to the compelling evidence in its favor) as to make one wonder whether anything ever gets resolved in this field, no matter the power of relative evidence. Against Müller, it has been argued (see sources cited at B&J p.28 n.34) that Tacitus Ann was published in 116 AD, which is after the (inexplicably-widely-believed) upper-limit date (110 AD) for Marinos. (But the 110 date is so far from firmly established that one should reverse the situation: instead of using the date to exclude H.Müller's finding, use the HM finding to help establish a lower limit for Marinos' date.) So we recognize that H.Müller's discovery contributes importantly to the evidence suggesting that conventional wisdom on Marinos' date is suspect, and thus that there is little trustworthy evidence against our proposal that Marinos was much nearer Ptolemy's contemporany than is now generally understood.

J Tyre: Missing Home-City of Book 8's Once-Supposed Source

J1 The most peculiar coincidence in the history of ancient geography will turn out to be a lucky break for scholars of the GD: incredibly, Marinos' native Tyre is absent from GD 8. (Curiously, this telling point has been overlooked in the literature.) And, in a context of questionable authorship, we must likewise notice (§E4) that Ptolemy's alleged home-city (Alexandria) is missing from GD 1.

J2 Marinos is clearly identified as of Tyre (GD 1.6.1). Indeed, Tyre (Phoenicia) is cited doubly and with accurate latitude — highly exceptional on each count — at GD 5.15.5&27: 61° E of Blest Isles, 33°1/3 N of Equator. (The latitude is correct [see similarly at §K11] if we account for refraction of pole-star light and 5° rounding.)

J3 Thus, we conclude that GD 8 (in the form we have it) was not compiled by Marinos.

over-imaginative later mis-read of a fragmentary ancient stone inscription (found in the catacombs of Rome on 1802/5/25): “LUMEN PAX TECUM FIT”, which was “restored” as a reference to FILUMEN or Philomena. This was enough to launch (starting c.1805 in the super-religious Kingdom of Naples) a cult, special novenas, the usual “miracles”, and (from devotees’ revelations) a detailed biography of her life & martyrdom. The Roman church creditably removed her from the list of saints about a 1/2 century ago.


Similarly, when (1999/101) dim the atmosphere proponent B.Schafer imparted to DR his intention of testing the Ancient Star Catalog’s authorship by assuming 0.23 mags/atm opacity, DR immediately suggested that it would be far more fruitful to use Hipparchos’ authorship (which had by then been obvious to serious astronomers for centuries) to test for ancient atmospheric opacity. BS didn’t listen, so this important and revealing project — proving beyond any question that man (not nature) is the prime cause of present atmospheric opacities ominously higher than ancient skies’ — was instead masterfully and independently established by Pickering 2002A [§D2-D5 [pp.11-12].

Tyre’s absence from GD 8 has several non-neatnesses. While Tyre is also missing from the Important Cities lists in late copies of Ptolemy’s Handy Tables (Halma ed.), Tyre does reside in two 9th century copies (published in Honigmann 1929), which are far older than our earliest mss of the GD, and each contains (fn 17) c.100 more sites (than GD 8): Tyre is city #307 in Vat 1291, #160a in Led.LXXVIII. In the latter ms, Tyre is counted secondarily; which suggests that, if paring occurred, Tyre was expendable. The superficially attractive interpretation is to wonder if GD 8 is a Byzantine-era add-on, which reflected a shrinking of the number of sites from nearly 500 to just 360. The problem with that theory is format: GD 8 differs generically (from all other surviving Important Cities lists, which uniformly are in longitude degrees east of the Blest Isles and latitude degrees north of the Equator) by: [1] using Alexandria (fn 14) as prime meridian (astrologer Ptolemy’s preference); and [2] providing data entirely in hours, just as ancient astronomers preferred (§G2 [a]). This argues strongly that GD 8 goes back in time at least as far as Ptolemy.

K Landubby Ho! Wrapped China Negates the Pacific

K1 It is well-known that the farthest-east region of the GD, China, portrays a non-existent roughly-north-to-south coast (making any route to the Pacific) beyond the South China Sea, near longitude 180° (12° east of the Blest Isles or 120° (8°) east of Alexandria, stretching from near the Tropic of Cancer, all the way south to Kattigara at 8°1/2 S. latitude — effectively wrapping China around the Indian Ocean’s eastern outlet. Longitude-longitudes for 18 China sites are found in GD 7.3 (Renou 1925 pp.62-65).

K2 But, according to the previously-brouched §D1 theory, all of this geography hinges upon the underlying grid-network: GD 8 and-or its kin. If we look at the GD 8.27.11-14 China list we find that the situation of all China hinges upon just 3 cities’ hour-data (longest day & longitude east of Alexandria, according to Diller 1984’s XZ mss): Aspithra [D354] (13°1/8, 7°2/3), Thinai [D355] (12°5/8, 8°), Kattigara [D356] (12°1/2, 7°3/4). Anything wrong with GD’s China is wrong in this trio.

K3 For Thinai (D355), GD 7.3.6’s latitude (3°5) jars with GD 8.27.12’s longest-day 12°3/4 north, which would be correct for about latitude 12°1/2 N.

K4 Fortunately, Vat 1291’s Important Cities (fn 17) lists the same 3 cities (only for China. (Honigmann 1929 p.206: cities #443-#445; no China listings in Led.LXXVIII.) And on Thinai, it provides confirmation of GD 8 (not GD 7), listing Thinai at 13°N. Which suggests that the 3°S of GD 7 is either a scribal error (missing the iota for ten) or perhaps is differential: 3° south of Aspithra (16°1/4N). Either way, it seems that 13°N is correct, as listed by Vat 1291.64 (S&G 2.734 for Thinai has GD 7.3.6’s 13° latitude.)

K5 Finally, we observe that Kattigara’s latitude in degrees is the same in both Vat 1291 and GD 7.3.3 & GD 8.27.14. The matter gets even more interesting when we check our latitudinally-corrected position for Kattigara: 177°E (of the Blest Isles) & 8°1/2 N — that is precisely the GD 7.3.2 position of Rhabana. Therefore (not for the 1st time: §H5), the GD may have used two (or more) names for the same place.

K6 Thus, when we examine the underlying-grid trio for China, the two negative (southern) latitudes both appear so shaky that we can dispense with all negative signs for China — which eliminates the above-cited fantastic N-S coastal-bar to the Pacific.

K7 There is a disturbing pattern to the GD 7 latitudes of the only four cities in the Southeast Asia region which are listed in GD 8 (in order N-to-S): Aspithra, Thinai, Kattigara, Zabai. These cities’ GD 7.2-3 latitudes are, resp, about equal to: 16°1/4, 13°, 8°1/2, 4°3/4 — which are suspiciously close (though not exactly equal) to what one would compute indoors via sph trig (eq.1) from a quarter-hour-interval klimata table: Aspithra (D354) 13°, Thinai (D355) 12°3/4, Kattigara (D356) 12°1/2, Zabai (D348) 12°1/4. (And, indeed, these are the values Diller found in GD 8’s UNK mss-tradition.) This looks even fishier when one recalls (above) that these are the only SE Asia cities east of the Golden Peninsula which are listed in GD 8, where only longest-days (the stuff of klimata-tables) are provided for N-S position. (Even the precise 13°1/8 variant discussed in fn 44 for Aspithra, perfectly matched what may [idem] have been merely a scribal error: 18°1/4.) Obviously assuming exactly-correct latitudes here is risky when dealing with such rounded data. Conclusion: we must also use verbal descriptions, if we wish to have any chance of solving this section of the GD.

44 The same Vat 1291 list gives 18°1/4 N latitude for Aspithra (not the 16°1/4 N latitude of GD 7.3.2, corresponding to longest-day 13°1/8 [§K7], the very Aspithra longest-day value listed in Diller’s XZ-tradition mss. (One is tempted to ask if 18°1/4 N latitude [idem] was the true original latitude — or was later forced to agree with M = 13°1/8? But it could have just come from a scribal error.) In Nobbe, GD 8 lists Aspithra at longest-day “about” 13°, which corresponds to latitude 16°2—, agreeing with the GD 7.3.5 Aspithra latitude in Nobbe and Renou: 16° and 16°1/4 N, respectively.
K8  GD 7.3.3 specifically refers to Kattigara (which has a 1st syllable like Cathay's) as a Chinese harbor, near walled cities and mountains. So it is on the Asian mainland.

Note: The rest of this explicitly specifies reconstructed region was nottrivially re-analysed & revised in 2009. See DIO 5 fn 68 for numerous SE Asia site-identifications.

Our interpretation of GD 1.13.9 (B&J p.75): Marinos is saying that an ancient sea voyage from Malay's Sabara-Tamala region (Phuket, Malay) to the Golden Peninsula (Sumatra's NW tip) is roughly 200 mi, which is about right. (Marinos' sailing direction [c.SE] is ignored here, since based on his distorted map.) GD 1.14.1 says the rest of the trip to Zabai (Singapore) takes 20³. Going around Sumatra (instead of sailing between Malay&Sumatra) would require c.20⁴. (Speed c.100mi/day; already established at B&J p.76 via GD 1.14.4: Aromata to Prason. Made more exact by checking Phuket-to-Singapore.) The original report is due to “Alexandros” (geographer? explorer? admiral?) who says the trip from Zabai across to Kattigara (Saigon) takes merely “some days” (GD 1.14.1-3), roughly consistent with the c.6⁵ it would've taken at the previous speed.

K9  The GD's supposed direction to Kattigara (left [east] of south) is obviously confused. I suspect that the ancient cause was a common land-lubber misinterpretation: “south wind” (which means wind from the south) was taken as towards the south — thus, the report of going somewhat east of a “south wind” (GD 1.14.1; B&J p.75) was misinterpreted at GD 1.14.6) to mean sailing with a wind blowing southwest. (Compare to B&J p.76.)

K10  Kattigara (D356) was probably about where resides the harbor long called Saigon. (Re-named Ho Chi Minh City. For now.) The real Saigon's latitude is just north of 10⁶N, so the GD is off by c.2⁷, which is about as big an error as one will find caused (δS) in this region by computing latitudes (eq.1) from 1/4-joint interval-kmata. Whoever originally cubby-holed Saigon so found that its L didn't fall exactly on a klima: the nearest such klima for rounded L = 10⁷ would in a region rounding to 1/4 put L at 8³1/2. This, in microcosm, is the secret of why the GD's mean latitude error is so poor: ordmag 1⁸ (δDS), despite contemporary astronomers' achievement of knowing their latitudes ordmag 100 times more accurately. (See citations: Rawlins 1982G, Rawlins 1982C, Rawlins 1985G.)

K11  For the four above-cited SE Asia cities with klima-afflicted latitudes, our tentative identifications follow. Barely-inland Aspithra (D354, L: 16⁹1/4) = Thailand Gulf’s Chanthaburi (real L: 12°7.7), more deeply inland Thainai (D355, L: 13°) = Cambodia’s Phnom Penh (real L: 11°6.1). Kattigara (D356, L: 8³1/2) = Saigon (real L: 10°8.8). Zabai (D348, L: 4°3/4) = Singapore (real L: 1°3). The GD's failure to notice prominent Hainan Island (which nearly blocks off the east side of the broad Tonkin Gulf) suggests that the report Marinos used did not extend beyond Saigon (which is in fact the farthest point of Alexandros' narrative), so Alexandros & thus the GD never reached Hanoi or Hong Kong.

48 Would linguistic problems (in the babel of antiquity) have contributed to these errors? (Marinos likely wrote in Greek; otherwise, Ptolemy could not have used him for a whole book.) For Ptolemy, it probably wouldn’t have been the 1st time. He appears to have sloppily misordered (GD 1.4.2) simple, well-known data regarding the famous lunar eclipse that occurred shortly before the Battle of Arbela (D261 [modern Irbil, lately a north Iraq hot-spot]) also seen at Cartaghe (D131), by (www.doi.org/cot.htm#ptxt) screwing-up Latin text of (or like) Pliny’s accurate description of that ~330/90/20 event, thereby attaching Arbela’s eclipse-time to Cartaghe! Despite lunar eclipse after lunar eclipse occurring in Ptolemy's lifetime (three recorded at Alexandria in under 3 months), this antique record was his sole example (!) of how to determine longitude astronomically.

(See fn 25.) Further suggestion of patch-workery (also [L1]): the Ptolemy account of these eclipses is in itself not too unusual. He quite well knew the real sky Well worth-owning his own listen-solar-tables. See similar situations for Polaris at fn 31 and for Venus at Rawlins 2002V [B3 (p.74)]. And his solar faces also show the same sympathy to swift-simple, not-even-tabular fraud and plagiarism. (Anyone researching Ptolemy should keep ever in mind that he was shamelessly capable of every brand of deceit. See, e.g., fn 8; also Thurston 1998A 1:2; [p.14].) This eclipse was so famous that one would suppose it was widely-written-of. Thus, it is doubly weird that Ptolemy could make such an error. The suggestion here is that, as an astrolabe for a Serapeic temple, he was isolated from real scientists. (As perhaps Hipparchos had also been: [B1].

49 A consideration which alone could serve to gut the entire long-orthodox Neugebauer-group fantasy (§D4) that high or even low Greek math-astronomy was derived from Babylon. Note that the same Strabo passage shows that Erotathenes’ latitude for Babylon was as erroneous as Hipparchos’ but in the other direction. I.e., the entire Greek tradition had no accurate idea of where Babylon was, despite by-then long-standing contacts that but just as great, i.e., invaluable Babylonian eclipse records (Dicks 1960 p.134 notes that Babylon had no interest in geographical latitude, not even its own.)

It has been remarked that the Strabo 2.5.34 intro to his discussion of Hipparchos’ klimata appears to state that Hipparchos was computing celestial phenomena every 700 stades (i.e., every degree) north of the Equator. But since the lengthy klimata data immediately following are almost entirely spaced at quarter-hour and half-hour intervals, DR presupposes that the original (of the material Strabo was digesting) said that Hipparchos was providing latitudes (for each klima) in stades according to a scale of 700 stades/degree, a key attestation that Hipparchos had adopted Erotathenes’ scale.
M Ptolemy’s 1st Planar World-Map Projection
From Where-in-the-World Arrived That 34-Unit Vertical Strut from Its Top (ε) to Its “North Pole” (η)? Ancient Averaging. And Weights?

M1 In GD 1.24, Ptolemy twice attempts to design a planar portrayal of a broad spherical geographical segment, representing the known world — the ekumene — covering 180° of longitude from the Blest Isles (0° longitude) to easternmost China-Vietnam (180°E. longitude) and 79°5/12 (GD 1.10.1) of latitude from Thule [Shetlands (Mainland)] (63°N. latitude) to anti-Meroë (16°5/12 S. latitude, a klima as far south of the Equator as Meroë is north of the Equator). It is the 1st of his two projections (GD 1.24.1-9) which will concern us, since it involves a hitherto-unsolved mystery. This projection (page opposite: Figure 1) re-creates Ptolemy world-projection.

Beyond the Equator, instead of continuing to extend the radiating meridians of his fan-projection, Ptolemy decides to bend all meridians inward — resulting in the oddly-shaped, dark-bounded ekumene of Fig.1. This kink-step enables Ptolemy to force the Thule semi-circle (latitude 63°N) = ξ-ο-π; the Rhodos (GD M6) semi-circle (latitude 36°N) = θ-κ-λ; the semiEquator (latitude 0°) = ρ-σ-τ; the anti-Meroë semi-circle (latitude 16°5/12 S) = μ-ζ-υ. (Repeating M1: though each arc in Fig.1 is only c.98°, it represents 180° of longitude in the Ptolemy world-projection.)

M2 For the 1st Projection’s conversion of the spherical-segment ekumene to planarity, the degree-distance $T = 63°$ from Equator to Thule is made (GD M4) into $T = 63$ linear units; likewise for the $S = 65°5/12$ from Equator to anti-Meroë, etc. In Fig.1, representations of several latitude-semi-circles are depicted as Ptolemy’s source intended (fn 54): the Thule semi-circle (latitude 63°N) = ξ-ο-π; the Rhodos (GD M6) semi-circle (latitude 36°N) = θ-κ-λ; the semiEquator (latitude 0°) = ρ-σ-τ; the anti-Meroë semi-circle (latitude 16°5/12 S) = μ-ζ-υ. (Versus fn 51.)

The fan is fairly neatly ekumene semi-circles are represented by 98° arcs. Thus, all north-latitude is a fan, opened slightly more than a right angle: c.98°-arcs. (Versus fn 51.) The fan is fairly neatly ekumene semi-circles are represented by 98° arcs. (Versus fn 51.) The fan is fairly neatly.

The odd duality here is effected by two rough expedients:

[a] Denoting the fan’s units by forcing the distance $T$ from Equator to Thule = $R = 63°$ units of space. $(T = 63$ is henceforth both a distance and an angle-in-degrees.)

[b] Making the distance $H$, from the Thule circle to the fan’s pseudo-N.Pole (point η in Fig.1) proportional to $cos 63°$; i.e., equal to $cos 63°$ in units of $R$, the fan’s radius from “N.Pole” (point η) to Equator. Simply put:

$$\frac{H}{R} = cos 63°$$

These conditions produce $T = R - H = R - R cos T = R(1 - cos T)$. Thus:

$$R = \frac{T}{1 - cos T} = \frac{63}{1 - cos 63°}\approx 115.38 \ldots \approx 115$$

(The rounding is Ptolemy’s.) Which produces the radius $H$ of the Thule latitude-circle (centered at the pseudo-N.Pole η):

$$H = R - T = 115 - 63 = 52$$

$^{48}$ Ptolemy rightly scaled-down (§L3) Marinos’ eastern limit from c.225° (15°5/8 of circle) to 180° (12°8/12 of circle); southern limit, from 224° (Tropic of Capricorn) to 16°5/12 (anti-Meroë).

$^{49}$ This length-fidelity (perfectly reflected in our Fig.1 — and creating the absolute magnitude in eq.6) renders all other southern parallels of the GD ekumene virtually equivalent (in length, though not radius) to their northern counterparts.
Letting $S$ = the south latitude of anti-Meroë, Ptolemy further defines

$$E = R + S = 115 + 16.5/12 = 131.5/12$$  \hspace{1cm} (7)

This establishes all the fan’s dimensions.$^{50}$ We next turn to the more puzzling question of how wide-open the fan will be.

**M5** The openness of the fan is immediately determined when Ptolemy states (GD 1.24.2) that he will choose a vertical strut $Y = 34$ units, extending from $\epsilon$ (the top of the rectangle bounding the fan) to the pseudo-N.Pole $\eta$, which is the fan’s radiating center. And then — a very strange step appears.

**M6** Since Ptolemy follows Hipparchos and (GD 1.20.5) Marinus in taking the Rhodos latitude ($36^\circ$) or klima ($14^\circ 1/2$) as canonical for the mid-ekumene, he chooses (GD 1.24.3) the Rhodos parallel at latitude $36^\circ$N as the one along which he will (allegedly) adjust longitudinal distances precisely, just so that this parallel’s curved length (west—east arc) has the correct proportion $(4.5\cos 36^\circ$: GD 1.20.5 & 24.3) to the fan’s already-determined north—south radial distances ($\S$M4).

**M7** That step is odd because, when he earlier ($\S$M5) established $Y = 34$ units, this rigidly fixed the fan’s openness, and thus the proportion along the Rhodos parallel — i.e., there is no fan-openness flexibility left, once $Y$ is set at 34 units.

**M8** Well, you may suppose: Ptolemy must have chosen $Y = 34$ with this very point in mind — this of course has to be the precise value for $Y$ which will ensure proper Rhodos-parallel proportionality. But, no. He didn’t, and it isn’t. We can tell so by just doing the math.

**M9** If we let $L$ be the latitude of Rhodos or any other place, the following equation finds that value of $Y$ which will guarantee the desired proportionality at the given $L$’s parallel:

$$Y = H \cos \frac{16200 \cos L}{\pi (R - |E|)}$$  \hspace{1cm} (8)

($L$’s sign-insensitivity in this equation is due to Ptolemy’s kink-step: $\S$M3.)

**M10** But the truth swiftly reveals itself when we substitute Rhodos’ $L$ ($36^\circ$) into this equation: we get $Y \approx 31$ units$^{51}$ (nearly 32 without Ptolemy’s eq.5 rounding) — not 34 units. But $Y = 31$ corresponds to fan-spread $106^\circ$ (not the $98^\circ$ of $\S$M1), since

$$F = \text{Fan-Spread} = 2 \arccos(Y/H) = 32400 \cos \frac{L}{\pi (R - L)}$$  \hspace{1cm} (9)

so for $L = 36^\circ$, $F = 32400 \cos 36^\circ/79\pi = 106^\circ$.

---

$^{50}$ A list for ready reference. If we go up the mid-vertical of Fig.1, we find:

- $\alpha$-$\eta$ is of length $H = 52$ (as is $\xi$-$\eta$);
- $\sigma$-$\alpha$ is of length $T = 63$ (as is $\rho$-$\xi$);
- $\rho$-$\eta$ is of length $R = 115$ (as is $\mu$-$\eta$);
- $\zeta$-$\sigma$ is of length $S = 16.5/12$ (as is $\mu$-$\rho$);
- $\zeta$-$\eta$ is of length $E = 131.5/12$ (as is $\mu$-$\eta$).

We recall that $\epsilon$-$\eta$ is of length $Y$. Note that $\zeta$-$\epsilon$ is of length $Z$ ($\S$N3), as are the sides of the 2-1 rectangle: $\gamma$-$\alpha$ & $\rho$-$\beta$, also equal to $Z$ are: $\alpha$-$\epsilon$, $\epsilon$-$\beta$, $\gamma$-$\zeta$, $\zeta$-$\epsilon$.

$^{51}$ This accounts for the non-fitting & unintended aggravation that points $\xi$ & $\pi$ lie above the top ($\alpha$-$\beta$) sides of the rectangle in several modern depictions of the situation. (The discrepancy has long been recognized; see, e.g., Wilberg & Grashof 1838-1845 p.78.) The screwup is not by the drafters but by Ptolemy, who did not realize ($\S$M12) that $Y = 34$ units is not for the Rhodos parallel (corresponding via eq.9 to the $106^\circ$ fan-spread used by the non-fitting diagrams just cited) but was designed as an average fit ($\S$M14) to all ekumene parallels $L$. Note that for $L = 0^\circ$ (Equator) or $63^\circ$ (Thule), fan-spread $F$ would be $90^\circ$ by eq.9 ($Y = 37$ by eq.8). The average of $106^\circ$ & $90^\circ$ is $98^\circ$, which fits $Y = 34$ (the average of $31^\circ$&$37^\circ$: $\S$M13).

$^{52}$If we eliminate the southern latitudes, we yet find $Y = 34$, except for the non-weighted average with rounding, where $Y = 33 1/3$ instead.

$^{53}$See, e.g., B&J p.38.
N Impossible Dream: Symmetric-Rectangle-Bounded Ekumene Fan

There is an attractive alternate theory of the origin of $Y = 34$: the suggestion (§N6) that the $2\times1$ rectangle (§M1) bounding Ptolemy’s ekumene influenced the openness of the fan (Fig.1): “The length of 34 units . . . seems to have been empirically chosen to accommodate the largest map in the given $[2\times1]$ rectangle without truncation of the corners [$\rho & \tau$].” (B&J p.86 n.68.) We will now explore this theory, which takes us in a very different (but equally fascinating) direction from the previous section, §M.

Ptolemy says his projection nearly (§N6) fits neatly into a $2\times1$ landscape-oriented rectangle: see Fig.1.

Since the fan-projection is symmetric about the mid-vertical ($\epsilon - \zeta$), the rectangular condition can be equated with fitting the left or right half of the ekumene into a split-off square. (SPLITTING the rectangle into halves, we will use the left square during the following analysis.) Fitting the half-ekumene into a square will henceforth be referred to here as: the split-ekumene constraint or just The Split.

Having arranged that each half of Fig.1’s rectangular bound is a perfect square of side $Z$ (in §N), we take half of the horizontal straight line between $\rho$ & $\tau$ and call it $B$. Note: if The Split-condition is met, then $B$ should equal half of the rectangle’s top border ($\alpha - \beta$). But it obviously does not, for reasons to be seen: §N7.

Our aim is to (as closely as possible: §N21) meet the Split-condition, which can be expressed simply as:

$$Z = B$$

We then search for the value of $Y$ which ensures that Ptolemy’s ekumene-fan will satisfy The Split. The equation is (using the inputs already defined):

$$Y = E + (R/H)\sqrt{R^2 + H^2 - E^2}$$

Ptolemy starts (§M5) by assuming that the meridian-radiating center of the fan (the pseudo-N-pole: point $\eta$ in Fig.1) is $Y = 34$ units (GD 1.24.2) above the top of the rectangle that he proposes to contain his ekumene projection. (To repeat, we are saying that in Fig.1 the distance from $\eta$ to $\epsilon = 34$ units.)

Ptolemy admits (GD 1.24.1) that his $2\times1$ rectangle isn’t quite exact (§N2): the rectangle’s width is only nearly [επιστευτ] two-fold its height. But: why only approximately twice as wide? Why not adjust $Y$ such as to make the ratio exact? — since the priority here is suspected (§N10) to be The Split: a symmetric $2\times1$ rectangularly-bounded fan, for reasons either aesthetic (symmetry) or practical. (A portable map that is conveniently square after one protective fold?)

The hitherto-unrecognized answer is that, given Ptolemy’s specs for the projection’s essentials ($T = 63$ and $S = 16 5/12$), the $2\times1$ rectangle-bound condition (§N2) for the fan cannot be met. Mathematically speaking: for the cited Ptolemaic values of $T$ & $S$, the only solutions for $Y$ that can result from eq.12 are not real. This surprise finding will now lead us onto unexpected paths.

i.e., the ekumene-fan as Ptolemy ultimately constructed it cannot fit into a $2\times1$ rectangle, no matter how widely or narrowly the Thule-bounded ekumene-fan is fanned out, so long as $S = 16 5/12$. Try it for yourself. As $S$ is increased, we find (from eq.12) that the maximum ekumene southern-limit $S$ that allows $Y = 34$ and satisfies the symmetry of The Split is about $S = 6$.

Figure 2: Left: Trajan aureus, Vienna Kunsthistorisches Museum. Right: Antoninus Pius aureus (minted during Ptolemy’s lifetime), DIO Collection.

### Notes

54 Notice to those checking via-ruler the rectangle of the Nobbe 1843-5 p.47 illustration of Ptolemy’s 1st projection (reproduced at www.dioi.org/gad.htm#n0bm, with the ekumene bounded in green): its halves are accidentally drawn not quite square, though very close. Also, many modern diagrams have failed along the anti-Meroë parallel. Creditable exceptions are those of Wilberg & Grashof 1838-1845 Fig.8 (p.96c2), B&J p.36, S&G 1:122-123, 2:748-749. The present illustration (our Fig.1) is perhaps the 1st rigorously accurate illustration of the anonymous ancient cartographer’s full intended map-rectangle concept. (Where compatible choice of $Y = 34$ and fan-spread 98° allows meant area-proportionality while $\xi$ & $\pi$ lie on line $\alpha - \beta$: §§M14-M15.) Fig.1 is designed in pure Postscript (as was §1’s Fig.1).

55 For the 2nd projection, there is no such qualifier (GD 1.24.17), even though there might as well have been — since for both projections the $2\times1$ rectangular bound is slightly wider than necessary. But for the 2nd projection, there is no appearance that an adjustment might render the ekumene exactly twice as wide as high. Its definition is quite different from the 1st, and results in a fan opened only about 61° (vs the 1st projection’s 98°: §§M1), with a pseudo-north-pole c.180 units above the Equator (vs the 1st’s 115 units: eq.5).
N9 When the Fan Fit The Split
So the 2-1 theory has exploded in disaster: no choice of \( Y \) will satisfy Ptolemy’s \( S = 16 \frac{5}{12} \) and allow the fan-projection to fit the symmetric 2-1 rectangle. Indeed, the maximum \( S \) that will permit satisfaction of The Split (for any choice of \( Y \)) is found via the equation:

\[
S_{\text{max}} = T \sqrt{\frac{1 + \cos^2 T}{1 - \cos T}} - 1
\]

where for \( T = 63 \) (fan’s north bound at Thule) yields \( S_{\text{max}} \approx 11 \frac{1}{3} \).

N10 Things get even more intriguing if we assume (as some non-adamantly have: \( \S N1 \)) that \( Y = 34 \) was an empirical adjustment to The Split (the 2-1 rectangle condition: eq.11). We can test the theory by finding (\( \S N21 \)) the value of \( Y \) which best satisfies The Split. Answer: \( Y \approx 21 \) — a value not even close to 34.

N11 However, let’s keep exploring the theory that the 34 was chosen for The Split. (If Ptolemy was seeking any other type of symmetry, the obvious and nearby alternative would have been to make the fan-spread angle \([\gamma - \eta - \pi]\) equal to exactly 90° — not the seemingly pointless and peculiar [roughly 98°] spread we actually find: see fn 55 or Fig.1.) A 90° spread would make all longitude slices nearly 1/2 their real angular thickness.\(^5\)

N12 Our math for an attempted Split-inspired reconstruction of the process behind \( Y = 34 \) will, up to a point, be the same as Ptolemy’s — only simpler. We found R = 115.4 to 115 (just as in eq.5 or GD 1.24.4) but then use a simple fan — i.e., without\(^7\) Ptolemy’s equatorial kink.

N13 Once we dispense with Ptolemy’s clever kinky-projection scheme, we may easily find the \( S \) that produces \( Y = 34 \):

\[
S = H^2 \sqrt{\frac{1 - (Y/H)^2}{Y}} + 1 - R
\]

Substituting Ptolemy’s values, \( Y = 34 \) (\( \S M5 \) or GD 1.24.2) and \( R = 115 & H = 52 \) (eqs.5&6 or GD 1.24.4), we find:

\[
S = 24.7
\]

A provocative result, since that is virtually right on the southern tropic (24°).

N14 However, as noted: \( S = 24 \)° is Marinos’ value — according to Ptolemy himself (GD 1.7.1-2 & 9.6). Thus, we have found a potentially fruitful alternate-possibility for the source of the problematic \( Y = 34 \): a non-kinked fan-ekumene, with Marinos’ latitudinal breadth of the known world, though Marinos is said (\( \S N17 \)) not to have used a fan-projection.

N15 Having thus found an \( S \) that could have led to GD 1.24.2’s \( Y = 34 \), we may simply invert the process to follow in the theoretical math-footsteps of the hypothetical ancient scholar who hypothetically deduced said \( Y \). If we also dispense with intermediate variables, to show dependence purely upon the ekumene’s northern & southern limits (\( T & S \), resp), the inverse of the previous equation gives us what we need:

\[
Y = 2 \frac{S + T / (1 - \cos T)}{1 + \frac{1}{1 + (1 - \cos T) / T}^2}
\]

(Barely less than 34 1/2 without Ptolemy’s rounding [eq.5] of \( R \) to 115; or about 34 1/8, if that rounding is adopted.)

N17 But GD 1.24.4-5 denies that Marinos used the fan-scheme. If this report is to be trusted and if the Split-hypothesis is valid, then: at an early stage in the history of the development of the fan-approach, a scholar (working sometime between Marinos and the final version of GD 1.24) tried out a simple (no-kink) fan using Marinos’ southern limit (\( S = 24 \)).

N18 However, had he adopted \( S = 16 \frac{5}{12} \) without\(^5\) kinking his projection, he could easily have found (using eq.16) that for this case the appropriate \( Y = 36 \), which would in fact effect a perfect-Split circumscription of the (non-kinked) fan by the preferred symmetric 2-1 rectangle.

N19 So, if the Split-theory is valid, \( Y \) must have been frozen at 34 before any steps were taken to abandon either \[1\] assumption of \( S = 24 \) (Marinos: fn 48), or \[2\] the simple non-kinked fan-scheme.

N20 If Ptolemy adopted \( Y = 16 \frac{5}{12} \) before kinking his fan, then he could easily have arrived at \( Y = 36 \) by the same means that 34 was arrived at. (As already shown above: \( \S N18 \).) Since 36 is not what survived, it would follow that Ptolemy instead kinked his fan before bringing his southern boundary from \( Y = 24 \) up to 16 5/12.

N21 However, either way, he at some point would be faced with the problem of finding out that \( Y \) would most closely effect The Split if the kinked version of his ekumene projection were adopted. For this search, he had best be aware that the eq.11 Split-ratio \( (Z/B) \) is extremal when (on Fig.1) a line drawn from \( \zeta \) to \( \xi \) is perpendicular to the radial line \( \eta - \mu \). Thus, the best fit to The Split occurs when:

\[
Y = \frac{H^2}{E}
\]

For \( S = 16 \frac{5}{12} \), this equation yields, as noted previously (\( \S N10 \)), \( Y = 21 \), which corresponds (eq.9) to fan-spread 132°. For \( S = 24 \), \( Y = 20 \) — corresponding to fan-spread \( F = 135 \).\(^5\)

N22 Even if the foregoing Split-theory isn’t historical (and the prior \( \S M \) development — much-preferred by DR — obviously assumes that it is not), the mathematical development of it here has been thoroughly enjoyable.

---

\(^5\) The corresponding \( Y = H / \sqrt{2} = 37 \), obviously not Ptolemy’s choice.

\(^7\) That is, we do not immediately follow Ptolemy in suddenly bending all meridians inward after southward-crossing the Equator. That step eliminated (for Ptolemy: \( \S M3 \)) the extreme-outside points \( \mu \& v \). But we instead (\( \S N15 \)) keep it simple by letting lines \( \gamma - \rho \) and \( \eta - r \) in Fig.1 extend right straight out to \( \mu \& v \), respectively — and leave them be (i.e., no kink) — just as these two points are shown (slightly outside the 2-1 rectangle in Fig.1).

\(^8\) B&J p.87 n.69 point out the oddity that the GD 1.24 discussion refers only to pt. \( v \) not pt. \( \zeta \), though they are identical. (Both are shown in Fig.1.) This would appear to indicate that, at some moment during drafting, before arrival at the final version of the first projection, pts. \( v \& \zeta \) were separate. This could have happened during experiments ere the kink (when the 2-1 rectangle touched pts. \( \mu \& v \)) or ones where the projection’s southern parallel was the Equator (\( \S 12 \)) or the Tropic of Capricorn (fn 48).
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