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

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

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

From the International Herald Tribune 2008/1/12-13 p.1 obit for Edmund Hillary, 1955/3/29 co-conquerer of Mt.Everest: “In the annals of great heroic exploits, the conquest of Mt.Everest by Hillary and [Tensing] 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 (Sty & 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 history 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.)

Gerd Graßhoff’s 1986 University of Hamburg thesis (Springer Verlag 1990) was based upon his proposal, ultimately successful 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 cement-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) 306 f. (Six ft = 1 fathom is: outstretched hands’ tip-to-tip span, one of the least infirm ancient measuring units.) The most detailed eyewitness Pharos image we have (late 1st 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 opining that no conquest should count unless the conquerer returned to base. Hmmm. And just where would that leave Brit ultra-polarhero Rob’t Scott?
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, 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. 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 we even develop (§J) the 1st credible (if quite speculative [at least until p.12’s finale]) figure ever modestly proposed for the precise height of the Pharos itself. Further, we find (§F) that royals-catering Eratosthenes was a geocentrist who rejected obvious visual counter-indicia, to promulgate the anthrocentric delusion that the Earth is appreciably bigger than the Sun. Finally, it will be shown (§K2) that air-bending (“atmospheric refraction”) of horizontal light explains both of the equally erroneous but extremely disparate (in 8) ancient standard Earth-sizes (Eratosthenes & Poseidonios) within 1% in each case (§K4).

A4 Before beginning, it’s best to recall the four options available for ancient Earth-measurement, and each’s respective atmospherically-induced error:


(All these errors would be appreciably weaker for great heights’ thinner air: fn 1.)
N16 Substituting (into the above equation) $T = 63$ (Thule) and $S = 24$ (southern tropic), the hypothetical ancient computer (of the $Y$ that has come through to us) found

$$Y = 34$$

(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 $S = 16 5/12$ without 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


N20 If Ptolemy adopted $Y = 16 5/12$ 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: §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 what $Y$ 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 $S = 16 5/12$, this equation yields, as noted previously (§N10), $Y \approx 21$, which corresponds (eq.9) to fan-spread 132°. For $S = 24$, $Y \approx 20$ — corresponding to fan-spread $F = 135°$.

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

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**B Lighthouse Math**

B1 The math of the Pharos Method is so easy that it doesn’t even require a diagram, though we supply Fig.1 anyway. At whatever distant point the Pharos’ flame starts (due to Earth-curvature) becoming invisible to a receding observer on the sea, is where the Pharos’ light-rays skim (are tangent to) the sea. Let $v$ be this observer’s distance from the Pharos, and $r$ his distance from the Earth’s center, while the Pharos’ flame is $r + h$ from that center — $h$ being the Pharos’ height and $r$ the ideally-spherical Earth’s radius. At the observer’s position, it is obvious that the angle between the skimming-light-ray vector and the Earth-radius vector is a right angle.

B2 Assuming an airless Earth (which permits straight-line light-rays), we can use Pythagoras’ Theorem:

$$v^2 + r^2 = (r + h)^2 = r^2 + 2rh + h^2$$

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

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58 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 (§J2) or the Tropic of Capricorn (fn 48).
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 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:

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

(13)

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

N10 Things get even more intriguing if we assume (as some non-adamantly have: §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 (§N21) the value of Y which best satisfies The Split. Answer: \( Y \approx 21 \) — a value not even close to 34.

\( Y = 21 \) satisfies The Split to within 5%; that is, \( Z/B < 1.05 \). But Ptolemy’s \( Y = 34 \) cannot satisfy the 2-1 rectangle condition to better than 11%, i.e., \( Z/B > 1.11 \).

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 \([\xi - \eta] \) 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 neatly 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 round \( R = 115.4 \) to 115 (just as in eq.5 or GD 12.24.4) but then use a simple fan — i.e., without7 Ptolemy’s equatorial kink.

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

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

(14)

Substituting Ptolemy’s values, \( Y = 34 \) (§M5 or GD 12.24.2) and \( R = 115 & H = 52 \) (eqs.5&6 or GD 12.24.4), we find:

\[ S = 24.7 \]  

(15)

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-ekatome, with Marinos’ latitudinal breadth of the known world, though Marinos is said (§N17) not to have used a fan-projection.

N15 Having thus found an \( S \) that could have led to GD 12.24.2’s \( Y = 34 \), we may simply invert the process to follow in the hypothetical 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 ekatome’s northern & southern limits (\( T & S \), resp), the inverse of the previous equation gives us what we need:  

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

(16)

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

5 That is, we do not immediately follow Ptolemy in suddenly bending all meridians inward after southward-crossing the Equator. That step eliminated (for Ptolemy: §M3) the extreme-outside points \( \mu \) & \( \nu \). But we instead (§N15) keep it simple by letting lines \( \eta \) & \( \eta \) in Fig.1 extend right straight out to \( \mu \) & \( \nu \), 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).
Thus eq.3 gives us a pretty good idea of the Lighthouse’s height $h_L$:

$$h_L = \frac{v^2}{2AR} = \frac{200^2}{(2.4 \cdot 34400)} \approx 0.48 \text{ stade} \approx 1/2 \text{ stade} \approx 90 \text{ m}$$  (4)

### D Eusebius Bequeaths Us Eratosthenes’ Exact Earth-Radius

Eusebius, Bishop of Caesarea-Palestine, is most remembered for leaving us his *Ecclesiastical History* of the Christian church at its time of triumph. We will henceforth also owe him for the long cast-aside, here vindicated clue relayed in his *Præparatio 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): Eratosthenes had the Moon 780000 stades distant; and the Sun, 4080000 stades. We formally list these two Eratosthenes distances:

$$M_E = 780000 \text{ stades}$$  (5)

$$S_E = 4080000 \text{ stades}$$  (6)

The traditional Eratosthenes Earth-circumference $C_K$ is based upon the famous §A4[a] Kleo “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 “premiss”.) So:

$$C_K = 50 \cdot 5000 \text{ stades} = 250000 \text{ stades}$$  (7)

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

$$p_{BK} = \frac{2\pi S_E}{C_K} \approx 103$$  (8)

much too surround 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, Poseidionios made the solar distance 10000 Earth-radii: §2 §F2 eq.15.) If we instead adopt the Eratosthenes circumference $C_G = 252000$ 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 $p_{BG}$:

$$p_{BG} = \frac{2\pi S_E}{C_G} \approx 102$$  (9)

but this is also unacceptably non-round.

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

$$C_N = 256000 \text{ stades}$$  (10)

[Noted also at Rawlins 1985G p.259 & Thurston 2002S p.66.] When we check this vs Eusebius’s $S_E = 4080000$ 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:

$$p_{BN} = \frac{2\pi S_E}{C_N} \approx 100.1$$  (11)

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 2-1 rectangular bound is slightly wider than necessary. But for the 2nd projection, there is no appearance that an adjustment might render the *ekumene*-fan 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).
There is an attractive alternate theory of the origin of $Y = 34$: the suggestion (§N6) that the 2-1 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 [2-1] rectangle without truncation of the corners [p&R].” (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 2-1 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-constraint or just The Split.

Having arranged that each half of Fig.1’s rectangular bound is a perfect square of size $Z$ (in 50), 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):

$$ Y = E + \frac{(R/H)\sqrt{R^2 + Z^2 - E^2}}{(R/H)^2 + 1} $$

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.)

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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#nobs, 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 means 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).
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-oily 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.

Two obvious questions now arise: [a] Why didn’t Ptolemy know the origin of $Y = 34$? and [b] We get a clue to the actual origin when we substitute other latitudes $L$ into the foregoing equation: we find that $Y$ reaches a minimum very near Rhodos — and is considerably higher near the Tropics or the Arctic. The $Y$ for Thule ($L = T = 63^\circ$) is the same as for the Equator ($L = 0^\circ$), since substituting either of these two $L$-values into the general equation (eq.8) reduces it to:

$$Y = H \cos[162000/(\pi R)] \approx 37$$

So Eratosthenes was pretending that the Sun was 12 times smaller than the Earth!\(^5\) Such cosmology doubtless delighted (and offering justified 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 & \(\S H 1\).)

\(G\) \ 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 $CN$ (eq.10) generates the radius:

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

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

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

This contrast (eq.17 vs eq.18) confirms the \(\S E 3\) 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: \(\S A 4[a]\), because that method’s math (eq.7) produces circumference.

By contrast, the Pharos Method (\(\S A 4[c]\)) 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: \(\S K 2\) & fn 7.)

\(H\) \ Inventing the “Experiment”

\(H1\) As noted at Rawlins 1982N n.10, Eratosthenes was as 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

\(^5\) Note Sun-shrinker Eratosthenes’ Scylla-Charybdis narrows: bringing the Sun near enough to make it smaller than Earth, while putting the Moon not too close to the Sun (thereby inflating eq.4’s $\gamma$) but not too close to the Earth, since that would entail huge daily lunar 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 \(\S F 3\) 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.)

\(^5\) If we eliminate the southern latitudes, we yet find $Y \approx 34$, except for the non-weighted average with rounding, where $Y \approx 33 1/3$ instead.

\(^5\) See, e.g., B&J p.38.
Letting $S =$ the south latitude of anti-Meroë, Ptolemy further defines

$$E = R + S = 115 + 16.5/12 = 131.5/12$$

(7)

This establishes all the fan’s dimensions.50

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°) or klima (14°1/2) as canonical for the mid-eukheme, he chooses (GD 1.24.3) the Rhodos parallel at latitude 36°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°: GD 1.20.5 & 24.3) to the fan’s already-determined north—south radial distances (§M4).

M7 That step is odd because, when he earlier (§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:

$$Y = H \cos \frac{16200 \cos L}{\pi(R - |L|)}$$

(8)

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

M10 But the truth swiftly reveals itself when we substitute Rhodos’ $L$ (36°) into this equation: we get $Y \approx 31$ units51 (nearly 32 without Ptolemy’s eq.5 rounding) — not 34 units. But $Y = 31$ corresponds to fan-spread 106° (not the 98° of §M1), since

$$F = \text{Fan-Spread} = 2 \arccos(Y/H) = 32400 \cos L/|\pi(R - L)|$$

(9)

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

Ck ultimately upon use by another scholar (see, e.g., §I1) 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 titularly 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, $7°12'/2$ (ibid n.20 & Table 3), the rounding of which to $7°15'/2$stades=630/50 = became the purported basis of his ultimately canonical $C_k = 250000$ stades.6

H2 Instead of walking 5000 stades or 500 nautical miles (nmi), the actual Earth-measurer walked merely (eq.4) 200 stades or 20 nmi. Eratosthenes’ “experiment” was just an indoor theoretical exercise whose $C$ was swiped from Sostratos’ prior outdoor Pharses scheme, a grab self-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 unquantified in Sostratos’ era. Had he known of (corrective) eq.3, he would have found

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

(19)

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

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

(20)

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 rough estimate of it.)

The Pharos was a pioneering, literally-superlative civic-science project. So: was its height $h$ a proud world-lighthouse-record round number of Greek feet? (Greek foot = 12°1/7 English.) We already have evidence (§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 $r = v^2/2h_L$ becomes simply:

$$r = v^2$$

(21)

So anyone could find the Earth’s radius $r$ in stades, just by pacing $v$ in stades and squaring it. The most metallic ring in Alexandria’s Square Stoa was a public-sciences equinox-detector (Alm 3.1), so could the sailor-beacon Pharos have doubled as a huge round-Earth-measurer 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 apogeon: t2 fn 33) by Pharos-builder Sostratos & fellow scientists, who thus should (§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, rootin $r_E$:

$$\sqrt{40800} = 201.99$$

(22)

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 $p-\xi$);

$\sigma-\eta$ is of length $R = 115$ (as is $\rho-\eta$);

$\zeta-\sigma$ is of length $S = 16.5/12$ (as is $\mu-\rho$);

$\zeta-\xi$ is of length $E = 131.5/12$ (as is $\mu-\eta$).

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

51 This accounts for the non-fitting & unintended aggravation that points $\xi$ & $\pi$ lie above the top ($\alpha-\beta$) 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 (§M12) that $Y = 34$ units is not for the Rhodos parallel (corresponding via eq.9 to the 106° fan-spread used by the non-fitting diagrams just cited) but was designed as an average fit (§M14) to all eukheme parallels $L$. Note that for $L = 0°$ (Equator) or 63° (Thule), fan-spread $F$ would be 90° by eq.9 ($Y = 37$ by eq.8). The average of 106° & 90° is 98°, which fits $Y = 34$ (the average of 31&37: §M13).
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) sea-level Pharos-visibility distance \( v \) was

\[ v = 202 \text{ stades} \tag{23} \]

(Not far from the crude \( \frac{1}{2} \text{C1 estimate used in eq.4.} \)

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

\[ r_E = \frac{v^2}{2h_L} = \frac{(202 \text{ stades})^2}{2 \cdot 1/2 \text{ stade}} = 40804 \text{ stades} = 40800 \text{ stades} \tag{24} \]

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 Pharsos 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 \( r \) is squared in eqs.2&3) 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 Pharsos theory, and applies also to the Sunset Method (§A4[d]), whose resulting \( C_P \) (eqs.26&28) likewise depends upon the shape of the crucial measurement. (Inverse-square 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 unempirical: 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 even mentioned it. [Engels 1985 makes it.] Amusing details of testimony-twisting (used to carry on 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, Poul built something or the other on the assumption that 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 Almagest Ptolemy was under Hipparchos’ influence, so he presumably adopted his C which was (Strabo 2.5.34) Eratosthenes’ C.C (§D3). When Ptolemy switched (§3 fn 13 & §L3) to C.P (eq.26) for his later GD, he obviously used travellers’ east-west distance-estimates more than astronomically based longitudes and thus (in order to switch

**M Ptolemy’s 1st Planar World-Map Projection From Where-in-the-World Arrived That 34-Unit Vertical Strut from Its Top (\( \epsilon \)) to Its “North Pole” (\( \eta \))? Average 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 erukeme — covering 180º of longitude from the Blest Isles (0º longitude) to easternmost China-Vietnam (180ºE. longitude) and 79ºS/12º (GD 1.10.1) of latitude from Thule (Shetlands [Mainland]) (63ºN. latitude) to anti-Meroê (16º5/12 S. latitude, a klima as far south of the Equator as Meroê is north of the Equator). It is the 1º 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) is a fan, opened slightly more than a right angle: c.98º (§N11). Thus, all north-latitude erukemas semi-circles are represented by 98º arcs. (Versus fn 51.) The fan is fairly neatly placed within a rectangle about twice (fn 55) as wide as high, as we see from Fig.1, where the four corners of the rectangle are (clockwise from upper left) points \( \alpha, \beta, \delta, \gamma \).

**M2** For the 1º Projection’s conversion of the spherical-segment erukeme to planarity, the degree-distance \( T = 63º \) from Equator to Thule is made (§M4) into \( T = 63 \) linear units; likewise for the \( S = 16º5/12 \) from Equator to anti-Meroê, etc. In Fig.1, representations of several latitude-semi-circles are depicted as Ptolemy’s source intended (fn 54):

- The Thule semi-circle (latitude 63ºN = \( \xi, \alpha - \pi \))
- The Rhodes (§M6) semi-circle (latitude 36ºN = \( \theta - \kappa - \lambda \))
- The semi-Equator (latitude 0º) = \( \rho - \sigma - \tau \)
- The anti-Meroê semi-circle (latitude 16º5/12 S) = \( \mu - \omega - \upsilon \)

(Repeating [M1]: though each arc in Fig.1 is only c.98º, it represents 180º of longitude in the Ptolemy world-projection.)

**M3** 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 erukemae of Fig.1. This kink-step enables Ptolemy to force (GD 1.24.7) the length of the anti-Meroê parallel (south of the Equator: latitude –16º5/12) to be exactly 49 as long as its northern equivalent, the Meroê parallel (latitude +16º5/12).

**M4** Ptolemy’s angular—linear duality here is effected by two rough expedients:

- [a] Defining the fan’s units by forcing the distance \( T \) from Equator to Thule circle — 63 degrees of latitude — to be 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 \( \eta \) in Fig.1) proportional to cos 63º: i.e., equal to cos 63º in units of \( R \), the fan’s radius from “N.Pole” (point \( \eta \)) to Equator. Simply put:

\[ \frac{H}{R} = \cos 63º \tag{4} \]

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

\[ R = \frac{T}{1 - \cos T} = \frac{63}{1 - \cos 63º} \approx 115.38 \ldots \approx 115 \]  

(5)

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

\[ H = R - T = 115 - 63 = 52 \]

(6)
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 1' 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:

\[ C_0 = 216000 \text{ stades} \]

(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

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

(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-zenith 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:

\[
\frac{C_N}{C_0} = \frac{256000}{216000} = 5.93/5 \quad \frac{C_N}{C_P} = \frac{216000}{180000} = 6.00/5
\]

(28) 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 ancients’ huge shift from \( C_G \) to \( C_P \) is only scantily attested: Strabo 1.3.11 & 1.4.1.

Other problems for accepting 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 nmi) 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 be 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 years after the experiment was actually done [elsewhere], successfully.) [Did an ordmag 1000-stade Nile-parallel version occur c.300 BC? See DIO 20 1 2.11] For those who cannot immediately see why the two methods yield such different results (one over 40% higher than the other!): see DIO 2.3 [§A, 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 mt-height.) That is, if Earth’s sea-level 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 how far one receded, and this infinite \( r \) 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).
L. Brief Comments & Hypotheses on Several Subjects

L1 Parts of the GD show familiarity with the Euphrates River by name. (E.g., GD 1.12.5, 5.20.1-36.) So: why does GD 5.20.6 refer to Babylon as merely being “on the river that goes through Babylonia”? This appears to be just an unconsidered quick-info-transplant from an uncredited source — and yet another (see §D3, etc) hint of patch-workery.

L2 Noting that from GD 5.13 on, the most trustworthy ms (O) bears no coordinate data. Since the dataless lands were acquired late (after 100 BC) if at all by the Roman Empire, one might wonder if this oddity reflects dependence of the GD’s data (up to that point) upon early Greco-Roman lists, maps, or globes. Perhaps of Hipparchos’ epoch.

L3 Marinos’ ekumene was overbroad: a 225°-wide known-world, 5/8 of a wrap. This was justly revised at GD 1.12-14 and a smaller and much more accurate half-wrap breadth of 180° (see fn 48 or GD 1.14.10), though B&J n.53 (p.76) rightly note the over-roundness here: Ptolemy aimed to get 180° — “by hook or by crook”. Had Marinos-Ptolemy not implicitly trusted (1 §J4; Rawlins 1985g n.14) E-W stade-measures over eclipse-measures of longitude (contra priority promo-announced at Rawlins 1985g p.264) when switching from 700 stades/degree (§L6) to 500 stades/degree, then the known-world’s breadth in degrees would have been quite close to the truth — as was Ptolemy’s breadth in distance (error merely ordmag 10% high): 90000 stades = 9000 nmi from BlestIsles-W.Europe to Java-E.China.

L4 Thus, strangely (since latitudes were much easier for the ancients to measure accurately: §D6), the Ptolemy ekumene (Fig.1) longitudinal stades-distance-across is not less trustworthy than his latitudinal stades-distance-across.

L5 We met a similar surprise earlier in finding (§D6 [2]) original longitude error-noise not worse than that in that latitude. The upshot of both findings is an important broad insight: the merits of the GD are more geographical than astrographical.

L6 Some scholars aver that an ambiguous discussion at Strabo 2.1.34-35 shows that Hipparchos knew Babylon’s true latitude, 32°1/2. But the argument is vitiated by the high sensitivity of its key triangles’ north-south sides, to slight uncertainties of ordmag 100 stades in other sides. (Contrariwise, if false: §L1, 2) unambiguously, unsensitively reports that Hipparchos placed Babylon over 2500 stades north of Pelusium (D150), which was well-known (in reality [31°01’N] & at GD 4.5.11 [31°14’] to be near the same 31° parallel as Alexandria (GD 4.5.9). (Opposite sides of the Nile Delta: Alexander-Canaan on the west, Pelusium on the east. Contiguous entries in GD 8.15: items 10&11 = D149&150, respectively.) At Hipparchos’ 700/1” scale (Strabo 2.5.34), this puts Babylon (D256) rather north of 31°1/4 + 2500 stades/(700 stades/1”) = 34°5/6-plus — i.e., at 35°N, just the grossly erroneous value we find at GD 5.20.6 and (effectively) at GD 8.20.27 (fn 16) and on all other extant ancient Greek Important-City lists.46 More germane to the present investigation: this finding leaves still-uncontradicted our proposal (Rawlins 1985g p.261) that Hipparchos was (in 10) the ultimate source of the corrupt state of the GD’s network’s key latitudes.47

46 A consideration which alone could serve to gut the entire long-orthodox Neugebauer-group fantasy (GD 4.5.9) that high or even low Greek math-astronomy was derived from Babylon. Note that the same Strabo passage shows that Eratosthenes’ 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 had transmitted, e.g., invaluable Babylonian eclipse records.

47 It has been remarked that the Strabo 2.5.34 intro to his discussion of Hipparchos’ climata appears to state that Hipparchos was computing celestial phenomena every 700 stades (i.e., every degree) north of the Equator. But since the lengthy climata data immediately following are instead almost entirely spaced at quarter-hour and half-hour intervals, DR presumes that the original (of the material Strabo was digging through) 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 Eratosthenes’ scale.
K8  GD 7.3.3 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.

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 mis-taken (at GD 1.14.6) to mean sailing with a wind blowing southward. (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 10°N, so the GD is off by c.2°, which is about as big an error as one will find caused (GD5) in this region by computing latitudes (eq.1) from 1°/4-interval klimata. Whoever originally cubby-holed Saigon so found that its L didn’t fall exactly on a klima: the nearest such klima for rounded L = 10° would in a region rounding to 1°/4 put L at 8°1/2. This, in microcosm, is the secret of why the GD’s mean latitude error is so poor: ordmag 1° (GD5), 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: 16°1/4) = Thailand Gulf’s Chanthaburi (real L: 12°7.7), more deeply inland Thinai (D355, L: 13°) = Cambodha’s Phnom Penh (real L: 11°6). Kattigara (D356, L: 8°1/2) = Saigon (real L: 10°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.

45 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 reordered (GD 1.4.2) simple, well-known data regarding the famous lunar eclipse that occurred shortly before the Battle of Arbela (D261 [modern Erbil, lately a north Iraq hot-spot]) also seen at Carthage (D131), by (www.dioi.org/cot.htm#ptxt) screwing-up Latin text of (or like) Pliny’s accurate description of that — 330/9/20 event, thereby attaching Arbela’s eclipse-time to Carthage! Despite lunar eclipse occurring in Ptolemy’s lifetime (three recorded at Alexandria in under 3rd at Almagest 4.6: 133-136 AD), 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 is in cross-disagreement with not just the real sky but with his own lunisolar-tables. See similar situations for Polaris at fn 31 and for Venus at Rawlins 2002V [B3 (p.74)]. And his solar fakes also show the same propensity 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 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 Serapic temple, he was isolated from real scientists. (As perhaps Hipparchos had also been: [B1].)

‡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-discrimination 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 Muffia Vision

A1  Today, it’s widely supposed that the astronomy of Aristarchos of Samos¹ (c.280 BC) was mostly theoretical; i.e., he is viewed within the constraints established by the flabbergasting 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 Aristarchus has “little to do with practical astronomy”⁴. The famous “Aristarchos Experiment” based its ratio of the distances of the SunXMoon upon the half-Moon’s occurring 3° onward (www.dioi.org/cot.htm#xptx) screwing-up Latin text of (or like) Pliny’s accurate description of that 330/9/20 event, thereby attaching Arbela’s eclipse-time to Carthage! Despite lunar eclipse after lunar calculation. (Without rounding: said factor will be an ordmag less.)

¹Likewise, 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.

²From 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 Symphony Op.18. It should be stated explicitly that DR shares none of the mysticism of either artist. And I note that Dionysios the Renegade (c.300 BC), for whom I suggest (DOI 1.1 fn 23) Aristarchos named the 365¹/4 Dionysios 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 Almagest 9.3 tables of those 2 planets, were originally designed for epoch Kleopatra 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.

⁴Unlike 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 Samos”? (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.66-68 n.8) claims that this is “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 nothing but arithmetically convenient parameters [§33], 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 Muffina — themselves ignore the crucial significance of a glaringly “unpublished”, the demonstrably falsity of the long-time attribution to Aristarchos-known pseudo-Aristarchos’ grossly overblown unempirical 2° solar diameter. It is not a JHA-scorned 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.3 4 §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 1986/9 remarks (12° after the Zürich meeting) at Gingerich 1996 — projecting his own bizarre Aristarchos-dementing fantasy onto Hugh Thurston, who has informed me, in further astonishment (plus DIO 6:3 §H1) at the JHA’s old habit of careless mentalism (Rawlins 1991W §B1&B2, DIO 2.1 ddg 3 §C9), that this is naturally just Gingerich’s imagination at work. Art Levine’s satire comes to life yet again in the unique JHA!4 What follows will suggest that these Neugebauer-Muffia 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 in §§A1&A2, the Neugebauer-overall-ancient-astronomy-conception’s perversity-pinnacle: rebeld/heliocentrist-pioneer Aristarchos was a non-observing fabricator, while go-along-geocentrist&data-faker Ptolemy was antiquity’s

3 Indoor-Neugebauer 1975 p.642 astonishingly claims that “one would be lucky to determine the night on which dichotomy fails.” Contrary this (§ fn 19), sharp eyes can discern lunar halfness whenever the ≈ 3° Aristarchos’ 3°, as DR & K.Picker have 1°-2°-1°°-2°°-1°° of times.

4 DR deliberately chooses the very phrase removed from the JHA by Lord Hoskin & O.Gingerich, whose political circle is dedicated to handing out AAS medals to those who got-it-wrong on Ptolemy’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.dio.org/] — but in Leid. LXVIII.) And on Thaini, it provides confirmation of GD 8 (not GD7), listing Thaini at 13°N. Which suggests that the 3°S of GD7 is either a scribal error (missing the iota for ten) or perhaps is differential: 3° south of Aspithra (16°1°N). Either way, it seems that 13°N is correct, as noted by生姜 1996 & S&G 7.234 for Thaini has GD 7.3.6’s 13° latitude.

K Landlubber Ho! Wrapped China Negates the Pacific

K1 It is well-known that the farthest-east region of the GD, China, portrays a non-existent continuous roughly-north-to-south coast (blocking 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.

K2 But, according to the previously-brouched §D1 theory, all of this geography hinges upon the underlying grid-network: GD 8 and-its kin. If we look at the GD 8.27.11-14 area, 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°23), Thaini [D355] (12°5/8, 8°), Kattigara [D356] (12°1/2, 7°3/4). Nothing wrong with GD’s China is wrong in this trio.

K3 For Thaini (D355), GD 7.3.6’s latitude (3°S) jars with GD 8.27.12’s longest 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 Leid.LXXVIII.) And on Thaini, it provides confirmation of GD 8 (not GD7), listing Thaini at 13°N. Which suggests that the 3°S of GD7 is either a scribal error (missing the iota for ten) or perhaps is differential: 3° south of Aspithra (16°1°N). Either way, it seems that 13°N is correct, as listed by生姜 1996 & S&G 7.234 for Thaini 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 — but in the former it is north latitude (which makes way more sense for a Chinese city), correctly contradicting the impossible southern latitude of both GD 7.3.3 & GD 8.27.14. The matter gets even more interesting when we check our latitudinally-correction 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 1°1 time: §B5), 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-continental 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, Thaini, 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°, Thaini (D355) 12°3/4, Kattigara (D356) 12°1/2, Zabai (D348) 12°1/4. (And, indeed, these are the Diller found in GD 8’s UNK mss-transformation.) This looks even fishier when one recalls (above) that these are the only four 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 have wish to have any chance of solving this section of the GD.

5 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 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 Noffee, GD 8 lists Aspithra at longest-day “about” 13°, which corresponds to latitude 16°0, agreeing with the GD 7.3.5 Aspithra latitude in Nobbe and Renou: 16°0 and 16°1/4 N, respectively.
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 contemporary 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 (§4) 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 K27: 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.

[the Aristarchos Experiment] addressed only the problem of the sizes and distances of the two great luminaries [Sun & Moon]. No comparable geometric methods, however inadequate by our standards, were at hand for determining the sizes and distances of the other heavenly bodies. . . . he [Aristarchos] chose convenient [DR: this astounding uncomprehending word is taken straight from Neugebauer: §A1] upper limits for cosmic distances [eq. 14 here] . . . very little astronomy was involved . . . however, [Mufa] scholars have discovered much about Hipparchus’ achievements . . . and how he improved on Aristarchus’ approach to the problem of sizes and distances.

Comments on these precious Van Helden 1985 remarks follow:

A5 There is no sign here or elsewhere (e.g. fn 70) of Mufa appreciation for the critical point (made prominent in Rawlins 1987 andassertively detailed in Rawlins 1991P) that heliocentrists such as Aristarchos obviously knew the planets’ mean distances from the Sun in AU (merely the ratio of epicycle/dentir radii for inner planets, inverse for outer planets), since the elimination of epicycles was, after all, the prime (Occamite) motivation for converting to heliocentrism! (See fn 7.) This is perhaps the most crucial achievement of concept (as against measurement: §1 fn 9) made by anyone in ancient astronomy. (See prevent heliocentrist heresy from sullying his readers’ minds, Ptolemy at Almajest 9.1 discusses the question of whether Mercury and Venus circuit points 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.)

A10 See, e.g., the bizarre attempt at Neugebauer 1975 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 Comn commonly uses roundings which are much more crude than those in the Catalog or those in Hipparchos’ declinations (Almajest 7.3). Furthermore, these apologia utterly and entertainingly conflict with those emitted by Huber (DIO 2.1.2 §J, 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 intellectual 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 too-easy spoong by getting into such pretzel-thought?

A11 Despite Ptolemy’s 1991P F1L, Gingerich 1992K p.105 nonetheless persists in stating that there was “an absence of proof” of heliocentrism even as late as the 16th century. This though Gingerich 1992K (earlier on the same page) notes that the outer planets’ motion exhibited a peculiarity 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 and Berggren & Sidoli 2007.) Perhaps realization of the contra-outdoor-sky results (§C1) of such calculations stopped pseudo-A from continuing his ms.
§2 item [c], Rawlins 1987, & Rawlins 1991p] Yet one looks in vain for mention of it in classic Muffia output, including Neugebauer 1975 & Van Helden 1985. Centrist historians have long insisted that Greek ephemerides 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 rigidity — 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 understand 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 Aristarchian 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 Muffia 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 modern liberals & cardigans who try to cast those poor souls as idiots when we have indication that both the GD’s data-sections (GD 2.7 and GD 8), previously added to date Marinus to c.110, actually contain material from the 130s or later.59

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 Poltemy’s distance (ordmag 10 AU), vs. Aristarchos’ ordmag 10000 AU. (See §E5. Actual distance of Proxima Centauri = 270000 AU.) In brief, Muffia13 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 n.53 (p.76) note an even more revealing careless retention.56 Marinro’s Aroma latitude. So, what should be tested isn’t whether all but whether any post-Trajan geography appears in the GD.

I5 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-merenary.) 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-rape1 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 Poltemy’s geographical work). These points recommend some caution before we draw conclusions on Marinos’ date from lack of the-very-latest Parthian information.

I6 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 “Ierosolomma [Jerusalem], which is called Ailia Capitolias”. And GD 8.20.18 lists “Ailia Capitolias Ierosoloma” without further comment but obviously reflecting the same up-to-date information. Therefore, the (far briefer) natural-selection fight as one of the focal points of the rise of science (B.Rawlins wonders if selling putative astronomy” as Aristarchos’ Experiment: eq. 4.

A8 Over 4 centuries of botheration, Parthia repelled three Roman invasions: [1] swallowing the army (Actually, the discovery of this revealing coincidence goes back at least to Delambre 1817 2:207. (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 “Ierosolomma [Jerusalem], which is called Ailia Capitolias”. And GD 8.20.18 lists “Ailia Capitolias Ierosoloma” without further comment but obviously reflecting the same up-to-date information. Therefore, the (far briefer) natural-selection fight as one of the focal points of the rise of science (B.Rawlins wonders if selling putative astronomy” as Aristarchos’ Experiment: eq. 4.

A9 The Hartner-RN citation sequence might be accidental. What is certainly not accidental is the total omission, from the Van Helden 1985 discussion of Eratosthenes, of 2 prominently published DR discoveries regarding ancient’s work. (DR’s name does not foul a single page of Van Helden 1985 Standard for Muffia archons’ output.) Van Helden 1985 p.5: “Since we do not know the precise length of the stade [Eratosthenes] used, it is fruitless to speculate on the ‘accuracy’ of his result. Suffice it to say that beginning with Eratosthenes the size of the Earth was known to the right order of magnitude.” Suffice it also to say that Van Helden 1985’s discussion is dense with misunderstandings. I regard the failure to cite here either Rawlins 1982G or Rawlins 1982N as a conscious, Muffia-kissing misleading of the reader, by suppression of evidence against the Muffia view propounded. I.e., the usual.13

13[Recently, O Gingerich has been trying to cope with this point. Without citation of DIO. Again.]

14 Van Helden 1985 p.19 appears to credit Hartner with the discovery that Poltemy’s 19-to-1 Sun-Moon distance ratio was taken from Aristarchos, by quoting Hartner 1980 p.26 before quoting R.Newton 1977 p.199 (see also p.173 and R.Newton 1973-4 pp.382 & 384) with the same result. (Actually, the discovery of this revealing coincidence goes back at least to Delambre 1817 2:207. As suggested here at §F5: the coincidence may mean nothing more than that the resulting RS was the lowest value then current among competent [read: heliocentrist] scientists, which made it current enough even with geocentrists that it survived. It is also a fun coincidence that the Aristarchian ratio 19 to 1 [eq.9] helps set up a neat fit for Poltemy’s geocentric nested-sphere scheme. Regardless, the implied solar parallax still survived in Tycho’s work — at the dawn of modern astronomy. Given that Tycho openly branded Poltemy a plagiarist [DIO 1.2 fn 154]: which of the 2 men [Aristarchos & Poltemy] is more likely to have been the one Tycho trusted, when Tycho adopted this [inaccurate ratio]? The Hartner-RN citation sequence might be accidental. What is certainly not accidental is the total omission, from the Van Helden 1985 discussion of Eratosthenes, of 2 prominently published DR discoveries regarding ancient’s work. (DR’s name does not foul a single page of Van Helden 1985 Standard for Muffia archons’ output.) Van Helden 1985 p.5: “Since we do not know the precise length of the stade [Eratosthenes] used, it is fruitless to speculate on the ‘accuracy’ of his result. Suffice it to say that beginning with Eratosthenes the size of the Earth was known to the right order of magnitude.” Suffice it also to say that Van Helden 1985’s discussion is dense with misunderstandings. I regard the failure to cite here either Rawlins 1982G or Rawlins 1982N as a conscious, Muffia-kissing misleading of the reader, by suppression of evidence against the Muffia view propounded. I.e., the usual.13

15E.g., Swerdlow (fn 70), Neugebauer (§A1), & Van Helden faithfully following (fn 70 & §A4).
I Marinos Mis-Dated?

11 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.32 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-1990s, would you speak of him so? (GD 1.17.1 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’ latest 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 “looks into account names of sites reflecting the changing of the names of 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 could such a mix of innocence and prejudice (e.g., fn 14) adorn a standard (gov’t funded) history-of-astronomy survey volume, written by historian (& sometime Adv Editor) A.Van Helden? The answer is found in the ancient astronomy archives he depended upon. Van Helden 1985 p.xvii (see also p.168 n.2): “In the course of this project I incurred many debts. . . . A Research Fellowship from the [NEH] . . . . For the material used in this paper [epigraphs] of this story I have relied heavily on the researches of [Neugebauer capos] 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 μ = 1/10000 of a radian

\[ (1) \]

then (for null visible stellar parallax), shouldn’t his distance r 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.

\[ [1] \]

17The cause of this imposition (and presumably of the who-cares-who-was-right-or-brave-or-ethical-or-original idée-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 superstitious 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 the defensive of Ptolemy has lately been less defensive and more rigorous. See esp. Alex Jones’ analyses.)

18 For the terminator to deviate more than 1/10000 of a radian from straightness, the line connecting the Moon’s horns must deviate 1/5000 of a radian from the middle of the terminator (§C4). The arc of the ratio of this to Aristarchos’ lunar semi-diameter (1/14: eq. 3) equals 2°38’ ≈ 3°. (Rawlins 1991W §R9’s analyses used 0.4 instead of 1/10000 of a radian, yielding 2°57’ by the same equation.)

Note that DR has not arbitrarily conjured-up μ = 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 arc of 0.45° is 3°26’ ≈ 3°.) I found by experiment long ago that the eye’s primitive visual limit is about 1/3. (The arc of this divided by 1/4 is 2°33’ ≈ 3°.) Aristarchos presumably performed just such an experiment to arrive at his value for μ. These estimates agree closely with Dawes’ limit (consistent with diaphrastic Airy disk) for a human eye’s pupil-size, and all flutter around μ = 1/10000 of a radian, the value underlying (§B2) all Aristarchan celestial scales. [Note added 2010: Was 87° computed from a null experiment? See www.dioi.org/cot.htm#nxhm.]

16 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 §SR9-R11), also exists here (Archimedes p.1975 p.646). Realizing that the full stellar parallax baseline was really 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 preference of whole or arbitrary numbers — 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 precession. Namely, the 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 Hipparchos or a little over 1/3 of an arcmin. (And this is about right for raw human vision: see fn 17.)

NB: It is attested 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 nonMuffia scholars who try.) Such work is more apt to enliven an encyclopedia-bibliographer, than to think among scholars. (Few Muffia 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 Muffia miscalculation. 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 Muffios.)

C Moon & Historians in Retrograde

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.

Since a hallmark of the Neugebauer sales-cult is its consistent confusion of superstitious ravings (e.g., §§A3&A7) with genuine science, one can readily understand how this clique got into the habit of dismissing the very idea of attempting to relate real science to ancient texts. See, e.g., Gingerich 1976’s hyperagnosto-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 archdotal naivete to the realities of SF9 and §1.) This astonishing bit of mis-megahistory (definitively vaporized at §1 §§E2&K4 and fn 9) was dished up to excuse Ptolemy’s Almajest 5.14 analysis, a fudgepot so incredible that even genial centrist W.Hartner calls it a “fair-tale” (Hartner 1980 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 slighting or ignoring this honest labor, instead preferring astrologers’ lazy false-observations & other plagiarisms, maybe ripoffs of the shade’s own original genuine work. 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 slighting or ignoring this honest labor, instead preferring astrologers’ lazy false-observations & other plagiarisms, maybe ripoffs of the shade’s own original genuine work. Just the sort of appreciation scientists ought out for. (See fn 67 & Rawlins 1993D §B3.)

One 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 nonMuffiosi not to emulate such arrogance and to instead appreciate that even ill-amannered bigots can make genuine contributions, which should be treated strictly on their merits.

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.

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), since he is the earliest ancient cited to two different year-lengths. Aristarchos flourished c.280 BC; 1 1/2 centuries before Hipparchos, who is generally regarded as precession’s discoverer. Both of Aristarchos’ year-lengths are provided at Rawlins 1999 §B7 [p.33]; see also Rawlins 2002A fn 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°25’. Hipparchos’ long-precession-obsolete NPD (North Polar Distance = declination’s compliment) for α UMi. (i.e., modern “Polaris”: the brightest star in UMi, and the most northern ever-visible UMi star for us; the most southern for Hipparchos.) And α UMi’s NPD actually was 12°27’ (Decl = 77°33’) at Hipparchos’ chosen epoch, −126.278 (128 BC Sept.24 Rhodes Apparent Noon: Rawlins 1991H eq.28 [p.58]). Marinos further states that this parallel is 1° north of Okelis, which he misplaces (§C1) at 11°25/2 N latitude. (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 statements 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 judgment-degeneration that has cursed modern history of ancient astronomy. See, e.g., Gingerich 1976’s hyperagnostic-alibi-quotes defending Ptolemy (taken from Neugebauer 1975 pp.107-108), e.g., “Greatest Astronomer of Antiquity” [§2 §G2] have known this? — especially since he pretended he’d cataloged the whole sky’s stars: “Greatest Astronomer of Antiquity” [§2 §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, ironically) of being the outrider-star whose NPD determines 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°1/2]; so, creditably, the GD’s Okelis latitude was closer to reality than to Marinos. Note also that 12° is almost exactly the theoretical

27Note: 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 predictability of astronomy. (See §2 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 Similar cases at fn 45.) Thus, γ UMi had long since assumed the distinction (one interjected by Marinos, ironically) of being the outrider-star whose NPD determines 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°1/2]; so, creditably, the GD’s Okelis latitude was closer to reality than to Marinos. Note also that 12° is almost exactly the theoretical
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 patently 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: *Almagest* 1.12), the inaccuracy of the latitudes in GD 2-7 show that these data had been corrupted by subjection to crude rounding (§D5) for astrollogers’ longest-day tables in *hours*, before being computationally converted into the latitude-degree data that ended up in GD 2-7.

[b] All astronomically-based latitudes in GD 2-7 were originally in *hours*,[25] as noted in GD 1.4. This, because based upon comparisons of lunar-eclipse local-times,[26] we thus 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 longest­days (§D5) and Earth­scale shifting (§L3), these hour-data became the semi­competent­occultist conduit (§D1) for data-corruption which tragically destroyed a sophisticated civilization’s laboriously accumulated high-quality astronomically-based ancient geographical data.

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has been universally accepted[23] as genuinely his. Rawlins 1991P (fn 6) and Rawlins 1991W (§R10 & fn 220) have challenged this incredible myth by exposing several internal problems of the pseudo-Aristarchos’ theory. Perhaps pseudo-A’s hazy perception of Aristarchos’ astronomy is related to his resented corpus’ near­extinction by the geocentrist establishment of his day. (See below: fn 69.) If we take “Sizes” as truly being Aristarchos’, we must accept that one of the most eminent astronomers in history believed all of the following five nonsense­propositions (Heath 1913 pp.329f & 352f; Neugebauer 1975 pp.635f):

[a] The Sun & Moon are 1/15th of a zodiacal sign or 2 width in angular diameter (nearly 4 times the correct value), thus pseudo-A’s semi-diameter was:

\[ \theta_R = 1/15 \]

(2)

obviously false & explicitly contradicted by Archimedes, who reported[24] that Aristarchos’ solar diameter was indeed the very accurate value 1°/2 (vs actually 32’), thus semi-diameter

\[ \theta_A = 1/\sqrt{4} \]

(3)

Rawlins 1991P fn 6 eliminated the contradiction by proposing that the factor­of­4 error was based on misreading the Greek word μῆκος (“part”) as a zodiacal sign (30°) rather than the Greek-measure unit called “part” (7°1/2: Neugebauer 1975 pp.652 & 671).

[b] Lunar eclipses can last half a day (vs 4° in reality; §C8.)

[c] Mean lunar parallax is c.3’. (Actually under 1’.) So an equatorial observer would see the Moon move (nearly) its own diameter from rising to setting, a hint of [e] to come.

[d] The Sun’s parallax is 9’ (60 times the truth), which would cause a parallax for Venus (near inferior conjunction) of over 1°/2.

[e] In Mediterranean climes (or nearer the Equator), the upper­culmination Moon MUST DAILY BE OBSERVED MOVING IN RETROGRADE[25] against the background of the stars. (Already noted at §1 fn 3&5.) Though this is an inevitable consequence of pseudo­Aristarchos’ work, it has not been noticed by commentaries, from Eratosthenes (c.230 BC) & Pappos (c.320 AD) through Neugebauer 1975, Van Helden 1985, & Evans 1998. (Note the precision of the irony here in the context of ON’s arrogant attack upon P.Duhem at Neugebauer 1957 p.206, emph added: “Duhem . . . has given a description of Ptolemy’s lunar theory according to which the moon would become retrograde each month . . . . flagrant nonsense . . . . Duhem’s total ignorance of Ptolemy’s lunar theory is a good example of the rapid decline of the history of science.”)[26]

C2 However, to give credit where it’s due: the National Geographic Society has gone so far as to publish photographic proof of moonrise in the west[27] (“Our World’s Heritage NGS 1987 pp.238­239, adorning an article by longtime Librarian of Congress Daniel Boorstin). But the photo is so ineptly faked that it provides unconvincing (not to mention irrelevant: fn 30) support for pseudo­Aristarchos’ implicitly revolutionary lunar theory.

23The failure of prior historians, to face the outlandish absurdities of the pseudo­Aristarchos 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 1913 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 Evans 1992 p.68.

24“Sand-Reckoner” p.223. With respect to the strange controversy (Rawlins 1991W fn 53) as to whether Aristarchos (also Timocharos & Aristyllos) 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°2/3 or 32°/3 (suns remaining: Rawlins 2002A eq.6).

25[Note added 2011. Archimedes’ (p.224) sunwidth limits, rt.angle fractions 1/200&1/164: 1°/2±1/200,] The “Upcoming” lists (inside-cover) of DIO 2.2 & DIO 2.3 published warnings of this bomb well over a decade ago (1992): “Hist.sci 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?

26See 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.

27In this handsome photo, the Moon is seen in its rising aspect (obvious to an outdoor astronomer) low behind the Sphinx, which looks at the camera. But the Sphinx faces eastward.
C3 Let us see how the deliciously zany retrograding consequence (§C1[e]) comes about. Pseudo-Aristarchos’ implicit\(^23\) mean lunar distance is (eq.5) \(r_{M} = 20^\circ.10\) (where \(1^\circ = 1\) Earth-radius). But it is well-known that the Moon’s sidereal period is \& was 27\(^{2}/3\) 32 (mean sidereal motion \(0^3.549/hr\)) 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\(^23\) than the Moon, which will thus appear to be moving in reverse at about 0\(^{\circ}\).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 §C1 suggested\(^31\) that the famous Aristarchos value

\[
\gamma_{A} = 3^\circ = \arcsin(\frac{r_{M}}{r_{S}}) = \arcsin(1/10) \quad (4)
\]

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

\(^{23}\)Heath 1913 p.339 \& Neugebauer 1975 p.637 perform the same method, understandably with less precision.

\(^{24}\)The pseudo-Aristarchos Moon, at mean geocentric distance 20\(^{\circ}\).10, will travel 20.1 times farther per Earth-circle — than will an observer on the terrestrial Equator. 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\(^{\circ}\).2 times farther 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) 2.12 times the mean geocentric sidereal lunar motion (\(0^3.549\)) or: −0\(^{\circ}\).2. (Obliquity’s cos = 92\%, ignorable for rough mean-situations: \(\gamma\) when the Moon is on the Celestial Equator, its motion is not parallel to the terrestrial observer’s equatorial motion; \(\gamma\) 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 §C2 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 angular speed when the actual Equator (not topocentric-theoretical) Moon is high. I.e., there is a retrograde tendency, due to the Earth’s spin; but in reality this supereposed parallactic motion’s speed is — due to the Moon being about 60\(^{\circ}\) (not 20\(^{\circ}\)) away from the Earth’s center — not fast enough to overcome the Moon’s sidereal motion. For the real equatorial equatorial Moon at mean distance \& mean sidereal speed, the equatorial observer will be traveling only 27.4/60.27 times the Moon’s sidereal speed, so the Moon’s mean geocentric \(0^3.56/hr\) speed is slowed to a relative angular speed of about 0\(^{\circ}\).3/hr. (When the Moon is near the equatorial nadir, this relative speed would be seen — if it were visible — to be 0\(^{\circ}\).8/hr. Over time, the speed must of course average out to the mean lunar geocentric sidereal speed: 0\(^{\circ}\).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 (§C1) of the Moon’s distance \(r_{M}\). Yet another reason for the incredible 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\(^{\circ}\)) 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 \(\gamma_{A} = 3^\circ\) 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 §A1.

\(^{32}\)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 seems 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 §C5] \& or 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

\(^{31}\)Thanks to Alex Jones for reminding DR of this.

\(^{32}\)E.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 5th of the 6 islands listed at GD 6.4.34 is named “Anarcr Neso’s.”
D6 Suggested Solution to Two Mysteries As shown in the tables of Rawlins 1985G p.262, GD latitude-errors for major cities are often sph-trigonometrically consistent with the §D1 theory. See eq.1 or Rawlins 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 GD8'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 local maps 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.28 Which argues against GD 8 being computed directly from27 GD 2-7 or vice-versa.

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

E4 DR has noted something similar: throughout GD 1, there is no mention of Alexandria.29 Ptolemy's claimed home and his Alm's prime meridian. By contrast, GD 1 mentions such sites as: Thule (D1), Ravenna (D56), Lilybaeum (D67), Carthage (D131), Rhodos (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 using Pseudo-A's false data (§C8 & eq.2): shadow-Moon ratio v_P = 2 and solar semi-diameter d_S = 3.

F Aristarchos: Ancient Vision

F1 We have seen earlier from Eusebius (§1 eq.14) 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., §1 fn 4] cannot be traced back beyond Eratosthenes.)31 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 pediatric geometric proofs. By contrast, an exact computation (e.g., Neugebauer 1975 p.637) finds 20 Earth-radii instead of 19:

\[ r_M = \frac{1 + \sin \gamma_A}{(1 + v_P) \sin \theta_P} = 20 \text{rd}.10 \]  

(5)

40 Ptolemy's GEOGRAPHY 2008 March DIO 14 §3

41 Rawlins 1982G p.263 fn 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 lasting a local tradition (§C1) of geographical tables, which Marinos (note GD 1.17.2's semi-connexion 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 felt forced to go along (in the Almabest) with Hipparchhos' flawed but long-pagan-sacred eccentric-model solar tables.


21 See §G1. For the consistent sites, either there were calculations of one section's data from the other (in one or both directions) or scrupulous attention was paid (fn 25) to math-consistency between the two sections (whether at the outset or during later editors' touchings-up) — though there are occasional inconsistencies, e.g., the longitude of Rome (D49): GD 3.1.61 puts Rome 36° 23/23 west of the Fortunate Isles, while GD 8.8.5 puts Rome 1°5/8 east of Alexandria. (Iself 60° 1/2 east of Blest Isles by GD 4.5.9, or 4° [60°] by GD 8.15.10. See Rawlins 1985G n.25.) But (60° 1/2 - 36° 23/23)\(15/8\) hour) \(\approx\) 1°7/12 < 1°5/8. Similar incompatibility: Salinae (GD 3.8.7, 8.11.4, D79). See also §K3.

31 Has it been previously noted that Aristarchos' near-contemporary Archimedes (probably a few years older or younger than Eratosthenes) reports none of the follies of pseudo-Aristarchos? (Which perhaps sandwiches the time of pseudo-A's origin into the 2nd half of the 3rd 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 \(\approx\). Note also the clash between Archimedes-Aristarchos (eq.15) and pseudo-Aristarchos (Heath 1913 pp.339 & 350) on \(r_S\): 10000° vs 360°, respectively. Were Aristarchos' works more welcome in Archimedes' Syracuse than in Eratosthenes' Alexandria (by then of less-Greek rulership, and fisically strained from funding wars, e.g., Pyrrhos 1st. See §F3. (What Alexandria instrumental star data survive from the 100th after Aristylos, 260 BCE?)
C6 In addition to the flock of pseudo-A difficulties cited above (§C1 & fn 32), Rawlins 1991W §R10 also revealed a hitherto-unnamed internal contradiction in the pseudo-A work: the explicit (and false) statement that 1/3960 of a rt angle is too small to be visually discerned (Heath 1913 p.370, Neugebauer 1975 p.640). However, 1/3960 of a rt angle is 4 times bigger than 1/10000 of a radian. So, this pseudo-A statement wipes out the entire visual basis (fn 17) of Aristarchos’ Experiment!

C7 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 astronomic identity of pseudo-A.

C8 Having thus already (§C1[a]: “μετοπος”) cleared up pseudo-Aristarchos’s most obvious absurdity (eq.2: 1°Junilor semi-diam $p_0$), we check another highly suspect pseudo-A statement, namely, that, at the Moon’s distance, the pseudo-Aristarchos ratio $v_2$ 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 $v$ would (eq.10) cause central eclipses’ Entirety (Partiality + Totality) to be 3 times longer than Totality. Letting $\rho$ stand for the Entirety/Totality ratio, we have pseudo-A’s $p_\rho = 3$ (eq.10). But it is well known that an eclipse’s maximum possible Entirety is instead just under $4^1$, while maximum possible Totality is slightly more than $1^3/4$ — that is, roughly $2^0$ — creating an $Em/Tot$ ratio $\rho$ of barely 2 (far short of $Em/Tot = 3$). For the mean distance situation, the actual shadow/Moon ratio $v$ is 2.7 (corresponding to $Em/Tot$ ratio $\rho = 2.16$; fn 35). And we know that Hipparcos used $v = 2.5$ (Almajest 4.9), while Ptolemy used $v = 2.6$ (Almajest 5.14). So how could an observing astronomer set $v = 2.7$? The basis for estimating $v$ is eclipse records. (And Aristarchos may have researched and drawn wisdom from such records more than any other Greek of his day: DIO 111.1 fever.)

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 $\rho$ of the time of an Entire unbral eclipse to time of Totality (for central eclipses), which is (crudely) $4^2/2^2 = 2$, a figure that reveals (via eq.7) $v$ to be much nearer 3. Even aside from Aristarchos’ access to centuries of Babylonian eclipse records, he could have observed first-hand the 21-digit eclipse of -2865/5/20 ($\rho = 2.15$); and-or the 19-digit eclipse of -2796/6/30 ($\rho = 2.14$), which occurred just a few days after his famous S Solicite 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 $\rho$ (something about in-shadow, wasn’t it 1.7?) to be $v$. (We already know from §§A1&1C1 how easily confused pseudo-A was.) Keep in mind: the Entire/Totality ratio $\rho$ 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 $\rho$:

$$v = \frac{\rho + 1}{\rho - 1} \quad \quad \rho = \frac{v + 1}{v - 1} \tag{7}$$

C9 Eq. 7 is a special case (where constant $a = 1$) of what I’ll call the “Reversible Fractional Function” (RFF):

$$y = R(x) = (x + a)/(x - 1) \tag{8}$$

34 It is possible that pseudo-A was an uninformed hyperdant (as Neugebauer 1975 p.643 speaks of Aristarchos, believing him to be the author of “Sizes”) — as politically powerful as he was incompetent. Poseidonios is also connected (Neugebauer 1975 pp.654) to $v = 2$, perhaps while assuming cylindrical shadow (which ON naively relates to null parallax). Did $v = 2$ evolve from such mis-geometry? Alternate route: if a key pseudo-A slip misconstrued $r_0/r_0$ $= 19$ (eq.4) as $r_0/R_0 = 10$ (eq.6), then eq.6 could have produced $v = 2$. (Less likely: eq.10 and $FM = 19^\circ$ [into eq.6] caused $dp = 1^\circ$.)

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&J p.29 n.37 rightly 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?) later). Not to mention Babylon (D256) by 2° 1/2 (fn 10; Rawlins 1985G n.13) — a discrepancy which is difficult to reconcile with a modern historian-cult’s non-empirical insistence (fn 46) that Greece had high-astrology debts to Babylon. DR suspects (§A4) that the latitudinal shortcoming of the GD’s hypothesized are mostly from astrolger Hipparchos (not Marinos or Ptolemy): see at GD 1.4.2 (& 8.1.11) on Hipparchos’ listing-clumping of cities of differing latitudes under the same klimata (§A3), for astrolgers’ convenient entry (§A4) into common longest-day-based tables of houses. This degenerative step typified the fatefully laxity (www.dioi.org/cot.htm#twvr) which DR’s §D1 theory proposes was the prime source of latitude-accuracy’s corruption in GD 2-7.

D5 Rounding klimate 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-days tables often rounded $M$ to the nearest 1/4. (See, e.g., Almajest 2.6, Neugebauer 1975 pp.728f.) But when using eq.1 in the Mediterranean region, a longest-day error of merely 3 timemin would cause an error of nearly a full degree. And ordmag 1° is the actual (terrribly 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 klimata) one of the best arguments for the Rawlins 85G theory that underlies GD 8 (GD 8’s or its type) were the basis (§G3) for the key-city latitudes of GD 2-7. Note the historically vital (if paradigmist-verboten) 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.
there are plenty of hints (e.g., Memphis’ 13°57′: fn 7) that the majority of GD 8’s non-major cities may have been directly computed (via eq.1) from the data of the sort found in GD 2-7. (Note strong evidence that neither section was directly computed from the other: §E2.) E.g., the greater precision of GD 2-7 data is obviously often impossible (fn 26) to derive by computing from GD 8 — while the reverse is frequently possible (see §D5 for cause). Further, late copies of Ptolemy’s Handy Tables (a work probably earlier than the GD) contain a list of c.360 Important Cities’ (364 in Halma’s ed.) latitudes and longitudes in degrees, very similar (though not identical) in selection, bulk, and sequence to GD 8. It may be that Ptolemy simply computed the non-key sites of GD 8 from something like this list, as a handiest-possible (§A4) add-on to crown his GD.

D2 However GD 8 was accomplished, it was an astrologer’s-dream Handy Tables (§A4 [2]), the only example of its type that survived from classical antiquity:

[1] All latitudes expressed in longest-day, for (§A4 & eq.2) easy entry into tables of houses.

D3 B&J p.29 notes (as did Rawlins 1985G pp.261f) specific cases where key cities’ latitudes must have been computed14 from longest-day. Regarding the preface to GD 8:

[a] The preface’s comments on map-distortions belong with parallel material back in GD 1.
[b] One of the most obvious arguments against GD 8’s data being for (non-warped) maps is that longest-day data are not linearly related (§A4) to latitude. (Note shrinking of klimata-bands with recession from the Equator at, e.g., S&G 2:748-751.)
[c] The GD’s regional maps have come down to us. Granted, they are not originals; nonetheless, their fidelity to the GD’s regional dividers strongly suggest that these are the originals in essentials. Though the maps’ margins bear longest-days marks (inevitably at large latitude intervals), the densely-marked, nominal south-north co-ordinate (linearly related to up-down distance on each map) is latitude in degrees. Which is necessary because these maps depict the locations of thousands of cities (not the hundreds of GD 8), the great majority of whose positions are not given at all in GD 8, while all their longitudes and latitudes are in GD 2-7. More indicative yet, the maps measure longitude not in GD 8’s hours east or west of Alexandria, but in GD 2-7’s degrees east of the Blest Isles. (See the beautiful reproductions of several such maps between pp.128&129 of B&J.)

So: why would GD 8’s preface be discussing the construction of regional maps actually based upon the data of GD 2-7? Is this more residual evidence (see further yet at fn 17) of patch-work authorship? What evidence connects Marinus to the construction of GD 8? The absence15 of his native coastal Phoenicia from GD 8 proves his non-authorship of it.

D4 Tyre’s absence from GD 8 only adds to the evidence (§E6 & §G1) that GD 8 is not directly connected to Marinus-of-Tyre’s Books 2-7. So it would be wrong to over-claim that GD 8 is the father of GD 2-7. Uncle or cousin might be nearer the mark: §E1 §G2. For, longest-day data (the stuff of GD 8) are obviously the basis of the full work’s flawed shift. Thus (§1 §2), there is not only no case-for but no longer even any need-for the literature’s ever-reappearing attempts (see, e.g., Rawlins 1996C §C14 & fn 47 [p.11]) to claim that Eratosthenes got the-right-answer for the Earth’s circumference but expressed it using an undersized stade.

14 A semi-ambiguity: Almajest 2.13 predicts the upcoming GD and refers to degrees vs the Equator for latitudes (like GD 2-7) but speaks of placing sites by degrees (the measure of Books 2-7) while using (fn 43 [1]) the Alexandria (D149) meridian of Book 8 (and of E.Mediterranean astronomers & astroglogers); so it conclusively favors neither side on the relation between the GD’s two data-sections.

15In Nobbe’s edition, at GD 8.20.18 (Jerusalem D247) the spelling of “east” changes from αντανακλασ[ανατρανκ]ατος to ενα (en) for most of the rest of GD 8. If the switch (which occurs only in some mss) is meaningful, it is possible that it is connected: [a] to the compiler’s departure (at about this point) from a map of the Roman Empire to an extra-empire map of different format (and less reliability), and this perhaps led [b] to the accidental omission of coastal Phoenicia, possibly due the two maps’ different order of site-listing around the nearby seam. More patch-workery?

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 $x = R(y)$.

C10 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 $\rho$ just35 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 $\rho = 4/\pi$26 ratio noted in §C8 as too plain to miss, that is: $\rho_A = 2$. 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:

$$v_0 = 2 \quad \rho_p = 3 \quad (10)$$

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

[a] misunderstood a reference to $\rho$ (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 others equals 2. Note also cylindrical-shadow confusion at fn 34.) Let us now explore the consequences of this simple (though speculative) hypothesis.

C11 We substitute eqs. 3 & 9 into the usual eclipse diagram equation36 (e.g., eq.5) and thus obtain:

$$r_M = \frac{1 + \sin \gamma_A}{(1 + v_A) \sin \theta_A} \approx 60^\circ \text{ or } 51^\circ$$

for $\gamma_A = 3^\circ$ (eq.4) or $\gamma_A = 0^\circ$ (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 $r_M \pm 60^\circ$ 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 $\rho$ 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 was so ignorant as to mistake $r_M$ by a factor of roughly 3 (20°: §C3 & eq.5) — or even a factor as large as 4/3 (80°: Rawlins 1991W eq.31) — is difficult to countenance, since these blunders would require almost impossibly large errors in $\rho$ and (especially) $\theta$.

D Solar System Scale

D1 We next find what the foregoing implies for solar distance $r_S$. From eqs. 4 & 11:

$$r_S = \frac{r_M}{\sin \gamma_A} \approx 60^\circ / \sin 3^\circ = 1146^\circ \approx 1000^\circ$$

35 In reality, mean $\rho \geq 2.1/6$, as one will find from a glance through an eclipse canon or by substituting $v = 2.7$ (§C8) into eq. 7.

36Almajest 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 \( \gamma \) justifies rounding\textsuperscript{37} \( 1146' \) to \( 1000' \).) Such a step could have triggered the later tradition — discovered at Hipparcian eqns.23&24 of Rawlins 1991W — of dividing\textsuperscript{38} the \( \text{AU} \) into units of thousands: \( 1 \text{AU} = 1000' \).\textsuperscript{39}

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

D3  Previous attempts to deduce Aristarchos’ \( r_S \) (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 value was based upon only 1 of the 2 emendations to \( \phi_0 \) by Al-Battani (fn 12 above) directly attested. Thus, given Al-Battani’s use (§D2) of 1146’ (eq. 12), we may conclude that: [a] the \( 1146' \) is the preferred choice (of those discussed here) for Aristarchos’ early \( r_S \) (see also fn 37), thus [b] our 2 emendations (eqs. 3&9) are not disconfirmed.

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

E1  The irony is that Aristarchos’ famous Experiment was far inferior\textsuperscript{40} 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

\[ \text{GD} = \frac{\text{Earth-circumference}}{\text{Sun-earth separation}} \]

and effectively parallax-free latitude-determination method which may (§B1) go back to the time of the Great Pyramid.\textsuperscript{10} Was the purpose of Marinos’ geography naval? Or natal?\textsuperscript{11}

D  Astrolgers’ Handiest Tables, InterRelations, Accuracy Degraded

D1  \( \text{GD} 8.2.1 \) 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 \( \delta \) 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-ruled longest-day \( \delta \)-data (§A4) of just the sort\textsuperscript{37} we see in Book 8. Flagrant examples appear below, e.g., Babylon (fn 16) & SE Asia (§K7). The suggestion here is that distortions in \( \delta \) values go back at least to Hipparchos, while the distortions in longitude probably occurred later than Hipparchos, since they involve a shift (fn 25; §J4) from the Hipparcos 252000-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 Taitsbak 1974 eruditely wonders if this switch wasn’t much later.) The \textit{Almajest} was still using the larger Earth-size during the mid-2nd century AD, and the earliest rock-certain attestation of the smaller value’s use is by Marinos, around the same time.\textsuperscript{13} (Columbus’ belief, that the shortest trip to China’s Kattigara \( [D356] \) was the same time.

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naked-eye-visible stellar parallax showed that the stars were at vastly greater distances than geocentrics had realized.

E2 How much greater? Well, according to Archimedes (d. 212 BC), the previous (still then-current) definition of "universe" was such that its radius was 1 AU. Aristarchos realized that, since the Earth (not the Sun) was moving in a circle of this radius, then: the invisibility of stellar parallax demanded that $r_s$, the closest stars' rough mean distance (in AU, where $r_s \equiv 1$ AU), be as great or greater than the inverse of the limit of human vision (in radians). From "Aristarchos' Experiment", we have already shown independently ($B_1$) that he used $1/10000$ of a radian for that limit. Thus, from eq. 1, he would have set

$$r_s < r_s / \mu = 10000 - r_s = 10000 \text{ AU}$$

(13)

So it is gratifying to find this result is actually tested to ($B_1$) as a limiting distance by Archimedes' "Sand-Reckoner".34 But such a scale, though ($E_1$) much more important than the famous "Aristarchos Experiment", is far less known today. Exceptions are Heath 1913 (p.348) & Neugebauer 1975 (pp.646&656). But, following the usual misconception that Greeks were non-empirical, neither author considers the possibility suggested here (eqs. 1&13), namely, that this figure was founded upon systematic scientific observations.

E3 Yet it is not difficult to reconstruct the empirical basis. Aëtios (a late source) appears to indicate that Aristarchos regarded the stars as suns,42 saying (Heath 1913 p.305) that he "sets the sun among the fixed stars and holds that the earth moves around the [ecliptic]". Aristarchos would probably regard stars’ distances as being as randomly varied as their brightnesses.

E4 Thus, the simplest experiment for measuring stellar parallax would be that which was later vainly attempted by W.Herschel (during the project which led him instead to his historic accidental backyard 1781/3/13 discovery of Uranus): look for annual oscillation in the relative positions of false double stars (i.e., two stars which happen quite by chance to be so situated that a line through them passes very nearly through the Solar System), where one of the stars is much nearer the Sun than the other. Some good examples: Giedi, Mizar-Alcor, and Shaula-Lesath. Giedi (the east horn of the SeaGoat, Capricorn) is probably the best example. In the time of Hipparchos, the separation between the Giedi pair ($\alpha^1 + \alpha^2$ Cap, respectively) was merely 5 arcmin: $3^\circ.7$ in longitude, $3^\circ.3$ in latitude.43 The searched-for parallactic motion would be almost entirely in longitude. Yet it is certain44 that no such relative motion was ever observed. An ancient might alibi this by supposing that Giedi’s 2 stars were of similar distance; however, repeated experiments all over the sky would give the same result. Which meant that annual parallax was invisible either from: [i] all stars being at same distance (or [ii] stars’ remoteness & thus invisible parallax. The former option would probably be rejected:46 if the seven “planets” were all at different distances, why should thousands of stars all be at only one distance?47 If Giedi’s nearer star ($\alpha^2$ Cap

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34 Archimedes (p.232): Neugebauer 1975 (p.643) calls this his most famous work, even while not realizing its empirical significance.

42 PlanHyp 1.2.5 has some speculations on celestial bodies’ volumes. Sun a bit larger than the brightest stars, which themselves exceeded all the planets. Jupiter & Saturn were a little smaller, yet still much bigger than Earth. Notably for a geocentric work, Ptolemy had even Mars slightly larger than Earth. (And c.60 times bigger than Venus.)

43 From the excellent elliptical tables of K.Moesgaard-L.Kristensen Centaurus 20:129 (1976).

46 Yale BSC parallaxes: for $5 \times$ Cap (HR7747) $0^\prime.006$; for 6 Cap (HR7754) $0^\prime.034$.

47 Perhaps to refute arguments such as those considered here, Ptolemy taught that stars were all at one distance (fn 47; PlanHyp 1.2, B.Goldstein 1967 p.9, Van Helden 1985 p.24), but ancient opinion was not unanimous. (See J.Evans’ new edition of Geminos, or Neugebauer 1975 p.584 n.37a.)

48 See fn 45 and conclusion of [E3].

49 Even aside from its Earth-immobility: Ptolemy’s conception had all the stars’ distances the same ($Almagest$ 7.1, Van Helden 1985 p.27), so the Giedi experiment here described would doubtfully make no impression on him. But one suspects that his demand for uniform stellar distance was designed to defuse (by anticipation) heliocentrists’ potentially troublesome parallactic-questions.
were, say, 1000 AU distant and $\alpha^3$ Cap much 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 eclipsical 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 eclipsical parallax — which thus predicts the unmissable spectacle of $\alpha^2$ Cap oscillating semi-annually, from eclipsical 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 repertoire of astute results. For heliocentrists, said null-parallax result would rule out the premis that the stars were merely 1000 AU distant — and thus supplied the empirical basis underlying ancient heliocentrist’s “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 (*Almajest* 2.1 (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”. So the logical conclusion for heliocentric visionaries 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 ($R_T = \text{Earth radius}$):

$$r_2/r_3 = r_5/R_T = 10000$$

(14)

This would, 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),

**F2** Kleomedes 2.1 reports (Heath 1913 p.348, Neugebauer 1975 p.365) that Kild 1988 p.445 that Poseidonios (1st century BC) considered the possibility that the Sun was (at least: fn 18) 10000 AU distant. This is already given in eq. 14, namely:

$$r_5 = 10000^\circ$$

(15)

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**26** 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 have 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: Plyndychkhurmen ‘grumbled’ at nonutility; seeking universe’s scale.

**53** Archimedes’ (“Sand-Reckoner” p.223) connects Aristarchos to eq. 12, not eq. 15. See fn 32.

**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 reflects prevailing heliocentrist opinion, in

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### B The Greek GD’s Best Latitudes: Non-Greek Egypt & Phoenicia

**B1** Fortunately, some cities’ accurate latitudes appear to have survived; two particular groups are consistent (if we include 2° stellar refraction & 1°/2 rounding) with that optimistic conjecture. In Lower Egypt (Rawlins 1985G p.260; GD 4.5.53-55): Memphis (DIO 22 B1 

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**55** It will help to provide an example, using the *Almajest* 2.8 table for Rhodes (D189) at Sidereal Time (the Right Ascension of the meridian, or Hour Angle of the Vernal Equinox) 21°23’36” = 320°54’ (which is chosen to avoid over-complication in step 1), as will be evident: Adding 6° 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 Rhodes (longest-day $M = 14\frac{1}{2}$, 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°/GEM 0° = ephemerally 70°, so that is the Ascendant. The Descendant (ecliptical point that is setting) is opposite: 250° or 10°/SQR 00’ (10° of Sagittarius). The Midheaven (long distance to transcendent zodiac point) is then found by linear interpolation on Toomer 1984 p.100: in the “Accumulated Time-Degrees” column, under the “Sphaera Recta” head, we find 312°32’; 320°54’ (ST) exceeds this by 8°22’ of the 9°58’ interval corresponding to the 10° interval between 10°ACR 00’ and 10°ACR 00’ (in the column “10° Intervals”), so: add 10° (8°22’9°58’)’ = 8°24’ to 10°ACR 00’, which yields Midheaven = 18°ACR 24’ (18° 4° of Aquarius) on the zodiac or eclipsical 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 astrolabes have for geographical latitude. (Ancients also used latitude to enter parallax tables, but such scrupulousness is rare among today’s astrologers.) Geographical longitude was used merely for additively converting (§D2 [3]) local time to ephemeredes’ standard zero-meridian, presumably that of Alexandria.

**56** All 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°3/4 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. Suggestively, the correct latitude is associated with the ancient Egyptian name, not the later Greek one. Details at Rawlins 1985G p.260.
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:

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

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

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 Marinus’ manual or map, presumably after his (though see §C1) systematic tacit mass allocation (GD 2.5 & 2.6) to force macro-geographical accord (through eq.1). Baker assumed the above-hypothesized (§A1) network-grid-basis, which had been severely pre-corrupted by roundings (§D1&D5) in tables long used by astrologers. Remarks at, e.g., GD 1.18 suggest that, like (following?) astrologer Hipparchos, Marinus clumped (§D4) cities under parallels. Also, Marinus gave primacy (GD 1.20.3 & 24.3; and below at §M) to Hipparchos’ 36° parallel (arc \( \theta - \kappa - \lambda \) in Fig.1 [p.50]) through the east-Mediterranean island of Rhodes, suggesting both an astrological-tradition connexion and even the possibility that Marinus’ 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 Rhodes (D149), probably just north of the town of Lindos. (See Rawlins 1994 §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 ephemeredes 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 1984a 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 Susa/Deous, while Diller semi-omits Limyra. (Though, see end of this fn.) Merging the lists, we have exactly 360 site 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 site D53, Susa/Deous (GD 3.4.8, 8.3.9) 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 5.5.6, 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 8.17.25. D192 is UNK’s item #22, “Myra” (GD 5.3.6). Note that one finds “#22a” in Diller’s hand in the left margin of his p.X31, 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!

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 Tetraabhis. Note that the geographical table in his astrologer-oriented Handy Tables was at this stage still inconveniently in degrees.]
F6 We will next show that the superiority of Poseidonios’ conception was probably based on observation, not “naïve” 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 also be 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\(^{58}\) must have frequently occurred near enough to stars that the invisibility of the predicted parallactic shift was repeatedly verified.

F7 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\(^{59}\) can be more than triple the Sun’s. But for 3° solar parallax (§F6), Venus’ greatest diurnal parallax\(^{60}\) should be as high as about 10°. If Venus passed near a star, then one need only compare observations taken, say, at Meroê (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.

F8 I propose that our fragmentary record (§F4) of ancient planet-star occultations is part of Aristarchans’ systematic empirical\(^{61}\) testing — which eventually converted heliocentrists, c.270 BC (sometime between Aristarchos’ Experiment & the “Sand-Reckoner” data) to a 1146° (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 (SC.3.)

F9 Summing up the evidential situation: we have examined all 3 of the surviving astronomical scales connectable to ancient heliocentrists (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 present coincidence lends more credibility 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 naïvete. (Which is spoofed at DIO 2.1 §1 §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 theory explaining how allegedly indoor Greek “theorists” came into possession of the sidereal year and the periods of the Moon (synodic, anomalistic, draconitic), Mars & (probably Venus) which are accurate to 1 part in ordmag a million or better. (See Rawlins

\(^{58}\)Venus has 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.

\(^{59}\) Almajest 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.

\(^{60}\) Hartner 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.\)

\(^{58}\)These investigations were posted on the DIO website in 2006-2007, at www.dioi.org/gad.htm. Unless otherwise indicated, GD section-numbering here follows that of Karl Nobbe 1843-5 (henceforth cited as merely “Nobbe”), numbering which is also followed as closely as possible by the excellent new edition, Stückenberger & Graßhoff 2006 (henceforth “SG&G”). Note that the present paper forgoes the use of accents for Greek words. Diller himself pointed out accents’ superfluity, since classical-era Greek lacked them. During a DR 1987/8/1 visit to the Vienna Papyrus collection, the same view was expressed by the collection’s chief, as well as by the able Dutch scholar Peter Sijpesteijn, who happened even to be visiting the same day.

\(^{59}\)DIO’s people are amazed at a long tradition of suggestions that the GD may well be the earliest geographical work ever to use spherical coordinates. This is less scholarship than a relic of Neugebauer-salesmanship for Ptolemy. (Origin: Neugebauer 1975 pp.337, 846, & 934; and see p.280 for parallel celestial semi-claims for the Almajest, despite the 2nd century BC Hipparchos Commn’s listing of dozens of stellar Right Ascensions & Declinations.) Long before Ptolemy, Strabo reported a Nile map consistent (Rawlins 1982C) with use of spherical geographical coordinates, and which goes back at least to Eratosthenes (3rd century BC) — a map so antique that it does not even use degrees.
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1984A p.984, Rawlins 1985K, Rawlins 1985G §5, Rawlins 1991H fn 1, DIO 11.1-2, DIO 15.1, www.dioi.org/hrh.htm.) DR evidently was the 1st to publish these startling facts, since the Mufa had willfully overlooked this remarkable 62 achievement. After all, the Mufa has decreed62 in Science that accuracy is irrelevant to ancient astronomy.

F10 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 consciencelessness 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 Prison

G1 We recall O’Gingerich’s suggestion (§A2) 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 persuasiveness64 of the arguments. Thus, Aristarchus will undoubtedly continue to be remembered as ‘The Copernicus of Antiquity’, rather than Copernicus as ‘The Aristarchus of the Renaissance’.”

G2 The most obvious problems with these typically anti-revolutionary OG comments (on 2 brave revolutionaries):

[a] To suggest that we might slight Aristarchos, merely because attacks on his heresy and on his intellectual freedom65 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-querks?

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.

65Fn 20. See also Gingerich 1976 ( & even valuable Grazalloff 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 similar excusing of discovery-misattribution in OG’s 64 values (statement by Lord H & OG). One senses just how upset the Hist.sci may have been its archon T.Kuhn’s launching of the buzzword “paradigm”. When I was...

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64See similar excusing 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 staff gets at plagiarism.


67Besides the present findings, see e.g., Rawlins 1991P fn 1 and Rawlins 1991W §N17 & eqs.22-24.

68If 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 (fnn 6, 20, 64) so knee-jerk anti-judgemental regarding its subjects (though not its turf-competitors) that it has lost sight of the fact that vindication-by-future-experimentation is not anachro-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 become 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 & dysfunctional 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 exploitive cultists who longed to obscure and allib the failures & fakeries of astrologers & other pseudoscientists.)
(Rawlins 1984A pp.972, 981) C.Ptolemy: supreme faker, sellout, lawyer-crank, i.e., the ideal Muffa choice for its “Greatest Astronomer of Antiquity”69⁶⁸ (Neugebauer 1957 p.191 on Ptolemy’s Almajest: “one of the greatest masterpieces of scientific analysis ever written.”) Cultist Van Helden 1985 p.41 genflects to Ptolemy: “the master himself.”

Above, we have found evidence that, even under the shadow of Cleanthes’ notorious threat, Aristarchos reasoned out & promulgated the epochemical implications of heliocentricity. It is selfevident (§A5) that, e.g., he realized that heliocentricity gave (in AU) the correct distances to the planets (not knowable from Ptolemy’s crankpotastronomy), the key step (Rawlins 1987 & Rawlins 1991P) ultimately yielding Kepler’s 3rd Law (discovered & suppressed in antiquity?) & so Newton’s universal gravitation.

And beyond this, we have the Aristarchos heliocentric theory’s more overwhelming implications for the size of the stellar universe, a conception which demonstrably impressed the greatest of ancient mathematicians, Archimedes — an influence which by itself earns Aristarchos first rank even by the JHA’s own corrupt criterion (fn 64). Since OG has raised (§G1) the question of the relative superiority of Aristarchos & Copernicus, I will note that Copernicus 1543 (De Rev 1.10) did not quantify at all the critical fact that heliocentricity necessitated an expansion of the universe by several orders of magnitude. But, as we have seen (eq.14), Aristarchos did. Nonetheless, modern hist.astron. (e.g., Van Helden 1985 pp.41, 46-47) pretends that Copernicus, not Aristarchos, was the first to realize that heliocentricity implied a huge universe. Well, what else would one expect from a cult which pretends to salvage & purify ancient scholarship, even while trying (DIO 1.1 § 5C) to destroy the reputation of any scholar (ancient or modern) whom it happens to disapprove of?

H Heroes & Zeros

H1 Since most great work is the tip of a pyramidal anonymiceberg, it is risky (& 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 a trillion (10⁷³) times larger than hitherto understood, which reveals him to have done even more for our spatial perspective than what 19th century geology & biology did for our temporal vision. His

69 DIO 1.1 15 fn 24, 16 167, 17 92B.
68 See fn 65. Heath 1913 p.304 (also DIO 1.1 § D3) recounts Cleanthes’ attempt (paralleling later threats against Galileo) to have a charge of “impiety” brought against Aristarchos — which, in those bennighted pagan times, could mean terminal consequences for a career. (Socrates was executed for “impiety.”) Of course, today, as our readers are aware (e.g., DIO 4.3 § 15, DIO 6 § 3), we live in an era of free intellectual discourse; for example, even an offense as serious as insufcient brainkissing of hist.astron archons will have no effect whatever upon a scholar’s career.
67 Neugebauer-Muffa genii discern none of this. Swerdlow 1968 p.96: “There is nothing even approximating a reasonable theory of planetary distances in pre-Ptolemaic literature.” Van Helden 1985 p.9: “Aristarchos’ treatise [‘Sizes’] ... addressed only [. . .] the Sun & Moon. No comparable geometric methods ... were at hand for determining the sizes and distances of the other heavenly bodies. Indeed, even the order of the planets was a question without a definite answer.”
66 If this seems too strong, see Rawlins 1991P & Thurston 1998 MMS & © 16.
65 Cubing 10000 yields a trillion — and “Sand-Deckoner” (Archimedes p.232) says that Aristarchos’ stellar universe was a trillion times the Earth-orbit sphere, but without explaining the observational base. Geocentrist preferred r₉ = ordmag 10⁸⁰ and extant geocentrist schemes (3 are tabulated in Van Helden 1985 pp.27, 30, 32) placed the stars ordmag 10⁸₉ distant, while Aristarchos-Archimedes held (eq.14) for 10⁸₀⁰ and 10⁸₀₀₉ distant, respectively; so the net heliocentrist-vs-geocentrist stellar-universe linear expansion factor is ordmag (10⁰₀₀₀·1₀₀₀₀) ≈ 10⁰₀₀₀.
64 The tiny universe-scale dominant among geocentrist remarks reminds one of a joke told by Jake Lamotta about fellow-pug Rocky Graziano. Both were gifted actors after — and before — their retirement from

Epiolog

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 (genn published at Rawlins 1991W fn 272), during which of the Neugebauer clan did what it could to damn the research. But that cult’s political influence has waned, while among its prime present legacies are G.Toomer’s scrupulous Alm edition, and Toomer’s prot´eg´e, 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 malinuence has been somewhat replaced by the Gingerich-Amm edition, and Toomer’s prot´eg´e, the brilliant and creative classicist, Alex Jones, of New York University’s hugely endowed new Institute for the Study of the Ancient World.

We here thank all those who helped effect this productive amicability, which most of us thought might never come to pass in our lifetimes.