Since 1991 inception, has gone without fee to leading scholars & libraries.
Contributors include world authorities in their respective fields, experts at, e.g., Johns Hopkins University, Cal Tech, Cambridge University, University of London.


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

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

Entire DIO vol.3 devoted to 1st critical edition of Tycho’s legendary 1004-star catalog.
Investigations of science hoaxes of the 1st, 2nd, 16th, 19th, and 20th centuries.

Paul Forman (History of Physics, Smithsonian Institution): “DIO is delightful!”
E. Myles Standish (prime creator of the solar, lunar, & planetary ephemerides for the pre-eminent annual Astronomical Almanac of the US Naval Observatory & Royal Greenwich Observatory; recent Chair of American Astronomical Society’s Division on Dynamical Astronomy): “a truly intriguing forum, dealing with a variety of subjects, presented often with [its] unique brand of humor, but always with strict adherence to a rigid code of scientific ethics, . . . [and] without pre-conceived biases, . . . [an] ambitious and valuable journal.”
B. L. van der Waerden (world-renowned University of Zürich mathematician), on DIO’s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: “marvellous.” (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)

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

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

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

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

Page:
1 Sostratos-Pharos Empirical Truth Behind Eratosthenes-Alexandria-Aswan Myth 3
2 Aristarchos Unbound: Ancient Vision 13
3 The Ptolemy GEOGRAPHY’s Secrets 33

Back Issues
Over the last few years, numerous library subscribers have inquired about the availability of back issues, in order to render their DIO collections as complete as possible. We have now established procedures to get this promptly accomplished. So, libraries desiring back issues may simply email to dio@mail.com, specifying the DIO issues desired and the address of the serialsdep’t that will receive them. If a not-yet-subscribing library wishes a complete run to date, that is OK, too. In either case, there is no charge for issues or postage.

The present DIO 14 may be read online at http://www.dioi.org/vols/web.pdf, whence it may be downloaded gratis; likewise, the booklet version of it and all other published DIO issues may be found via http://www.dioi.org/bk.htm; readers preferring instant hard copy can have any of these printed-stapled-trimmed for trivial cost at a local photocopy shop.

News Notes
From the International Herald Tribune 2008/1/12-13 p.1 obit for Edmund Hillary, 1953/5/29 co-conqueror of Mt.Everest: “In the annals of great heroic exploits, the conquest of Mt.Everest by Hillary2 and [Tenzing] Norgay ranks with the first trek to the South Pole by Roald Amundsen in 1911 and the first solo nonstop trans-Atlantic flight by Charles Lindbergh in 1927.” In the era B.D. (Before DIO) this would instead surely have read: Peary-N.Pole & Lindbergh-Atlantic. Popular history takes far too long to reach accurate equilibrium. And all-too-often never does. But we may savour justice as it blossoms.

[Note added 2008 Dec.] DIO’s newest winners of its $1000 R.R.Newton Award for Scientific History are S.Albers & G.Graßhoff, honoring their originality and fruitfulness. Steve Albers was first to propose (Sky & Telescope 1979 March) the ingenious notion of investigating the ms records of earlier-era astronomers who had searched for satellites of any known planet at times when it had been near conjunction with then-unknown planets — in order to find out whether the latter had been accidentally recorded as possible satellites. Albers’ nomination for this DIO prize was due to the R.R.Newton Award Committee’s Charles Kowal, who (with Stillman Drake) had in 1980 taken up Albers’ suggestion and made the sensational discovery of Galileo’s 1612-1613 observations of planet Neptune. The remarkable 1980 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 a partly successful, ultimately no one (including R.Newton & DR) had thought of) to detect mass-statistical correlations between the hundreds of star-positions in Hipparchos’ Commentaries, & 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+5 = 50 fathoms high, or (contra PBA conversion) 300 ft. (Six ft = 1 fathom = 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 300-306 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 conqueror returned to base. Hmmmm. And just where would that leave Brit ultra-polarhero Rob’l Scott?
References


Ernst Honigmann 1929. Sieben Klimata und die Πόλεις Επιστήμη, Heidelberg U.


C.Müller 1883&1901. Claudii Ptolemaei Geographia, Paris. (Bks.1-5 of GD, plus maps.)

O.Heube & Toomer 1984, Ed. History of Ancient Mathematical Astronomy (HAMA), NYC.


Keith Pickering 2002A. DIO 12:3.


D.Rawlins 1982N. ArchiveHistExactSci 26:211.


D.Rawlins 1999. DIO 9:1 3. (Accepted JHA 1981, but suppressed by livid M.Hoskin.)


D.Rawlins 2002V. DIO 11:3 36.


Louis Renou 1925. La Géographie de Tolémée: l’Inde, Paris. (Bks.7.1-4.)


Hugh Thurston 1998A. DIO 8 1.

Gerald Toomer 1984, Ed. Ptolemy’s Almajest, NYC.

Friedrich Wilberg & Carl Grashof 1838-1845. Claudii Ptolemaei Geogr, Essen. (Bks.1-6.)

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

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 1:1 fn 2.]

A2 Rawlins 1982N (p.217 & n.26) discussed two easy stay-at-home methods which would account for the overlargeeness 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, until ultimately falling to earthquakes and their aftershocks.

A3 With this glimmer of where we’re headed, we now plunge into solving the entire Eratosthenes Earth-measure mystery: method, place, all his data (terrestrial and celestial), and we even develop (§J) the 1st credible (if quite speculative [at least until p.12’s finale]) figure ever modernly 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 (fn 8) ancient standard Earth-sizes (Eratosthenes & Poseidonios) within c.1% in each case (§K4).

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


Summarizing the respective methods’ errors: c.0%, +20%, +20%, ~17%.

(All these errors would be appreciably weaker for great heights’ thinner air: fn 1.)

Aubrey Diller is generally acknowledged to have been the 20th century’s leading authority on ancient geographical mss. We will be ever grateful that he in 1984 bequeathed to DR his final work: 1st establishment of the text of crucial Book 8 of Ptolemy’s Geography.

Further thanks are due to DIO Editor Dennis Duke for getting our GD Book 8 project re-started early in 2006, as well as for restoring the original 1984 Diller mss (DIO 5 [2006], www.dioi.org/diller8/diller8.htm) to a publishable state. And expert advice from Alex Jones and Len Berggren headed off potential mis-steps in the foregoing.

Also to be thanked: a longtime family friend, the late Prof. Emeritus Jimmy Poultney (for many years one of the stars of the Classics Dep’t of Johns Hopkins University), who kindly oversaw DR’s early work on Diller’s final opus; and our friend David Rockel, who patiently assisted in the collection of materials used in DR’s research.
Eratosthenes’ Large Earth & Tiny Universe

2008 March

DIO 14

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:

\[
v^2 + r^2 = (r + h)^2 = r^2 + 2rh + h^2
\]  

(1)

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

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 \( S = 24 \). However, had he adopted \( S = 16 \text{ 5}/12 \) without\(^{58} \) 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 \( Y = 16 \text{ 5}/12 \) 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 \( 16 \text{ 5}/12 \).

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:

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

(18)

\(^{58}\)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. \( \mu \& \nu \)) or ones where the projection’s southern parallel was the Equator (§I2) 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:

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

(13)

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

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

Y \( \approx 21 \) 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 \( \xi \) equal to exactly 90° — not the seemingly pointless and peculiar [roughly 98°] spread we actually find: see fn 55 or Fig.1.) A 90° spread would make all longitude slices nearly 1/2 their real angular thickness.

N12  Our math for an attempted Split-inspired reconstruction of the process behind Y = 34 will, up to a point, be the same as Ptolemy’s — only simpler. We found \( R = 115.4 \) to 115 (just as in eq.5 or GD 1.24.4) but then use a simple fan — i.e., 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:

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

(14)

Substituting Ptolemy’s values, \( Y = 34 \) (\$M5 or GD 1.24.2) and \( R = 115 & H = 52 \) (eqs.5&6 or GD 1.24.4), 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 alterate-possibility for the source of the problematic Y = 34: a non-kinked fan-ekamene, with Marinos’ latitudinal breadth of the known world, though Marinos is said (\S N17) not to have used a fan-projection.

N15  Having thus found an S that could have led to GD 1.24.2’s Y = 34, we may simply invert the process to follow in the 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 ekamene’s northern & southern limits (T & S, resp), the inverse of the previous equation gives us what we need:

\[ Y = \frac{2(S + T)/(1 - \cos T)}{1 + \left[1 + 1.16(1 - \cos T)/T \right]^2} \]  

(16)

1 While seeking an explanation of Eratosthenes’ result, DR has in recent years been inexplicably distracted by the \$A[4][b] Mountain Method. (Thurston 2002S p.66 evidenced better memory and sense.) Yet it is obviously inferior (to the \$A[4][c] Pharaoh Method): it involves measuring a small angle — and the 1% precision of agreement with Eratosthenes’ actual Earth-radius would require 1° measuring accuracy under difficult seeing conditions. (Also, the great height required to get an angle large enough to render observer-error negligible would lead to weakening of refraction due to decreased atmospheric density-gradient, yet the error in \( C_{\text{hk}} \) is closely \$E [3] consistent with virtually full-strength seallevel refraction.) Advantageously, the Pharaoh Method does not even get involved with angles at all, and the requisite relative precision is attained with ease. Note: the Mountain Method would lead to two-significant-digit results: the Pharaoh Method, three. So the very fact that Eratosthenes expressed his Earth-radius to three (eq.13) provides yet another indication that it was based on the Pharaoh Method.

C  Pharaoh’s Approximate Height
C1  Josephus J.War 4.613 says the flame of the Pharaoh was visible to ships for 300 stades (obviously a round figure for v), which would by eq.3 make it the world’s then-tallest building (exceeding the Great Pyramid); yet it was never so described. Solution to Josephus’ datum: the crow’s-nests of tall ancient ships were roughly 1/4 of the Pharaoh’s height, meaning (eq.3) that approximately 1/3 of Josephus’ 300 stades was due to ship-height; so \( v \approx 200 \) stades is an adequate rough estimate for the Pharaoh’s visibility-distance \( v \) at seal level.
Thus eq.3 gives us a pretty good idea of the Lighthouse’s height $h_L$:

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

D Eusebius Bequeaths Us Eratosthenes’ Exact Earth-Radius

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

**D2**
We will henceforth also owe him for the long cast-aside, here vindicated clue relayed in his *Prerapatio 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)

**D3**
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 $(90^\circ \text{ minus altitude } h)$ was 1/50 of a circle at Alexandria but null at Aswan-Elefantine (very near Tropic of Cancer) where legend had vertical sunshine reaching well-bottom (though see Rawlins 1985G p.258) — 2 cities 5000 stades apart in latitude. (NB: Kleomedes 1.10 doesn’t say that the 5000 stade distance was measured, merely calling it a “premis”.)

So:

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

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

$$p_{BK} = 2\pi S_E/C_K \cong 103$$ (8)

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

$$p_{BG} = 2\pi S_E/C_G \cong 102$$ (9)

but this is also unacceptably non-round.

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

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

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

$$p_{BN} = 2\pi S_E/C_N \cong 100.1$$ (11)

---

55 For the 2nd projection, there is no such qualifier (*GD* 1.24.17), even though there might as well have been — since for both projections the 2-1 rectangular bound is slightly wider than necessary. But for the 2nd projection, there is no appearance that an adjustment might render the *ekumene* exactly twice as wide as high. Its definition is quite different from the 1st, and results in a fan opened only about $61^\circ$ (vs the 1st projection’s $98^\circ$; §§M1), with a pseudo-north-pole c.180 units above the Equator (vs the 1st’s 115 units: eq.5).
N Impossible Dream: Symmetric-Rectangle-Bounded Ekumene Fan

Ptolemy’s Geography

DIO 14

54

Ptolemy’s

GEOGRAPHY

DIO 14

Eratosthenes’ Large Earth & Tiny Universe

DIO 14

57

This is a hit that carries us right into the heart of the Earth-measure mystery.

The obvious conclusion from eqs.6&11 is that Eratosthenes had the Sun’s distance equal to

100 Earth-radii, so

\[ S_E = 100r_E \] (12)

the only 3-significant-digit Eratosthenes figure for the Earth’s size directly based on empirical data. (Compare eq.13 to eq.7.) All pre-Pharos C were 1-significant-digit-rough: 4000000 stades (Aristotle c.350 BC), 3000000 stades (Dikaearchos c.300 BC). Yet (§11) after the Pharos’ debut, we find ordigma 100 times greater precision in 3-significant-digit eq.13.

E Eratosthenes’ Moon

E1 While placing the Sun 100 Earth-radii distant, far short of Aristarchos’ solar distance, Eratosthenes nonetheless adopted the farcical lunar distance of pseudo-Aristarchos,\(^3\) 19 Earth-radii (Heath 1913 pp.339 & 350; but see \(\S\)2 [C5], as eq.13 verifies:

\[ M_E = 19\pi r_E = 775200 \text{ stades} \approx 780000 \text{ stades} \] (14)

which matches\(^4\) eq.5, Eusebius’ report. (The match is far better than that figured at Heath 1913 p.340, where \(2\pi/19\) is divided into the hitherto-conventional Eratosthenes C = 252000 stades, yielding about 760000 stades.)

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

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

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

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

F Eratosthenes’ Sun

F1 Remarkably, Eratosthenes had the Moon’s distance almost 1/5 of the Sun’s — which goes counter to easy visual checks, since if his 19:100 ratio were true, half-Moons would simply as:

\[ Y = \frac{E + (R/H)\sqrt{R^2 + H^2}}{(R/H)^2 + 1} \] (12)

we notice to those checking via-ruler the rectangle of the Nobbe 1843-5 p.47 illustration of Ptolemy’s 1st projection (reproduced at www.dioii.org/grad.htm?obn, with the ekumene bounded in green): its halves are accidentally 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 ancient anonymous cartographer’s full intended map-rectangle concept. (Where compatible choice of \(Z = 34\) and fan-spread 98° allows meant area-proportionality while \(\xi & \pi\) lie on line \(\alpha-z\). \(\S\)M14-M15.) Fig.1 is designed in pure Postscript (as was \(\S\)1’s Fig.1).

---

\(^3\) DR has long contended (\(\S\)2 [C1] etc) that Aristarchos’ supposed ms “The Sizes and Distances of the Sun & Moon” is not truly his but is by an uncomprehending pedant (follower, detractor, distractor?), since the work is vitiated by an error of a factor of four (mis-step’s amateurish origin explained at \(\S\)2 [C1]), leading to a 23°-wide Moon and thus (\(\S\)2 [C5]) a 4° wide Earth-shadow at the Moon, which would imply central lunar eclipses’ Entirety (partiality-start to partiality-end) lasting half a day, with c.4 Totality (durations too high by factors of about 3 and 2, respectively). Pseudo-A’s 19° lunar distance required the Moon to visibly retrograde daily, and this joke-astronomy became the royally approved lunar theory in the Alexandria that elevated Eratosthenes to top academic. (Full incredible details below at \(\S\)2 [C]). Eratosthenes’ adoption of this way-too-low lunar distance (vs DR’s reconstruction of c.60 Earth-radii for Aristarchos: \(\S\)2 [C11]) suggests that the acceptance of pseudo-Aristarchos’ work as genuinely Aristarchos’ goes way back. (It also suggests little comprehension by Eratosthenes of his lunar distance’s two most ludicrous implications, as just remarked. Perhaps lunar parallax was not recognized by some scholars of the 3rd century BC, though it is obvious that Hipparchos had parallax tables only a century later: Rawlins 1991W fn 28B.) Note that, by contrast with Eratosthenes (and modern scholars), Archimedes didn’t fall for any of pseudo-Aristarchos’ bizarre astronomy: \(\S\)2 fn 33.

\(^4\) A lunar distance of 19° implies 3° Earth semi-diameter as seen from the Moon, which itself was anciently gauged as having semi-diameter 1°/4 as seen from the Earth; that is, seen at the same distance, the Moon has merely 1/12 the Earth’s angular sd. Thus (by the same symmetry argument we’d use at \(\S\)3), the Moon’s radius is 1/12 the Earth’s so (in adopting pseudo-Aristarchos’ lunar distance of 19 Earth-radii: \(\S\)2 [C5] Eratosthenes had the Earth’s volume about 12\(^4\) 1700 of the Moon’s!\)
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 (royally­oily Eratosthenes was a fav 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.

From Eratosthenes’ 100 Earth­radii solar distance (eq.12), we see that the Earth’s angular semi­diameter as seen from the Sun would be 180°/100π = 0°.573, while the semi­diameter of the Sun (seen from the same 100 Earth­radii distance) was pretty accurately estimated (§2[C1] to be 0°.25. Therefore, the implicit solar size s in Earth­volumes is:

\[ s = (0°.25/0°.573)^2 \approx 1/12 \] (16)

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 anti­progressive view of the universe dominated the world 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 2 fn 33 & §H1.)

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

\[ C_N/2\pi = 256000/2\pi \approx 40700 \text{ stades} \neq r_E \] (17)

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

\[ 2\pi r_E = 2\pi \cdot 40800 \text{ stades} \approx 256000 \text{ stades} = C_N \] (18)

This contrast (eq.17 vs eq.18) confirms the §E3 finding, so that we now have double­evidence that Eratosthenes’ radius generated his circumference CN, not the reverse.

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: §A4[a], because that method’s math (eq.7) produces circumference. By contrast, the Pharsos Method (§A4[c]) directly yields the Earth’s radius: eq.2. Thus, the clear implication of the radius’ computational priority is that the Pharsos 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: §K2 & fn 7.)

As noted at Rawlins 1982N n.10, Eratosthenes was possibly unsure of whether the Mediterranean Sea’s curvature matched the world’s. If so then (ibid p.216) he may have unwittingly based his 5000 stade supposed­meridian (Alexandria­to­Aswan) & his

5 Note Sun­shrinker Eratosthenes’ Scylla­Charybdis narrowers: bringing the Sun near enough to make it smaller than Earth, while putting the Moon too close to the Sun (thereby inflating §2 eq.4’s γ) 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 §F3 used smaller solar sd (Heath 1913 p.312­314), perhaps also rounding π to 3, then the computed Earth/Sun radii­ratio could be ≈ 3, the cube of which is 27.)

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

\(^5\)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.\(^50\) We next turn to the more puzzling question of how wide-open the fan will be.

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

M6 Since Ptolemy follows Hipparchos and (GD 1.20.5) Marinos in taking the Rhodos latitude (36\(^\circ\)) or klima (14\(^\circ\)1/2) as canonical for the mid-eukeme, he chooses (GD 1.24.3) the Rhodos parallel at latitude 36\(^\circ\)N as the one along which he will (allegedly) adjust longitudinal distances precisely, just so that this parallel’s curved length (west—east arc) has the correct proportion (4:5 \( \equiv \cos 36\(^\circ\) = GD 1.20.5 & 24.3) to the fan’s already-determined north—south radial distances (\( \text{eq.}\)M4).

M7 That step is odd because, when he earlier (\( \text{§}\)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 just by 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:

\[
Y = H \cos \left( \frac{16200 \cos L}{\pi (R - [L])} \right)
\]

\( \text{eq.}\)8

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

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

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

\( \text{eq.}\)9

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

\( \text{eq.}\)9

\( \text{§}\)M4. A list for ready reference. If we go up the mid-vertical of \( \text{Fig.}\)1, we find:

\( \alpha\)–\( \eta \) is of length \( H = 52 \) (as is \( \xi\)–\( \eta \));

\( \alpha\)–\( \rho \) is of length \( T = 63 \) (as is \( \rho\)–\( \xi \));

\( \alpha\)–\( \sigma \) is of length \( R = 115 \) (as is \( \sigma\)–\( \rho \));

\( \zeta\)–\( \sigma \) 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 \( \gamma\)–\( \beta \) is of length \( L \). Note that \( \zeta\)–\( \epsilon \) is of length \( Z \) (\( \text{§}\)N3), as are the sides of the 2-1 rectangle: \( \gamma\)–\( \alpha \) & \( \beta\)–\( \eta \); also equal to \( Z \) are: \( \alpha\)–\( \epsilon \), \( \epsilon\)–\( \beta \), \( \gamma\)–\( \zeta \), \( \zeta\)–\( \epsilon \).

\( \text{§}\)M4. 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 (\( \text{§}\)M12) that \( Y = 34 \) units is not for the Rhodos parallel (corresponding via \( \text{eq.}\)9 to the 106\(^\circ\) fan-spread used by the non-fitting diagrams just cited) but was designed as an average fit (\( \text{§}\)M14) to all eukeme parallels \( L \). Note that for \( L = 0\(^\circ\) \) (Equator) or 63\(^\circ\) (Thule), fan-spread \( F \) would be 90\(^\circ\) by \( \text{eq.}\)8 (\( Y \approx 37 \) by \( \text{eq.}\)8). The average of 106\(^\circ\) & 90\(^\circ\) is 98\(^\circ\), which fits \( Y = 34 \) (the average of 31\&37: \( \text{§}\)M3).
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) seallevel Pharos-visibility distance \( v \) was
\[
v = 202 \text{ stades} \tag{23}
\]
(Not far from the crude §C1 estimate used in eq.4.)

I2 When these values are substituted into eq.2 (or eq.21), the result is:
\[
r_E = \frac{v^2}{2h_L} = (202 \text{ stades})^2 / (2 \times 1\text/2 \text{ stade}) = 40804 \text{ stades} \approx 40800 \text{ stades} \tag{24}
\]
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 \( R = 34400 \) stades of §B6, and (somewhat over-ideally taking the equation’s 1.2 factor as exact) find that a perfect Pharos Experiment for a 1/2-stade Lighthouse would have measured \( v = 203 \) stades. Not only does this (compared to eq.23) evidence the care of the Greek scientists who performed the necessary measurements, but it also reminds us that (because \( v \) is squared in eqs.2&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 exponential error of not 1% but roughly half that. NB. This point is independent of the 1/2-stade Pharos theory, and applies also to the Sunset Method (§A4[d]), whose resulting \( C_P \) (eqs.26&28) likewise depends upon the square of the crucial measurement. (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’ \( r_E = 40800 \) stades: eq.13) and Poseidonios’ \( C_P = 180000 \) 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 & humble to the ubiquitous modern cult that has misled generations of young scholars into accepting the fantasy that ancient science was unempirical: see, e.g., §2 §§A1, A6, B3, & especially the priceless gem at §2 fn 20.

J Playing-Accordance with the Stade

J1 There has been a long tradition of attempting to force agreement of the Eratosthenes and Poseidonios values with each other and with reality by arguing for whatever stade-size would make-E&P-right. But it is encouraging to report that this sort of manipulation is no longer taken seriously by most specialists. Dicks, Neugebauer, Berggren, & Jones never fell for it. [Engels 1985 marshes it.] Amusing details of testimony-twisting (used to carry out such programmes) are exposed at Rawlins 1982N App.B and Rawlins 1996C fn 47.

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

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

J4 In the Almagest Ptolemy was under Hipparchos’ influence, so he presumably adopted his C which was (Strabo 2.5.34) Eratosthenes’ \( C_P \) (§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 to planarity)

M Ptolemy’s 1st Planar World-Map Projection From Where-in-the-World Arrived That 34-Unit Vertical Strut from Its Top (\( \varepsilon \)) to Its “North Pole” (\( \eta \))? Ancient Averaging. And Weights?

M1 In GD 1.24, Ptolemy twice attempts to design a planar portrayal of a broad spherical geographical segment, representing the known world — the \( \text{ekumene} \) — covering 180° of longitude from the Blest Isles (0° longitude) to easternmost China-Vietnam (180°E. longitude)\(^{48}\) and 79.5°S/12° (GD 1.10.1) of latitude from Thule (Shetlands [Mainland]) 63°N. latitude to anti-Meroë (16°5/12 S. latitude, a klima as far south of the Equator as Meroë is north of the Equator). It is the 1st of his two projections (GD 1.24.1-9) which will concern us, since it involves a hitherto-unsolved mystery. This projection (page opposite: Figure 1) is a fan, opened slightly more than a right angle: \( \varepsilon^9 \) (§N11). Thus, all north-latitude ekumena semi-circles are represented by 98° 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 1st Projection’s conversion of the spherical-segment \( \text{ekumene} \) to planarity, the degree-distance \( T = 63° \) from Equator to Thule is made (§M4) into \( T = 63 \) linear units; likewise for the \( S = 16°5/12 \) from Equator to anti-Meroë, etc. In Fig.1, representations of several latitude-semi-circles are depicted as Ptolemy’s source intended (fn 54):

\begin{align*}
\text{The Thule semi-circle (latitude 63°N) } \ &= \xi \cdot \alpha \cdot \tau, \\
\text{the Rhodes (§M6) semi-circle (latitude 36°N) } \ &= \theta \cdot \kappa \cdot \lambda, \\
\text{the semi-Equator (latitude 0°) } \ &= \mu \cdot \sigma \cdot \tau; \\
\text{the anti-Meroë semi-circle (latitude 16°5/12 S) } \ &= \nu \cdot \zeta \cdot \upsilon.
\end{align*}

(Repeating [M]: though each arc in Fig.1 is only \( \varepsilon^9 \), it represents 180° 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 \( \text{ekumene} \) of Fig.1. This kink-step enables Ptolemy to force (GD 1.24.7) the length of the anti-Meroë parallel (south of the Equator: latitude \( \Delta = 16°5/12 \)) to be exactly \( 49 \) as long as its northern equivalent, the Meroë parallel (latitude \( +16°5/12 \)).

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. 

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

\[
\frac{H}{R} = \cos 63° \tag{4}
\]

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

\[
R = \frac{T}{1 - \cos T} = \frac{63}{1 - \cos 63°} \approx 115.38 \ldots \approx 115 \tag{5}
\]

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

\[
H = R - T = 115 - 63 = 52 \tag{6}
\]

\begin{align*}
\text{48) Ptolemy rightly scaled-down (§L3) Marinos’ eastern limit from c.225° (15°b = 5/8 of circle) to 180° (120° = 1/2 of circle); southern limit, from c.24° (Tropic of Capricorn) to 16°5/12 (anti-Meroë).} \\
\text{49This length-fidelity (perfectly reflected in our Fig.1 — and creating the absolute magnitude in eq.6) renders all other southern parallels of the GD \text{ekumene} virtually equivalent (in length, though not radius) to their northern counterparts.}
\end{align*}
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:

$$C_o = 216000 \text{ stades}$$

(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:

$$C_N/C_o = 256000/216000 = 5.93/5 \quad C_o/C_p = 216000/180000 = 6.00/5$$

(28)

which shows how dramatically successful the refraction theory has proven⁵ — 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 $C_o$ to $C_p$ is only scantily attested: Strabo 1.3.11 & 1.4.1.

⁵ Other problems for accepting the Assuan-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) Assuan due south of Alexandria (see also Rawlins 1982N), though travel straight from Alexandria to Asswan would have to be knowingly steered 20° east of south to hit Asswan. 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 1000¹ later the experiment was actually done [elsewhere], successfully.) [Did an ordmag 1000-stade Nile-parallel version occur c.300 BC? See Dio 20 21 n.2.]⁶ 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 18 §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 sealevel atmosphere-density gradient were high enough, horizontal Pharos-light-rays’ curvature could be the same as Earth’s, so (for null extinction) the Pharos would be visible no matter how far away one receded, and this infinite $r$ 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).

Figure 1: Ptolemy’s 1st projection. Ekmene demarcated by dark bound. Proceeding south, we successively encounter arcs representing the ekmene portions of six latitudinal circles: Thule = $\xi$-o-π, Rhodos = $\theta$-κ-λ, N.Tropic, Meroë, Equator = $\rho$-$\sigma$-τ, anti-Meroë = $\mu$-$\zeta$-υ.
K3 But given the cascade of startling new matches above [& at this page’s end], little doubt can remain that the unattested Pharos & Sunset Methods underlay the only 2 standard ancient Earth-sizes, $C_p$ & $C_p$, resp. Which tells us what has often been shown in these pages (see, e.g., fn 9, §§ fn 38, §3 §A3): much of high ancient science has been lost & so is only recoverable by reconstruction, a finding unsurprising to most of us, yet which nonetheless eludes induction-challenged chauvinists who whenever convenient will (DIO 11.1 p.3 & §f 7) pretend that they cannot accept anything without extant textual explanation.

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

References


Eusebius PE. Preparatio Evangelica c.310 AD. Ed: E.Girard 1903.


S.Newcomb 1906. Compendium of Spherical Astronomy, NYC.

Pliny the Elder. Natural History 77 AD. Ed: H.Rackham, LCL 1938-62.


D.Rawlins 1982N. ArchiveHistExactSci 26:211.


Hugh Thurston 2002S. Isis 93:1.58.

Gerald Toomer 1984, Ed. Ptolemy’s Almagest, NYC.

InductionQuake AfterShock

This paper was 1st posted and referees alerted on 2008/3/12. But on 2008/3/15, DR happened upon the obscure sole extant ancient estimate of the Pharos’ height $h$: 306 fathoms (Steph.Byz T35a [1825 ed. 3:1251]; Strabo [H.Jones 1790, §2:46], taller than any building ever. Unless Greek feet were meant. If so, $h$ is within 2% of our eq.21, and $v = 204$ stades. But it’s suggestive that 306 & 40800 are both unrounded by factor 1.02. Did a later scholar try estimating $h$ by putting $r_q$ = (252000 stades)/$2\pi$ = 40000 stades (Neugebauer 1975 p.654) and $v = 202$ stades (eq.23) into eq.2 to find $h = 0.51$ stades = 306 ft? Regardless, after years of exaggerations, we now have double evidence for a conservative estimate:

Pharos flame’s height $h_k = 93m \pm 1m$

46 A consideration which alone could serve to gut the entire long-orthodox Neugebauer-group fantasy (§D4) that high or even low Greek math-astronomy was derived from Babylon. Note that the same Strabo passage shows that Eratosthenes’ latitude for Babylon was as erroneous as Hipparcos’ 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. (Dicks 1960 p.134 notes that Babylon had no interest in geographical latitude, not even its own.)

47 It has been remarked that the Strabo 2.5.34 intro to his discussion of Hipparcos’ klimata appears to state that Hipparcos was computing celestial phenomena every 700 stades (i.e., every degree) north of the Equator. But since the lengthy klimata 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) was that Hipparcos was providing latitudes (for each klima) in stades according to a scale of 700 stades/degree, a key attestation that Hipparcos had adopted Eratosthenes’ scale.


8 Ptolemy’s GEOGRAPHY 2008 March DIO 14 ≥3

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. [Note: The rest of this explicitly speculative reconstruction was nontrivially re-analysed & revised in 2009. See DIO 5 fn 68 for numerous SE Asia site-identifications.] Our interpretation of GD 1.13.9 (B&J p.75): Marinos is saying that an ancient sea voyage from Malay’s Sabara-Tamala region (Phuket, Malay) to the Golden Peninsula (Sumatra’s NW tip) is roughly 200 mi, which is about right. (Marinos’ sailing direction [c.SE] is ignored here, since based on his distorted map.) GD 1.14 says the rest of the trip to Zabai (Singapore) takes 20°. Going around Sumatra (instead of sailing between Malay&Sumatra) would require c.20°. (Speed c.100mi/day; already established at B&J p.76 via GD 1.14.4: Aromata to Prason. Made more exact by checking Phuket-to-Singapore.) The original report is due to “Alexandros” (geographer? explorer? admiral?) who says the trip from Zabai across to Kattigara (Saigon) takes merely “some days” (GD 1.14.1-3), roughly consistent with the c.6° it would’ve taken at the previous speed.

K9  The GD’s supposed direction to Kattigara (left [east] of south) is obviously confused. I suspect that the ancient cause was a common land-lubber misinterpretation: “south wind” (which means wind from the south) was taken as towards the south — thus, the report of going somewhat east of a “south wind” (GD 1.14.1). B&J p.75) was 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 (EDS) 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° (EDS), 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/), more deeply inland Thinai (D355, L: 13°) = Cambodia’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 misordered (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 (D313), by (www.dioi.org/cot.htm#ptxt) screwing-up Latin text of (or like) Pliny’s accurate description of that –330/90 event, thereby attaching Arbela’s eclipse-time to Carthage! Despite lunar eclipse after lunar eclipse occurring in Ptolemy’s lifetime (three recorded at Alexandria in under 3° 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 in cross-disagreement with not just the real sky but just as much his own lunis-solar 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 11:2:0 [p.14].) This eclipse was so famous one would suppose it was widely-written-of. Thus, it is doubtfully weird that Ptolemy could make such an error. The suggestion here is that, as an astrologer 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 athisr 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, 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 geocentrist’s.3

A Muffia Vision

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

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

2From the Indian poet R.Tagore. This particular poem inspired Vienness 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 (DIO 1.1 1 fn 23) Aristarchos named the 365°1/4 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 22.22 & 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 (519/5). 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 Hipparcus. 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.6&167 n.8) claims that this is “a purely fictitious number” (part of a “purely mathematical exercise”), and that the data of a supposed lone extant Aristarchos ms., “On Sizes & Distances” — which DR ascribes to an otherwise unknown soon-after indoor mathematical pedant pseudo-Aristarchos — “are nothing but arithmetically convenient parameters [§3], chosen without consideration for observational facts which would inevitably lead to unhandy numerical details.” (One might as well straight-out call Aristarchos an idiot. Such pontifications by the ever-intolerantly arrogant Neugebauer-cult — formerly known here as the Mufa — themselves ignore the crucial significance of a glaringly “unhandy detail”, the demonstrable falsity of the longish attribution to Aristarchos’ pseudo-Aristarchos’ grossly overblown unempirical 2½ 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.4 §C1 item [a.]).

Similarly, on 1984/6/28, O.Gingerich astonished a small Zurich gathering (including van der Waerden, myself, my wife Barbara, and others), by supposing aloud that Aristarchos’ heliocentricity was not really a full-fledged theory: perhaps he’d merely broached the idea one day while chatting with another scientist.

A2 See OG’s similar 1996/8 remarks (12ª after the Zurich meeting) at Gingerich 1996 — projecting his own bizarre Aristarchos-demoting fantasy onto Hugh Thurston, who has informed me, in further astonishment (plus DIO 6 §3 [SH1]) at the JHA’s old habit of careless mentalism (Rawlins 1991W §5B1&B2, DIO 2.1 ddag 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!5 What follows will suggest that these Neugebauer-Mufa appraisals are ac- correct & perceptive as ever. (See also fn 70.)

A3 But I must call a brief interlude at this point, in order that the reader not miss the weird inversion going on here in §A1&A2, the Neugebauer-overall-ancient-astronomy-conception’s perversity-pinnacle: rebeld&heliocentrist-pioneer Aristarchos was a non- observing fabricator, while go-along-geocentrist&data-faker Ptolemy was antiquity’s

4 Indoor-Neugebauer 1975 p.642 astonishingly claims that “one would be lucky to determine the night on which dichotomous failed”. Contra this (§ fn 19), sharp eyes can discern lunular halfness whenever > ~ > Aristarchos’ 3º, as DR & K.Picking have 1st-hand verified outdoors 100s of times.

5 DR deliberately chooses the very phrase banned 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/III.htm#ghss]. And note its debts to O.Neugebauer & O.Gingeri’s fn 20.) Evans 1992 p.458 still takes the pseudo-A 2½ solar diameter bound to the pseudo- that this author of Oxford Univ Press’ History and Practice of Ancient Astronomy draws over convincon — not to mention indefensible — conclusions about the evolution of ancient astronomy during its two most productive centuries. (The usual for culptists who think great ancient astronomy only flowered with the faker Ptolemy.) See also fn 16.

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

6 See DIO 6 §3 fn 11, which relays Levine’s spoof of his own WashMonthly’s penchant for projection, chuckling that fellow writers reading WM accounts of their output “find themselves espousing ideas they’ve never even heard of, much less agree with.”

7 Ptolemy’s fraudulent tendencies did not end at mere fabrication of data. He had also a proclivity for suppressing all manner of inconvenient facts. E.g., when he pretended (Almajest 3:1) that the solstices of Aristarchos & Hipparchos were consistent with the Hipparchos PH solar theory (Rawlins 1991W §K10) adopted for the Almajest, he suppressed (DIO 1.1 §6 [A5]) the time of each of these 2 solstices and no other, of the score of equinox-solstice data provided thereabouts — thereby hiding the fact that each disagreed with said theory. (Each by the same amount: minus 1½.) Likewise, to

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 continuously rough-north-to-south coast (blocking any route to the Pacific) beyond the South China Sea, near longitude 180º (12º east of the Blest Isles or 120º (8º) east of Alexandria, stretching from near the Tropic of Cancer, all the way south to Kattigara at 8º1½ S. latitude — effectively wrapping China around the Indian Ocean’s eastern outlet. Latitude-longitude coordinates for 18 China sites are found in GD 7.3 (Renou 1925 pp.62-65).

K2 But, according to the previously-broached §D1 theory, all of this geography hinges upon the underlying grid-network: GD 8 and-or its kin. If we look at the GD 8.27.11-14 China data, 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) (13º1/8, 7º2/3), Thinai (D355) (12º5/8, 8º), Kattigara (D356) (12º1/2, 7º3/4). Anything wrong with GD’s China is wrong in this trio.

K3 For Thinai (D355), GD 7.3.6’s latitude (3ºS) jars with GD 8.27.12’s longest day 12º3/4N, which would be correct for about latitude 12º1/2 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 Thinai, it provides confirmation of GD 8 (not GD 7), listing Thinai at 13ºN. Which suggests that the 3ºS of GD 7 is either a scribal error (missing the iota for ten) or is differential: 3º south of Aspithra (16º1/4N). Either way, it seems that 13ºN is correct, as listed by Vat 1291. (S&G 2.734 for Thinai has GD 7.3.6’s 13º 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-counter-statement for Kattigara: 177°E (of the Blest Isles) & 8º1/2 N — that is precisely the GD 7.3.2 position of Rhabana. Therefore (not for the 1ª 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-mentioned fantastic N-S coastal-bar to the Pacific.

K7 There is a disturbing pattern to the GD 7 latitudes of the only four cities in the Southeast Asia region which are listed in GD 8 (in order N-to-S): Aspithra, Thinai, Kattigara, Zabai. These cities’ GD 7.2-3 latitudes are, resp, about equal to: 16º1/4, 13º, 8º1/2, 4º3/4 — 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) 13º, Thinai (D355) 12º3/4, Kattigara (D356) 12º1/2, Zabai (D348) 12º1/4. (And, indeed, these are the Diller found in GD 8’s UNK mss-position.) This looks even firsher 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 13º1/8 variant discussed in fn 44 for Aspithra, perfectly matched what may [idem] have been merely a scribal error: 18º1/4.) Obviously assuming exactly-correct latitudes here is risky when dealing with such rounded data. Conclusion: we must also use verbal descriptions, if we wish to have any chance of solving this section of the GD.

44 The same Vat 1291 list gives 18º1/4 N latitude for Aspithra (not the 16º1/4 N latitude of GD 7.3.2, corresponding to longest-day 13º1/8 [§K7], the very Aspithra longest-day value listed in Diller’s XZ tradition mss. (One is tempted to ask if 18º1/4 latitude [idem] was the true original latitude — or was later forced to agree with M 13º1/8? But it could have just come from a scribal error.) In Nobe, GD 8 lists Aspitha at longest-day “about” 13º, which corresponds to latitude 16º—, agreeing with the GD 7.3.5 Aspitha latitude in Nobbe and Renou: 16º and 16º1/4 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 (§E4) that Ptolemy’s alleged home-city (Alexandria) is missing from GD 1.

J2 Marinos is clearly identified as of-Tyre (GD 1.6.1). Indeed, Tyre (Phoenicia) is cited doubly and with accurate latitude — highly exceptional on each count — at GD 5.15.5&27: 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 FI”, 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.

Note the Velikovskian context.

J4 Similarly, when (1990/101) dim atmosphere proponent B.Schafer imparted to DR his intention of testing the Ancient Star Catalog’s authorship by assuming 0.23 mags/attm opacity, DR immediately suggested that it would be much more fruitful to use Hipparchus’ authorship (which had by then been obvious to serious astronomers for centuries) to test for ancient atmospheric opacity. BS didn’t listen, so this important and revealing project — proving beyond any question that man (not nature) is the prime cause of present atmospheric opacities ominously higher than ancient skies’ — was instead masterfully and independently established by Pickering 2002A §§D2-D5 [pp.11-12].

J5 Tyre’s absence from GD 8 has several non-neatnesses. While Tyre is also missing from the Important Cities lists in late copies of Ptolemy’s Handy Tables (Halma ed.), Tyre does reside in two 9th century copies (published in Honigmann 1929), which are far older than our earliest mss of the GD, and each contains (fn 17) c.100 more sites (than GD 8): Tyre is city #307 in Vat 1291, #160a in Leid.LXXVIII. In the latter mss, Tyre is counted secondarily, which suggests that, if pairing occurred, Tyre was expendable. The superficially attractive interpretation is to wonder if GD 8 is a Byzantine-era add-on, which reflected a shrinking of the number of sites from nearly 500 to just 360. The problem with that theory is format: GD 8 differs generically (from all other surviving Important Cities lists, which uniformly are in longitude degrees east of the Blest Isles and latitude degrees north of the Equator) by: (1) using Alexandria (fn 14) as prime meridian (astronomer Ptolemy’s preference); and (2) providing data entirely in hours, just as ancient astroglogers preferred (§G2 ii[a]). This argues strongly that GD 8 goes back in time at least as far as Ptolemy.

ABLE observer. For once, analogies fail me. No other fantasy in scientific historical analysis has ever been so Orwellianly wild. If some oddities are more unique than others, then this one is uniquely unique. A4 Only in recent years have glimmers been detected (e.g., van der Waerden 1970 & Rawlins 1987) 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.

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 and assertively detailed in Rawlins 1991P) that heliocentrist such as Aristarchos obviously knew the planets’ mean distances from the Sun in AU (merely the ratio of epicycle/deferent 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 for 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.)

A6 Similar, 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 Hipparchus. Obvious to the 2 mens’ relative errors, random & systematic: Rawlins 1999 §§E3-E4. This particular hyper-inversion (started by Vogt 1925) is based merely the fact that semi-popular Hipparchus Comm commonly uses roundings which are much more crude than those in the Catalog or those in Hipparchus’ declinations (Almajest 7.5). Furthermore, these apologia utterly and entertainingly conflict with those emitted by Huber (DIO 2.1 §2 JH), Swerdlov 1989, Graßhoff 1990, & Gingerich 2002, who contend that Ptolemy’s greatness in data-reportage was shown not at all by his alleged observations’ superior accuracy but rather through the 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?

A7 Despite the 1999 Valins §F1, 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.)

A8 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.
Aristarchos: Ancient Vision 2008 March DIO 14 12

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.232). 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 Aristarchean universe reported by Archimedes p.232 has “as little to do with practical astronomy” as Aristarchos’ Experiment: eq. 4." B.Rawlins wonders if sellingputative Babylonian originality and genius has led Mufosiso 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 Mufosiso’s obsessive pretense, that geocentrist astrologers were brilliant, is glorifying the side that suppressed the actual great scientists of their time. Even the lily-white types & cardinals who popped up trying to cast those poor popes as heroes and added Gallileo as the actual top intellects of the medieval helio-vs-geocentrist dispute. So, in the field of outrageous historical-revision-apologia, the Mufosiso outdoes even the master.)

A7 The claim that Hipparchos “improved” heliocentrist Aristarchos’ measure of the universe is particularly curious, since Hipparchos and other geocentristis probably put the stars at roughly Ptolemy’s distance (ordmag 10 AU), vs. Aristarchos’ ordmag 10000 AU. (See §E5. Actual distance of Proxima Centauri = 270000 AU.) In brief, Mufosiso 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:39 Marionos’ Aroma latitute. So, what should be tested isn’t whether all but whether any post-Trajan geography appears in the GD.

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

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 “Ierosolomu [Jerusalem], which is called Ailia Kapitolias”. And GD 8.20.18 lists “Ailia Kop titias Ierosolomu” without further comment but obviously reflecting the same up-to-date situation. Therefore we have indication that both the GD’s data-sections (GD 2.7 and GD 8), previously added to date Marionos to c.110, actually contain material from the 130s or later.39

An example of the fruitfulness of the foregoing: Almost 2 centuries ago, H.Müller made the brilliant observation that a GD-listed N.German town “Siatoutanda”, was probably non-existent, just (another: fn 45) Ptolemy-compilation mis-read of a foreign language: Tacitus’ Latin description (Annales 4.73) of a N.German battle-retreat (“ad sua autuanda”). This does not stop our ancient geographer from providing (§C1) highly specific coordinates: latitude 29°1/3, latitude 54°1/3 (GD 2.11.27). As is 39These situations remind one of the common modern mis-interpretation (Rawlins 2002B fn 7 [p.12]) of Almajest 3.7 to mean that no Babylonian astronomical records came through to Ptolemy prior to 747 BC, though the actual statement is rather that continuous records went back that far. 37Over 4 centuries of botheration, Parthia repelled three Roman invasions: [1] swallowing the army of Crassus (suppressor & crucifier of Spartacus, and member of the 1st triumvirate), [2] exhausting emperor Trajan, and (after a temporary setback at Marcus Aurelius’ hands) [3] slaying last pagan emperor Julian the Apostle (unless he was fragged). And, yes, “parting shot” is thought to come from Parthian archers’ tactic of shooting arrows even when retreating or pseudo-retreating. 38Such an explicit update is rare in the GD’s data-body. Another such passage, even more unusually discursive, is found at GD 7.4.1, where it is stated that Tappobane (modern Sri Lanka [though known as Ceylon in Diller’s & DR’s youth]) was formerly called Simoundou but is now called Salike by the native. Comments are even (very atypically) added, describing Salike’s women and local products ranging from meal & gold to elephants & tigers. It seems likely that the mention of both Ailia Kop titias and Salike were late additions to the GD, a point we will shortly (fn 39) make use of. (Note: Tappobane [GD 7.4.1] is the same town in the GD, though [as in all of GD’s data] it should obviously have been covered before the GD listings get to China. I.e., we have here yet another symptom of a late add-on.) 39Following the revolt’s suppression, Judaea was re-named “Syria Palestine” and Romo henceforth (c.135) eliminated the term “Judaica”. The fact that it is retained in both the body (G2-G7) and Book 8 of the GD, taken together with the re-naming of Jerusalem leaves us with a bracket-argument in favor of dating Marionos to about 135, which is indeed of Ptolemy’s time — as he said. 40The “Siatoutanda” goof reminds one of St.Philomena, of whose “life” whole books were used to be written (DR possesses a copy of one), though she never existed: “Philomena” turned out to be just an
Aristarchos: Ancient Vision

2008 March

DIO 14 ¶2

numerological guesswork — even while the worthless & demonstrably (§F7) false numerological speculations of a succession of geocentrists and-or astrologers (see tables of Van Helden 1985 pp.27, 30, 32) are palmed off on the modern scholarly community as the best science available in antiquity,16 without even referencing dissenting literature.

A8 How could such a mix of innocence and prejudice (e.g., fn 14) adorn a standard (gov’t funded) history-of-astronomy survey volume, written by historian (& sometime JHA Adv Editor) A.Van Helden? The answer is found in the ancient astronomy archons he depended upon. Van Helden 1985 p.vii (see also p.168 n:2): “In the course of this project I incurred many debts. . . . A Research Fellowship from the [NEH] . . . . For the medieval and especially the ancient [episodes] 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-dispersion limit of man’s vision to be about

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

(1) then (for null visible stellar parallax), shouldn’t his distance \( r_2 \) 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.

16 The cause of this imposition (and presumably of the who-cares-who-was-right-or-brave-or-ethical-or-original idee-fixe of the modern ancient-astronomy establishment: fn 67) is simply that the number of extant ancient texts created by competent scientists is tiny compared to the lot of superstitious pseudo-science that survives. Thus, realistic grantsmanship 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 this defensive line of attack has been less defensive than expected. See esp. Alex Jones’ analyses.]

17 For the terminator to deviate more than 1/1000 of a radian from straightness, the Moon’s horns must deviate 1/5000 of a radian from the middle of the terminator (§C4). The arcsin 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 1/1000 of a radian, yielding 2°57’ by the same equation.) Note that DR has not arbitrarily conjured-up \( \mu \approx 0.4 \) for the purposes of this paper: Rawlins 1982G (p.263, in a quite different context) noted that the mean angular separation of the retina’s foveal cones is 0.4° – 0.5°. (The arcsin of 0.45/15’ is 3°26’ ≈ 3°.) I found by experiment long ago that the eye’s primitive visual limit is about 1/3. (The arcsin of this divided by 1/14 is 2°33’ ≈ 3°.) Aristarchos presumably performed just such an experiment to arrive at his value for \( \mu \). These estimates agree closely with Dawes’ limit (consistent with diffractive Airy disk) for a human eye’s pupil-size, and all fluctuate around \( \mu = 1/10000 \) 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.)

18 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 certain, but I lean to believing that the original use of 10000 in eq. 13 was Aristarchos’.
B2 Thus, I realized at a stroke that all the famous Aristarchos astronomical scale measures could turn out to be consistent with the very same empirical base, namely, the limit of human vision was experimentally realized by Aristarchos to be about 1/10000 of a radian, 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.)

B3 It may seem remarkable that no one previously noticed this. But such an astonishing oversight is, in fact, precisely20 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 science21 behind either A or B. (Muffia disability here is seasoned with naked contempt22 for non-Muffia scholars who try.) Such work is more apt to encyclopedist-bibliographers, than to thinking scholars. (Few Muffia capos are scientists. They naïvely presume that some mathematics background will suffice to protect23 them from misperceiving ancient methods; but: this presumption is just one more Muffia misperception. The idea that practical experience in relating empirical data to theory might be of use in doing history of science would seem to be self-evident. Not to Muffiosi.)

C Moon & Historians in Retrograde

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

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

20 Since a hallmark of the Neugebauer sales-cult is its consistent confusion of superstitious ravings (e.g., §§A3&A7) with genuine science, one can readily understand how this clique got into the habit of typing at the very idea of attempting to relate ancient texts. See, e.g., Gingerich 1976’s hyperagonistic-alibi-quotes defending Ptolemy (taken from Neugebauer 1975 pp.107-108), etc.: “It makes no sense to praise or condemn the ancients for the accuracy of or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure”. (Compare such added archdial naivete to the realities of §F9 and §1.2.) This astonishing bit of mis-megalithism (definitively vaporized at §§E2&K4 and fn 9) was dished up to excuse Ptolemy’s Almagest 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 naïvely 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. Just the sort of appreciation scientists pour out for lives for. (See fn 67 & Rawlins 1993D §B3.)

21 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 non-Muffiosi not to emulate such arrogance and to instead appreciate that even ill-mannered bigots can make genuine contributions, which should be treated strictly on their merits.

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

23 Note: not a single historian has yet indicated publicly that he understands this rather self-evident point. (Though some have privately.) Which gives us hope that sociology can yet attain to the predictivity of astronomy. (See 12 Epilog [p.31].)

24 GD 8.2.2 by the arrangement of B&K or 8.B.2 in Diller 1984 (the only reliable English translations) at DIO 5.

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

26 This is a revised & mutilated re-hash of an original Hipparchos estimate that Okelis was on the arc (eccentric-visible) circle of α UMi? — which would have been correct in 170 BC and OK to ordnag 0°.1 during Hipparchos’ time (130 BC).

27 GD 8.2.3, 28 Thus, the GD 1.7.4 discussion seems artificially strong52 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-precessionally-obsolete NPD (North Polar Distance = declination’s complement) for α UMi. (i.e., modern “Polaris”: the brightest star in UMi, and the most northern 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’ closest epoch, −126.278 (128 BC Sept.24 Rhodos Apparent Noon: Rawlins 1991H eq 28 [p.58]). Marinos further states that this parallel is 1° north of Okelis, which he mis-places (§C1) at 11°25’ N latitude.

29 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 agreement with the Almajest certainly knew (Alm 7.2-3) the math of precession. Comments:

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

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

H5 Has it been noted that, by the time of Marinos & Ptolemy, α UMi was (thanks to precession) no longer the most southern of UMi’s seven traditional stars?! — η UMi and especially 3rd magnitude γ UMi were much more so. Indeed, for the time of the GD, γ UMi was over a degree (1°04’ at 160 AD) more southern than α UMi. (Shouldn’t the “Greatest Astronomer of Antiquity” [12 §G2] have known this? — especially since he pretended he’d cataloged the whole sky’s stars: Almajest 7.4, i.e., the GD 1.7.4 statement on α UMi disagrees not only with the sky but with Ptolemy’s own tables.31) Similar cases at fn 45.) Thus, γ UMi had long since assumed the distinction (one interjected by Marinos, ironically) of being the oldest star whose NPD determined whether a geographical region was far enough north to attain UMi-ever-visible. (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 paltry 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,5 as noted in GD 1.4. This, because based upon comparisons of lunar-eclipse local-times,26[c] Thus we have arrived at a hitherto-unappreciated realization (obvious example at fn 16): ironically, every jot of the astronomically-determined data of the basic network of cities underlying GD 2-7’s thousands of degree-expressed positions, was at some point (during its mathematical descent from its empirical base) rendered in time-units: hours. As proposed in Rawlins 1985G.

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

25Wrongly (fn 45), Ptolemy believed (GD 1.4&12-13) that eclipse-based longitudes were rare. (The method of finding longitude-differences between sites by comparing local times of simultaneously-observed eclipses was obviously well-known. See, e.g., Strabo 1.1.12 or GD 1.4.2. Least-squares tests on ancient longitudes showed that the eclipse method had been extensively used by genuine ancient scientists: Rawlins 1985G §§5&89 [pp.258-259 & 264-265].) And so he assumed that generally-accepted longitudes were primarily based upon travellers’ stade-measured distances (terrestrial) instead of eclipse-comparisons (celestial) — a crucial, disastrous error, which undid generations of competent scientists’ eclipse-based accurate longitudes-in-hours and thereby wrecked (§L3) the GD’s longitude measurement accuracy in angle. (Though not in distance: idem.) Note: said mis-step must have occurred before the hypothetical dovetailing (fn 21) of GD 2-7 and GD 8, perhaps (§D1) in the 1st century BC.

26A number of network-cities’ GD 2-7 longitudes could have been calculated directly from GD 8 or its source, using Alexandria (D149) longitudes (east-of-Blest-Iles) 60°/2 (GD 4.5.9) or 60° (GD 8.15, 10). Some examples:

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

However, there could as easily have been computed in the other direction. The majority of less grid-critical sites’ degree-coordinates couldn’t (§D1&D5) have been computed directly from those of GD 8 (at least in its present state), but could’ve gone the other way; e.g., Smyrna (5.2.8, 8.17.11, D179) & Pergamon (5.2.14, 8.17.10, D178).

Given the GD as it stands, if GD 8 is contended to be the direct ancestor of GD 2-7’s longitudes, one would have to argue that the underlying network-basis was far less in number than GD 8’s 360 sites — which, if we are speaking of sites whose longitudes (vs Alexandria) had been astronomically determined, would not (in itself) be an unreasonable contention.

Aristarchos: Ancient Vision 2008 March DIO 14 ¶2

has been universally accepted23 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 treatise. Perhaps pseudo-A’s hazy perception of Aristarchos’ astronomy is related to his resented corpus’ near-extinction 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 nonsense-propositions (Heath 1913 pp.329f & 352f; Neugebauer 1975 pp.635f):

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

\[ \theta_R = 1/2 \]  (2)

obviously false & explicitly contradicted by Archimedes, who reported24 that Aristarchos’ solar diameter was indeed the very accurate value 1°/2 (vs actually 32°), thus semi-diameter

\[ \theta_A = 1/4 \]  (3)

Rawlins 1991P fn 6 eliminated the contradiction by proposing that the factor of-4 error was based on misreading the Greek word μεγαλόν (“part”) as a zodiacal sign (30°) rather than the Greek-measure unit called “part” (7°/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 (net) barely 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 upper-culmination Moon MUST DAILY BE OBSERVED MOVING IN RETROGRADE25 against the background of the stars. (Already noted at §1 fn 3&5.) Though this is an inevitable consequence of pseudo-Aristarchos’ work, it has not been noticed by commentators, 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.”)

24The 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.

25The failure of prior historians, to face the outlandish absurdities of the pseudo-Aristarchos ms, is a mystery. (None has previously realized that it entailed a retrograde Moon, despite our broad knowledge this comes nearest to fully realizing the ms’ folly — but then attacked Aristarchos instead of the ms’ attribution); also Evans 1992 p.68.

26See the equally-ironic comments at 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?

27The equally-ironic comments at DIO-JHA 1.2 fn 284. With respect to the strange controversy (Rawlins 1991W fn 3) 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 (sars remaining: Rawlins 2002A eq.6).

28The “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?

29See the equally-ironic comments at DIO-JHA 1.2 fn 284. The Neugebauer 1957 p.192 passage (there compared to p.206) was first brought to DR’s attention by the late R.Newton.

20In this handsome photo [www.doi.org/jha.htm#mms], the Moon is seen in its rising aspect (obvious to an outdoor astronomer) low behind the camera-facing Sphinx. But the Sphinx faces eastward.
C3 Let us see how the deliciously zany retrograding consequence ([C1][e]) comes about. Pseudo-Aristarchos’ implicit28 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$^{3/2}$.32 (mean sidereal motion 0$^{\circ}.549$/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 27.4/20.1 = 1.36 times faster29 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] suggested31 that the famous Aristarchos value

$$\gamma_A = 3^\circ = \arcsin(r_M/r_S) \approx \arcsin(1/10)$$

(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

29The 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$^{\circ}.1$ distant from pseudo-A’s Moon, so the Moon’s relative hourly angular “topocentric” or observer-centered motion is (20.10 – 27.4/20.10 – 1) times the mean geocentric sidereal lunar motion (0$^{\circ}.549$) or – 0$^{\circ}.2$. (Obliquity’s cos = 92%, ignorable for rough mean-situations: [a] when the Moon is on the equator, the Equator in front of it is parallel to the terrestrial observer’s equatorial perspective; [b] when the Moon’s geocentric motion is parallel to the Equator, the Moon is not on the Equator.)
30Maximum 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 lunar angular speed when the center is on the Equator (not anywhere actually). But this is true only when the Moon is in the observer’s zenith. When the Moon is near the equatorial nadir, 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 ([C11] of the Moon’s distance $r_M$. Yet another reason for the credibility 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.

31A 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 $3^\circ$ — faithfully converting a physical argument (“perception”) into the orthodox Neugebauerism cited above at §A1.

32As 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

Ptolemy’s GEOGRAPHY 2008 March DIO 14 ¶3

F Blest Isles Ignored & Identified: the Cape Verde Islands

F1 Conversely, the Blest Isles, the GD ekumene’s west-bound (and GD 2.7’s implicit prime meridian), have no GD 8 entry. In GD 8, this linch-pin site is only mentioned at two places, rather in-passing: at GD 8’s prime meridian Alexandria (GD 8.15.10) and at the GD ekumene’s east-bound, Thinai (GD 8.27.13), where it is noted that Thinai is 8$^{\circ}$ east of Alexandria and thus 12$^{\circ}$ east of the Blest Isles.

F2 Yet another oddity: the GD repeatedly states that the Blest Isles are the west bound of the ekumene. (Though, curiously, not at GD 7.5.2., even while soon after saying so at GD 7.5.14.) When the writer of GD 1 does not explicitly state that all the longitudes of GD 2.7 will be measured from the Blest Isles; and the Blest Isles have no entry in GD 8. Its position appears33 under Africa at GD 4.6.34. Additionally, one notes that there is not a single absolute longitude in GD 1 — every longitudinal value is given in strictly differential terms. Now, if one is writing a preface to a compendium that provides the longitude-east-of-Blést-Isles of 8000 sites, one would think that the east-of-BI part just might get mentioned somewhere. Instead, GD 1 is completely non-committal regarding what will be the prime meridian of the work. And GD 2.1 (the preface launching the reader into the 8000 sites) is likewise. (If one were just grabbing — virtually unedited — a preface to another work, something like this could easily happen.)

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

F4 GD 4.6.33 lists some non-zero-longitude off-shore islands, incl. “Kerne” at 5$^{\circ}$E & 26$^{\circ}$N, latitudinally & phonetically near the Canaries which at (actually) c.28$^N$ are the better part of a thousand miles north of Ptolemy’s six “Blest Isles”, listed by him (Nobbe ed. GD 4.6.34 at longitudes 0$^{\circ}$ (four) or 1$^{\circ}$ (two), at latitudes ranging from 10$^{\circ}$1/2 to 16$^{\circ}$6; about right for the Cape Verde Islands. (Actual CVI latitudes: c.75 nmi N of Dakar, Cap Vert) at latitudes that are again a convincing match for the Cape Verde Islands, which are therefore firmly identified as the Blest Isles.

F5 The GD’s knowledge of the Cape Verde Islands stands as a testament to ancient explorers’ courage: they are indeed c.400 mi from Cap Vert, the mainland’s nearest point. (By contrast, eastern Canaries are barely off the NW-Africa shore.) So the islands’ discoverer was himself the nearest thing to an ancient Eriksen or Columbus. Over 1000$^{\circ}$ before sailors discovered taking, trips there were presumably extremely rare and hazardous. Possibly galley-slave rowing-power was the key to the ancients’ knowledge of the Cape Verde Islands. And perhaps they were regarded as Blest because European civilization had not yet significantly uplifted the inhabitants by the introduction of their ever-brewing wars & their ever-rewarding slavery.

28Thanks to Alex Jones for reminding DR of this.
29E.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 5$^{\circ}$ of the 6 islands listed at GD 6.4.34 is named “Kanaria Nesos”.
23E.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 5$^{\circ}$ of the 6 islands listed at GD 6.4.34 is named “Kanaria Nesos”.

E  **GD’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 correlates 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. Which argues against GD 8 being computed directly from GD 2-7 or vice-versa.

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

E4 DR has noted something similar: throughout GD 1, there is no mention of Alexandria. 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 multiple-conjuring plagiarist (Pickering 2002A; Duke 2002C), one may ask: is it credible that

---

19 Rawlins 1982G p.263 fn 17. That 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 a lost geographical tradition (§C1) of geographical tables, which Marinus (note GD 1.17.2’s semi-connexion of astrologers’ klimata to Marinus) and or-Ptolemy forced to assign to the flawed important-cities latitudes of? Just as usually-equant-prefering Ptolemy may’ve felt forced to go along (in the Almajoest) with Hipparchus’ 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 sections (whether at the outset or during later editors’ touchings-up) — though there are occasional inconsistencies, e.g., the longitude of Rome (D49): GD 1.16.1 puts Rome 36° 23/2 west of the Fortunate Isles, while GD 8.8.3 puts Rome 1°5/8 east of Alexandria. (Itself 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° 23/3) (15°/hour) ≈ 1°7/12 < 1°5/8. Similar incompatibility: Salinae (GD 3.8.7, 8.11.4, D79). See also §K3.

22 Nobbe 1.46 inserts Alexandria at the 14th klima (GD 1.23.9), but it is clear from Müller 1883&1901 (1883) 1:57, &B&J pp.85&111, and &S&G 1.116 n.4 this that was not the original, which (in GD 1.23) named only four klimata north of the Equator: Meroë [D165] (13°), Syene [D154] (13°1/2), Rhodos [D189] (14°1/2), Thule [D11] (20°). Selection repeated GD 7.5: &B&J p.111. Note that Alexandria [D149] is mentioned at GD 7.13.14.

---

D6  **Suggested Solution to Two Mysteries** As shown in the tables of Rawlins 1985G p.262. GD latitude-errrors for major cities are often sph-trigonometrically consistent with the §B1 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.19

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

C5  We have seen earlier from Eusebius (§I.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., §I fn 4] cannot be traced back beyond Eratosthenes). And this is the figure computed from pseudo-A’s propositions 11&17 at Heath 1913 pp.338-339. Yet Heath bases this upon averaging depressingly crude brackets associated with needlessly pedantic geometric proofs. By contrast, an exact computation (e.g., Neugebauer 1975 p.637) finds 20 Earth-radii instead of 19:

\[
\tau_M = \frac{1 + \sin \gamma_A}{(1 + \sin \theta_P) \sin \theta_F} = 20.10
\]

using pseudo-A’s false data (§C8 & eq.2): shadow-Moon ratio \(v_P = 2\) and solar semi-diameter \(\theta_F = 1^\circ\). Question: if you wished to find 1/1° or (virtually the same) the distance/size ratio for something subtending 1°, wouldn’t you just figure that the circumference is 2\pi times the distance and 1° is 1/360 of that, so distance/semi-diameter = 360/2\pi = 57.3? (The pseudo-A brackets instead can only put the number somewhere between 45 & 60! It’s hard to accept that Aristarchos was this limited.) Is there more a reasonable explanation for why a very simple computation which should have produced 20 instead got 19? [Our next speculation parallels known Hipparchan researches: Alm 5.11.] Try this: since DIO has for years pointed out (§C4) that \(\gamma = 3^\circ\) is an upper bound (not an exact figure), why not explore the obvious consequence of this assumption, namely, that Aristarchos (not knowing where \(\gamma\) was in the range 0° to 3°) simply made it null for solar distance \(r_S \approx \infty (\gamma = 0°)\). In that case, eq.5 becomes:

\[
\tau_M = \frac{1 + \sin 0^\circ}{(1 + \sin \theta_P) \sin \theta_F} \pm 19.100
\]
C6 In addition to the flock of pseudo-A difficulties cited above (§C1 & fn 32), Rawlins 1991 W §R10 also revealed a hitherto-unnoticed 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, 24 is the astronomy behind pseudo-A.

C8 Having thus already (§C1[a]: “μερος”) cleared up pseudo-Aristarchos’s most obvious absurdity (eq.2: 1° iradial semi-diam 6θ0), we check another highly suspect pseudo-A statement, namely, that, at the Moon’s distance, the pseudo-Aristarchos ratio r2 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 ρ stand for the Entirety/Totality ratio, we have pseudo-A’s ρ8 = 3 (eq.10). But it is well known that an eclipse’s maximum possible Entirety is instead just under 4, while maximum possible Totality is slightly more than 18/3/4 — that is, roughly 25 — creating an Entailing/Tot ratio ρ of barely 2 (far short of Ent=Tot = 3). For the mean distance situation, the actual shadow/Moon ratio v = 2.7 (corresponding to Ent/Tot ratio ρ = 2 1/6: fn 35). And we know that Hipparchos used v2 = 2.5 (Almajest 4.9), while Ptolemy used v2 = 2.6 (Almajest 5.14). So how could an observing astronomer set v2 = 2 ?! The basis for estimating ρ = Ent/Tot is eclipse records. (And Aristarchos may have researched more from such records than any other Greek of his day: DIO 11.1 [1].) The simplest method would be to use central eclipses (Earth-shadow & Moon concentric at mid-eclipse): those for which the lunar path virtually bisects the shadow. By averaging a few empirical duration data from such central events, one may (eq.7) compute v from the ratio ρ of the time of an Entire umbral eclipse to time of Totality for (central eclipses), which is (crudely) 4/13 ρ2 = 2, a figure that reveals (via eq.7) v to be much nearer 3 than 2. Even aside from Aristarchos’ access to centuries of Babylonian eclipse records, he could have observed first-hand the 21-digit eclipse of −286/5/20 (ρ = 2 1/5): and-or the 19-digit eclipse of −279/6/30 (ρ = 2 1/4), which occurred just a few days after his famous Sun eclipse observation. Such easy observations would make it clear that v was nowhere near 2. One possible cause of pseudo-A’s wacky v2 = 2 is amateurish confusion: pseudo-A carelessly took ρ (something about in-shadow, wasn’t it . . . ?) to be v. (We already know from §§A1&C1 how easily confused pseudo-A was.) Keep in mind: the Entire/Totality ratio ρ is an easy raw-empirical number, while v is derivative. Another possible explanation of the pseudo-Aristarchos v-vsp foulup arises quite naturally from an examination of the neat inter-relationship between v and ρ:

\[
v = \frac{\rho + 1}{\rho - 1}
\]

(7)

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

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

(Eq. 1) as ρ = v + 1 v − 1 (eq. 10) caused δp = 1. 

Equation 2, 3, 4, & now 6 manuals: astronomical tables, geographical tables, & interpretational handbook. Ptolemy’s prime works were: Almajest, GD, & Tetrabiblos.

[15] It is common knowledge (§L6) that the longest-day value (GD 8.20.27) for Babylon (D256), 14°5/12, is a rounding of 14°2/5 — which is 3/5 of a day and the M basis of computation (82G2 (c)) of the revealingly inaccurate latitude L = 35°5 (GD 5.20.6), 2°28’ (148 nautical miles) too far north.

[16] There remains the question of whether Hipparchos was responsible for the fateful step of converting (via eq.1) crude tabular longest-day M values from hours to degrees of latitude L. In the light of Dr. 2007 realization (www.dioi.org/cot.htm#hrbc) of just how admirably accurate Hipparchos’ longitudes may’ve been, the odds that he was not the culprit are enhanced. Has the remarkable irony been noted that the Geographical Directory (at GD 8.1.1) itself scoffs at the common folly of chumping cities under parallels? Or that this contradicts GD 1.4.2, where Ptolemy is praised for his alleged aloineness in performing the very same chumping? Of course, the GD 8.1.1 complaint is merely that parallel-lists [like the pre-Ptolemy one of Pliny (77 AD); analysed at Rawlins 1985 p.262] waste time and space, but the statement is valuable in its suggestion of ancient currency of the very lists upon which the DR theory is founded. (Said currency could help a defense of Hipparchos as not-necessarily the unique source of the GD’s macro-errors; however, his attractive fame and his citation by both Marinos (GD 1.7-4) and Ptolemy [Almajest, passim] argue in favor of his culpability here, though see speculation above [in this fn], on his longitudes.) We needn’t speculate anyway, on the existence of lists of a few hundred key cities’ coordinates. Just such a list survives, e.g., in the Handy Tables, the Important Cities table of which (N.Halma 1.109f [1822]), appears closely related (§K4) to GD 8 in both quantity and sequence: 364 sites in all, with 12 not in GD 8, and 8 missing in HT. See also the two Important Cities lists (in 43) provided in E.Honigmann 1929 [pp.1936]; Vatican 1291 [493 sites] and Leidenix LXXXVIII [a comparable number of sites]. These lists’ positions are [like GD 2-7] given entirely in degrees east of Bllest Isles and north of the Equator.

16See [2 fn 67 & DIO 2.1 §13] [C10.31].
there are plenty of hints (e.g., Memphis' 13°57′n: 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 astrolabist's dream Handiest 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 hours.


D3 B&J p.29 notes (as did Rawlins 1985G pp.261f) specific cases where key cities' latitudes must have been computed[14] 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 was 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 essence. Though the maps’ margins bear longest-days marks (inevitably at large latitude intervals), the densely-marked, nominal north-south 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 patchwork authorship?[3] What evidence connects Marinos to the construction of GD 8?[5] The absence[1] 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 (§E & §G1) that GD 8 is not directly connected to Marinos-of-Tyre’s Books 2-7. So it would be wrong to over-claim that GD 8 is the father of GD 7-2. 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 (11 §D2), 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 & astlogers); so it conclusively favors neither side on the relation between the GD’s two data-sections.

[15] In Nobbe’s edition, at GD 8.20.18 (Jerusalem D247) the spelling of “east” changes from αντωτολας (anatolas) to εαο [eo] for most of the rest of GD 8. If the switch (which occurs only in some miss) 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 to the 2 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 \( \nu = 2 \), 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 \( \rho \) just\(^35\) exceeds 2, and no lunar eclipse datum is easier to find. Thus, it is not credible that Aristarchos opted for \( \rho = 3 \) — 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 \( \rho = 4/\sqrt{2} \) 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 \( \nu_A \), so we can be pretty sure Aristarchos used:

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

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

\[ \nu = 2 \quad \rho = 3 \quad (10) \]

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

[a] misunderstood a reference to \( \rho \) (commonly known to be about 2) as a reference to \( \nu \), or
[b] simply confused Aristarchos’ \( \rho_A = 2 \) & \( \nu_A = 3 \) with each other! (Easy mix-up for an amateur, since, as eqs. 7&8-9 have revealed: when either of the two variables equals 3, the other 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 equation\(^36\) (e.g., eq.5) and thus obtain:

\[ r_M = \frac{1 + \sin \gamma^M}{(1 + \nu_A) \sin \theta^M} \approx 60^\circ \text{ or } 51^e \]

(11)

for \( \gamma^M = 3^\circ \) (eq.4) or \( \gamma^M = 0^\circ \) (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 \( r_M \approx 60^\circ \) 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 \( \nu \) (or \( \rho \)) and \( \theta \) and both of these are easy to find accurately enough for that purpose. (Keep in mind that Aristarchos knew the Moon’s period to a precision that certainly doesn’t sound like a mere “theoretical” math-pedant: §F9 vs. §A1, fn 20, & fn 34.) In fact, the idea that Aristarchos was so ignorant as to mistake \( r_M \) by a factor of roughly 3 (20² : §C3 & eq.5) — or even a factor as large as 4/3 (80² : 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:

\[ r_S = r_M/\sin \gamma_A \approx 60^\circ / \sin 35^\circ \approx 1146^\circ \approx 1000^\circ \]

(12)

\(^35\) In reality, mean \( \rho \approx 2.13 \), as one will find from a glance through an eclipse ‘canon’ or by substituting \( \nu = 2.7 \) (§C8) into eq. 7.

\(^36\) Almajest 5.15 or Rawlins 1991W eq.27. This equation depends upon setting the solar & lunar semi-diameters equal to a common \( \theta \).
The obvious large uncertainty in $\gamma$ justifying rounding\textsuperscript{37} 1146¢ to 1000¢. Such a step could have triggered the later tradition — discovered at Hipparchos eqs.23&24 of Rawlins 1991W — of dividing\textsuperscript{38} the AU into units of thousandths: 1 AU = 1000¢.

D2 About 900 AD, Al-Battani’s solar work, explicitly building upon the remains of Greek solar theory, exhibited precisely $r_S = 1146¢$ (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 his general formula.)\textsuperscript{40} It is not clear whether any of these values were directly attested. Thus, given Al-Battani’s use (§D2) of 1146¢ (eq. 12), we may conclude that: [a] the value 1146¢ is the preferred choice (of those discussed here) for Aristarchos’ early $r_S$ (see also fn 37), thus [b] our 2 emendations (eqs. 3&9) are not disconfirmed.

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

E1 The irony is that Aristarchos’ famous Experiment was far inferior\textsuperscript{49} to his greatest heliocentrism scale-contribution. As remarked here at §B1, Aristarchos thought out the implications of heliocentricity to their astonishing and historic conclusion: the absence of

\textsuperscript{37} The hypothetical rounding of $r_S = 1146¢$ (to 1000¢) would produce a slight inconsistency in eq. 12; for $r_S = 60°$, one would yet imply $r_S = 3°26'\approx 3.4°$. Note that 1146¢ is much nearer 1000¢ than any previous scholar’s estimate of Aristarchos’ value for $r_S$; §D3. From fn 18 or eq. 13, we see that Aristarchos ultimately may have ordmag-rounded $r_S/1000$ to 10000. In any case, Rawlins 1991W eqs.23&24 prove that he (at least initially) and/or later followers rounded 1146¢ to the nearest ordmag, 1000¢, or divided the AU into a thousand milli-AU: 1000¢. Whether or not these ancients’ micro-measure was Earth-radii, the 1991 analysis shows that their macro-measure was heliocentrically AU-based.

\textsuperscript{38} Whatever its origin, this standardization does not imply perpetual consistent identification of $\pi$ with $\pi$, though such an equation may well have had at least passing popularity. It seems that, during the 3rd century BC, $r_S$ was initially (from Aristarchos’ Experiment) set at ordmag 1000¢; and then later (due to failure to observe planetary diurnal parallax, as noted here at §F), heliocentrist astronomers (contra geocentrist: §F5) enhanced $r_S$ an ordmag, up to 10000¢ — the same Archimedian myriad ratio also adopted for $r_S/r_S$ at eqs. 13 & 14.

\textsuperscript{39} It is always possible that the values broached above ($r_M = 60°$ & $r_S = 1146¢$) actually came from a completely different source than here suggested. Swerdlow 1969 has made a persuasive argument that Hipparchos’ $r_S = 490¢$ was based on an adopted solar parallax of the rounded value 7¢. Similarly, if an ancient had adopted a rounded solar parallax of 3¢, he would (as independently noted at Van Helden 1985 p.31) deduce $r_S = 180°60'/3(\pi/2) = 1146¢$ (a figure later used by Al-Battani: §D2 & fn 57) — and he could then, from a rearanged version of eq. 12, arrive (backwardly & shakily) at $r_M = 60°$. On the other hand, it might be that, if Hipparchos concluded for $r_S \approx 490¢$ (Swerdlow 1969), he did so (as he did so much else, e.g., Rawlins 2002A fn 14, 16, & 17) following Aristarchos’ lead, which in this case would probably mean building upon $\gamma$ rather than solar parallax. If he adopted $r_M = 60°$ from Aristarchos (eq. 11), and believed he had measured $\gamma = 7°$, then he would revise eq. 12 to yield $30° = 60°/7° = 1146¢$ (a figure later used by Al-Battani: §D2 & fn 57) — and then shift to $\gamma = 9°$; he might have inconsistently computed $77°/sin 9° = 492° \approx 490¢$. For Hipparchos’ $r_S = 77°$, see, e.g., Swerdlow 1969 pp.289.) Van Helden 1985 p.167 n.8 supplies similar speculations.

\textsuperscript{40} The intimate relation of Aristarchos’ Experiment to heliocentricity is seldom mentioned in modern textbooks (perhaps due to the ironic geocentrist-preference noted at fn 27), though obvious from the Experiment’s large implied solar volume: Rawlins 1991P §C3. That the Experiment & heliocentricity are due to the same scientist is thus implicitly regarded as merely a coincidence!

10 See Rawlins 1985G pp.255-256, as well as Rawlins & Pickering 2001; see also DIO 13.1 [2003] (www.dioi.org/vols/wd1.pdf) [pp.2-11]. Similarly, Hipparchos knew his own latitude, but seems (§B1) to have been weak elsewhere, e.g., placing Athens a degree south of its actual latitude (Hipparchos Comm 1.1.13&8) and Babylon 2°1/2 north (§L6) of its — both values copied (fn 10) by the GD.

11 This is an important consequence of the GD’s low $r_S$ and high $r_M$, which would raise ‘torres’, илиtorres’ or ‘solars’ in a different way, if at all. (An astrophor.) Possible, but — as already noted [§C1] — his reckoning longitude in degrees and from Best Isles is contra this idea.) Were famous ancient astronomers analogous to modern popular-science writers and publications (www.dioi.sno.htm), where ubiquity, lucre, and hype obscure innumercy, thereby nourishing blind-leading-blind multi-generational replication (e.g., www.dioi.stt56.html#r8b) of unreliable scholarship?

12 This is not to say that some GD calculations went in the other direction (§D1) — nor even to reject the distinct possibility that GD 8 was entirely computed from GD 2-7 (data themselves already corrupted by calculations from a prior pool of longest-day data) as alleged. But some differences (fn 7) in the two mss-traditions (Diller’s XZ & UNK) occasionally remind us that post-2nd-century AD revisions of the GD 8 values may have attempted arranging consistency, in the same spirit that latitudes in GD 2-7 were adjusted at some point (at or before Hipparchos’ era), according to the DR theory of the GD. Note that the latter theory (§D1) has here been limited to proposing the high likelihood that data of the sort (§D4) provided in GD 8 underlay GD 2-7’s major cities.

13 B&J p.14 n.10 show excellent judgement in rejecting a misguided but persistent tradition of manipulating the stanze, to force disparate ancient Earth-measures to agree with each other or reality. See also §1 J1; Rawlins 1982N; Rawlins 1996C §1 §C14 & fn 47 [p.11]. The formerly unpopular but evidently-inconsistent fact (§1 §C5) that Eratosthenes’ Earth-circumference was (slightly) (not illusorily) high by 1/5, and Marinos-Ptolemy’s too low by 1/6, is shown by at least 5 considerations: [1] Ptolemy’s 4/3-expansion (130°=172°1/2) of the Rome-Babylon longitude-difference, between Aby & GD. [2] The GD’s similarly large (33%–40%) systematic over-estimate of many actual longitudes, See the least-squares test of Rawlins 1985G p.264, leading to p.265’s table of reconstructions. (The first scholar to sense that ancients had multiplied their adjustments by adjustment-constants when adopting new Earth-measures, was Ptolemy. Or, if Hipparchos indefensibly stuck by an early value [Rawlins 1991W §R] $r_M = 77°$ [itself based on $\gamma = 3°$] and then shifted to $\gamma = 9°$, he might have inconsistently computed $77°/sin 9° = 492° \approx 490°$. For Hipparchos’ $r_S = 77°$, see, e.g., Swerdlow 1969 p.289.) Van Helden 1985 p.167 n.8 supplies similar speculations.
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 \( r_S = 1 \) 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 (§B1) that he used \( 1/10000 \) of a radian for that limit. Thus, from eq. 1, he would have set

\[
\frac{r_S}{r_S - \mu} = \frac{10000}{r_S - 10000} \quad \text{AU}
\]

(13)

So it is gratifying to find this result is actually testified to (§B1) as a limiting distance by Archimedes’ “Sand-Reckoner”.\(^{41}\) But such a scale, though (§E1) much more important than the famous “Aristarchos Experiment”, is far less known today. Exceptions are Heath 1913 (p.348) & Neugebauer 1975 (pp.646&656).

E3 Yet it is not difficult to reconstruct the empirical basis. Àetios (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 on the same 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: 3.7° in longitude, 3.3° in latitude.\(^{43}\) The searched-for parallactic motion would be almost entirely in longitude. Yet it is certain\(^{44}\) that no such relative motion was ever observed. An ancient might aibi 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\(^{45}\) 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?\(^{47}\) Giedi’s nearest star (\( \alpha^2 \) Cap)

\(^{41}\)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 geo-centric 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).

\(^{44}\)Vale BSC parallaxes: for 5017 Cap (HR7747) \( 0^\circ.006 \); for 607 Cap (HR7754) \( 0^\circ.034 \).

\(^{45}\)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.)

\(^{46}\)See fn 45 and conclusion of §E3.

\(^{47}\)Even aside from its Earth-immobility: Ptolemy’s conception had all the stars’ distances the same (Almajast 7.1, Van Helden 1985 p.27), so the Giedi experiment here described would doubly make no impression on him. But one suspects that his demand for uniform stellar distance was designed to defuse (by anticipation) heliocentrics’ potentially troublesome parallactic-questions.
were, say, 1000 AU distant and \( \alpha^3 \) Cap much\(^{48} \) more remote, then the 2 stars’ relative positions in April vs. October would correspond to baseline 2 AU (see fn 18) — and thus: a
total eclipsal parallactic swing of about 2.3438/1000 or 7'. As noted above, the eclipsal 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 eclipsal parallax —
which thus predicts the unmissable spectacle of \( \alpha^2 \) Cap oscillating semi-annually, from
eclipsal near-conjunction (October) with \( \alpha^1 \) Cap, to being (April) distant by an angle equal
to c.1/2 the lunar semi-diameter! Obviously, no such effect was observed — and
careful occult monitoring of Giedi and similar star-pairs would have produced an ample
rebuttal of all results. For heliocentrists, said null-parallax results would rule out the
premise that the stars were merely 1000 AU distant\(^{49} \) — and thus supplied the empirical
basis underlying ancient heliocentrist’s \( \textit{scientific} \) (not “theoretical”)\(^{50} \) conclusion for eq. 13:
stars without annual parallax had to be at least another ordermag distant, namely, 10000 AU.
E5 But we need not speculate on the existence of such observations, since it is obvious
from \textit{Almajest} 7.1 (c.160 AD) that, indeed, the ancients had carefully measured lineups and
relative positions between stars. And the same source is clear that no such stellar shifts
had ever been observed — which is why (until Halley) the stars’ relative positions were
regarded as “fixed”\(^{51} \). So the logical conclusion for heliocentric visionaries\(^{52} \) would be
that the stars were roughly 10000 AU distant (or more), as already expressed in eq. 13.

F Later Heliocentric Improvements

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

\[
r_\text{parallax} / r_\text{parallax} = r_T / 10000
\]

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)?\(^{53} \) we know (§F2 & eq. 14) that astronomers did so shortly thereafter.

F2 Kleomedes 2.1 reports (Heath 1913 p.348, Neugebauer 1975 p.656, Kidd 1988 p.445) that Poseidonios (1st century BC) considered the possibility that the Sun was (at
least: fn 18) 10000 AU distant.\(^{54} \) This is already given in eq. 14, namely:

\[
r_\text{parallax} = 10000 \text{AU}
\]

\(^{48} \) Apparently dimmer \( \alpha^1 \) Cap is (fn 44) roughly 6 times more distant than \( \alpha^2 \) Cap.
\(^{49} \) To attain to an appropriate perspective on vying ancients’ relative intelligence, recall from §A7: [a] Geocentrist 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 1000 AU. §A7.
[b] The real distance of Proxima Cen, nearest extra-Solar System star, is ordmag 10000 AU. §A7.
[c] Sidereal Time, one can calculate:

\[
cot A = (\cos[15M/2] - \sin ST) \cos \epsilon / \cos ST
\]

\(^{50} \) It will help to provide an example, using the \textit{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 parallax 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 \textit{Almajest} 2.8’s “Accumulated Time-Degrees” column for Rhodes (longest-day \( M = 141/2 \), the basis of this column’s ancient computation and arrangement): \textit{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 00' = 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 (polar longitude of transiting zodiac point) is then found by linear interpolation on Toomer 1984 p.100: in the “Accumulated Time-Degrees” column, under the “Sphaera Recta” heading, we find 312°32'; 320°54' (ST) exceeds this by 8°22' of the 9°58' interval corresponding to the 10° interval between 10°AQR 00' and 20°AQR 00' (in the column “10° Intervals”), so: add 10°(8°22'/9°58') = 8°24' to 10°AQR 00', which yields Midheaven = 18°AQR 24' (18°4 of Aquarius) on the ecliptic or sidereal 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 Thesidos of Bithynia’s “Houses”, 2nd century BC. Finding
Ascendant & Descendant (and thus house-divisions) is the sole use most modern astrologers have for
geographical latitude. (Ancients also used latitude to enter parallel tables, but such scrupulousness is
rare among today’s astrologers.) Geographical longitude was used merely for additively converting
§D2 [3] local time to ephemerides’ standard zero-meridian, presumably that of Alexandria.

\(^{51} \) 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°74
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 6 (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:

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

(1)

where obliquity 6 was usually taken to be 23.5/6 or [the discovery of Diller 1934] 23.2/3).

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

[1] Books 2-7, like the Important Cities part of Ptolemy’s HanTabl, are in the form of Marinus’ manual or map, presumably after his (though see §C1) systematic tectonic mass-allocations (GD 5.6.2-3). B&K p.4-60 refer to these macro-geographical accord (through eq.1). B&K p.44-51 quote the above-hypothesized (§A1) network-grid-base, which had been severely 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 §M6) to Hipparchos’ 36° parallel (arc 0°-23° in Fig.1 [p.50]) through the east-Mediterranean island of Rhodos, 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 Rhodos (D149), probably just north of the town of Lindos. (See Rawlins 1994L §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 Alexander (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 istsing data for maps. (To the contrary: §D3 [b].) However, astrological tables of the outdoor-invisible “Ascendant”

3See Aubrey Diller 1984’s scrupulously-wrought establishment of the text of the entire contents of Book 8 at www.dio.org/gad.htm. The total of his site-lists is 359. Nobbe’s total is 358. But Nobbe omits Tarentum and Sousaleos, while Diller semi-omits Limyra. (Though, see end of this fn.) Merging the lists, we have exactly 360 sites in 26 sections, corresponding to GD 2-7’s 26 maps. Sections: 10 of Europe (118 GD 8 sites), 4 of Africa (52 GD 8 sites), 12 of Asia (190 GD 8 sites). I propose scholars’ agreement upon a conventional numbering of all 360, based upon the sequence of Diller’s XZ Codices, dovetailing with the UNK Codices (to cover sites either skipped), which follows Diller’s desire to give primacy to the former. We use prex D, to number every site D53, Sousaleos (GD 3.3.4, 8.8.4) as site D63. (Note that we are dovetailing these two sites into Nobbe in passages that [exceptionally] already list more than one site — which may help explain these two oversights.) To Diller’s version, we add Limyra (GD 5.3.6, 8.17.25) as site D193, Diller XZ Codices Asia-Map 1 site #22 ->#22a. “Myra”, whose coordinates are identical to Nobbe’s “Limyra” at GD 8.17.25. D192 is UNK’s item#22, “Myra” at GD 3.3.6. Note that one finds “22a” in Diller’s hand in the left margin of his p.X13, showing that he suspected the need to add this site as the final touch to perfecting his epochal document. I.e., even at age eighty-plus, his sharp eye was still missing nothing!4

4The very choice of longest-day (instead of latitude) as GD 8’s measure of notherliness 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 Tetrabiblos. Note that the geographical table in his astrologer-oriented Handy Tables was at this stage still inconveniency in degrees).

As Heath notes, this is in the right ballpark (only off by a factor of about 2). It implies a solar volume of ordmag 100,000 Earths! Given the sheer solar mass obviously indicated, this would suggest (Rawlins 1991P §C3) to anyone outside the Munich55 that Ptolemaios was teaching a heliocentric conception of the universe — as also did Seleukos. (Heath 1913 p.305 cites several of the ancient testimonies on heliocentrist.) And Ptolemaios also suggested that the stars can match or even exceed the Sun in size (Neugebauer 1975 p.965).

F3 What can have caused the shift in heliocentrist’s adopted rS from 1146° (ordmag 1000°) to 10000°? The answer is obvious the moment one has recourse to observation, which (if rS is assumed equal to c.1000°) produces a radical empirical contradiction, similar to that found via Giedee’s (§E8) by assuming a stellar distance of 1000 AU.

F4 It is a striking fact that all 3 extant reported ancient planet-star occultations are Hellenistic and are near or not long after Aristarchos’ time. One is by his contemporary Timocharis (Almajest 10.4): Venus in –271. The other two are for Mars (Almajest 10.9) the same year, and of Jupiter (Almajest 11.3) in –240; both are recorded according to the Dionysios calendar. (DIO 1.1 §1 fn 23 identified Dionysios for the first time, and uncovered evidence of the very heliocentrist connexion [to Dionysios] long suspected by DR & van der Waerden. See van der Waerden 1984-p 130.)

F5 From §F4, we conclude: it is not a wild speculation to suppose that Aristarchos were examining planet-star occultations — which just happen to have been the last hope for ancients’ gauging rS in Earth-radii. In a moment, we will show (§F6) how such observations will swiftly eliminate Aristarchos’ initial idea that rS = 1146° (eq. 12). After this value was rendered obsolete, it evidently lingered on anyway among psychologically-receptive geocentrist, e.g., Al-Battani.57 He, like Hipparchos & Ptolemy, preferred rS to be as small as possible so the Sun wouldn’t be so embarrassingly bigger than the tiny alleged terrestrial Center of the Universe. (And Eratosthenes had the universe even snugger: §F3.) Ironically, this geocentrist tradition misled the first modern public heliocentrist, Copernicus, who set rS = 1142°, close to Aristarchos’ initial 1146° value (& not far from Ptolemy’s). Later, public-geocentrist Tycho used 1150° (Thoren 1990 pp.302-304). So: [a] Aristarchos’ Experiment was the basis of Solar System scales for nearly 2 millennia, adopted (at least roughly) by Ptolemy, Battani, Copernicus, Tycho, successively. [b] Poseidones’ rS = 10000° (eq. 14) was, in accuracy, superior to all these later figures. Its double use (eq. 14) of 10000 as the key scale ratio of the system. Note that Archimedes speaks of 10000 as an upper limit for both ratios of eq. 14; but Poseidones does not do so. He instead goes on (§F2) to propose that stars’ sizes can exceed the Sun’s. (A similar statement regarding brightness would be more indicative. After all, even Ptolemy taught that stars were nearly as big as the Sun: fn 42. Van Helden 1985 p.27.) This slight alteration may reflect post-Archarites refinements (e.g., larger terrestrial baseline) for the planet-star occultation observations discussed at §F7.

55See the precise puzzlement of Toomer 1984 (p.257 n.66 embled): “There is no point in estimating the relative volumes of the bodies, but it was evidently traditional in Greek astronomy”. The incomprenension here (by the very scholar whom Munich satellite P.Huber calls “the expert” on the Almajest; PH’s emphasis) beautifully typifies the Munich’s uncanny non-intuition regarding what real ancient scientists were about.

56Poseidones taught several conflicting schemes: Neugebauer 1975 p.656. One of his values, rS = 1625°, is more consistent with 1536° (§D3) than with 1146° (idem). An accurate ancient Earth-circumference is implicit in one of Poseidones’ schemes: 600 stades/degree (Neugebauer 1975 p.656 n.3; or, with p.655 eq.11: 625 st/degr). Yet his math at Neugebauer 1975 p.656 eq.20 presumes 700 st/degr; and Poseidones is known from Strabo 2.2.2 to have promoted 500 st/degr. Note another 600 st/degr suggestion in Pliny. (Nobbe’s Table 1.18.23, “Myra” at GD 5.3.6). Note that one finds “22a” in Diller’s hand in the left margin of his p.X13, showing that he suspected the need to add this site as the final touch to perfecting his epochal document. I.e., even at age eighty-plus, his sharp eye was still missing nothing!

57At least 1187° (eq. 12) was, in accuracy, superior to all these later figures.
We will next show that the superiority of Poseidonios’ conception was probably based on observation, not “naive” guesswork (Neugebauer 1975 pp.655-656). For solar distance 1146° (eq. 12), the Sun’s diurnal parallax is 3°. Now, when Mars reaches a station and is roughly near perihelion, it can be less than 0.5 AU from the Earth — which means that a 3° solar parallax corresponds to about 6° of Mars parallax. At Alexandria’s latitude, 31°N, while Mars is visible during the night, an observer will be transported well over 1 Earth radius (transversely to the Earth-Mars vector) by the Earth’s axial rotation. So, for rs = 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 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 must have frequently occurred near enough to stars that the invisibility of the predicted parallactic shift was repeatedly verified.

There is another planet-star method which requires (not the neat timing of hitting on a station but) a wide geographical range of observations. When Venus is near inferior conjunction, it can be less than 0.3 AU from the Earth. (About 1/3 of an AU at stations.) I.e., Venus’ diurnal parallax can be more than triple the Sun’s. But for 3° solar parallax (§F6), Venus’ greatest diurnal parallax should be as high as about 10’. If Venus passed near a star, then one need only compare observations taken, say, at Meroe (latitude L = 17°), vs. ones taken, say, at Byzantium (L = 41°). The north-south angular distance between planet & star at conjunction should differ by about 5’ — simply detected by the naked eye.

I propose that our fragmentary record (§F4) of ancient planet-star occultations is part of Aristarchans’ systematic empirical testing — which eventually converted heliocentrist, c.270 BC (sometimes between Aristarchos’ Experiment & the “Sand-Reckoner’s Data”)

Summing up the evidential situation: we have examined all 3 of the surviving astronomical scales connectable to ancient heliocentrist (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 μ = 1/10000 of a radian. This present coincidence lends more credibility to the empirical-base theory proposed here, than most current astronomy-historian archaeoastronomists will ever admit. However, these archaeoastronomists’ 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 naiveté. (Which is spoofed at DIO 2.1 §1 §J. 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 after the 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.

---

3 The Ptolemy GEOGRAPHY’s Secrets

[Which GEOGRAPHY Secrets Were Secret from Ptolemy?]

Distillate from 3 Decades of DR Researches into Ptolemy’s Geographical Directory, 1979-2007

Zero Longitude Revealed: Cape Verde Isles

Old Egyptian Accuracy vs Greek

Marinos’ Date and Authorship

Astrologers’ Handiest Tables

In Memory of a Brilliant Friend

AUBREY DILLER 1903-1985

---

A Why a Network of Exactly 360 Sites’ Geographical Hours?

A1 The famous Ptolemy Geographical Directory (henceforth “GD”), popularly called “The Geography” or “Geographia”, is in eight Books. It was commissioned in the 2nd century AD for the use of Serapic astrologers (§D5). We will here adopt the fine English edition of its text by Berggren & Jones 2000 (henceforth “B&J”). But don’t miss the lovely new complete Stückelberger & Graßhoff 2006 edition (henceforth “S&G”). If you know German. And even if you don’t. The GD begins with explanatory Book 1, which tells of Ptolemy’s incorporation of thousands of sites’ geographical places from the work of an earlier geographer, Marinos of Tyre. Then, Books 2-7 list about 8000 sites’ positions, expressed consistently in degrees to 1/12th degree precision: longitudes in degrees west or east of the Equator.

The GD then concludes with what DR contends (§A4) resembles and-or partly constitutes the data-base grid-network computationally (eq.1) underlying the precision-corrupted (§D1, D5, & K10) positions of GD Books 2-7, namely: Book 8, whose data are expressed entirely in hours (not degrees), a list of 360 sites’ longitudes in hours west or east of Alexandria (not the Blest Isles); and, instead of latitude, longest-days M (for Sumner Solstice) in hours, where parallels at 1/4° or 1/2° intervals of M were called “klimata”. E.g., longest-day M = 14°1/2 was called the Rhodos klima where L = 36° (via eq.1).

---

1These investigations were posted on the DIO website in 2006-2007, at www.dio.org/ged.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, Stückelberger & Graßhoff 2006 (hereafter “S&G”). Note that the present paper forges the use of accents for Greek words. Diller himself pointed out accents’ superiority, since classical-era Greek lacked them. During a DR 1987/6/1 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 to be visiting the same day.

2 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 Catalog of dozens of stellar Right Ascensions & Declinations.) Long before Ptolemy, Strabo reported a Nile map consistent (Rawlins 1992b) 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.
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 persuasiveness 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 slight Aristarchos, merely because attacks on his heresy and on his intellectual freedom succeeded in virtually burying his work — despite his high ancient reputation (Rawlins 1991W §Q1) & achievements — is effectively to endorse dictatorial bullying & idea-imprisonment. I cannot begin to imagine why the Mufa would do such a thing.

[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-quackery?
Aristarchos: Ancient Vision

2008 March

DIO 14 ¶2

Aristarchos: Ancient Vision


[c] Above, we have found evidence that, even under the shadow of Cleanthes’ notorious threat, Aristarchos reasoned out & promulgated the epochal 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 crankpot1 astronomy), 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 ¶5G5*) 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 anonyomizeberg, 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 ordmag a trillion (10^12) 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

1 Dio 1.1 ¶15 fn 24, 16 [H], 17 [B2].

2 See fn 65, Heath 1913 p.304 (also *DIO 1.1 ¶1D3*) recounts Cleanthes’ attempt (parallelling later threats against Galileo) to have a charge of “impiety” brought against Aristarchos — which, in those bentigated 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 insufficient brainkissing of hist.astron archons will have no effect whatever upon a scholar’s career.

3 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 [. . .] 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.”

4 If this seems too strong, see Rawlins 1991P and Thurston 1998 GMS & ¶16.

5 Cubing 10000 yields a trillion — and “Sand-Reckoner” (Archimedes p.232) says that Aristarchos’ stellar universe was a trillion times the Earth-oorb sphere, but without explaining the observational base. Geocentrist preferred r = ordmag 10000r and extant geocentrist schemes (3 are tabulated in Van Helden 1985 pp.27, 30, 32) placed the stars ordmag 10^16 r distant, while Aristarchos-Archimedes held (eq.14) for 10000r and 10000^2 r distant, respectively; so the net heliocentrist-vs-geocentrist stellar-universe linear expansion factor is ordmag (10000/10000)- (10000/100) ≈ 10000.

6 The tiny universe-scale dominant among geocentrists reminds one of a joke told by Jake Lamotta about fellow-pug Rocky Graziano. Both were gifted actors after — and before — their retirement from

boxing. Jake and professionally-punchy Rocky leave the gym together, and Jake points up into the sky and asks: “Hey, Rocky, what’s that big bright thing — the Sun or the Moon?” Rocky: “Aaah . . . Aaah . . . Aawww, Jake, how would I know? I don’t live in this neighborhood.”

7 There are exceptions, for which our gratitude is frequently expressed in *DIO*.