‡3 Continued Fraction Decipherment: the Aristarchan Ancestry of Hipparchos' Yearlength & Precession

The Aristarchos Sidereal Year's High Accuracy
His pre-Hipparchos Knowledge of Precession
Consistency & Cause of Greek Tropical Year's Error

[Eighteen years ago, this paper was doubly refereed & accepted for publication by the Journal for the History of Astronomy *but was then suppressed for its §E heresy on Ptolemy. (Details below at §F.) Substantial changes & additions to the 1981 version are in brackets.]*

A Introduction

A1 Not long ago, Neugebauer republished and considered a couple of largely undeciphered lists[1] (see Table 1) of ancient values for the year's length, taken from the Vatican Greek mss collection.
A2 He comments[2] on the mysterious state of the numbers:

> The first number is the traditional value for the Greek version of the Metonic cycle. The remaining numbers are obviously corrupt; the number of Aristarchus in [list] A could perhaps be rescued by interpreting ξβ′ as [sixtieths of the second power] i.e. "seconds," but the resulting 365;15,20 would still leave [the Aristarchus year in list] B unexplained, nor is there any relation to another supposedly Aristarchean value 365;15,2,13, . . . [$365^{\rm d}$1/4 + 3/4868 or $365^{\rm d}$1/4 + 1/(1622 2/3)].

[The previously puzzling digits of the mss are rendered in both Greek and Arabic in Table 1.]

B Solutions

B1 I have elsewhere already shown[3] that Eratosthenes' famous value for the obliquity (c.200 B.C.), 11/83 of a semicircle ($23°51'20''$), is derived from a continued-fraction process (upon his empirical result, $23°51'15''$).[4] And I find that others[5] have suspected that the continued-fraction technique (for approximating any number by a rational expression, as closely as desired) goes back at least as far as Eratosthenes' Alexandrian predecessor, Aristarchos of Samos (fl. 280 B.C.).

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[1] Otto Neugebauer, *History of Ancient Mathematical Astronomy* (3 pts, New York, 1975), p.601. Lists published earlier by W.Kroll and E.Maass. Neugebauer (*loc. cit.* note 1) gives detailed citations, including the information that list A is from Vat. gr. 191 fol. $170^{\rm v}$; list B, from Vat. gr. 381 fol. $163^{\rm v}$.

[2] *Ibid.*, 601-602.

[3] For details, see Robert Newton, "The Sources of Eratosthenes' Measurement of the Earth", *Q. Jl Roy. astr. Soc.*, xxi (1980), 379-387, pp.386-387; or [Rawlins, "Eratosthenes' Geodesy Unraveled" *Isis*, lxxiii (1982), 259-265, pp.262-263. See also *DIO 2.1* ‡3 fn 26.]

[4] *Almajest* (henceforth [usually] abbreviated *Alm.*), I 12.

[5] Starting with Fortia d'Urban. See Thomas Heath, *Aristarchus of Samos* (Oxford, 1913), 336, and *Greek Mathematical Works* [Loeb C L], English translation by Ivor Thomas, ii (London, 1941), 14-15.

Table 1: The Vatican Mss Lists of Ancient Yearlengths in Days

List A:

Calendarist	Manuscript Data	
Meton, Euktemon, Philip	365 5/19	365 5/19
Aristarchos of Samos	[365] 1/4 κ′ ξ β′	[365] 1/4 20′ 60 2′
Chaldeans	365 1/4 ε′ ζ′	365 1/4 5′ 7′
Babylonians	365 1/4 1/144	365 1/4 1/144

List B:

Calendarist	Manuscript Data	
Euktemon, Philip, Apollinarios	365 5/19	365 5/19
Aristarchos of Samos	365 1/4 ι′ δ′	365 1/4 10′ 4′
Babylonians	365 1/4 ε′ ζ′	365 1/4 5′ 7′
Sudines	365 1/4 γ′ ε′	365 1/4 3′ 5′
———	365 1/4 ρ′ σ′[	365 1/4 100′ 200′?

Table 2: Continued-Fraction Interpretations of Table 1's Data

List A:

Calendarist	Year	Continued Fraction	Restored Year
Meton, Euktemon, Philip	$Y_{\rm E}$	365 1/(4 − 1/5)	365 1/4 + 1/76
Aristarchos of Samos	$Y_{\rm At}$	365 1/(4 + 1/(20 + 2/60))	365 1/4 − 15/4868
Chaldeans	$Y_{\rm Bt}$	365 1/(4 + (1/5 − 1/7))	365 1/4 − 1/c.285
Babylonians	$Y_{\rm Bs}$	365 1/(4 − 1/(9 + 1/4))	365 1/4 + 1/144

List B:

Calendarist	Year	Continued Fraction	Restored Year
Euktemon, Philip, . . .	$Y_{\rm E}$	365 1/(4 − 1/5)	365 1/4 + 1/76
Aristarchos of Samos	$Y_{\rm As}$	365 1/(4 − 1/(10 − 1/4))	365 1/4 + 1/152
Babylonians	$Y_{\rm Bt}$	365 1/(4 + (1/5 − 1/7))	365 1/4 − 1/c.285
Sudines	$Y_{\rm St}$	365 1/(4 + (3 1/5)/60)	365 1/4 − 1/304
———	$Y_{\rm Ag}$	365 1/(4 − 1/(100 + 100/60))	365 1/4 + 3/4868
———	$Y_{\rm Ag'}$	365 1/(4 − 1/100)	365 1/4 + 1/1596

B2 Until the revelations to follow (below), I would say that the starkest hint of ancient use of continued fractions was the Archimedes approximation-bracketing of π:

$$3 + 10/71 < \pi < 3 + 10/70 \tag{1}$$

Why else would the variation appear in the denominator?
B3 Continued fractions are the key that unlocks Table 1. If we presume that the table's data evolved from an uncomprehending scribe's[6] copying numbers which represented continued-fraction expressions (in whole or in part), then we may easily find the solutions set forth here in Table 2 (where I render the unmysterious cases also as continued fractions, just to provide extra illustrative examples).
B4 At first glance, it may appear that the sign-choices are arbitrary. However, upon reflection, it is obvious that without some negative signs, all the years listed in Greek notation would be shorter than $365^{\rm d}1/4$ (and thus sidereal years would be unrepresentable) by the continued-fraction interpretation of Table 1. And, without some such interpretive hypothesis, the figures of Table 1 are the gibberish that Neugebauer rightly calls them.[7] An obvious point: no normal Greek fractional expression used fractions in non-descending order of size, as in, e.g., the case of list B's Aristarchos year, $365^{\rm d}1/4$ 1/10 1/4.
B5 This case also illustrates why flexible use of plus or minus signs is efficient (and thus would be attractive to those ancients who worked with continued fractions): if one tries to express $365^{\rm d}1/4 + 1/152$ using only the usual plus signs, the result is:

$$365^{\rm d} + \cfrac{1}{3 + \cfrac{1}{1 + \cfrac{1}{8 + \cfrac{1}{1 + \cfrac{1}{3}}}}} \tag{2}$$

But simply permit sign-flexibility, and the outcome is much compacted:

$$365^{\rm d} + \cfrac{1}{4 - \cfrac{1}{10 - \cfrac{1}{4}}} \tag{3}$$

which is the expression for Y_{As} in Table 2.
B6 Anticipating the inevitable question regarding the influence of preconception, let me say that I started investigating Table 1 in search of values for the *actual* tropical year $Y_{\rm t}$. [See fn 18 for real ancient tropical and sidereal yearlengths.] I never found a one. The only plausible tropical results instead kept coming out near the old standard (but highly erroneous) Hipparchos-Ptolemy value (eq. 10), $Y_{\rm Ht} = 365^{\rm d}1/4 - 1/300$.

[6] An alternate hypothesis is that a deliberate attempt was made to disguise-encode the values of the length of the year so that non-initiates could not understand them. The legendary secrecy of ancient science (or pseudoscience) has been knowledgeably ridiculed by Neugebauer (*op. cit.*, 566): "all these 'secrets' were eagerly written down and have survived in countless copies . . . ." But the inherent sampling-completeness infirmity [*DIO 4.2* ‡9 §I] of this reasoning reminds one of Count Fosco's retort to the confident idea that crimes always out: "Yes — all the crime *you* know of. And what of the rest?" (Wilkie Collins, *The Woman in White*, 1859-1860.) Whatever the degree of success, it is undeniable that some ancient cults treated knowledge as an elitist treasure not to be lightly shared. See, e.g., #143 of *The Letters of Synesius of Cyrene*, English translation by A.Fitzgerald (Oxford Univ, 1926).

[7] See Neugebauer *op. cit.*, p.602. Also R.Gillings *Historia Mathematica*, viii (1981), 456-457 on Egyptian writing of fractions (a different context, though Alexandria is geographically Egyptian): "Scribes never used signs" (brought to my attention by Owen Gingerich in 1982 February).

B7 Some of the Table 2 interpretations of Table 1's data may look arbitrary; however, considering the relatively small number of interpretive options available, one must ask what the odds are on: [a] The exact appearance of two known Aristarchos (calendaric) numbers, 152 (§C8) and 4868 (§C3). [Note that these numbers may be related: 32 cycles, of $152^{\rm y}1/8$ each, constitute $4868^{\rm y}$. Note also that Hipparchos' final lunisolar tabular epoch (*DIO 1.1* ‡6 eq.28), −127/9/24 1/2, was $152^{\rm y}1/4$ after Aristarchos' −279 S.Solstice, which we know Hipparchos used to found his $Y_{\rm Ht}$: *ibid* §B4.] [b] The repeated appearance of integer-factor multiples of 76 (Kallippic), namely +1 (Euktemon), +2 (Aristarchos), −4 (Sudines).

C Comparisons

C1 For simplicity in explanation below, we set the convention that the year Y is related to a reciprocal remainder R (reflecting the deviation from the Kallippic-Julian year $Y_{\rm K} = 365^{\rm d}1/4$) by the easy equation:

$$Y = Y_{\rm K} + R^{-1} = 365^{\rm d}1/4 + 1/R \tag{4}$$

C2 The Kallippic cycle of $76^{\rm y}$ obviously figures in the Table 2 data. The Euktemon year $Y_{\rm E}$ ($365^{\rm d} + 1/R_{\rm E}$) exceeds $Y_{\rm K}$ by an accumulated total of $1^{\rm d}$ per Kallippic cycle — i.e., $R_{\rm E} = +76$. Aristarchos' sidereal year $Y_{\rm As}$ (list B): $1^{\rm d}$ excess every 2 Kallippic cycles (or $152^{\rm y}$), i.e., $R_{\rm As} = +152$. Sudines' (c.240 B.C.)[8] tropical year $Y_{\rm St}$: $1^{\rm d}$ defect every 4 cycles (or $304^{\rm y}$), i.e., $R_{\rm St} = -304$.
C3 The Aristarchos tropical year $Y_{\rm At}$ (list A): $1^{\rm d}$ defect every 18 Saros cycles, where we know[9] that Aristarchos' value for the Saros was

$$18^{\rm y}10^\circ 2/3 = (1622^{\rm y}2/3)/90 = 4868^{\rm y}/270 \tag{5}$$

Thus, $R_{\rm At} = -18{\cdot}4868/270 = -4868/15 = -324\ 8/15$. Since the number 4868 has long been associated with Aristarchos (see §A2, fn 11, and Neugebauer *op. cit.* p.603), its appearance in Table 2 under his name is particularly striking.
C4 The Babylonian sidereal year $Y_{\rm Bs}$ (list A) shows an excess over $Y_{\rm K}$ ($365^{\rm d}1/4$) of $1^{\rm d}$ every $144^{\rm y}$, which may be a rounding of 8 Saros (about $144^{\rm y}.2$). $Y_{\rm Bs} = +144$.
C5 The Table 2 expression for the Chaldean (list A)-Babylonian (list B) tropical year $Y_{\rm Bt}$ looks over-speculative at first; however, if we presume an intent to quantify a year which corresponds to a defect of $1^{\rm d}$ every 15 Metonic ($19^{\rm y}$) cycles, that is every $285^{\rm y}$ ($Y_{\rm Bt} = -285$), then a continued fraction solution would be (rounding 17.5625 to 17 1/2 or

[8] Neugebauer *op. cit.*, p.611 n.31.

[9] See fn 11 below. (*Alm.*, IV 2 has same Saros-length as eq. 5. The Tannery-reconstructed Aristarchos period was half of that equation's 4868. And 2434 years Y = 30105 synodic months M = 32265 anomalistic months A = 32670 draconitic months D = 32539 sidereal months. These figures use the relatively simple Saros relations (*Alm.*, IV 2): $223M = 239A = 242D$ (where $242 = 2{\cdot}11^2$). It is a provocative [though far from conclusive] coincidence that the simplest fractional expression which will yield precisely eq. 13's monthlength M (*Alm.*, IV 2) is 99902/3383, where we note that 99902 = 2·11·19·239. These familiar factors permit simplifying various periods via convenient cancellations:
$Y_{\rm M} = 235M/19 = 235{\cdot}2{\cdot}11{\cdot}239/3383 = 365^{\rm d}1/4 - 43/13532$.
$A = 223M/239 = 223{\cdot}2{\cdot}11{\cdot}19/3383 = 27^{\rm d}1873/3383$.
$D = 223M/242 = 223{\cdot}19{\cdot}239/(11{\cdot}3383) = 27^{\rm d}7892/37213$.
(In an age of tedious computational means, relatively short fractional expressions were preferable to long sexagesimal ones. [Similarly: since 3383 is 17·199, cancelling 17 in the key eq.1 of *DIO 6* ‡1 finds: $2{\cdot}11{\cdot}17{\cdot}19{\cdot}239{\cdot}251/(17{\cdot}199) = 126007^{\rm d}9/199$.]) For a completely different explanation of M (also to all sexagesimal precision given in *Alm.*, IV 2), see Noel Swerdlow, "Hipparchus's Determination of the Length of the Tropical Year and the Rate of Precession", *Archive for History of Exact Sciences*, xxi (1980), 291-309, pp.307-308. [Also simple DR solution bracketed below at §D4.]

35/2, to avoid the lengthy exact solution):

$$365^{\mathrm{d}}1/4 - 1/285 = 365^{\mathrm{d}} + \cfrac{1}{4 + \cfrac{1}{17.5625}} \approx 365^{\mathrm{d}} + \cfrac{1}{4 + \cfrac{2}{35}} \qquad (6)$$

The ancient preference for unit fractions [& compactness] would quickly tranform the last expression to $365^{\mathrm{d}}1/(4 + (1/5 - 1/7))$ — as shown in Table 2.[10]

C6 Attempts to solve the last item in list B of Table 1 must contend with the lacunae in data and even name. A guess is that this year might be related to the Aristarchan year relayed by Censorinus:

$$Y_{\mathrm{Ag}} = 365^{\mathrm{d}}1/4 + 1/1623 \qquad (7)$$

which Tannery reconstructed[11] more exactly as

$$Y_{\mathrm{Ag}} = 365^{\mathrm{d}}1/4 + 3/4868 \qquad (8)$$

C7 One possible explanation of the Table 1 version is: $365^{\mathrm{d}}1/(4 - 1/(100 + 100/60))$, which equals Y_{Ag} precisely.[12] Another explanation is: the nearest integral multiple of R_{Ag} in Kallippic cycles is 21 cycles, which is 1596^{y}, the alternate figure found in Table 2 ($R_{\mathrm{Ag'}}$ =1596), where it is implicitly speculated that σ' is not a number but part of a mutilated word on the original record list B was copied from. This value is consistent with the fact that all other years of list B have here been found to be based upon fit-roundings to the Metonic cycle (19^{y}) or Kallippic cycle ($76^{\mathrm{y}} = 4{\cdot}19^{\mathrm{y}}$), not the Saros.

C8 The relating (in Table 2, list B) of the Aristarchos sidereal year

$$Y_{\mathrm{As}} = 365^{\mathrm{d}}1/4 + 1/152 \qquad (9)$$

($R_{\mathrm{As}} = +152$) to him is as definite as was the case with his tropical year (above) — since we know[13] that Aristarchos' 280 B.C. Summer Solstice [−279/6/26 noon][14] was exactly 152^{y} after the famous Meton 432 B.C. Summer Solstice.

[10] Subsequent to positing this (in the 1st draft of this paper, transmitted for me by Michael Hoskin to the 1980 June Aristarchos conference on Samos [where it was not read]), I realized the likely origin of $R_{\mathrm{Bt}} = -285$. It is clear from *Alm.*, IX 3 that [the ancients quite commonly] rounded to 60ths of a day (i.e., one sexagesimal place) when reporting astronomical period relations. If one expresses the Metonic cycle so, it is [by eqs. 12&13] $6939^{\mathrm{d}}41'$. Divide by 19, and one gets exactly $365^{\mathrm{d}}1/4 - 1/285$. (Note also that 285^{y} [underlies both Ptolemy *Alm.*, III 1 equinox fabrications: *DIO 8* ‡1 Table 1].)

[11] See Heath, *op. cit.*, pp.314-315, and Neugebauer, *op. cit.*, pp.603-604. [It is also possible that the last entry in List B is related to the anomalistic year. See *DIO 6* ‡1 eq.6 & fn 40.]

[12] The precise agreement has of course been achieved only because σ' has been arbitrarily presumed to be a scribe's error for $100'$ or 100/60. (Sole defense: the astonishing agreement with Y_{Ag}.)

[13] *Alm.*, III 1. [Alternate theory for Aristarchos B: $365\ 1/(4 - 1/(10 + 1/4)) = 365\ 1/4 + 1/160$. If this is the right interpretation, it might be based on the −1079/6/24 eclipse. See *DIO 6* ‡1 §I2.]

[14] [See *DIO 1.1* ‡6 eq.8. The occurrence of the actual 280 B.C. Summer Solstice was the better part of a day later. See §E3. Thus, the precise observed solstice-time may have been rounded to equal the noon of the calendar day *containing* the event. See Rawlins *Bull. Amer. Astron. Soc.*, xvii (1985), 583, where it is suggested that Aristarchos indeed truncated the observed Summer Solstice's time to diurnal epoch-hour (noon for him) a practice that (the 1985 paper theorizes) would be a natural tradition for calendaric astronomers since Meton — and which obviously tended to cause later astronomers (who had to use older solstice data for computing mean yearlength) to systematically overestimate Y. See also *DIO 1.1* ‡6 §E4.] Proximity to the Dionysios era suggests Aristarchos' connexion to the $365^{\mathrm{d}}1/4$ Dionysios calendar, which was used [by Dionysian heliocentric astronomers: *DIO 1.1* ‡1 §D] to date eight *Alm.* data (3rd century B.C.), mostly observations of Mercury. We take epoch (start of Dionysios year 1 = Ptolemy II year 1) = −284/6/26 noon and adopt the reconstruction of August Böckh, *über die vierjährigen Sonnenkreise der Alten* . . . (Berlin, 1863), 286-340. The lone non-fitting *Alm.* date is the Mars-βSco conjunction, off by 1^{d} — but R.Newton has found that the real conjunction was 1^{d} off the *Alm.* report. Thus, Böckh's scheme seems completely vindicated. (Noon epoch is my speculation [based on noon's diurnal analogy to the Summer Solstice's annual effect].) I suspect that the 280 B.C. observation was taken to help establish the Dionysios calendar. By this scenario, the 1^{h}-precise observed time is now lost (and was not even known to Hipparchos or Ptolemy).

C9 Both this value and the Babylonian sidereal year Y_{Bs} ($R_{\mathrm{Bs}} = +144$) are close to that of the tables of the *Almajest* (where $R_{\mathrm{Ps}} = +147$)[15] and to the actual[16] sidereal year Y_{s} of that epoch, when $R_{\mathrm{s}} = +153$.

C10 Notice that all the tropical years of Table 2 (Y_{At}, Y_{St}, Y_{Bt}) are very erroneous yet all are quite near the rounded value later used[17] by Hipparchos and Ptolemy, eq. 10. And $R_{\mathrm{Ht}} = -300$, whereas actual[18] $R_{\mathrm{t}} = -133$ in [the former's era, becoming −132 by Ptolemy's time].

D Conclusions

What do we learn from the foregoing?

D1 Table 1 is the oldest extant material expressed in continued-fraction format, albeit corruptly.

D2 The veil has been pulled aside from a flock of long-lost ancient values for the length of the year.

D3 The sidereal-year estimates (Y_{s}), being strictly astronomers' values, are unforced to civil considerations. [Which is part of the reason that ancients' Y_{s} are so much more accurate than their Y_{t}.] These Y_{s} values are probably based [see, e.g., Rawlins, *Vistas in Astronomy* xxvii (1985), 255-268 §5, also *DIO 6* ‡1] upon lunar eclipse observations, which do not require high precision visual measurements to determine Y_{s} to good accuracy. [See *DIO 1.1* ‡6 fn 1.] As seen above, Aristarchos' value, Y_{As}, is astonishingly accurate (R_{As} = 152 vs. actual R_{s} = 153 for his epoch), correct to a few time-seconds. However, this proximity may be much an accident of rounding.

[15] Perhaps $R_{\mathrm{Ps}} = +147$ was also Hipparchos' value. However, there is evidence that he used the nearby value, $R_{\mathrm{Bs}} = +144$. See Neugebauer, *op. cit.*, 293, and Swerdlow, *op. cit.*, 300. [See also *DIO 6* ‡1 §C.]

[16] This figure includes both the secular variation of the Earth's revolution about the Sun and [the greater (but oft-ignored)] effect of the Earth's spin acceleration [*DIO 6* ‡1 fn 53] upon the length of the day itself. [(Gingerich 1981 *Q. Jl Roy. astr. Soc.* 22:44 ignores both.) See also fn 18. It is sometimes helpful to have at-hand rough values for the accumulated effect; so, accurate to ordmag 10^{m}, I give ΔT (and the Besselian date) for each of several calendar epochs: Meton (−430.527) 4^{h}, Kallippos (−328.525) $3^{\mathrm{h}}3/4$, Dionysios-Aristarchos (−283.527) $3^{\mathrm{h}}1/2$, Hipparchos (−126.278) 3^{h}, Antoninus-Ptolemy (+137.547) $2^{\mathrm{h}}1/4$. Since empirical estimates of Y were based on data extending backwards in time, the apt R_{s} for comparisons would be a little less than that for the astronomer's own era. For Aristarchos, we make no adjustment, since R_{s} was likely on the high side of 153 in 280 BC.]

[17] *Alm.*, III 1 — though Censorinus (Heath *op. cit.*, 297) rendered Hipparchos' year as $365^{\mathrm{d}}1/4 - 1/304$ (which is Y_{St} of Table 2).

[18] See fn 16. [R_{s} & R_{t} are obviously not exact. Each is figured for the era of Aristarchos and rounded to the nearest integer, being uncertain by a few tenths of a unit in the last place. (Both figures grow more positive in time, roughly 2 units/millennium, sidereal rather more slowly than tropical.) Note that the most applicable R — that for Summer Solstices — is not really −133 (a value based upon mean solar motion) but is instead −124. (So the ancients' values [§D4] were even farther from empirical truth than is indicated by superficial analysis.) This is because all knowledgeable ancients used Summer Solstices to gauge yearlength Y. (See fn 25.) It is seldom appreciated by modern investigators that (due to the variation of eccentricity & apse in the Earth's orbit) each seasonal event has its own yearlength Y (differing from the others by ordmag 1^{m}) and so (by eq. 4) its own R. I here provide these for 280 BC: S.Solstice $R_{\mathrm{t}} = -124$; A.Equinox $R_{\mathrm{t}} = -138$; W.Solstice $R_{\mathrm{t}} = -144$; V.Equinox $R_{\mathrm{t}} = -129$. (The harmonic mean of opposite values of course yields −133 for each pair.) The sidereal year will also differ from star to star, around the zodiac. (And Y_{s}'s periodic variation, as a function of chosen zero-point longitude, will be similar to Y_{t}'s.) So our analyses here of R_{s} implicitly assume a broad enough ancient observational data-base, that mean solar sidereal motion was being measured.]

D4 Ancients' estimates of the tropical year (Y_t) ought to have been based upon centuries of high-precision transit observations.[19] However, the tropical year was the civil and religious year, in an age of strong traditional reliance on lunar calendars. Thus, Tobias Mayer long ago suggested[20] that Hipparchos' yearlength,

$$Y_{Ht} = 365^d 1/4 - 1/300 \quad (10)$$

(6^m too long), was based on a fit to the Kallippic cycle (established 330 B.C.) relating tropical (civil) years Y to synodic (civil) months M, by defining a Metonic year Y_M from the lunisolar equation

$$76Y = 940M \quad (11)$$

which is the same ratio as the older Metonic cycle (established 432B.C.),

$$19Y_M = 235M \quad (12)$$

The length of the month had become [*DIO 1.1* ‡6 fn 1] very well established by Aristarchos' time at the excellent estimate:

$$M = 29^d;31,50,08,20 \quad (13)$$

(accurate to well within 1^s, both in antiquity AND today). [*DIO loc. cit.* speculates that he was responsible for determining this famous value — by the extremely simple method explained at *DIO 6* ‡1 §A5 & fn 18.] So the [Metonic "year"] was

$$235M/19 = 365^d 1/4 - 1/315 \quad (14)$$

i.e., $R_M = -315$ very nearly. That Mayer's theory is no longer just a speculation may be discerned from the tight cluster of tropical year values Table 2 has showered upon us all at once; adding[21] in already-wellknown Y_{Ht} (eq. 10), we have the following tropical R_t values: $R_{Bt} = -285$, $R_{Ht} = -300$, $R_{St} = -304$, $R_{At} = -324$ 8/15. All four are very near the Mayer-Metonic calendaric-numerological value, $R_M = -315$, and all four are very far from the correct ancient value, $R_t = -133$ [actually -124: fn 18]. As we have already seen, each astronomer rounded in a different fashion (using, e.g., 1st or 2nd place sexagesimal roundings *en route* to Y_t, or rounding R_t to the nearest multiple of Metonic, Kallippic, or Saros cycles, or centuries). But obviously all shared the *a priori* prejudice that $R_t = 315$ (or some nearby transformation of it) was about right. [Even accounting for the systematic effect noted at fn 14], it is impossible to believe that all 4 of the R_t values just reprised (-285, -300, -304, -324 8/15) were based upon independent *solar* observations that just happened by chance to arrive in the vicinity of the same (very wrong) *lunar*-based (eq. 14) value, $R_M = -315$. A classic contrast of numerological forcing vs. what was to be expected from [neutral] empirical observation ($R_t = -133$). The overwhelming vindication here of Mayer's supposition ensures that we now know the source of the wellknown $+6^m$ error in Y_t — which accumulated [(*DIO 8* ‡1 ⊙1) to 26^h] in the interval between the solar theories of Hipparchos (c. -130) and Ptolemy [whose date is almost irrelevant here, since all of his alleged +132-140 AD solar "observations" were faked], producing the infamous solar mean longitude error (over 1°, negative) that infected all of Ptolemy's tables (the fundamental astronomy for which was borrowed entirely from Hipparchos) — the fatal error which ultimately revealed the truth behind Ptolemy's pretense that he was a regular observer of the sky.[22]

D5 Annual precession p (the difference between the sidereal and tropical years) [was understood by Aristarchos, though not by Meton or even Kallippos (who proposed only one yearlength each), so Aristarchos was the earliest known scientist to recognize precession. The difference was also evidently (Table 2) accounted for by Babylonians — who are, we note, listed later than Aristarchos, in *chronologically-ordered* Table 1. (See *DIO 1.1* ‡6 §§B11-B13.) Aristarchos gauged p] at very nearly 0°.01 (or 0^d.01) per year (in rough terms: 1/150 + 1/300 = 1/100), the mistaken figure adopted over a century later by Hipparchos (hitherto universally credited with the discovery of precession). We may even derive exact figures for these early precession values by using the following equation for finding centennial precession P:

$$P = 100(360°/Y)(Y_s - Y_t) \quad (15)$$

From this and the numbers developed in this paper, we find for Aristarchos [whose Y_{As} & Y_{At} in Table 2 are fortunately the firmest decipherments there]

$$P_A = 100(360°/Y) \cdot (1/152 - [-1/(324\ 8/15)]) = 0°.952 \quad (16)$$

and for Babylonia:

$$P_B = 100(360°/Y) \cdot (1/144 - [-1/285]) = 1°.03 \quad (17)$$

— both deduced values being close to that of Hipparchos-Ptolemy [$P_H = 1°.00$], but far from the truth for antiquity (1°.38).

E Prejudices [The Part Totally Suppressed by Hoskin]

E1 Hipparchos' lack of originality in his tropical year (eq. 10) and his centennial precession of $P_H = 1°$, should not entirely surprise us.[23]

E2 However, to forestall potential misunderstandings of the implications of this paper, I must here stress that there is little comparison between his failings and Ptolemy's with respect to the issue of non-originality.

E3 His month (eq. 13) was accurate (thus agreement is not suspicious — his own researches indicated a trivially different value).[24] His Y_{Ht} was based upon a comparison of solstices (wiser than using equinoxes):[25] his own -134/6/26 1/4 Summer Solstice [error -1^h] and Aristarchos' -279/6/27 1/2 Summer Solstice [error -16^h] (The total $+15^h$ error[26] of the time-difference, for 1 1/2 centuries, led directly to the fateful error

[19] Transit-observations of ordmag 1′ accuracy were within the capability of some ancient astronomers. See *Alm.*, I 12 [also Rawlins 1982 (of fn 3) n.17, and Rawlins 1985 (of §D3) §§3&5.]

[20] Swerdlow *op. cit.*, 292; other scholars ([including Swerdlow and] myself) have since rediscovered Mayer's finding, primarily Kristian Moesgaard and Raymond Mercier (see *ibid* 293), contra R.Newton.

[21] Swerdlow (*ibid.*, 292) suggests that $R_{St} = -304$ (fn 17, above) might have been rounded to $R_{Ht} = -300$ via a rounding of a Kallippic cycle of $940M$ to $27758^d 45'$, followed by a 2nd-sexagesimal-place rounding of a 76th of the result, which would indeed give $365^d 14' 48''$ (*Alm.*, III 1). [This speculative reconstruction is not at all impossible (similar discussion on rounding at *DIO 1.3* fn 274);] but without the 1st rounding, the upshot would've been $365^d 14' 49''$, thus $R \approx -327 \approx R_{At} = -324$ 8/15 (Table 2 list A). Alternate theory: 432 B.C. to 135 B.C. ≈ 300^y (fn 14 above).

[22] See fn 29 below. Perhaps the most obvious proof that Ptolemy was not a regular observer is the simple fact that he did not know his own alleged observatory's latitude! (*Alm.*,V 12-13 — an error of $-14'$.) [See Rawlins 1987 (of §E3) p.236 item (2).]

[23] [See, e.g., *DIO 1.3* §N16.]

[24] *Alm.*, IV 2. Note that use of lunar eclipses for finding the synodic month and the sidereal year allows far greater accuracy (from modest visual work) than the use of solar transit observations (*Alm.*, I 12 and III 1) allows for the determination of the tropical year (see above at §§D3&refsec-aD4). See Rawlins (of §D3); also *DIO 6* ‡1.

[25] See R.Newton, *Crime of Claudius Ptolemy* (Johns Hopkins Univ, 1977), 81-82 and 85-86. Also Rawlins here at fn 14 [and at *DIO 1.1* ‡5 fn 20].

[26] See fnn 14&16, above.

in Y_{Ht}: $0^{\mathrm{h}}.1 = +6^{\mathrm{m}}$.) Hipparchos could be faulted for (probably [see *DIO 1.1* ‡6 §E5]) prejudiced selection of the required ancient-to-him [S.Solstice] (Aristarchos'), but nothing more. His own solstice's error was trivial. (Aristarchos' was not. [For cause, see fn 14.]) Hipparchos' 21 [extant] solar observations are of fair accuracy (rms error 8^{h}) — normal random and systematic[27] errors. By contrast, Ptolemy's 4 solar "observations", which agree with Hipparchan data to within less than a half-hour in all cases, disagree with reality by [21^{h}, 33^{h} (twice), and 36^{h}]. The Ptolemaic data's [31^{h}] rms deviation from reality is over 50 times [their $0^{\mathrm{h}}.6$ rms] deviation from Hipparchos' solar tables. For Hipparchos himself, this ratio is only about 2 (instead of 50+) and even much of that is due to the fact that of course his tables were fit to his observations. [Thus, the common blanket-slander that ancient scientists were non-empirical is simply one more Hist.sci-establishment fantasy. See Rawlins, *Amer. J. Physics*, lv (1987), 235-239, n.12. See also fn 19 here.] (Even so, Ptolemy's "observations" adhere 5 times more closely to Hipparchos' tables than do the Hipparchan observations on which the tables were founded!)

E4 The point to keep in mind here is that whereas gauging the *year* entails dependence upon a predecessor's work (e.g., Hipparchos' use of Aristarchos'), finding solar *positions* has no such dependence. (Thus, the impact of the above-cited ratios.) The reality of Hipparchos' data for position is clear from a glance at his solar theory: despite its fallacious year-length (mean motion), its longitude-at-epoch is very close to the truth for his own time. [See *DIO 1.1* ‡6 §D7.] On the other hand, all of Ptolemy's positional "observations" are consistent with this same theory — a theory that gave correct positions only for Hipparchos' time, but was slow by over 1° by Ptolemy's epoch.[28]

E5 R.Newton's analyses of Ptolemy's data (especially solar and stellar)[29] have concluded that he was not simply prejudiced but that he systematically deceived (to high precision) in support of these prejudices. Newton's conclusion has been attacked with such passionate disbelief in a variety of journals (by commentators all whom had, in earlier publications, prejudged Ptolemy as a great astronomer), that many onlookers may not be aware that a number of scholars agree that Ptolemy has indeed been shown to have been a liar. These include B. L. van der Waerden and myself.

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I thank Willy Hartner and Kristian Moesgaard for their useful comments upon the preliminary (1980/6/15) draft of this paper, and Robert Newton for his helpful advice on the 1981 draft.

[27] All of Hipparchos' Vernal Equinoxes are a few hours early; all his Autumnal Equinoxes are a few hours late. (This is perfectly consistent with his transit instrument's equator having been set a few arcminutes low. [But see the attractive alternate theory implicit in Swerdlow's proposed Hipparchos solar parallax: *DIO 1.3* fn 280.]) By contrast, all of Ptolemy's equinoxes (Vernal *and* Autumnal) are late. (Very. The smallest of Ptolemy's three Equinox errors exceeds the largest of Hipparchos' twenty Equinox errors — Ptolemy's rms error was 4 times Hipparchos'.) This circumstance cannot be explained by misplacement of the transit instrument, which Ptolemy says he used, *Alm.*, III 1 (unambiguous ref. to *Alm.*, I 12; see R.Newton, "Comments on 'Was Ptolemy a Fraud?' . . . ", *Q. Jl. Roy. astr. Soc.*, xxi (1980), 388-399, pp.389-390).

[28] This is illustrated, to devastating effect, by Raymond Mercier at p.215 of *British Journal for the History of Science*, xii (1979) 211-217, his review of R.Newton, *Ancient Planetary Observations . . .* (Johns Hopkins Univ, 1976). Note too that almost all of "Ptolemy's" *Alm.* tables end in 82 AD [a fact first broadcast by A.Rome] .

[29] See R.Newton, *opera cit* (fnn 14, 25, & 29) and "On the Fractions of Degrees in an Ancient Star Catalog", *Q. Jl. Roy. astr. Soc.*, xx (1979), 383-394; also Owen Gingerich, "Ptolemy Revisited . . .", *Q. Jl. Roy. astr. Soc.*, xxii (1981), 40-44, p.42.

F *JHA* Editorial Integrity. Again . . . . [Note Added 1999 Dec]

F1 The foregoing is the paper long referred to (semi-facetiously at first) as "Rawlins 1999", in numerous articles appearing in this journal. (Some of the history of its persistent suppression was previously recounted at *DIO 1.2* §B2 & fn 9.)

F2 Since the paper's main math opposed (fn 20) a specific R.Newton contention, Owen Gingerich encouraged the expansion of a brief 1980 version of it for publication in the *Journal for the History of Astronomy*. Almost two decades ago, this was submitted (1981/8/7) in the version printed here at §§A-E and was promptly accepted[30] (1981/9/17) by *JHA* Editor Michael Hoskin. However, the 1981 version ended with a section (§E) that defended Newton's general view. In retrospect, it is obvious that the *JHA* wanted only the anti-Newton part — but didn't want to say so. Thus, there ensued a ridiculous series of publication-postponements (during which time the paper could have been before an honest journal), accompanied by increasingly thespian expressions of sorrowful apology.

F3 DR repeatedly inquired as to whether the *content* was a problem. Consistent denials of this (indicating *JHA* reluctance to *appear* censorial, evidently hoping that enough delay might push DR into angry withdrawal of the paper, thereby neatly solving the *JHA*'s theological dilemma) were followed by a sudden 1982/7/27 demand for *immediate* DR assent to a version chopping off the concluding section (the pro-Newton part: §E above), which had committed the special sin of pointing out (what the *JHA* was loathe to reveal) that some reputable scholars such as van der Waerden (fn 34) did not agree with the *JHA* crowd's rejection of Johns Hopkins Univ physicist Robert Newton's recent charges that Ptolemy was a liar. The *JHA* continues to imitate its hero's ethics in its private explanations of this incident. (See *DIO 1.2* §B1. The attendant ugliness, false slander, and censorship led ultimately to the birth of *DIO*.) [In 2002, *JHA*'s smear was published nationally. See *DIO 11.1* p.2.]

F4 The *JHA* has quite recently shown that its integrity has not changed: it continues to dishonor its own original agreement to publish the full paper, as it *even today* insists that §E5 must be censored. Disturbed that the paper's remarkable results were being kept permanently from the academic community, and increasingly convinced (by the behavior described below) that the *JHA* would exercise interminably its idea of high creativity by continuing to find some excuse or other to impede the prospect of publication (thereby throwing away a perfect opportunity for that journal to improve relations with *DIO* — and, more important, to demonstrate the large-minded universality to which it seems to aspire), DR has decided to publish here in *DIO* the unbowdlerized paper.

F5 Readers who expect (hope?) to be shocked at the original prose will be badly let down. The true shock here is how afraid and one-sided the *JHA* continues to be. It cannot print any paper that says in so many words that Ptolemy was a liar. [But: no-problem for papers opposing that view; see, e.g., §F8.] It cannot even admit that there is a serious controversy about whether he was. (Check for yourself what the *JHA* finds so hideously offensive at §E5.) Instead of falsely laying blame (§F6) on others, for a problem that arose entirely because of its own intellectual and ethical limitations, the *JHA* might consider surprising its critics (e.g., *DIO 4.3* ‡15 §H10), by rethinking its goals, and reflecting on how the devil it became so politically narrow that it would actually suppress key new findings

[30] DR had even gone to the trouble of putting the paper into the *JHA*'s inexplicably-preferred style. Some of the additions to it here are done *DIO*'s way; thus the odd mix of styles in the present version. The paper has been improved and augmented here&there. Serious additions are in brackets. None of these alterations have anything to do with the offending sentence in §E5.

(certified as such by both of its own eminent referees)[31] and *for years* hold these results hostage to an insistence that DR betray an honest scholar (R.Newton) and the obvious truth — and consent to **join in the *JHA*'s obsessive decades-long try at misleading academe regarding the state of the Ptolemy controversy**. (How can Hist.sci be taken seriously as a discipline, when one of its most prominent journals resembles a church? — banning ideas, even researchers. When did the Hist.sci community begin uncomplainingly taking it for granted — as an enduring & unalterable Reality — that a politically prominent academic journal will rigidly promote a certain viewpoint, boosting only one side of a controversy while handicapping another?)

F6 A good start at reform here would be a retraction of the behind-the-back falsehood (*DIO 1.2* §B1) that DR was "impossible to deal with" regarding the present paper. (Check *idem* or here at fn 31 to see just how perversely opposite the actuality was.) Several years ago DR warned in *DIO* that (with the millennium ending), the paper's increasingly-unhumorous tag as "Rawlins 1999" meant that time was running out for Hoskin to "fulfill his written agreement" (*DIO 6* ‡3 fn 24) to publish the paper. Honest Hoskin made no reply. (Since *DIO* began, Hoskin has just mailed back all issues, unopened. He thinks this proves something. And it does. Question: Is anyone in the Hist.sci community concerned at what is done to the reputation of academe, when the publisher of the leading British journal for astronomical history refuses to communicate with his US counterpart? For over 16 years now.)

F7 With 1999 passing, and while there seemed to be growing hope[32] of encouraging peace, mutual tolerance, and *open discourse* between the formerly warring camps and journals, DR asked *JHA*-Number-Two-Leader about the "on-ice" paper (as he had come to call it). He said (1999/7/3) that THE problem with the paper was a single terrible sentence — though he couldn't quite remember what it was. Unable at first to find his original copy, he wrote DR, carefully attempting to commit the *JHA* to nothing (forgetting that an honest journal would already be committed, after it had thoroughly refereed and accepted the paper — especially in light of Hoskin's airs about being bound by referees' advice, whenever this suits *JHA*'s wants). Number Two said (1999/10/7, emph added): "I would certainly be willing to urge a *reconsideration* of [the paper] upon receipt of a clean copy of it with the offending sentence removed." (This is an important clarifier of what had originally gone on behind the scenes, because Hoskin had never even been frank enough to admit that this sentence was the problem. He'd originally gotten rid of the sentence by just wide-broom-sweeping-out the whole pro-Newton section [§E] that included it.) DR sent along a photocopy of the paper, knowing that no sentence in it was improper — and therefore challenging Number Two to find such (1999/10/15): "I look forward to learning what the intolerable sentence in it is." Reading the paper, Number Two soon learned that no sentence in it could be defensibly condemned. He then dug out his original 1981 copy — but still found no impossible sentence. So, he shifted to a new ploy: he excerpted just a portion, a **phrase**, from the sentence in question! — and used that as an excuse to keep on[33] arguing for censorship (1999/10/25): "You will not have a ghost of a chance of getting your paper published in the *JHA* as long as it contains the phrase [at §E5] 'that Ptolemy has indeed been shown to have been a liar' ". However, the full sentence is not a claim of proof (as the excerpting makes it sound), but is rather a description of the Hist.sci sociological situation on the issue. (Must the *JHA* mutilate a sentence, in the cause of mutilating the journalistic truth of its attempted mutilation of an article on a long-mutilated historical

[31] The paper was submitted in final form 1981/8/7. Hoskin soon reported (1981/9/17) that the "paper is accepted in principle but we need to work at *getting your message across to the reader*. Moesgaard has very kindly agreed to try his hand at a draft modified version for your consideration and I hope to send this to you in ***a few weeks***." (Emph added.) The referees were Willy Hartner & K.P.Moesgaard. Hartner had (after initially nixing the paper) directly informed DR of his positive verdict on 1980/8/15, but Moesgaard's prompt referee report on the 1981 version was long held private — even when its content was requested. The report recommended that the paper be softened. (Readers here of the original 1981/8/7 DR version [keeping in mind that brackets indicate post-1981 changes & interpolations] can now judge for themselves the validity and purpose of that suggestion.) This intent was kept private for a half-year — despite DR's direct 1981/11/27 questions about what Moesgaard was to do. Instead of imparting his plans, Hoskin denied to DR that any tampering with the content was to occur (a denial Hoskin maintained right up to the 1982/7/27 moment when the *JHA* rush-demanded tampering, and simultaneously relayed Moesgaard's report at last). After DR offered in late 1981 to have a friend (the former Editor of the American Geographical Society) do the ever-elusive clarity-enhancing revision, that was allegedly delaying the paper month after inexplicable month, Hoskin declined, saying (1981/12/27): "It's totally a matter of making the message clearer to the reader. That's all." And: "The paper won't present any problems. . . . everything's OK and there's no problem. And you'll be hearing from me within the next month." More months passed, along with two more apologies. (E.g., "especially to apologize for my delay in revising your paper & to thank you for your patience. It must certainly go in the October issue . . . . Many apologies!") DR treated Hoskin with such extreme politeness throughout this charade that it bordered on obsequiousness. DR actually offered (seriously) to visit Cambridge to assist the work of revision; Hoskin declined the help, but replied (1982/5/30, emph added): "I feel very guilty about this. It's simply a pressure of other things. . . . *it's nothing more than that* . . . . You've been extraordinarily patient. . . . again my thanks for your patience."

[32] See inside cover of this issue. Also, the *JHA* had in 1998 published (without the slightest interference) John Britton's erudite torching (*JHA 29*:381-385) of the gas in Swerdlow's 1998 P.U. book, though Swerdlow is one of the *JHA*'s "Advisory Editors". And a paper from Russia (an earlier version of which was commended at *DIO 4.3* ‡14 fn 4) giving evidence for Hipparchan authorship of the Ancient Star Catalog is said to be in the offing at *JHA*. One hopes that the authors will not be cajoled into warping the logical conclusions of their own research — as has happened in the recent past in Hist.sci. (See, e.g., *DIO 1.2* §I8. Process satirized [barely] at *DIO 6* ‡3 fn 11.)

[33] Hmmm. Why does such censorship mean so much to the *JHA*? Is the intent to aggravate DR until co-existence breaks down? (And then blame the result on him?) Or is the *JHA* just now so deeply into this wringer that it thinks it must resort to any available means of conjuring up a non-existent problem with the paper, because the journal can't admit that it never had cause to censor this article in the 1st place? Keep in mind that Hoskin's 1983 severance of communication with DR was caused not by the paper's contents but by DR's openly — not behind Hoskin's back — and gently (note old-proverb at *DIO 1.2* p.96) pointing out the *JHA*'s poor refereeing of a miscomputed paper it published late in 1982 (*DIO 1.2* §B2 & fn 8). But the heaviest underlying factor behind *JHA* censorship of the DR paper is elementary: keeping control of centrist-forum discussion of the Ptolemy-as-liar issue can be accomplished by the simple scheme noted at *DIO 2.1* ‡3 fn 8: "tape one side's mouth shut." For the unfettered other side, see here at §F8; see also the gratuitous false attack on Newton's character (pointed out by Thurston at *DIO 8* ‡4 §A19), appearing in a 1998 book that was vetted and aggressively promoted by two *JHA* archons. [Remainder of note added 2000 Jan.] This attack is in fact based upon 1993 remarks first appearing in the *JHA* itself: "Newton's arguments were based on apparently dispassionate statistical tests, but the concluding sections of the book [*Crime* 1977] were marked by a personal animosity of surprising intensity toward Ptolemy" (*JHA 24*:145). Note that neither of these *JHA*-inspired attacks on Newton's character (& of course his competence — it doesn't get any more ironic) has given us *any quotes whatever* from a pure-*JHA*-fantasy hatemonger-Newton, to justify *JHA*'s charge of "personal animosity". I.e., this is simply more *JHA* smearing of Newton, intended to convince the reader that Ptolemy's fraudulence is nothing but the figment of a lone (§F8) nut's enraged & unbalanced imagination — when the sharply ironic truth is that the mirrorlessly-projecting *JHA* is itself the party whose undeniable personal animosity is doing the imagining. No one has ever presented any evidence to support the lie that Newton's work was motivated by personal anger — though dislike of massive cheating wouldn't merit condemnation, anyhow — or was (*ibid* p.146) "ahistorical", except insofar as the *JHA* (again the actual fantasizer here) megalomaniacally supposes that it blesses its lessers by defining for them what is and isn't legitimate history. (The *JHA*'s central historical Principles [!] are ever so solemnly set forth at *JHA 11.2*:145; 1980 June; reactive comments at *DIO 2.1* ‡3 §B2.) While Newton presented proof after proof (see H.Thurston's summation at *DIO 8* ‡1) of his theory of Ptolemy's character — *which is a legitimate and major science-history question* (fn 34, contra *DIO 2.1* ‡3 §B7) — no proof at all is required by the *JHA* whenever the urge seizes it to trash Newton's character again. For the ***explicit*** smear-'em-back logic that governs the *JHA* circle's behavior in this controversy, see *DIO 2.1* ‡2 §H17.

truth? See ‡1 fn 10.) The actual, entire §E5 sentence: "Newton's conclusion has been attacked with such passionate disbelief in a variety of journals (by commentators all whom had, in earlier publications, prejudged Ptolemy as a great astronomer), that many onlookers may not be aware that a number of scholars[34] agree that Ptolemy has indeed been shown to have been a liar." That's a very different statement than is indicated by the snippet-phrase.

F8 So: does the even-handed *JHA* stifle *all* scholars who try to describe in its pages what the academic community thinks about the issue of whether Ptolemy was a liar? Wellllllll — no. And certainly not if a scholar is "safe" — i.e., can be trusted to conclude in favor of the *JHA*'s two granite religious tenets in this connection: [a] Ptolemy was not (provably) dishonest, and [b] Reputable academic consensus agrees. E.g., in a book review, the *JHA*'s most servile pawn on the Ptolemy debate, is permitted to discuss the *very same issue* — general academic opinion on Ptolemy's lying — which the (rather more important) above paper was long suppressed for mentioning! His comment at *JHA 24*:145 (1993): "Newton denounced Ptolemy as a liar and a plagiarist, and called into question his competence as astronomer and mathematician. . . . Very few historians have accepted Newton's conclusions in their entirety."

F9 We conclude with questions which deserve provident consideration.

Question 1. What should we think of a journal that would ever-so-deftly[35] resort (while suppressing vital research) to excision-sleight and inequitably-applied pseudo-rules — as a means of convincing onlookers that it is (of all things!) playing fair with contributors?

Question 2. Do any of the *JHA*'s decorative "Advisory Editors" or its occasional high-quality contributors[36] even care?

[Note added 2001: In the foregoing, there lurks (at §B7) yet another precise appearance of the Aristarchan number 4868. (This one was missed by all previous scholars, including two decades of DR's own researches.) Simply check the number of years between Meton's famous bedrock −431 Summer Solstice and the Hipparchos Ultimate-Orbit epoch −127 Autumn Equinox: it's $304^y 1/4$. This number is *exactly* one sixteenth the number of years in the "Great Year" (fn 9 & §A2) of Aristarchos. That is, the Meton-Hipparchos epoch-interval is $4868^y/16$ — on the nose.]

[Note added 2013: The full, astonishingly extensive Aristarchan-Hipparchan geometric-series context, into which $304^y 1/4$ neatly dovetails, is set forth at *DIO 11.1* ‡1 fnn 14, 16, & 17.]

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[34] One of these scholars was the great B. L. van der Waerden. Before concluding (in his final book, 1988) that Ptolemy lied (see also *DIO 1.1* ‡6 fn 37), a previously neutral van der Waerden was shocked that O.Pedersen's otherwise valuable 1974 book on Ptolemy had not even dealt with the issue; van der Waerden commented in *Annals of Science 32.6*:603 [1975]: "the question of whether Ptolemy was a liar is important for everyone who wishes to appreciate the value of Ptolemy's work. Pedersen does not even tell the reader that Ptolemy's sincerity has been doubted by serious scholars, and that extensive calculations have been made to check his statements." (Similar defense tactics discussed at *DIO 4.3* ‡15 §§D2&D7.)

[35] Shades of the ineptitude of another of History-of-science's anti-DR censorial moves (by Robert Kargon), described at *DIO 2.1* inside cover. (Note: *DIO* has since been restored to the library shelves at Johns Hopkins University.)

[36] R.Stephenson is both.