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DIO

Babylonian Lunar Periods' Lost Sources:

The Most Ancient Recoverable Eclipse-Record: 1274 BC

Aristarchos' Great Year

Hipparchos' Use of an Eclipse of 1245 BC

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Thanks&News [SmearBotch, McCarthyism, Lie & Telescope]

Hugh Thurston has contributed a sharpeyed and extremely helpful proofing job upon this issue’s scholarship, leading to material improvements at numerous crucial places within.[1] [And Leroy Ellenberger has impressively kept nailing *DIO* typos.]

In the 2002 March issue of the History of Science Society’s *Isis* (93.1:58-69), Thurston has done the historical community a high service by quite extensively explaining and summarizing some among the most important of a broad canvas of new findings and viewpoints in ancient astronomy created during recent decades by Robert Newton, DR, and *DIO* — with additional perceptive comments and revelations by Thurston (esp. pp.62&64).

[Missman Dep’t: hitman-wannabe *Sky&Telescope*’s 2002 Feb (p.40) Owen Gingerich-copying[2] (*Discover*-reject) smear of *DIO* betrayed advanced dementia in cliques who dread *DIO*’s increasingly wellknown & top-expert-backed open forum, which OG’s semipopsci cult has long affected to ignore. (Some *S&T*-published falsities were corrected at *S&T* 2002 June p.12, by the world’s leading planetary-motion expert, E.M.Standish of CalTech-JPL & *DIO*.) Never in its 60^{y} history had *S&T* run such an ad-hominem attack upon a capable astronomer. Worse, its dirt was based on documents it *hadn’t even consulted*, **merely echoing Gingerich’s gossip instead**. *S&T* pretends to possess an abusive, war-starting DR letter to *J.Hist Astron*’s M.Hoskin, but MH is loathe to release it publicly (despite *DIO* challenges to MH-*S&T*) since DR’s letter triggering MH’s abusive 1983/3/3 rage (charging a “damned lie” by DR & intimating libel-suit & ostracism: trifles omitted from *S&T*’s telescoped story!) was just sympathetic but pointed constructive criticism. (DR’s “lie” was that refereeing of a misbegotten 1982 Oct *JHA* paper was poor; but, to *JHA*’s chagrin, after unhinged MH’s 83/3/3 tantrum, the paper’s memorably honest author agreed with DR [on math & reffing], and re-computed his main results: 1984 June *JHA*.) *S&T* went on: DR’s “abuse” was revenge (classic projection-fantasy) for *JHA* rejecting a 1982 DR paper, a paper in fact recommended by both *JHA* refs & *JHA*-accepted (see *JHA* ad: 1982/3 *Isis*). But again: *S&T* silently lies[3] low, after *DIO* requested the date of *JHA*’s “rejection”.]

[1] [Alex Jones’ advice, on paper ‡3 here, caused enormously implication-laden June expansion of its investigation: §D1. (For erudite doubts [of our work] by Jones & John Britton, phone 416-978-0483 & 307-734-0881, resp. Both seem rather odds-immune to some of *DIO*’s reconstructive matches, e.g., ‡1 eqs.12&13, and *DIO 11.2* p.33; yet Jones’ skepticism has proved justified in a key area: see *idem*.)]

[2] [See Gingerich-vs-*S&T* textual comparison at www.dioi.org (2002/2/20 link) plus *DIO*’s (*S&T*-suppressed!) letter to *S&T*. Hmmm. *S&T*’s author has owned (email 2002/4/23) DR’s fraud analyses of Vespucci, Tycho, Neptune, Cook, Peary & Byrd are all valid. (He dissents on Ptolemy, deeming it not fraud to pass off 100s of indoor-plagiarized star-data as one’s own 1sthand outdoor work.) Yet *S&T*’s version of his article painted DR as a volatile crank, of no judgement or achievement, *just* the 25^{y} slanderous tack of self-imprisoned (*DIO 1.2* §D4) careerist-heresyhound OG, history-gooroo to *S&T* & the AmerAstrSoc’s blithely-unsupervised HAD, which clubbily shuns the pro-free-speech winners of the field’s hottest (*S&T: loc cit*) controversy, while blessing with Doggett-Prize cash the censorial losers: needy politboss-smearbunglers OG & (highschoolmath-level businessman) Hoskin. *S&T* is the 3rd journal in barely 1^{y} led into error by OG. Other cases: *DIO 13.1* p.3 fn 1 & *DIO 11.3* ‡6 §E13.]

[3] [Familiar act: bully-attacking heresy with here-in-my-hand hidden documents. After 4^{y} of 1950s terror, the Senate censured witchhunter Sen. McCarthy. But, faced with *S&T*’s ***inverted*** (*DIO 11.2* p.30 fn 3) fable-smear: the understandably ethics-committeeless AmerAstrSoc & its HAD (HistAstronDiv) simply hid. Equally revealing: AAAS’ *Science 260*:1587 story on Roald Amundsen was based upon *WashPost* 93/6/1 coverage citing a ***scientific***-journal (*DIO 2.2*) as primary source, yet *Science* cited only the ***pop*** secondary-source (*Post*); *Science* can’t name another such incongruity in its long history. Should *DIO* feel perversely honored at such unique focus from *S&T* & *AAAS*, 2 forums which for 30^{y} have also repeatedly defended (ancient) astronomy’s #1 liar, printing zero dissent? Happily un-perv: *DIO*-DR appreciation by, e.g., *Nature*, *Astronomy*, *NYTimes*, *Isis*, etc; & prime scientific justification of 2004 Dec *Scientific American* cover-billed story “Stealing a Planet”: honestly cited to *DIO 9.1*.]

DIO

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

Contributors should send (expendable photocopies of) papers to one of the following *DIO* referees — and then inquire of him by phone in 40 days:

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Afterthoughts

This *DIO* issue produces unexpected & compelling new evidence that backs a favorite *DIO*-opposition viewpoint (a Hipparchos-Babylon connexion), suggesting an enormously greater astronomical-recordkeeping achievement (than hitherto known) by the very Babylonian culture so precious to said opposition. Thus, we have an open shot at shrinking some long-running, hitherto-intractable academic divides (p.3). Will the chance be seized?[1]

Today's formerly-dominant ancient-historian clique has 2 loves: C.Ptolemy and Babylon. But: what happens when one lobby clashes with another? (For parallel instance, see *DIO 4.3* ‡13 §G5.) Answer: it turns out that even the testimony of historians' longtime Greatest Astronomer of Antiquity, Ptolemy (whose reliability historians have for decades defended against *DIO*), has become vastly suspect and expendable.[2] Let's review the astonishing breadth (and temporal length & cranial-paleo thickness) of historian-denial here. In *Almajest* 4.2, Ptolemy explains how the best ancient monthlengths were achieved in pre-100 BC times. He there states that the Babylonian synodic and anomalistic monthlengths (‡1 eq.2) came from observations of the 4267 month eclipse-cycle. Certain historians don't believe it. He says that the Babylonian draconitic month (‡3 eq.1) came from observed equal-magnitude eclipse-cycle data. Same historians don't believe it. He says that this draconitic month was due to Hipparchos. Same historians don't believe it. Summing up: all the high-accuracy monthlengths Ptolemy reports (*Almajest* 4.2) from before his own time were directly based upon long eclipse-cycles. Same historians don't believe any were. And what ancient attestations of alternate methods' use do these stalwarts produce? None.

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[1] [Note added 02/9/26f, long after 5/31 posting (www.dioi.org) of this *DIO* & its olivebranches (above, p.10, etc) & alerting of top Muffiosi to them.] Cliquish reaction rules on the present *DIO* issue and (more revealingly&enduringly) on far less reasonably-arguable (*DIO 11.2* p.33) Muffia-offending achievements. (*DIO* is reacting&nonreacting regretfully but aptly.) Esp. sad: [i] Muffia 2000-2002 failure to engage *DIO* despite *Isis*' & (with generous debate-terms) *DIO*'s invitations. [ii] The low-comprehension unappreciativeness of most of the all-Muffiose ref-reports upon Thurston's *Isis 93.1*:58 paper. (Though one report deemed *DIO 1.1* ‡6 [incl. our 1000000000-to-1 fit: above p.2] valid & "brilliant".) [iii] OG's all-too-typical "reply" (*Isis 93.1*:70) was 100% ungenerous. (See also *DIO 11.3* A3.) [iv] Gratuitous continuation at *JHA 33*:15-20 [2002] p.17 of unanimous rejection ***for 70 yrs now*** of A.Diller's greatest discovery, as *JHA* junks Diller's lovely 12 latitude-data-based triumph (key to sph-trig-inception chronology) & accurate ancient measure of the Earth's obliquity & trashes (n.9) a 13th datum ($12^h3/4$ klima) without noting Diller's theory fits it too, *on the nose*. All this in order to push a (*nonexclusive*) theory, based on 1 latitude's 1 datum, which doesn't even fit! (Thurston notes also solstice-equinox confusion: *JHA* 2002 p.15 line 6 [similarly at *ibid* p.16 line 4]; *JHA*-mythic-referee-hifi-déja vu: *DIO 1.3* ‡10.) Diller ironlock-vindicated & double-newdata confirmed: *DIO 4.2* p.56 Table 1; but as always uncited at *JHA 33*:15f [& *JHA 35*:71f].

[2] [Note added 2002/8/13-9/9.] As perceptive scholars will see right off, it's *inherently* likely that the several lunar periods here investigated were based upon huge eclipse cycles. I.e., the only serious question here is: *which* multiple of a relation recovers the underlying eclipse cycle, not *whether* eclipses are the foundation of extant pre-Ptolemy lunisolar relations. (Does any scholar really think that ancients didn't know about the 781^y & 800^y eclipse cycles?! — when: [a] one-fifth of the 800^y relation is anciently attested [‡3 §E1]; [b] we actually possess records of two famous ancient eclipses separated by 781^y [Rawlins 1996C §I5]; [c] the 781^y & 800^y cycles are arithmetically linked by the famous 19 yr cycle [‡1 eq.9]; [d] 800^y is an especially vital eclipse-related period, ***the shortest time in which lunar eclipses return to the same star*** [Rawlins 1996C §I1]; [e] the key 781^y lunisolar relation was known to Ptolemy [*ibid* eqs.27-31], so who'd say he didn't know it was an eclipse cycle?!) As noted (‡2 fn 2 & §A2), the relations' accuracy and the eclipse-cycle method's attestation-exclusivity should've made the truth clear long ago. (In general, the Muffia has [1] failed to find compelling solutions for the major parameters of its own field, [2] can't even *recognize* such when handed them, instead [3] shunning, suppressing, and-or slandering the discoverers.) Why would balanced, provident scholars reject the plainest path-to-solution *as utterly valueless* and thereby commit-for-life to an inevitably-doomed deny-deny policy? (Instruction via J.Bishop at Rawlins 2003J p.32.) Analogous to National Geographic (*DIO 10* §R3) & Gingerich (*DIO 11.3* ‡6 fnn 12 & 57) attempts to fool observers into accepting that *altering empirical-data records* is acceptable, "ingenious", and-or "brilliant"!

Hopes & Apprehensions

Some comments prefatory to the revolutionary (simultaneously counter-revolutionary) key discoveries revealed for the first time in this *DIO* issue, where:

[a] In a simple analysis (‡1), which DR delivered on 2001/6/27 to a British Museum conference, the preeminent Babylonian System B monthlength is mathematically traced (redundantly: ‡1 §§A9&B5) to Aristarchos of Samos, the immortal 1st public heliocentrist. This parameter's astonishing accuracy (‡1 §A3 [b]) is shown to be based upon an attested (‡3 §E5) and surprisingly elementary evidential foundation: long-eclipse-cycles (‡2 §A3).

[b] A persistent succession of evidences unfurls (‡3 §§C3-C8), culminating in awareness that Hipparchos (who worked on the isle of Rhodos) may have had access to priceless 13th century BC eclipse data — preserved for a millennium by the priests of Babylon — which he used to find the draconitic month to an accuracy of 1 part in ordmag 10,000,000.

[c] We ultimately solve for (and date) the empirical sources of *all five* precise Babylonian monthlengths (in some cases identifying the specific eclipses involved): the System A synodic & anomalistic months (‡2), the System B synodic & anomalistic months (‡1), and the System B draconitic month (‡3). At least some (I believe the majority) were Greek.

For those obligatorily pre-ordained to resist our findings, various excuses for invincible innocence will have to be conjured-up. Most are easy to anticipate; e.g.: there's-not-a-jot[1] of *direct* evidence that the Greeks had the "Babylonian" month first. Or, obviously: there's-not-a-jot of *direct* evidence that any 13th century BC Babylonian record survived until the 3rd-2nd centuries BC. (Etc. See, e.g., ‡2 fnn 2&7.) But the word "direct" will be omitted from such lawyering. Those lacking mathematicians' probability-sense (note well who *does* have it: Thurston 2002S pp.60&62), can't see that when a wide spectrum of disparate mysteries simultaneously finds potential solution from a narrow[2] spectrum of tight-fitting new postulates, this ***is*** encouraging Occamite evidence in favor of those postulates.

Some will be delighted to share in the happy surprise attendant to the following startling & very recent (2002/3/18 & 4/3-4) development of evidences that Babylon's priests tried (amidst wars' ravages) to preserve lunar cuneiform records with the same millennial-scale reverential (& perhaps proprietary: ‡3 §D4 [vs *DIO 13.1* ‡2 §E4]) dedication which Egyptian priests devoted to hoarding pharaonic corpses. What separates those who can&can't see is that above-cited sense (& see ‡2 fn 3) of what is&isn't probable, an instinct for when data-fits are *so* extra-chance (*DIO 11.2* p.33) that the fitting theory should [despite omnipresent risk of falsity] be taken seriously, even if it annoys the inertial anointed.

I'm reminded of one of Hugh Thurston's favorite jokes, about the Voice-of-Authority who decreed: "There are three kinds of people. Those who can count, & those who can't." We will soon enough find out whether those who shun (e.g., the ‡1 eq.12-eq.13 match) belong to the 3rd or to the 2nd group cited. (I.e., what will we have: an empty set, or a set of empties?) As an ancient-historian who obviously hasn't learned much from the recent socio-history of his own field, I'm earnestly hoping (pp.10 & 26) it's the 3rd.

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[1] Among counter-arguments: there's just-as-jotless a dearth of direct evidence that Babylonians used eclipse cycles for finding period-relations (only method capable of producing the accurate extant lunar relations), as we know Greeks did: see ‡1 fn 2, where item [b] alone reveals Greek priority with M_A or virtual equivalent (virtuality eliminated by *ibid* eqs.12-13 match [**NB**: §B6]).

[2] E.g., [a] Positing classically-extant 13th century BC Babylonian eclipse records can explain *both* the System A anomalistic month (‡2 eq.2) and the Hipparchos draconitic month (‡3 eq.3). [Note added 2003. A 3rd ancient mystery is now soluble via 13th cy BC data: ‡2 fn 21 or *DIO 13.1* ‡2 §E3.] [b] The *single* hypothesis of long-eclipse-cycle-basis (see, e.g., ‡1 fn 2 item [c]) offers a solution for every high-precision lunar monthlength, Greek or Babylonian. This approach had already achieved 3 neat successes in finding eclipse-cycles [Rawlins 1996C eqs.11, 20 & 21] underlying known ancient lunar data, the 3rd case such a spectacular hit [*ibid* eqs.27-31 !] that those historians who've long denigrated mere physicists' contributions to ancient astronomy, can only react to it by silence & fleeing debate with DR. Such tactics have long been aimed at damaging noncultists' credibility. But, given *DIO*'s steadily accreting achievements, readership, and prominence [plus its board's extremely high eminence]: **just whose credibility is dying**? Formerly, the field's longtime "owners" could be effectively impedimental to its essential progress. Will their future be just a fade to irrelevancy?

‡1 Aristarchos & the "Babylonian" System B Month

The Empirical and Calendaric Bases of Ancient Astronomy's Prime Parameter

by Dennis Rawlins

A Derivation from the 345 Year Cycle & Aristarchos' Great Year

A1 Greek civilization achieved technological superiority over Babylon — even conquering & permanently occupying the city (fn 2 [f]) — ordmag a century before our earliest records of the famous and highly accurate (§A3 [b]) System B "Babylonian" month:

$$M_{\mathrm{A}} = 29^{\mathrm{d}}31'50''08'''20'''' = 765433^{\mathrm{d}}/25920 = 765433^{\mathrm{h}}/1080 \qquad (1)$$

Current orthodoxy has been assuming that eq.1 is due to Babylon. But, using Greek relations (empirical & conventional), the following[1] will trace a very few steps of plain arithmetic leading from Greek relations (eqs.6&7) precisely to eq.1. This will be accomplished on the theory that eq.1 was due to the daring Greek astronomer and innovator, Aristarchos of Samos, the earliest scientist to teach heliocentrism widely (long-persisting ancient influences of which are noted at Rawlins 1987 p.238 & nn.34-38, Rawlins 1991W eqs.23&24, & Rawlins 1991P). The Aristarchan connexion can be made more swiftly, fully, & exactly than is possible for any Babylonian-data-based explanation (even though extant Babylonian data outnumber Aristarchan data by a factor of thousands). And we'll enjoy a series of reality-checks along the way to discovering and independently confirming the origin of this, the most central of all ancient astronomical parameters.

A2 The sole empirical foundation upon which one could firmly base a monthlength as remarkably accurate as eq.1, is the very same one which Ptolemy (from Hipparchos) cites

[1] [The present paper has evolved for over two decades. (The derivation of M_{A} from a saros-Kallippic calendar [without realization yet of Aristarchos' involvement] initially appeared in eqs.69-70 of a 1983 DR ms, which Gingerich suppressed at that time and van der Waerden was 1st to appreciate: *DIO 7.1* ‡6 fn 3. (Rest of paper now published: Rawlins 2003J.) Some of the 2nd half of the paper (culminating in eq.12's match to eq.13) was likewise suppressed (also 1983, for an "indefinite" period, by the *JHA*'s M.Hoskin [MH to DR 1983/3/21] — details at Rawlins 1999 §F [and Rawlins 1991W §B3]). Its key findings eventually appeared in this journal: e.g, *DIO 1.1* ‡6 fn 1 & *DIO 9.1* ‡3.) Thanks to the encouragement of Christopher Walker and John Steele, the reconstruction was first delivered in more mature form on 2001/6/27 at the British Museum, as part of the conference *Under One Sky*, and was then published (condensed & simultaneously with the present edition) at *Alter Orient und Altes Testament 297*:295. During the writing of this paper, the author has repeatedly benefited from the learned input of Christopher Walker, Alexander Jones, John Britton, John Steele, and Hugh Thurston.]

$$1300{:}\ \nu = [1-(3/4)^7]\cdot[1-(8/9)^5]\cdot[1-(3/4)^5]\cdot[1-(3/4)^2]\cdot[1-(3/4)^1] = 1/31 \quad (5)$$

$$1400{:}\ \nu = [1-(3/4)^8]\cdot[1-(8/9)^5]\cdot[1-(3/4)^5]\cdot[1-(3/4)^2]\cdot[1-(3/4)^1] = 1/30 \quad (6)$$

$$1500{:}\ \nu = [1-(3/4)^9]\cdot[1-(8/9)^6]\cdot[1-(3/4)^6]\cdot[1-(3/4)^2]\cdot[1-(3/4)^2] = 1/14 \quad (7)$$

E4 So, unless we are prepared to fish beyond c.1400 BC (or 1564 BC: see big fn 17 parenthesis) it's unlikely at a statistically significant level (under 1/20) that our proposed (§E1) parent eclipse-cycles are accidental. And the results that have accumulated here draw credibility from other indicia, e.g., §D1 item [d]; also, what are the odds that independent analyses would narrowly (due to the involved cycles' delicate rarity) zero-in on two [now three: see *DIO 13.1* ‡2 §§E2&E3] early eclipses over 1000^{y} before the period under discussion, yet within just a few years of each other? Plus: we have a remarkable flock of Hipparchos-related coincidences (§C9); and we already know that Babylon used Hipparchan material (& vice-versa): Rawlins 1991H §§B11-B12 (& fn 10).

E5 A subtext point: what other compelling explanation (besides long eclipse-cycles) has ever come forth — during all the centuries these mysteries have lain unsolved — for the ancient equations which we have, in this paper and preceding ones (p.3 fn 2), produced potential solutions for? The main mystery now requiring explanation is: why does a certain cult keep cohesively & adamantly spurning an obviously attractive avenue? Why so unreceptive to our long-eclipse-cycle key to explaining both the origin & the high accuracy of: [1] the ancient draconitic month (eq.1), [2] the System A anomalistic month (‡2 eq.1), & [3] the System B synodic month (‡1 eq.2)? (Revealingly, some cultists can't admit even *the possibility* that *DIO* is right here.) I hope to see the day when historians will be asking aloud why this approach was ever particularly controversial. After all, *an ancient example of each of these 3 types of months is anciently **attested** to have been founded upon long eclipse-cycles*: [1] above at fn 9 (draconitic), [2] ‡1 eq.2 & fn 4 (anomalistic), [3] ‡1 §A10 (synodic). (The last attestation is of the very monthlength which is pointlessly in question! See ‡1 §A2.) One can wish that such precedents and explicit ancient testimony (as well as some idea of how astronomers actually think & work: ‡2 §A2) will ultimately cause appreciation (vs ‡2 fn 5) for what has been achieved here.

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E Odds

E1 The technique used by *DIO* throughout the present papers and Rawlins 1996C is: tracing known ancient lunar period-relations to parent eclipse-cycles. *DIO* has done this for five precise long lunar cycles known to the ancients, of lengths: $1979^{\rm u} = 160^y$ (Rawlins 1996C §I2 [& Jones 1999G]), $2729^{\rm u} = 2961^{\rm w}1/2$ (above, eq.2), $3277^{\rm u} = 3512^{\rm v}$ (Rawlins 1996C eq.10), $6247^{\rm u} = 6695^{\rm v}$ (‡2 eq.1), $9660^{\rm u} = 781^y$ (Rawlins 1996C eq.21), where $y =$ sidereal years. But could the here-alleged parent-relations be merely a set of accidents?

E2 One approach (parent→child) asks: how likely is it that each original directly-empirical eclipse cycle (eq.3 and ‡2 eq.2) just happened to have a common prime factor(s) on both sides of the equation? (Any such shared factor[s] was of course removed [by division] when the relation was published, to simplify&compact the ratios as much as possible, a perfectly natural mathematical step, but one which inadvertently left a *disguised* eclipse-cycle to posterity. E.g., the $345^{\rm y}$ cycle, ‡1 eq.2, was of course divided by 17 to produce the famous and misleadingly [‡1 fn 5] roundish [but extremely accurate] simple relation: $251^{\rm u} = 269^{\rm v}$. See ‡1 fn 21.) For 2 numbers roughly of size N, the probability $D(N)$ that they Don't share a prime factor is nearer 50-50 than one might suspect. I find:

$$D(N) \rightarrow 6/\pi^2 \quad \text{as} \quad N \rightarrow \infty \tag{4}$$

(converging rapidly). Of the original empirical cycles we assert underlay §E1's relations, only one ($9660^{\rm u} = 781^y$) doesn't primeshare; but the 2tailed probability, of chance deviation (from expectation: 3) by 2 or more prime-shares, is statistically insignificant (c.1/6).

E3 Further, §E2 may reflect a defective viewpoint: e.g., the most accurate submillennial period-relation for any given lunar motion will probably not happen to be an eclipse cycle — and thus it will require several recurrences (multiples) before an eclipse-return appears. Compared to §E2, this reflection starts us into an inverse perspective (child→parent), which will ultimately tell us whether our five results are chance or not: what are the odds that ‡2 eq.1 (obviously not itself an eclipse cycle) would have an unknown simple integral multiple (parent) that happens to be an eclipse cycle — just by pure chance? Well, for any given period-relation, the odds[17] are roughly 1/4 that eclipse-pairs are possible for it. Now, the relation in question is $505^{\rm y}$ long — so the only possible multiplicative factors (short of a $1515^{\rm y}$ base) are twofold: 1 & 2. Taking all such factors into account (see math at fn 17), and setting the dates for our §E1 relations as not later than, respectively, 25 BC, 120 BC, 160 AD, 49 BC, 160 AD, we can compute the net pure-chance probability ν that all 5 ancient period-relations would happen to have valid eclipse-cycle parents by pure chance. We do this for several retrosearch cutoff-dates (each given [in BC reckoning] at equation's left):

[17] E.g., in a relation such as ‡2 eq.2, the draconitic remainder could have been $0°\pm25°-$ or $180°\pm25°-$. But we are here stretching things rather too near the extreme outer bound of eclipse possibility: anything beyond about 23° would so limit an eclipse-pair's frequency that a relation thus founded would be valueless. Thus we will use $\pm22°1/2$, which allows 90°/360° or about 1/4 of the ecliptic for eclipse-pair-possibilities. Going back no further than 1300 BC for the 1st eclipse, for the $3277^{\rm u}$ cycle (known from Ptolemy, 160 AD, $1460^{\rm y}$ later than 1300 BC), there are (since $1460^{\rm y}/265^{\rm y} = 5.5$) 5 potential parent cycles for this $265^{\rm y}$ cycle, found just by multiplying the $3277^{\rm u}$ relation (§E1) by 1, 2, 3, 4, or 5. (By including 1 here, we're bending over backwards not to stretch odds: after all, had the right-on-the-record-all-along period-relations [$1979^{\rm u}$, $2729^{\rm u}$, $3277^{\rm u}$, $6247^{\rm u}$] been undisguised eclipse cycles, *they'd long since have been spotted as such*. Dropping 1 from their possible-multiple ranges ***more than doubles eqs.5-7's odds: to 1/72, 1/69, 1/30, resp***.) So the chance of thus fortuitously bumping into a valid eclipse-cycle is $1-(3/4)^5 = 0.783$. Next, we find the odds on a multiple of eq.2 accidentally having $\Delta v = 0°$ or 180° (within 10°). Fraction of the zodiac involved = 1/9; so for this $220^{\rm y}$ cycle (limit 1300 BC, $1180^{\rm y}$ before 120 BC), since $1180^{\rm y}/220^{\rm y} = 5.4$, one can get a potential parent relation when multiplying eq.2 by any of 5 integers (1, 2, 3, 4, or 5); thus, the odds are $1-(8/9)^5 = 0.445$ for hitting upon an integral or half-integral anomalistic return. In series, the odds against known lunar period-relations leading us to eclipse-cycles in all 5 cases, are significant (eqs.5-6) though not dramatic. The highest improbabilities here are otherwise-based: §E4.

as eq.1's source, the precious Greek[2] $345^{\rm y}$ eclipse period-relation (*Almajest* 4.2),[3] which was (and is) of striking constancy in duration (to appreciate this point fully, compare to ‡2 §§B7&B8), due to virtually integral returns in synodic and anomalistic (lunar & nearly solar) revolutions.

$$4267^{\rm u} = 4573^{\rm v} = 345^{\rm g} - 7°1/2 = 4630^{\rm w}1/2 + 11° = 126007^{\rm d}01^{\rm h} = 3024169^{\rm h} \tag{2}$$

(Abbreviations adopted throughout: d = days, h = hours, u = synodic months, v = anomalistic months, w = draconitic months, g = anomalistic years.)

A3 Cutting past myriad confusing perturbations, this neat relation handed its discoverer (who could have found it by simple arithmetic centuries before Aristarchos, especially [see fn 7] after c.400 BC): [a] Realization of the stability possible in celestial mean motion (historically of high import: Rawlins 1996C fn 18) — this cycle may even have helped initiate the entire concept of mean motion. [b] A highly accurate value for the synodic monthlength, correct then and now to a fraction of a timesec (*ibid* fn 12). [Previous attempts to explain this accuracy have openly admitted failure. See, e.g., n.21 of Britton

[2] Several contextual points (additional to those cited elsewhere here, e.g., chronology: §A1 & fn 14) add to this paper's mathematical reconstruction, in further suggesting a Greek origin for eq.1: [a] The attestation (§A2) of its empirical basis (eq.2) is purely Greek. (Or, to mirror certain Babylonianists' obsession with text-paucity: there's not-a-jot of evidence that Babylonians knew anything about the $345^{\rm y}$ cycle that underlay "their" monthlength. Had Babylonian wisdom been at work, a highly accurate M would have existed in Babylon long before Greece conquered it. [See §A6, Rawlins 1991W fn 81 & Rawlins 2003P §H.]) [b] Other than eq.4, every equation in A is virtually equal to the others; but the oldest attested one is eq.6 — *which unquestionably was used by Aristarchos* (§A7). [c] There are several unsubtle indications of early Greek mathematical use of eclipses. See Rawlins 1985G pp.264-265, Rawlins 1991H fn 1, and Rawlins 1996C fn 34. Scientific Greek lunar observations — more sophisticated than mere eclipse-recording — certainly go back at least to c.300 BC (Timocharis): *Almajest* 7.3. The fact that the famous −330/9/20 Arbela eclipse was the last visible before the epoch of Kallippos' lunisolar calendar suggests that eclipses figured in Greek astronomical math well before Aristarchos. Our main Greek mathematical-astronomy source, Ptolemy, mentions no other Greek eclipses in this period — but, then, the *Almajest* cites no eclipses at all (Greek or Babylonian) between −381 and −200. (The not-a-jot contingent would leap all over this — except that a different Greek work cites [*GD* 1.4.2] the −330 Arbela eclipse for mathematical purposes. See ‡2 fn 7.) [d] The 1 hour remainder in eq.2 smells more of Greeks than Babylonians, since the latter's day-division was via degrees ($1^{\rm d}/360$), arcmin, arcsec, etc — while Greeks used our modern conventional hour (though Aristarchos' time-units aren't known): $1^{\rm d}/24$, the very amount of Greek-attested eq.2's revealing remainder. (Greek-hour rounding eases our path to $M_{\rm A}$. Since the actual average ancient 4267 month span exceeded $126007^{\rm d}01^{\rm h}$ by a few tenths of an hour, a Babylonian expression for this time-span would have likely been $126007^{\rm d}$ + c.20°, which by eq.3 would've produced a value for M discrepant with $M_{\rm A}$ already by the 3rd sexagesimal place: 9 instead of 8. [But *note well*: Britton's §A8 theory eliminates the problem.] [e] The most famous among the extremely small number of Babylonian tablets explicitly providing the "Babylonian" monthlength $M_{\rm A}$ is ACT 210 (BM 55555); but it is now generally acknowledged that the adjacent yearlength (on that very same 100 BC tablet: Rawlins 1996C fn 16) is of Greek origin, as discovered at Rawlins 1991H §A6. [f] By the time $M_{\rm A}$ is known to have been used, Babylon had been ruled by Greece for many decades. (See Rawlins 1991W §E3 & fn 73, *DIO 4.2* ‡9 §K9, Rawlins 1996C fn 128.) Are Babylonianists not implicitly contending that subjugation somehow made Babylonian astronomers more original & accurate than ever?

[3] A few of Ptolemy's most invincibly innocent and humorless admirers have repeatedly objected to my educational rendering of "Almagest" as "Almajest". Comments: [a] Toomer 1975 p.187 reasonably suggests that "Almagest" means not The-Greatest (as is commonly believed) but just the "big" astronomical collection (in contrast to Pappos' well-known little astronomical collection). Thus, the term may be no more a commendation than is the title of Schubert's "Great" Symphony-in-C, where the "Great" is supplied merely to distinguish this massive work from his earlier little Symphony-in-C. [b] It was the Arabs who contracted "the great compilation" by Ptolemy into a single word, but their rendition was "al-majistī " — later corrupted via Latin into the present spelling with a "g". (All explained at Toomer 1975 *loc cit*.) So, I trust that there will be no further complaint at my adoption of the "j" from the original Arabic contraction. [See *DIO 1.1* ‡1 fn 6.]

1999 (‡2 Refs).] (The anomalistic monthlength V determined by eq.2 is also quite accurate: good to about 1 timesec.)[4] The monthlength eq.2 provides is (see Copernicus 1543 [4.4]):

$$M_{\mathrm{e}} = 126007^{\mathrm{d}}01^{\mathrm{h}}/4267^{\mathrm{u}} = 3024169^{\mathrm{d}}/102408 = 29^{\mathrm{d}}31'50''08'''09'''' \ldots \tag{3}$$

In Aristarchos' era, the interval between an eclipse and its 345^{y}-ago partner was always within about an hour of $126007^{\mathrm{d}}01^{\mathrm{h}}$ with an rms scatter (around a value a few tenths of an hour higher) of less than a half hour ($0^{\mathrm{h}}.4$). So the error in eqs.2&3 was merely 1 part in ordmag 10 million.

A4 The Kallippic yearlength (see fn 5), basis of the Aristarchos circle's Dionysios calendar (van der Waerden 1984-5; *DIO 1.1* ‡1 fn 23), was Julian before Caesar:

$$Y_{\mathrm{K}} \equiv 365^{\mathrm{d}}1/4 \tag{4}$$

(Question-in-passing: why launch the Dionysios calendar at all? — since Kallippos' calendar had the same yearlength. Speculative answer: the Dionysios calendar incorporated *additionally* Aristarchos' typically large-scale-visionary Great-Year [§A7, fn 14]. He was famous in antiquity for proposing the largest universe, too: see Archimedes' *Sandreckoner*.)

A5 Eqs.3&4 determine the number of Kallippic years in each $\mathrm{M_e}$-based 223-month saros (where we will henceforth use superscript K for Kallippic years):

$$223M_{\mathrm{e}} \approx 18^{\mathrm{K}} + 10°40'00''.3 \tag{5}$$

A6 Reality-check: eq.5's remainder is astonishingly close to that in Aristarchos' (§A7) well-known saros equation (*Almajest* 4.2) — a relation which constitutes a prime instance of Greek scientists' clever proclivity (of which I have uncovered numerous hitherto-neglected examples)[5] for round numbers, sometimes expressing high precision (key rounding here: just 1-part-in-76 million!) in a deceptively round-looking & conveniently compact fashion:

$$S = 223M = 18^{\mathrm{K}} + 10°2/3 = 18^{\mathrm{K}} + 4/135 = 4868^{\mathrm{K}}/270 \tag{6}$$

By contrast to the Aristarchos cycle's smooth dovetailing: had we used Meton's monthlength in eq.5, the remainder would have come out as $10°57'$; computing analogously for Kallippos, it would've been $10°43'$. the Geminos (18:3-6) 19756^{d} exeligmos produces $10°41'$. The relative ordmag fits of these M to either eq.3 or eq.6 (or eq.5): Meton 10^{-4}, Kallippos 10^{-5}, Geminos 10^{-6}; whereas the agreement of eq.5 (empirical) with eq.6 (Aristarchos) is 10^{-8}. (For Meton's & Kallippos' monthlengths, see Rawlins 1991H fn 1.) NB: Aristarchan eq.6 agrees with later "Babylonian" eq.1 to *1-part-in-24 million*.

A7 Via Censorinus, P.Tannery, & T.Heath, the connexion of Aristarchos to eq.6's quite-particular large number 4868 is neither new nor at all controversial. (See, e.g., Neugebauer 1975 p.603.) The smallest interval containing an integral number of days, Kallippic years, and saros[6] is, by eq.6 (with eq.4), the following, which is the Great Year[7] GY of Aristarchos (as in eq.6, we again drop approximation-signs, in honor of eq.5's near-exactitude):

$$GY = 1778037^{\mathrm{d}} = 270S = 270 \cdot 223M = 60210M = 4868^{\mathrm{K}} \tag{7}$$

Thus, Aristarchos' happy realization that deceptively crude-looking eq.6 was *extremely* accurate (a super-trivial rounding of empirical eq.5 [from eqs.3&4]) made possible his 4868^{y} Great Year cycle (eq.7): the prime factor 1217 (1/4 of 4868) is embedded right in eq.6.

[4] See Rawlins 1996C eq.8 and attendant discussion. But, ironically, generally-inferior System A's anomalistic monthlength is more accurate. See ‡2 §C3.

[5] See eq.13, ‡2 §E5, ‡3 §E2, Rawlins 1984A n.27, Rawlins 1982G eq.7, Rawlins 2003J fnn 5&38 (esp.), Rawlins 1991W eqs.8&9; and Rawlins 1985H shows Kallippos thought $365^{\mathrm{d}}1/4$ (eq.4) was exactly correct, not rounded.

[6] The plural of saros is often rendered literally as "saroi"; but I think "saros" communicates more unambiguously.

[7] We note in passing that the span of Aristarchos' civil (though not Kallippic or sidereal) GY could have been half of 4868^{y} (as indicated by Censorinus, P.Tannery, & Heath 1913 p.314f), since halving eq.10 yields: 2434 tropical years $= 889011^{\mathrm{d}}$.

D2 Thus, if §C9 is true, future historians should be less casual in tacitly assuming (as we all unthinkingly have, until now) that ACT were always written very near the time their data were calculated-for. After all, Babylonians were more into tabular calculations than observations (see, e.g., Neugebauer 1957 p.97); and back-calculations were common in antiquity (see, e.g., ACT 122 & 135 [Neugebauer 1955 1:144 & 161], Neugebauer 1975 p.525, probably Pliny 2.53), as they are today (e.g., Meeus & Mucke 1992).

D3 The theory that Hipparchos used a Babylonian eclipse-record of -1244 (or -1280: §D5) suggests his access to material which we have no hint was known to Aristarchos.[13] For years, Gerald Toomer and circle have proposed that Hipparchos had a strong Babylonian connexion. To explain the foregoing, it may not be absolutely necessary to accept Toomer's entire theory (especially the idea that Hipparchos' math-astronomy was nontrivially Babylonian), but it is nonetheless only fair (and in accord with the principles set forth at, e.g., *DIO 10* ⊙2&⊙21) to own that our new results suggest that Toomer has been more perceptive than we previously thought. So: we wish his theory good luck down the road, with respect to future indications and perhaps even solid verifications. [Vs. *DIO 13.1* §E4.]

D4 And, to help launch that hopefully productive journey, we ask: how is it that a small cluster of surviving 13th century BC eclipse data seems never to have become public, though evidently accessible to a privileged few, such as System A's inventor & Hipparchos? We have discussed previously (Rawlins 1999 fn 6) the controversial question of insider-secrecy in ancient science. Neugebauer 1957 p.144 suggests it's just a myth, even while owning that some Uruk astronomical cuneiform tablets state that they should only be shown to "the informed". How could Hipparchos have known of an apparently-private Babylonian record of the $-1244/11/13$ eclipse, unless he had close links to the priests of Babylon? Again, such considerations tend to favor[14] the credibility of Toomer's daring hypothesis.

D5 The foregoing has potential utility for present science: if Hipparchos really used a -1244 eclipse-report, this would set an upper limit upon the era's ΔT[15] (perhaps favoring secular quadratic over cubic-spline in Morrison & Stephenson 2004 Figs.2-3) at the value where the $-1244/11/13$ eclipse (also $-1238/7/12$'s [fn 16]: did H use both?) would've ended for Babylon around moonrise. [But OK eastward: India, China.] Among the more interesting other approaches: possibly Hipparchos didn't use the -1244 & -140 eclipse-pair, but instead based his eq.1 upon another[16] viable eclipse-pair candidate on our §C2 list: $-1280/10/23$ & $-176/1/6$. This recourse carries the enticement that the latter pair's older eclipse (potentially underlying eq.1) is merely 7^{y} from our earlier-induced (‡2 eq.8) 13th century BC eclipse-record: $-1273/12/5$. And it would ease the cuneiform-calculation time-disjunct cited at §D1. I add these thoughts so as to provide all sides of the issue, even though I opt for the in-hand (guaranteed non-cloudy-weather) eclipse which we *know* Hipparchos observed (and used for just the sort of apogee-perigee analysis we're discussing), namely, that of $-140/1/27$; note also its superior anomaly (fn 11).

[13] If System A's 1010^{y} cycle was Aristarchos' 1st try, he dropped it for System B's better 345^{y} cycle.

[14] However, another possibility [see especially Rawlins 2003P §B etc] is that the early eclipses were publicly known — but later fell into disuse as more precise data became available. After all, this is not the first time we have encountered strong proof of important ancient data's existence — despite their total disappearance from the surviving literature. See, e.g., Hipparchos' EH & UH solar orbits (Rawlins 1991W §§K9&G10, respectively), and his several adopted obliquity-values (Diller 1934 & *DIO 4.2* p.56 Table 1, Rawlins 1982C eqs.27&28, Rawlins 1985G eq.9 & pp.262-263, Nadal & Brunet 1984 p.231, Jones 2002E) as well as the 781^{y} eclipse cycle (Rawlins 1996C eqs.20-31).

[15] The accepted spin rate's establishers don't claim tight validity back to 1300 BC, where ΔT's 2σ is ordmag $10°$. [Speculation added 2002/9/29. Earliest [alleged] Chinese astronomical records: c.1300 BC; reliable ones: c.720 BC. Babylon eclipse-record chronology very similar. Linkage?]

[16] The $-1238/7/12$ & $-135/9/24$ pair is also possible, but rather less attractive (than other pairs considered here) because little of the $-135/9/24$ eclipse was visible in Hipparchos' Rhodos.

C4 Next indicator: we emphasized at §C1 that Hipparchos is the only astronomer known to have used[9] a perigee-apogee pair of partial eclipses to determine the draconitic month.
C5 Which perigee eclipse did he use for this purpose? Again: the −140/1/27 eclipse!
C6 Of all these eclipses, which is nearest perigee? Check at §C2. (And the pair ending in −135 [Hipparchos' era] has the lowest mean absolute deviation from the apsidal line.)
C7 And, further, who is the only astronomer who is attested to have discovered eq.1? Hipparchos — at *Almajest* 4.2 (Toomer 1984 p.176 or Pedersen 1974 p.163).
C8 How does Ptolemy say (*idem*) Hipparchos did it? With equal-magnitude eclipses.
C9 Is it even necessary to re-cap the foregoing connexions to Hipparchos? Despite decades of consensus that eq.1 was a Babylonian creation, we now possess strong & coherent evidence (§§C3-C8) that Hipparchos discovered it (as Ptolemy informed us: §C7), presumably using the −1244/11/13 & −140/1/27 eclipse-pair.

D The Surprising Consequences of §C9

D1 Some System B Babylonian texts reflect use of eq.1 in calculations for lunar latitudes c.200 BC, well before Hipparchos (Neugebauer 1955 p.127, Neugebauer 1975 p.523). Which seems[10] to favor −195 (§C2) as eq.1's date.[11] However, those cuneiform records which tabulate eq.1-based lunar latitude data (from −205 to −75: ACT 100, 104, 122, 123, 150) are largely just calculation-lists, bearing no date-of-writing. [Yet, as helpfully noted by A.Jones, other tablets with c.200 BC latitude data do carry explicit dates: ACT 101, 102, 135. But, hitherto-overlooked: [a] With one exception, the *dated* latitude-function tablets *do not* exhibit eq.1's 5458^{u} period. [b] The exception is ACT 122; whose explicit date-of-writing is −102 (Neugebauer 1955 1:144), *post*-Hipparchos — and very close to the date of another Babylonian tablet (ACT 210) that unquestionably used Hipparchan data (Rawlins 1991H eq.9). [c] The *pre*-Hipparchos-dated tablets all *conflict with eq.1*. See tabular comparisons at Neugebauer 1955 pp.131, 135, 162, showing incompatibility of ACT 101, 102, & 135 with eq.1-based ACT 100, 104, & 150, resp. [d] Of the six latitude tablets computed for c.200 BC, *all* three eq.1-based ones are undated, while *all* three dated ones are non-eq.1-based. So the very tablets once taken as proof that Hipparchos swiped eq.1 from Babylon, now seem to favor his authorship of it,[12] *a point independent of eq.3*.]

[9] See *Almajest* 6.9, where Ptolemy justly criticizes Hipparchos for slips in seeking (or more likely confirming, as earlier guessed at Rawlins 1996C §D3) the draconitic motion from an apogee-perigee 579^{y}cycle eclipse-pair: −719/3/8 & −140/1/27. (If Hipparchos looked back another 579^{y} to the −1298/4/18 near-perigee partial eclipse, he found it occurred well below Babylon's horizon.) But his highly accurate synodic/draconitic ratio (eq.1) was not improved upon by Ptolemy or any other ancient. [Note (fn 7): the −1244/11/13 eclipse was much nearer apogee than the −719/3/8 one.]

[10] I have nothing against adjusting our findings to an earlier date, if future tests, e.g., discovery of an eq.1-based tablet dated to c.200 BC, point (like ACT 174, for ‡1 eq.1) to eq.1's currency around then (fn 10), hinting at, say, Babylon or Apollonios as originator. Note: our −140 date doesn't itself prove Hipparchos' authorship, since 13th century BC data's very antiquity can be seen as favoring invention by those with readiest access to early eclipses, namely Babylonians. If he took eq.1 from Babylon, he could've been using 13th century BC eclipse data unknowingly. But the theft-theory accepts a longshot coincidence: he mimicked another's apogee-perigee method for one eclipse-pair (579^{y}: fn 9), while stealing the fruit of the same method applied to a separate eclipse-pair (1103^{y}: §C2).

[11] But by modern theory, the matching −1298/10/12 eclipse's end most probably occurred well before Babylon moonrise. Plus: the −195 eclipse's anomaly was further from perigee [lunar anomaly $v = 6°$] than the neat −140 eclipse's [$v = -1°$]. The −176 & −171 eclipses are also rather mediocre in this regard: $v = 4°$ & $8°$, resp. [Another anomalistically inferior possibility: −231/12/4 [$v = 11°$] ($m = 0.19$) & −1334/9/21 [$v = 184°$] ($m = 0.29$).] But each of these pairs' 1st eclipse had v nearer apogee than did the −1244 & −140 pair, whose 2nd eclipse was nearest perigee. Yet, 2nd v being more knowable, its apse-proximity was primary to the ancients, which favors −140.

[12] Even aside from Hipparchan items [a]-[d]: there was no eq.3 eclipse-pair from −447 to −176 whose 2nd member was within 5° of the apse, further indicating that 200 BC cuneiform data based upon eq.1 were back-calculations; unless we say eq.1 was found c.500 BC but kept secret for 300^{y}.

A8 Once his Great Year was established, he needed to fit the monthlength M into his vast Great Year cycle, and so computed the M implied by eq.7's GY. Suppose he made the clever choice to perform the division of GY by 223 *first*, rounding the result Greek-wise (*nearest timemin*): $7973^{\mathrm{d}}06^{\mathrm{h}}15^{\mathrm{m}}$. (Apt precision: relative accuracy-degradation under 10^{-7}.) In then performing *last* his division of the rounded result by the flagrantly sexagesimalesque number 270, he ensured a neatly-terminating expression for M_{A} (analog: *DIO 1.2* §R12):

$$M = (1778037^{\mathrm{d}}/223)/270 \approx 7973^{\mathrm{d}}06^{\mathrm{h}}15^{\mathrm{m}}/270 = 29^{\mathrm{d}}31'50''08'''20'''' = M_{\mathrm{A}} \quad (8)$$

[But was eq.1 brother not son to eq.6? Despite fn 2 & p.3 fn 1, we admiringly note Britton's simple alternate (contra-Ptolemy&DR) route via degree-day division (Babylon convention [A's is not known]) from eq.3 or eq.6 to $M = 29^{\mathrm{d}}191°00'49''$ (or $48''$) $\approx 50'' \equiv$ eq.1.]
A9 This (eq.8) is just the eq.1 we wished at the outset to explain. As Aristarchos *could easily post-verify*: this value of cycle-convenient[8] M_{A} agreed with empirical eq.3 to 1 part in 36 million (an ordmag better than eq.3's accuracy).
A10 Though a sometime critic of Ptolemy, I am here in the happy position of essentially vindicating[9] his *Almajest* 4.2 assertion (repeatedly attacked since Copernicus *loc cit*) that eq.1 came from eq.2. Irony: *Almajest* 4.2 denigrates eq.6, and miscalls it & eq.2 sidereal (as H.Thurston notes), unaware that eq.6, not eq.2, is the more immediate source of eq.1.
A11 Ptolemy's misconception has been lately reborn in the sole plausible-looking point offered against the present paper: the at-first-attractive suggestion that eq.6 is just a rough approximation (merely 1/3 of an exeligmos[10] equation with whole degree remainder: 54 yrs plus 32°). But, even aside from other simple counter-arguments (§A9; fn 2, esp. item [b]), the truth can be fully established by just one single ultra-elementary but central consideration: upon fortuitously-neat eq.6, Aristarchos founded a lunisolar calendaric cycle which was ambitiously designed to extend *for thousands of years*. Why would he do this?[11] —

unless he believed eq.6 to be the most exactly accurate lunisolar equation known.

B Checks from Aristarchos' Metonic "Tropical" Year

B1 Given that Aristarchos' solstice is exactly two Kallippic and eight Metonic cycles (152^{y}: fn 14) after Meton's famous solstice, we know he accepted the Metonic cycle

$$19^{\mathrm{y}} = 235^{\mathrm{u}} \quad (9)$$

where we use superscript y for "tropical"[12] years. But his new monthlength implied (via eq.9) a "tropical" yearlength differing from those of Meton & Kallippos; so a Great Year containing 4868 Aristarchan "tropical" years equalled (melding eqs.8&9):

$$4868^{\mathrm{y}} = (235/19) \cdot 4868 M_{\mathrm{A}} \approx 1778021^{\mathrm{d}}12^{\mathrm{h}}44^{\mathrm{m}} \approx 1778022^{\mathrm{d}} \quad (10)$$

which is 15^{d} less than the Kallippic-year-based interval of eq.7 in his Great Year scheme — a scheme where everything was arranged to be integral: §A7.

[8] Yes, in eq.8 we are dividing by the very factors (270 & 223) we originally multiplied by, during the development of eqs.5-7. (Note that a paper communicated by A.Aaboe, Swerdlow 1980 [p.292 eqs.a&b], performs the very same kind of move, in order to derive Hipparchos' yearlength from eq.1 via the 76^{y} Kallippic Cycle.) Between Aristarchos' multiplication and his division, he established his Great Year as his dominant temporal unit (perhaps overoptimistically-intended as an eternal calendaric cycle); and a tiny rounding occurs (similarly at Swerdlow 1980 *loc cit*) when (eq.8) the month is hyperminutely adjusted and re-defined (according to the GY) through the re-division.

[9] This will happen yet again here — see below at ‡3 §§A2&C9.

[10] [Note added 2003. Rawlins 1985K (and the 1st edition of this note) proposed the possibility that some ancient computers might have used 1/45ths of a circle as a basic unit. But A.Jones' recent discoveries (*DIO 11.2* p.30) have severely reduced the acceptability of such speculation.]

[11] See parallel argument at ‡2 fn 25.

[12] For discussion of ancient confusion of Metonic and tropical years, see, e.g., the works of K.Moesgaard (or Rawlins 1996C & Rawlins 1999).

B2 Eq.10 determined Aristarchos' "tropical" year (dovetailed with his Great Year) as:

$$Y_{\mathrm{At}} = 1778022^{\mathrm{d}}/4868 = 365^{\mathrm{d}}1/4 - 15/4868 \tag{11}$$

B3 In continued-fraction format, tropical eq.11 could be written with its distinguishing number rendered sexagesimally:[13]

$$365^{\mathrm{d}} + \cfrac{1}{4 + \cfrac{1}{20 + \cfrac{2}{60}}} \tag{12}$$

B4 Now, it happens that the Vatican holds two rare and precious Greek ancient yearlength-lists[14] (Neugebauer 1975 p.601, cited from Vat. gr. 191 fol. 170^{v} & Vat. gr. 381 fol. 163^{v} [see Rawlins 1999 Tables 1&2]), which express various ancient astronomers' yearlengths as continued-fractions. Two listings are given for Aristarchos. One is obviously sidereal[15] and so would not apply here. The other is:

$$\text{Aristarchos of Samos: } [365]\ 1/4\ \kappa'\ \xi\ \beta' = [365]\ 1/4\ 20'\ 60\ 2' \tag{13}$$

B5 The remarkable match of eq.13 to eq.12 provides a gratifying extra reality-check on the foregoing proceedings.

B6 It might be thought that eqs.11&12 do not flow specially from eq.1 because proximate eq.2 would produce the same result. Not so: eq.2 would (if put into eq.10) yield $1778021^{\mathrm{d}}11^{\mathrm{h}}33^{\mathrm{m}}$, thus a remainder (in eq.11) of $-16/4868$, disagreeing[16] with the Vatican ms' Aristarchos data (eq.13): a final, *discrimination-test* reality-check, again revealing Aristarchos' pre-System B possession of the **precise** "Babylonian" month.

[13] According to a common modern style, this would be written 20;02. Britton has suggested interpreting the "tropical" numbers in eq.13 as $365^{\mathrm{d}}1/[4 + 1/(20 + 1/62)] = 365^{\mathrm{d}}1/4 - 31/10052$ instead of eq.12. (Though note: the idea that 60 in eq.13 indicated sixtieths did not originate with DR but with Neugebauer 1975 p.602.) This would destroy the appearance of the Aristarchan number 4868 in eq. 11 and thus imply that eq. 11's display of 4868 is just a spookily spectacular coincidence. But Britton's interpretation would (via eq. 9) actually move M_{A} closer than ever to eq. 1.

[14] These lists were long regarded as mysterious gibberish (Neugebauer 1975 p.602) — until 1980, when long-*JHA*-suppressed Rawlins 1999 treated them (sample analysis at fn 15) as consisting of continued-fraction expressions. (Notably, both mss list Greek yearlength values chronologically-prior to Babylonian values.) [Was the origin of the Greeks' highly useful fascination with cont'd fractions related to ancients' wide but only moderately-utilitarian use of unit fractions?] This analysis revealed both sidereal and tropical years listed under Aristarchos. (Thus, as emphasized in Rawlins 1999, Aristarchos had pre-Hipparchan knowledge of precession: indicating that, reasonably enough, the first public geomobilist was also first to perceive the Earth's precessional wobble.) These induced yearlengths exhibited two characteristic numbers: sidereal, 152 (fn 15); "tropical", 4868 (eqs.11-13), respectively. Both numbers are well-known to be Aristarchan: his solstice was 152^{y} after Meton's (§B1) and his GY was 4868^{y} long. (Note: 4868/32 = 152 1/8 [Rawlins 1999 §B7 bracket].) There are no signs in the Vatican lists, but note that even discarding the final numbers in both Aristarchos entries on the Vatican lists (and using merely the $10'$ [as $-1/10$] and the $20'$ [as $+1/20$]), we get remainders of $1^{\mathrm{d}}/156$ and $-1^{\mathrm{d}}/324$, which are obviously near the ancients' well-known estimates of the excess & deficit of their sidereal & "tropical" yearlengths with respect to the Kallippic $365^{\mathrm{d}}1/4$ calendar.
[***Note well*** the many doublings geometrically flowering in the Aristarchos-Hipparchos calendar (fn 17): 1^{d} diff between tropical & Kallippic calendars = $304^{\mathrm{K}}1/4$; saros-cycle return to same longitude = $608^{\mathrm{K}}1/2$; with solar return = 1217^{K}; with lunar return 2434^{K}; with diurnal return = 4868^{K} (eq.7)].

[15] Rawlins 1999 interprets $365^{\mathrm{d}}1/4\ 10'\ 4'$ as: $365^{\mathrm{d}}1/[4 - 1/(10 - 1/4)] = 365^{\mathrm{d}}1/4 + 1/152$. See fn 14 and fn 16.

[16] So, did Hipparchos opt for using eq.2 instead of eq.1? — thus producing his 16-based cycle. (See fn 17.) See another suggestion at Rawlins 1996C §D of his personal contribution [directly attested anyway at *Almajest* 4.2] to the evolution of 345^{y}-cycle-based lunisolar theory. Note: given the chronology-ambiguities implied in ‡3 §D1 below, it is possible that some details of what we have here been attributing to Aristarchos actually had a few debts (positive or negative; examples of latter: Rawlins 1991W §§N7&R12) to Hipparchos' investigations, adjustments, and-or transmissions.

B2 Then, using the fact (§A3) that eq.1's Δv is nearly 2/5 of a circle, I understood the key (2002/4/3-4, 00:50EST) — and immediately turned it: multiplication of ***half*** of eq.1 by 5 produces an eclipse-cycle, and one of length far more reasonable than 2200^{y}-plus.

B3 Still, at over 1103^{y}, the implied cycle is huge enough that it would formerly have been automatically discarded. Yet now, after ‡2 eq.1 delivered us naturally and inevitably to a 1010^{y}-cycle origin for System A (‡2 eq.2), millennial remoteness has come well within our purview: indeed, we're about to see that the early foundation-eclipse here will *fall right into* the very mid-13th century BC bin which the revolutionary findings of ‡2 (§A6) had already inadvertently prepared the way for. [Yet a 3rd 13th cy BC bin-hit (p.3 fn 1) at Rawlins 2003P §E2!] (If a regular *DIO* reader hasn't yet had a dawning of awareness of why ancient astronomy is an inductive scientist's cloverpatch, it isn't ever going to happen.) So, we simply multiply eq.1 by 5/2 (or eq.2 by 5) — and thereby hit the ancient-draconitic jackpot, namely, the hitherto-secret[7] eclipse-cycle that produced the well-known ratio eq.1:

$$13645^{\mathrm{u}} = 14807^{\mathrm{w}}1/2 = 14623^{\mathrm{v}}1/2 + 7^\circ = 1103^{\mathrm{g}} + 63^\circ = 402945^{\mathrm{d}} \tag{3}$$

C A Cascade of Verifications

C1 Anyone familiar with orbit theory knows the only type of eclipse-pair that can straightforwardly produce eq.3's odd (1/2-circle-anomaly!) lunisolar relation is: one eclipse near lunar perigee, the other around apogee — both partial eclipses of similar magnitude. To students of the history of ancient mathematical astronomy, this extremely special type of equation will instantly have a familiar smell: only one astronomer is known (*Almajest* 6.9) ever to have found the draconitic month by using a perigee-apogee partial eclipse-pair, namely, the internationally famous 2nd century BC Hellenistic astronomer, Hipparchos.

C2 As in ‡2 (§A7), we find ourselves with a delicate cycle: while eclipse-pairs satisfying the 1103^{y} cycle are not very rare, those with apsidal alignment are quite unusual. So we next list the near-equal-magnitude perigee-apogee eclipse-pair possibilities from c.500 BC to Hipparchos (again finding — as also in ‡2 §§B2&B4 and Rawlins 1996C §E6 etc — that the prospects occur in temporal bunches, far from randomly); and we give mid-eclipse anomaly v in brackets, magnitude m in parentheses (both data *DIO*-calculated), and the Meeus-Mucke (MM) numbers (consistently differing by 35) at left:

MM02&37:	−1604/09/05 [357°] (0.12)	&	−501/11/20 [184°] (0.18)
MM02&37:	−1550/10/08 [349°] (0.02)	&	−447/12/21 [176°] (0.16)
MM35&70:	−1298/10/12 [178°] (0.35)	&	−195/12/25 [006°] (0.20)
MM35&70:	−1280/10/23 [176°] (0.37)	&	−176/01/06 [004°] (0.21)
MM19&54:	−1274/06/21 [183°] (0.96)	&	−171/09/02 [008°] (0.93)
MM35&70:	−1244/11/13 [171°] (0.39)	&	−140/01/27 [359°] (0.26)
MM19&54:	−1238/07/12 [177°] (0.68)	&	−135/09/24 [002°] (0.81)

C3 We can readily dispense with the early pairs. (These were only listed in §C2 in order to illustrate how many centuries can go by with no appearance at all of eclipses satisfying the §C1 conditions required for utility in draconitic period-determination via eq.3.) So we concentrate upon the last 5 eclipse-pairs of §C2, where we of course note a coincidence which is delightfully indicative, since we are looking for a partial eclipse: of the three extant Hipparchos lunar eclipses, the only partial one he is known[8] to have reported (also used) was that of −140/1/27 *an eclipse which is right there* in the short §C2 list — and specifically stated (*Almajest* 6.5) as having been observed in Hipparchos' Rhodos (not Babylon, note).

[7] Probably never published even in antiquity: see §D4. Relatively, eq.3's anomalistic remainder is c.4 times better than the 579^{y} cycle's (fn 9; Rawlins 1996C fn 59): barely half the Δv (7° vs 13°), for twice the time-base. [Some believe that H quit fn 9's neat 579^{y} eclipse cycle for eq.1's *shorter* 441^{y} anomalistically-nonintegral eclipse cycle.]

[8] See *Almajest* 6.5&9. Note that Hipparchos could (with sufficient accuracy) know the anomaly of both eclipses by calculation from his already-established anomaly tables, founded upon ‡1 eq.2.

‡3 Hipparchos' Draconitic Month & 1245 BC Eclipse

Late Use of 13th Century BC Data Independently Confirmed Hipparchos' Debt to Babylon: Gerald Toomer Vindicated?

by Dennis Rawlins

A How the Ancient Draconitic Month-Source Got Investigated

A1 Shortly after 2002/4/3-4 midnight, while[1] pondering the prospect of the foregoing paper's inevitably meeting rejective stolidity from a certain group of scholars, I was mentally resorting to the point that all precise ancient lunar periods have by now been traced to eclipse periods. But then I suddenly recalled that such tracing had in fact not ever been accomplished for the ancients' ultimo (marvelously accurate)[2] synodic-draconitic relation (*Almajest* 4.2)

$$5458^{\mathrm{u}} = 5923^{\mathrm{w}} = 5849^{\mathrm{v}} + 147^{\circ} = 441^{\mathrm{g}} + 97^{\circ} = 161178^{\mathrm{d}} \qquad (1)$$

where I have here tossed in several extra later-useful items, additional to the well-known integral numbers of synodic & draconitic[3] months. (As previously in both ‡1 and ‡2, we adopt our standard abbreviations: d = days, h = hours, u = synodic months, v = anomalistic months, w = draconitic months, g = anomalistic years.)

A2 This famous relation (eq.1) is ascribed (see §C7) by Ptolemy to Hipparchos' analysis of eclipse-pair data chosen to avoid the effects of lunar-anomaly differences — though modern scholars have (again[4] wrongly, as we are about to see) rejected Ptolemy's 1st-hand report upon Hipparchos' work.

A3 As to wrong-headedness: as a matter of ironic personal confession, I should say that I've long presumed [e.g., Rawlins 1996C fn 59] eq.1's source would never be known. Why? Because initially the difficulty in finding an eclipse period here looks staggeringly intimidating: the Δv remainder is 147° — *about 2/5 of a circle*. Thus, searching analogously to ‡2 §A4, we see that the only multiplicative integer which has a hope of producing a useful eclipse cycle (from eq.1) is 5; but multiplying eq.1 by 5 would produce a cycle over 2200^{y} long! — much too remote (implying use of eclipse data from c.2500 BC).

B Draconitic Jackpot

But 3 considerations spectacularly rescued this at-first-seemingly-hopeless[5] situation:

B1 I realized that since the number of synodic months in eq.1 is even, we can find a possibly-useful relation just by halving[6] eq.1:

$$2729^{\mathrm{u}} = 2961^{\mathrm{w}}1/2 = 2924^{\mathrm{v}}7/10 + 1^{\circ}+ = 221^{\mathrm{g}} - 131^{\circ} = 80589^{\mathrm{d}} \qquad (2)$$

[1] I am thus again (see also ‡2 fn 1) deeply indebted to B.Goldstein 2002.

[2] See Rawlins 1996C fn 55. [Cause of accuracy (far superior to 579^{y} cycle): see fn 7.]

[3] The draconitic month ("eclipse month") is the time the mean Moon takes to return to a node.

[4] See likewise at ‡1 §A10.

[5] One recalls A.C.Doyle's penetrating observation (also acknowledged at Rawlins 1973 pp.148-149) that whatever seems most to complicate a problem, can be the key to solving it.

[6] I later noticed that I'd already come upon eq. 2 quite independently of Hipparchos — and had even published it as Rawlins 1996C eq.18 — noting only in passing that its double equalled eq.1. Therefore, in the six years since Rawlins 1996C eq.18 was published [1995/12/31], no one — myself most emphatically included! (note that Rawlins 1996C's expression of eq.18 obscured the key 5-factor) — has had the sense simply to follow DR's integral-multiple enhancement approach (e.g., Rawlins 1996C: eq.10→eq.11 [and §I2 in reprints]): mere multiplication by 5 — which would have produced the upcoming discovery of eq.1's source: eq.3.

C Comments on the Aristarchan Evidence

Several contemporary scholars have tried to portray Aristarchos' pioneering heliocentrism as a virtually valueless passing-blip on the ancient screen. But the opinions of Archimedes (*op cit*) & Apollonios (*DIO 11.2* ‡4 §L7) show that Aristarchos' bold originality was highly regarded by the brightest ancients. However, a few historians, in anticipation of the current paper (before even seeing its evidence), have suggested there's too little Aristarchan material to make much of. Actually, the ultra-lean evidential situation for Aristarchos only increases the strength of this paper: I can't be suspect of picking among a large Aristarchan corpus to select only the data agreeing with my thesis, since there *is* no such fat corpus to filter. Those who wish to reject the paper's proposals will now have the burden (which I have repeatedly laid on them in other contexts as well — see, e.g., the neat Aristarchos-based Hipparchos-transmitted[17] heliocentrist eqs.23&24 in Rawlins 1991W) of convincingly explaining how such mega-odds fits can meaninglessly emerge from such micro-slim materials.

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[17] There is no question that Hipparchos used Aristarchan data: see *Almajest* 3.1. And the famous $-1^{\mathrm{d}}/300$ remainder in Hipparchos' canonical yearlength is actually a rounding from a cycle of not 300^{y} but 304^{y} (Heath 1913 p.297, Rawlins 1985H, Rawlins 1999 fn 17), perhaps of peculiarly Aristarchan origin by a route revealed in a note appended (at p.42) to *DIO*'s 2001 reprinting (see www.dioi.org) of Rawlins 1999: "The number of years between Meton's famous bedrock −431 Summer Solstice [the epoch of the Metonic calendar] and the Hipparchos [solar] Ultimate-Orbit epoch −127 Autumn Equinox [Rawlins 1991H eq.28: the *only* equinoctial ancient calendaric epoch] is $304^{\mathrm{y}}1/4$. This number is *exactly* one sixteenth the number of years in the 'Great Year' [eq.7] of Aristarchos. That is, the Meton-Hipparchos epoch-interval is $4868^{\mathrm{y}}/16$ — on the nose." (See fn 14.) Now, recall: we found at eq.10 that the appearance of 15 instead of 16 in eq.11 was virtually a toss-up; it seems that Hipparchos opted for the 16, thereby adopting a 304 yr calendar that was simultaneously 1/16 of Aristarchos' cycle and [Heath *loc cit*] 16 times Meton's. Note: in the 1 1/2 centuries between Meton's 19 yr cycle and Aristarchos' 4868 yr Great Year, the growth of Greek astronomers' cycles reflected an octodoubling of scientists' temporal vision, up by a factor of *hundreds*; specifically for Meton-to-Aristarchos: about 256 [16-squared or 4-to-the-4th]), paralleling a huge expansion too of man's spatial conception of the universe, also initiated by Aristarchos: §A4, Rawlins 1991P fn 11, & Rawlins 1982G fn 284.

‡2 Babylon's System A & the 1274 BC Eclipse

The Oldest of All Traceable Eclipse-Records

by Dennis Rawlins

Gratitude to Opposites

This investigation and the one (‡3) immediately following it were both triggered by my recent fortunate encounter[1] with a learned analysis by Bernard Goldstein: the lead paper in the 2002 Feb *Journal for the History of Astronomy*. I am obliged and delighted to here acknowledge the debt. I've also, on numerous occasions, benefitted from chats with Alex Jones & John Britton regarding Babylonian lunar theory.

Goldstein's paper (following on the heels of DR's delivery of ‡1 at the British Museum the previous June) was clearly intended to encourage and stimulate the discovery of the long-unknown sources of the Babylonian lunar periods. BG expresses a becoming humility and amiability in carrying out his mission. True, if he follows his group's sad tradition, he will never take pleasure in the present unexpected potential fruits (‡2 & ‡3) of his own paper; but we are here expressing our thanks to him and to the *JHA*, regardless — and will continue to hope for some untraditionalism.[2]

[1] The 2002 Feb *JHA* arrived at The Johns Hopkins University's central library on 2002/3/14; I first saw the B.Goldstein paper 3/16. On 3/18 (13:02EST), I thought I faintly recalled that double eq.1's 1010^y (eq.2) was the length of a very long saros-series I'd encountered at some point in the past. Then, with bizarrely atypical unrushedness, I delayed 7 hours before finally getting around to running a global search through the whole *DIO* file, swiftly finding Rawlins 1996C §H2's 1010^y saros-series. This unleashed the present paper; and shortly thereafter (2002/4/3-4) also the following paper: ‡3.

[2] One can rationally dispute the precise date-estimates proposed in this paper and the next (‡3). But those experienced in astronomy will discern the obvious strength of the analyses' general foundation: [a] Long eclipse-cycles were the only reliable method which scientists of the era in question possessed (and attested) for determining their high-accuracy monthlengths, especially the difficult anomalistic month. [b] This firm basis takes us (via eq.2 here & ‡3's Hipparchos-redolent eq.3) inevitably into the previously-unknown region of 13th century BC eclipse data. Opposition to these findings will surely stress: [i] DR is an amateur in Babylonian "astronomy". [ii] The era suggested is extremely remote. [iii] No records of 13th century BC eclipses survive directly today. [iv] How could early calendars date them accurately, anyway? [v] Our new findings have forced us to the seemingly-risky (though see ‡3 fn 12) conclusion that three Babylonian tablets (ACT 100, 104, 150), computed for c.200 BC, were back-calculations actually performed at least a half-century later (after −140). See ‡3 §D1.

Contra these potential complaints: [i] DR openly boasts of being a green amateur (*DIO 1.2* fn 19 & *DIO 3* fn 197). (Are the "pros" also turning a little green, when one who doesn't even seek their grant-funds is solving some of their own field's mysteries?) [ii] The Ammizaduga Venus Tablets evidently bear pre-13th century data; and the strength (§§A2&A3) of the presumption of long-eclipse-cycle-basis is far stronger than a mere argument-from-absence (fn 7). [iii] As for attestation: there's not-a-jot of testimony describing any means used by Hipparchos or earlier astronomers for finding accurate lunar months, other than by the multiply-attested (‡3 §E5) method of long-eclipse-cycles. (Ptolemy's own alleged methods are later, were fabricated, and *don't generate integral period-relations*: §A3.) [iv] Babylon knew what day it was, despite its unsteady pre-Metonic calendar (fn 37). [v] Back-calculations were (and are) ordinary astronomical work (‡3 §D1); the only 200 BC Babylonian tablets based upon Hipparchos' draconitic equation also happen to be the only ones that do not bear a date-of-writing; the only Hipparchos-ratio-based material that *is* dated happens to be post-Hipparchos (*idem*).

A reader can make up his own mind regarding which arguments here are primary; but he shouldn't be surprised at a few unfalsifiable-adamantine reactions to the issues raised by these papers. We'll set a more scientific example by (‡3 fn 10 & §D5 [& see *DIO 11.2* p.31]) openness to alternate theories, plus ready acceptance that discovery of a 200 BC-inscribed tablet computed via ‡3 eq.1 would instantly disprove Hipparchos' authorship of that equation. [And, in case radiocarbon testing can tell 100 BC tablets from 200 BC ones, *DIO* will welcome such checks. (Also for the Ammizaduga copies.)]

G2 [First noted 2002/10/12.]
The c.1000^y size of most Ptolemy *PlanHyp* sidereal planet-cycles requires stationary-point data taken within ordmag 10^y of −830 (Rawlins 2003J §L1).]
G3 [First realized 2002/12/14.]
Jones 1999G's dating Geminos to 90-25 BC puts a lower limit of 825 BC for the date of the 1st eclipse establishing his 800^y eclipse cycle. (See ‡3 §E1, Geminos 8.40-41, & Rawlins 1996C §I2: post-1997 reprints.) This, too, is within ordmag 10^y of −830.
G4 Thus, we now have (§§G1-G3) three independent indications (plus another hint at Rawlins 1996C §D), all pointing to classical-era use of 9th century BC celestial data.

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H A General Theory of Ancients' Cyclicities

Certain Muffiosi are extremely upset at the present paper & ‡3, insisting (with classic-Muffia preternatural surety) that pre-8th-century-BC eclipse records could not possibly have been accessible to Hipparchos-Ptolemy. See *DIO 13.1* ‡2 §H on such opining's mote-beam imbalance, plus startling & crucial implications for the long-curiously-durable former orthodoxy that serious ancient math astronomy was born in Babylon. Muffiosi also carp at our fertile exploitation of long cycles. So let's go beyond ‡4 §B1 to propose a *DIO* general theory: Greeks expressed the mean motions of **all seven** wandering celestial bodies by integral math ratios ultimately founded upon empirical integral cycles: 5 planets (‡4), Moon (‡1 eq.2), & sometimes even the Sun (‡1 fn 17, *DIO 11.2* p.33 item 8).
For attestation & the generally sound reasoning-beneath, see, e.g., fnn 2&4, §A3, & ‡4 §B2.

E6 According to n.6 of Britton 1999 (an extensive study, by a scholar long deeply versed in Babylonian materials — and taking a totally different approach [vs ours] to System A: see esp. his pp.219-227), the earliest calculations found (so far) upon unquestionably-System A cuneiform tablets are those of ACT 70; which lists data starting with a full Moon that occurred only a few months after (the full Moon which was) the very −262/1/26 eclipse that we have just shown (§E4) probably launched System A. (The tablet examines full Moons from late −262 to late −251. See Neugebauer 1955 1:117, 3:47.) It is arguable[35] whether we can trust that the tablet was actually computed during that time range; but, of course, I cannot (and should not) refrain from remarking such a close temporal coincidence.

F System A: Babylonian or Greek?

F1 Following our evidence (§§E4&E6) on System A's date of birth, we turn to the question of *place*-of-birth. The obvious point in favor of Babylonian (as against Greek) origin is the upfront item: the −262/1/26 eclipse couldn't be seen in the Hellenistic world. However (even aside from the fact that this eclipse was also invisible in Babylon itself), we know that Babylonian observations were transmitted to the Greek world and were used by astronomers there. (See, e.g., ‡1, Rawlins 1991W fn 223.)

F2 Nonetheless, one ultimately senses that System A was Babylonian — at least in place. Summing up:

[a] The −262 eclipse was seeable in the Seleukid empire, not the Ptolemaic.

[b] Early System A lunar material exists only on Babylonian cuneiform tablets.

[c] And, by distinct contrast[36] to Babylon's System B synodic month (‡1 eqs.8&12) and draconitic month (‡3 §C), not-a-jot of (known) high-level Greek astronomy connects mathematically to System A.

[d] See also §D3.

So, the preponderance of evidence is in favor of our (necessarily very tentative) conclusion here that: System A probably originated in Babylon.[37]

G Appendix: Late Use of 9th Century BC Astronomical Data

[Two intriguing items (discovered after 2002/5/31 first-posting of this paper) add to mounting (& surprising) evidence for classical-era utilization of records of celestial observations from well before the epoch (747 BC) of Nabonassar, contra current perception (fn 7).]

G1 Both of these evidences point to the 9th century BC (§A5), near the −830 eclipse which Rawlins 1996C §E6 suggested on other grounds could have been [but see fn 21's appended bracket] used to derive Ptolemy's last lunar equation (Rawlins 1996C eq.10).

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that, though eq.4 shows 4^{s} precision, its V_{A} was accurate to within 1^{s}. However, the relative inaccuracy of associated M_{A} (eq.6) reminds us of the obvious possibility that V_{A}'s accuracy is simply an accident (1st hypothesis [§D]).

[35] Note: the very chronological implication which appears to fit so well here will be doubted in a different context during evaluation of new findings to follow. See ‡3 §D1. (The earliest explicitly dated System A lunar tablet in Neugebauer 1955 [1:100] is −48/47 [ACT 18].)

[36] Question: given ‡3 §§D1&D2, which of System B's monthlengths are we sure did not come to Babylon via Hipparchos?

[37] [Our finding (that 13th century BC eclipse data were usable roughly 1000^{y} later) has the implication that Babylon maintained calendaric continuity throughout its long astronomical history (our thanks to Alex Jones' skepticism, for triggering this DR realization), a magnificent accomplishment in itself, especially since the Babylonian calendar was irregular until late in the city's history. Yet, despite that apparent impediment, Babylon was evidently (vs. Rawlins 2003P §E5) able to keep its calendars straight: after all, the 8th century lunar eclipse-triad cited by Ptolemy (*Almajest* 4.6) is accurately dated, though it occurred centuries before Babylon's calendar became reliably Metonic.]

[Note added 2013. It seems likely that Babylonian eclipse-specialists privately depended upon the steady Egyptian calendar to avoid being misled by the vagaries of Babylonian & Greek civil calendars.]

A The Magnificent Durability of Babylonian Eclipse-Recordkeeping

A1 It is well-known that the central relation of Babylonian lunar System A is the ratio (see, e.g., B.Goldstein 2002 p.7f or Neugebauer 1975 pp.478&501):

$$6247^{\mathrm{u}} = 6695^{\mathrm{v}} \tag{1}$$

(As in ‡1, we will here use our standard abbreviations: d = days, h = hours, u = synodic months, v = anomalistic months, w = draconitic months, g = anomalistic years).

A2 Among the several professional historians who deal regularly with Babylonian materials, there has long persisted a strangely infectious notion (to the point of rather inflexible orthodoxy) that such long period-relations (more than 500^{y} in this case) are illusory, that centuries-long Babylonian lunar relations were instead built up by indoor mathematical manipulation from far shorter ones, an idea perhaps related to the unkillable popular myth (justly scoffed at by Neugebauer 1957 p.152 [vs Neugebauer 1975 pp.107&643]) of ancient scientists as a bunch of dreamy non-empiricists.[3] Yet to an astronomer, it is chapter-one obvious (see also Rawlins 2003J §J1) that celestial periods are found *most accurately* by using extremely long temporal baselines.[4] (This is simply *standard procedure* for astronomers. The preferability of such an approach was also self-evident to modern historians' own Hellenistic hero, C.Ptolemy, each of whose alleged derivations of his tables' periods [for Sun, the five planets, and all lunar cycles] used positions observed centuries apart.) This, because division by a lengthy time-interval reduces the effect of measurement-errors (at each end of the interval) to trivial proportions. See at ‡1 §A3 how ordmag 1^{h} errors in the empirical basis for M_{A} melt into an error of less than a timesec in M_{A} itself. (See also Rawlins 1996C fn 110.) Note: if one bases a long cycle upon a short one, empirical errors' effects will *obviously* be artificially inflated — what ancient astronomer would invite that? Is there even a single attested case of such ancient manipulation?[5] Why are certain historians so ready (p.26) to jettison self-evident proper scientific procedure (normal both anciently & modernly) even in the face of ancients' undeniable repeated & consistent success in getting results whose impressive accuracy is consistent only with such solid scientific means?

A3 Some scholars' antennae seem permanently unequipped to receive yet another extremely clear (and quite elementary) signal: *only eclipse-cycles automatically & exclusively produce integral*[6] *period-relations* — which is just how all ancient pre-Ptolemy (fn 2) lunar motions were expressed (‡1 eq.2 & ‡3 §E1). See §H.

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[3] The alibiing of Ptolemy's sins often goes in the direction of rapturously declaring it only natural — even outright admirable (Graßhoff 1990 pp.214-215, Rawlins 2002V fn 57) — that Greek theorizing submerged empiricism. Problem: how could the ancients have (centuries before Ptolemy) gotten all three of their key monthlengths (eq.4 or ‡1 eq.2 [anomalistic]; ‡1 eq.1 [synodic]; & ‡3 eq.1 [draconitic]) correct to one part in ordmag a million merely *by indoor logical conjuring*? [Least accurate month: anomalistic, as expected from our hypothesis. Analogy: Rawlins 1985G §5 ¶3.] Again (p.3): this is just another case of a lapse in common sense regarding probabilities — forgetfulness perhaps related to a common modern-historian confusion (Rawlins 2002V fnn 7&35) of ancient semi-comprehending *transmitters* with the brilliant *originators* of ancient astronomy's refined achievements.

[4] DR has long noted (Rawlins 1987 & Rawlins 2003J) that all five of the short planet periods cited at *Almajest* 9.3 are descended from long tropical cycles, themselves derived (via 1°/cy precession) from wellfounded similarly-long empirical sidereal relations (*DIO 2.1* ‡3 fn 17). [Note: a surviving papyrus (Jones 1999A 1:67, 69-80; 2:2-5, Pl.1) records a 104 AD astronomer observationally checking the Jupiter relation 315 syn revs = 344 sid yrs. (See *DIO 9.1* News Notes for our admiring astonishment at this find.)] To create *Almajest* 9.3's short relations, Ptolemy (or whoever) divided such long tropical cycles by integers (Rawlins 2003J): Saturn, 11 (or 7 or 9); Jupiter, 6; Mars, 8; Venus, 62 (or nearby); Mercury, 5. (I.e., long cycles bred short ones: the very inverse of historians' perception.) The truth is especially obvious in the case of Jupiter where, if a short tropical relation were primary, it would have to be the neat 83^{y} cycle, not Ptolemy's 71^{y} one — whose remainder is (relatively) 50 times bigger!

[5] Promoters of such unattested ancient math manipulation simultaneously reject the upsetting but now obvious implications of ancient math manipulation which *is* effectively attested, e.g., ‡1 eqs.6&12.

[6] Or half-integral period-relations — which mere doubling renders integral. (See ‡3.)

A4 Let us start with a Ptolemaic example of §A2's approach, demonstrating the fruitfulness of using extremely long eclipse-cycles — such being the obvious and natural empirical base for the determination of accurate lunar period-relations, a method which was explored in an earlier *DIO*: see Rawlins 1996C §E, where we found that merely tripling or quintupling Ptolemy's last synodic-anomalistic lunar relation (fn 7) found eclipse cycles. In the case of eq.1, just *a simple doubling* will do the trick, instantly producing the central equation upon which the Babylonian System A lunar periods were founded:

$$12494^{\mathrm{u}} = 13390^{\mathrm{v}} = 13558^{\mathrm{w}}1/2 - 22^\circ = 1010^{\mathrm{g}} + 42^\circ = 368955^{\mathrm{d}}1/3 \qquad (2)$$

(As to whether it could be an accident that eq.2 is an eclipse cycle: see analysis at ‡3 §E.)

A5 The foregoing's main surprise is swiftly apparent to any Babylonian-astronomy scholar: eq.1 was probably discovered in the 3rd century BC (§B3); therefore, eq.2 requires that the inventor of System A had access to (almost certainly Babylonian) eclipse records of the *13th century BC*, none later than (§B4) 1274 BC — a date which is more than 500 years before[7] what have been attested (& generally accepted) as the earliest eclipse records that came down to classical-era astronomers. (But see Jones' suggestion [& Rawlins 2002V §B3 vs Rawlins 2003P §E4] that Ptolemy had only 2nd-hand knowledge of early data.) It is over 400^{y} before the earliest record we previously had even good *in*direct evidence for ancient use of (via §A4's 795^{y} eclipse cycle): the −830/2/4 eclipse. [See §G, below.]

A6 Though the suggestion of 13th century data (surviving into the Seleukid era) may initially appear outré, there are considerations weighing strongly in its favor: [i] No other direct empirical basis for eq.1 (accurate to nearly 1 part in a million) has ever previously explained it. [ii] A remarkable confirmation of extremely ancient Babylonian eclipse records is about to arise quite independently in the paper immediately following this one (see ‡3 §B) — and the indicated record in that case is also from the 13th century BC: specifically 1245 BC (within just a few decades of the range suggested above at §A5). [Yet a *third* 13th-century-eclipse indication has now appeared: *DIO 13.1* ‡2 §§E2&E3.]

A7 As in the 795^{y}-eclipse-cycle case cited in §§A4&A5 (also exhibiting a 22° remainder [Rawlins 1996C eq.11] — which verges on the outer limit [‡3 fn 17] of eclipse-pair possibilities), the 1010^{y} cycle is an extremely fragile relation: a 1010^{y} eclipse pair occurs very, very seldom (unlike the quite common 345^{y} pairs of ‡1 eq.2). That infrequency presumably inconvenienced those ancient pioneers who were trying to establish eq.1 empirically — but it is a fortuitous boon to the modern historical detective: it severely restricts the number of eclipses that could have contributed to eq.1's ancient discovery. Therefore, we are assisted in narrowing the sample of eclipses (and thus the era) that could have underlain eq.1.

[7] Conventional scholars interpret *Almajest* 3.7 as saying that only from Nabonassar's time (747 BC) were observations preserved. But Ptolemy just says this is so "on the whole". (Toomer 1984 p.166 n.59 notes that extant cuneiform records are generally consistent with that date, though, given these records' thinness, one can hardly conclude anything firm in such fashion. [See 2003 note at end of this paper.]) In response: [a] Ptolemy does not claim that nothing at all survives from an earlier time. His statement appears to imply that continuous records start with Nabonasser [747 BC]; however, our proposal here is not that a continuous eclipse-record (from the 13th century BC down to Ptolemy) survived intact, but rather that a small bunch of 13th century BC data came through — either [i] exceptionally and in precious isolation, or [ii] as the oldest data (among centuries of spotty records between c.1300 BC and Nabonasser) then available, deliberately selected in order to found System A's central synodic-anomalistic period-relation (eq.1, as it turned out) upon as long a temporal baseline as possible. [b] Conservatives continue to be silent about the fact that the only solutions yet presented that explain (perfectly) Ptolemy's final lunar synodic-anomalistic equation ($3277^{\mathrm{u}} = 3512^{\mathrm{v}}$ [fn 21]) require eclipse data that cannot be later than 831 BC. (See Rawlins 1996C eq.10 & §E6 [or Rawlins 2003P eq.3]. See also Rawlins 2003J §L on antiquity of implied Babylonian planet records.) [c] Wise young Hugh Thurston is fond of quoting a wise old adage (especially wise in the study of ancient science, where over 99.9999% of the physical records are lost): absence of evidence is not evidence of absence. (See ‡1 fn 2. Or: did Alcor not exist in 128 BC?) [d] If we cannot accept any finding in ancient science without direct attestation, then: should we all park our brains at the entrance to the ancient science field? Is it forbidden to induce beyond the texts?

E3 Now, we already encountered above (§§B7&B8) the likely cause of a significant portion (roughly 1/4) of the total error; if eq.7 applies, then the remaining part (about 3/4) of this total comes from a factor which DR has elsewhere already adduced (Rawlins 1985H) to explain most of yet another astronomical-calendar systematic overstretching-tendency (Hellenistic astronomers' *always*-overlong [Rawlins 1999 §C10] estimates of the tropical year): an ancient scholar's use of ancient-to-him calendar-related astronomical data often forced him to use time-reports that recorded merely an event's *date*[29] — not its hour. In which case, he would — whether knowingly or not — use the epoch hour (i.e., starting or zero hour) of the day *containing* the event. This would incidentally pad the interval upwards (by a half-day on average).[30] Again: this would occur simply because the 1st-eclipse record was mis-cited implicitly or explicitly to the day-epoch of the calendar of the 1st eclipse's observer. In the case of Babylon, the day started at evening (Neugebauer 1975 p.1067): 1/4 day before the modern day-epoch, midnight.[31]

E4 Therefore, to begin the process of identifying the eclipse responsible (via eq.2) for System A's fundamental period-parameters, we merely subtract $1^{\mathrm{d}}/4$ (§E3) from the $7^{\mathrm{d}}/8$ remainder just realized at eq.7. This elementary arithmetic tells us that the computer estimated (not very accurately) that his eclipse's middle occurred half-way through the afternoon (i.e., 5/8 through the day modernly figured from midnight), which we'd call 3 PM. (Babylon would've quantified it as: 45° short of day's end.) A 1^{h} error would be unremarkable for Babylon. (See Dicks 1994 fn 46.) But, if occurring too early (c.2 PM), such an eclipse would be invisible even in the eastern Seleukid empire.[32] So the mid-time of the eclipse we are looking for would have to be c.4 PM in order simultaneously to satisfy ($\pm 1^{\mathrm{h}}$) eq.7 while being partly visible at least somewhere in greater Babylonia. Checking the times of every eclipse on our §B4 list of candidates (by direct calculation — or via published canons of eclipses or full moons), we find that only one eclipse-pair makes the cut: that whose later member is the −262/1/26 partial eclipse, the end of which was visible in the eastern part of the Seleukid empire (fn 10): Persepolis, Tehran, and beyond. For Babylon, this eclipse's middle occurred (invisibly) about 16^{h} (4 PM) Babylon Apparent Time.[33]

E5 Since for our chosen pair, we now possess the times for the 1st eclipse (§§B4&E3) and the 2nd eclipse (§E4), it is easy to reconstruct the interval t used by System A's inventor: since he thought the time of eclipse was −262/1/26 5/8, we have

$$t = [-262/1/26\ \ 5/8] - [-1273/12/4\ \ 3/4] = 368955^{\mathrm{d}}7/8 \qquad (8)$$

So the ancient founder of System A was able to calculate his anomalistic month:

$$V_{\mathrm{A}} = (368955^{\mathrm{d}}7/8)/13390 = 27^{\mathrm{d}}199^\circ 39'00''.4 \approx 27^{\mathrm{d}}199^\circ 39' \qquad (9)$$

which gloriously matches eq.4 (Saros Text) with a seemingly round result — (packing more precision than superficially apparent)[34] that evidently had a special appeal for ancient ephemeris-creators (see compendium at ‡1 fn 5), presumably for reasons of convenience and easy remembrance.

[29] See Rawlins 1991W fn 223 for brief discussion of the responsive progression of ancients' eclipse-report precision, as theorists' interest in accuracy advanced.

[30] And by about the same amount in our single case: fn 27.

[31] So, if the −1273/12/5 eclipse was believed by System A's originator to have occurred at the start of the Babylonian day, we would express said local time as: −1273/12/4 3/4 (eq.8) or 6 PM.

[32] Obviously, to be visible at all, an eclipse fitting our conditions should be a winter event — and, as well, it ought to be either a very long eclipse (not the case here) and-or was observed by astronomers situated to the east of Babylon.

[33] One should always keep in mind that ancients used apparent not mean time. (We are assuming that the calculator took account of converting seasonal hours to equinoctial hours.)

[34] Like ‡1 eq.6 (from eq.5). Note that if eq.4 is accurate despite its roundish appearance (our 2nd hypothesis [§E]), this has key implications: [a] The Babylonian-rounding argument of §D3 becomes irrelevant and valueless. [b] Since eq.9 is how V_{A} came out round-looking, the likelihood would be enhanced that eq.9 indeed produced eqs.3&4. [c] Eq.9's pseudoroundishness could explain the oddity

D Whole-Day Rounding at Both Ends of the Eclipse-Pair

D1 Since it is likely[22] that the hour of the 1st eclipse (a millennium earlier) did not survive, it is reasonable to ask whether it's coincidental that the $1'$ imprecision in V_{A} (eq.4) corresponds to the 1^{d} imprecision in eq.5's numerator and to the 4^{s} error in the System A synodic month M_{A} (eq.6): all three imperfections are roughly 2 parts in a million.
D2 If this approximate triple-coincidence is meaningful, then the inventor of System A rounded both[23] ends of his eclipse-pairs to the nearest whole day — and computed his anomalistic month V_{A} as follows:

$$V_{\mathrm{A}} = 368956^{\mathrm{d}}/13390 = 9919°39'13'' \approx 9919°39' = 27^{\mathrm{d}}199°39' \tag{5}$$

which matches the attested value (eq.4).
D3 Note: for Greek time-measure (fn 20), the key unit-rounding-step in eq.5 will not produce the attested eq.4 result (making it $27^{\mathrm{d}}13^{\mathrm{h}}18^{\mathrm{m}}37^{\mathrm{s}}$ instead).[24] Which suggests that the computer of V_{A} was Babylonian.

E Whole-Day Rounding at Only the Early End of the Eclipse-Pair

E1 Our other and potentially more precisely-fruitful interpretation starts by wondering: if it were known[25] that V_{A} was as crude (eq.5's rounding) as it appears, then why would the Saros Text's ancient calculator carry his figuring (via eq.1) of the System A synodic month M_{A} to so many places (see Neugebauer 1975 p.501)? —

$$M_{\mathrm{A}} = 6695V_{\mathrm{A}}/6247 = 29^{\mathrm{d}}31'50''19'''11''''\ldots = 29^{\mathrm{d}}.5306444\ldots \tag{6}$$

Starting with this consideration, we probe by testing eq.5 backwards — and find thereby that V_{A} will end up *looking*[26] remarkably round if the numerator in eq.5 is:

$$t = 368955^{\mathrm{d}}7/8 \tag{7}$$

E2 This t (eq.7) was seriously mistaken (high by roughly a a half-day),[27] an error which became[28] the main factor degrading the accuracy of the contingent System A synodic month; however, this slip may turn out to be of critical assistance in telling us today which of our eclipse-pair candidates produced the t which led to System A's monthlengths.

[22][But note Rawlins 2003P §E5's curiosity about the basis of Ptolemy's highly accurate 3277^{u} equation (fn 21).] For the scholar who established System A: assuming he knew of the 1st eclipse report's time-roughness (it actually occurred nearer 6 AM than 6 PM), then he reasoned (wrongly) that the benefit of the antiquity of the −1273/12/5 eclipse outweighed the disadvantage of its crudity. (Hipparchos was faced with a parallel dilemma when considering whether to use Meton's similarly corrupted epochal solstice-time: Rawlins 1991H §§B3&B8.)

[23] The problem here is that precedent consistently shows that a classical-era astronomer attempting to determine a very large period, by using a longago day-epoch-anchored 1st datum, did not round his own 2nd datum to the nearest whole-day. Two examples at Rawlins 1985H.

[24] Natural unit-roundings (occurring at key reconstructed steps) have been interpretively used earlier in this issue: in the ‡1 derivation of the System B month; there, roundings twice consistently suggested (§A8 [but note there Britton's simple Babylonian theory] & fn 2 item [d]) that the inventor of System B worked in Greek time-measure. Now, this same reasoning attracts us (in §D, at any rate) to the conclusion that Babylonian time-measure was used in the computations of the inventor of System A.

[25] The argument here is analogous to that of ‡1 §A11, except that there is more reason in that case to be sure that the computer (Aristarchos) knew 1st-hand the true precision of the crude-looking quantity (since he'd probably computed it himself). By contrast: it's unlikely that the flukishly-surviving Saros Text was authored by System A's originator, so the author may have known nothing about V_{A}'s actual precision or origins.

[26] See eq.9. [But keep in mind that eq.6's inaccuracy is apt to eq.4's apparent imprecision.]

[27] Compare t in eq.7 (System A) to t in eq.2 (real). (And see fn 30.)

[28] By the hypothesis of this section (§E).

B Behind System A: the Saros-Series-Pair Prime Suspects

B1 The eclipse-pairs satisfying eq.2 are few in number, and (rather like the situation for the 795^{y} cycle: Rawlins 1996C §F) the visible ones do not occur at all uniformly in time. For that very limited number of pairs which do occur: in every case, the 2nd eclipse belonged to a saros-series whose Meeus-Mucke (1992) number was 5 greater than that of the 1st eclipse's Meeus-Mucke number. So, we will group our data according to Meeus-Mucke saros-series numbers (using the prefix "MM" to signify those numbers):
B2 Some sample pairs[8] from before 480 BC:

MM18&23:	−1952/06/16 &	−942/08/08
MM15&20:	−1841/12/14 &	−830/02/04
MM21&26:	−1811/05/19 &	−801/07/11
MM31&36:	−1558/10/07 &	−548/11/29
MM27&32:	−1547/03/12 &	−537/05/05
MM31&36:	−1540/10/17 &	−530/12/10
MM33&38:	−1518/08/16 &	−508/10/07
MM33&38:	−1500/08/26 &	−490/10/19

B3 A systematic search was made for 1010^{y} pairs whose latter eclipse occurred during the centuries following 500 BC, where the earlier eclipse could be seen in Babylon and the latter either there or in Alexandria. Revealingly, no pairs at all were found where the 2nd eclipse occurred between −244/2/7 (useless, since its −1255/12/15 mate was entirely invisible[9] in Babylon), and +67/5/17 (both it and its −943/3/25 mate were invisible): a blank of more than 300^{y}! All of which suggests the 3rd century BC as the approximate origin-epoch of eq.1. In §C1, we will present further evidence for such a date.
B4 Now to the post-500 BC eclipse-pairs not already noted. From the MM30&35 group:

−1442/01/22 & −432/03/15

But, notably, by far our richest saros-series matchup here is MM34&39, which (due to a longterm-near-stable anomalistic relationship between the two series) handed a bunch of 1010^{y} pairs to any 3rd century BC astronomer *who had access to the rich eclipse-record heritage of Babylon*. This single group (MM34&39) produced three visible[10] pairs:

−1345/10/22 & −335/12/14
−1291/11/23 & −280/01/16
−1273/12/05 & −262/01/26

[8] Most not visible in Babylon at both ends. The −1841/12/14 & −830/2/4 pair was already noticed at fn 1. See Rawlins 1996C §H2. It might be fun to speculate that this very early pair was the basis of System A's eqs.1-2. But (even aside from enormous inherent improbability, e.g., a huge discontinuity in ΔT's variation and-or a Chinese [!] report of the prior event): such a stretch-recourse is quite needless when so many other 1010^{y} pairs are known to end much nearer the era of the first firm extant evidence of System A's existence.

[9] According to modern theory, which of course is subject to change in response to future findings. Just in case it ever turns out that the −1255/12/15 eclipse was recorded (and this would only be a decade before the −1245 eclipse of ‡3 §C9), we may here note that the −1255 & −244 pair parallels a complete 1010^{y}-long saros-series (§B5): the 2nd eclipse ends series MM39, and the 1st eclipse is adjacent to MM39's beginning: see §B3 method [b].

[10] The last eclipse listed (−262/1/26) was invisible west of around Persepolis but its conclusion could easily have been recorded in the eastern part of the Seleukid empire. (See §E4.) Lunar theory was by then advanced enough that an eclipse's mid-time could be found (to all required accuracy) from the umbral emersion-time, simply by correction from indoor tables.
[Note added 2008. The original edition's list carelessly (since no use was made of it) included the −1417/09/09&−407/10/31 pair; but the −1417 event was invisible in Babylon.]

B5 Given the 1010^{y} feature of eq.2 (not to mention §A1), we note in passing that both saros-series MM34 and MM39 lasted 1010^{y} — and we are pairing eclipses (from each series) which are themselves 1010^{y} apart. This suggests that the very choice of 1010^{y} as an interval (not an especially attractive one, otherwise) may have been related to Babylonian saros-series-tracking.[11]

B6 How would a classical-period scholar determine the lunar anomaly for a 13th century BC eclipse? Possibilities:

[a] As B.Goldstein 2002 p.3 notes, Lis Brack-Bernsen in 1994 very ably laid out a case [see, more recently, Brack-Bernsen 1999] that regular Babylonian non-eclipse data could've identified anomalistic variations. [Britton 1999 p.220 believed that eclipses underlay Babylonian lunar theory; but he has later come to have doubts on that point.] If such means permitted determining the day (hardly hour) of apogee near early eclipses, an eclipse that occurred on an estimated apogee-day could be paired with an eclipse 12494^{u} ($1010^{\mathrm{y}}+$) later to produce eq.2.

[b] A scholar of the 3rd century BC could have realized that, given the nearly steady pace of the gradual lunar-anomalistic shift (averaging $-3°$/saros: Rawlins 1996C eq.14), which accrues during a saros-series' duration (ordmag a millennium), one could simply take a very long (e.g., 1010^{y})[12] saros-series (fully visible or no) and compare the eclipse at one end of it to an eclipse 1/2 year beyond the eclipse at the other end — and the two eclipses' lunar anomalies would be roughly equal (within about $10°$). Such an approach could have produced eq.2.

[c] If an ancient scholar believed that eq.2 was a stable cycle — that is, if a set of 1010^{y}-spaced eclipse-pairs seemed to exhibit closely equal intervals — then he might use them as Aristarchos used the 345^{y} cycle to find the month's length (see ‡1 eq.3 & §A2).

B7 However, option [c] (using several eclipse-pairs — as against the two one-pair methods: [a]&[b]) would be based upon an illusion, since eq.2 is actually *not* very steady. True, as we saw in ‡1 §A2, the best idea for finding the anomalistic month from period-returns is the identification of a near-perfect return in both lunar and solar anomaly (which would indeed ensure the constancy of the pairs' intervals). But the 1010^{y} cycle's duration (eq.2) is much less stable than the 345^{y} cycle's (‡1 eq.3). Not only is eq.2 less accurate & less frequent (in eclipse-occurrence) than the 345^{y} cycle (so one doubts if enough data could allow even a try at showing eq.2's constancy); but it (eq.2) also has a far less perfect return in solar anomaly g, causing periodic error with serious amplitude: the solar anomalistic remainder is $\Delta g = 42°$ [eq.2], vs merely $7°1/2$ in ‡1 eq.3. Lunar anomaly remainders ($-8°$, $-1°$, resp) add lesser error-amplitude. For the 345^{y} cycle, these two unwelcome amplitudes' sum is merely $2^{\mathrm{h}}/3$ (rms even less: ‡1 §A3), while for the 1010^{y} cycle the sum is c.4^{h}. (For the 795^{y} cycle [Rawlins 1996C eq.11]: c.5^{h}.) [See Rawlins 2003P §F7 tabulation.]

B8 A further complicating factor for method [c]: the most fragile eclipse pairings (such as 1010^{y} & 795^{y}) cannot come off when apogee-proximity is too great, so an average of even the densest & most scrupulously-collected set of observed results will not yield a correct mean month. This is inevitable when a large and quite unrandom[13] fraction of the sample is comprised of eclipse-pairs which are not mutually umbral. (By contrast, this is not a serious problem for ‡1's 345^{y} case.) Since all the intervals for the 1010^{y} eclipse pairs in our key saros-series-pairing (MM34&39: §B4) were above-average,[14] the most exact ancient empirical averaging of 1010^{y}-pair records would have yielded a result a few hours too-high.[15] This is a provocative point (which we'll return to at §E3) — since the System A synodic monthlength *was* in fact seriously too high.

B9 All of which warns us of the invalidity[16] of applying method [c] (looking for a stable interval) to a set of 1010^{y} eclipse-pairs. The safest conclusion here is that one-pair methods ([a]&[b]) are likelier[17] [Note added 2013. On later reflection, DR inclines (uncertainly) rather to supposing that option [c] was the (flawed, as indicated) source of eq.1.]

[11] See fn 9 — and the now-somewhat-less-dreamy speculation at Rawlins 1996C §H6.

[12] A saros-series of length 1010^{y} is the 2nd longest in the period under examination. (Note §B5.) [An odd coincidence: the longest Ptolemy sidereal planet-cycle (Mars: Neugebauer 1975 p.906 Table 15) is 1010^{y} long.]

[13] See the huge gap in 1010^{y} pairs specified in §B3.

[14] Mostly just short of $368955^{\mathrm{d}}1/2$. Compare this to eq.2.

C Eq.1's 3rd Century BC Origin

C1 A starting consideration: as late as Kallippos' calendar (epoch $-329/6/28$), the month's true length seems not to have been known even within 20^{s} (Rawlins 1991H fn 1), while System A's synodic month was only off by 4^{s} (§§D1&E1); so we can probably eliminate the pair ending at $-335/12/14$ — and all the earlier ones.[18] Thus, the preferred candidates' 2nd eclipses are $-280/1/16$ and $-262/1/26$.

C2 Survival of the Babylonian "Saros Text"[19] luckily may help us probe further, if perhaps on rather thin ice. This text directly attests to the length of the System A anomalistic month V_{A} (Neugebauer 1975 p.501), by telling us (in degrees)[20] what half of it equals:

$$V_{\mathrm{A}}/2 = 1,22,39,49,30 = 4959°49'30'' = 13^{\mathrm{d}}279°49'30'' \qquad (3)$$

C3 So, simply doubling eq.3 produces the Saros-Text-attested System A anomalistic month V_{A}, which was (and is) correct within a fraction of a timesec:[21]

$$V_{\mathrm{A}} = 9919°39' = 27^{\mathrm{d}}199°39' \qquad (4)$$

C4 The noteworthy and perhaps revealing feature about eq.4 is the strikingly imprecise-looking Babylonian expression for V_{A}: $9919°39'$. But there are two distinct ways of interpreting this feature. The next two sections will investigate these in order.

[15] The 795^{y}-pair interval of Rawlins 1996C §E7 was (typically) even further *below*-average.

[16] Even if one preferred option [c]: the eclipse-pair ending at $-262/1/26$ is still the best guess for providing System A's date, since it ended (for 300^{y}: §B3) a hitherto-long-accumulating series of visible 1010^{y} eclipse-pairs — so the period following -262 was (for centuries thereafter) the time of maximal availability of fresh 1010^{y} eclipse-pair data.

[17] Option [c]'s several problems (§B7) have pushed us towards preferring the conclusion that a single eclipse-pair launched eq.2 and thus System A. And note that, even if [c] were accepted, there would be a most-recent pair of the data-set adopted; so we still end up trying to identify (among the data listed in §B6) the single 1010^{y} eclipse-pair that immediately launched System A.

[18] However, anyone inclined to date System A at around 500 BC, might make something of the density of 1010^{y} pairs ending in the period -548 to -490 (§B2).

[19] Note the provocative coincidence that we are here finding (eq.9) that the key parameter (V_{A}) revealed by the "Saros Text" is founded upon saros-series data (MM34&39: §B4).

[20] Babylonians divided the day into degrees, not (as the Greeks did) into our hours of $1^{\mathrm{d}}/24$.

[21] V_{A} is several times more accurate than the better-known Greek value for V: ‡1 fn 4. The occurrence of such a smack-on-the-mark hit, found *en route* to the determination of the less accurate System A synodic month (§§D1&E1), suggests a possibility: did System A's establisher have outside access to an accurate V_{A}? (A notion encouraged by [Rawlins 1996C §E7 & by]: the oddity that though V is much harder than M to gauge accurately, the ancient computer treats V_{A} as primary, with M_{A} then computed only secondarily *from* V. See *idem*.) More conservative view: by accident, the error in eq.1 ($6247^{\mathrm{u}} = 6695^{\mathrm{v}}$) nearly cancelled the error introduced by the System A inventor's use (in eq.2) of too high a value (eq.5 or eq.7) for the 1010^{y} interval. Note: eq.1 was about 3 times less accurate than the famous relation (Rawlins 1996C eq.3) $251^{\mathrm{u}} = 269^{\mathrm{v}}$ resulting from ‡1 eq.2 — or than the equally impressive if less-wellknown late-Ptolemy relation (Rawlins 1996C eq.10) $3277^{\mathrm{u}} = 3512^{\mathrm{v}}$.
[Note added 2003. Rawlins 1996C §E tripled 3277^{u}, finding eclipse cycle 9831^{u}, unusable for Ptolemy. But quintupling 3277^{u} gets eclipse cycle 16385^{u}. Improbably, 2 of Ptolemy's 4 eclipses, 125/4/5 & 136/3/6 (*Almajest* 4.9&6, resp), have mates (visible Babylon) 16385^{u} earlier: $-1200/7/11$ & $-1189/6/12$, resp. Details: Rawlins 2003P. Both pairs yield same pseudo-stable 483859^{d} interval (actually [pulsing] high by 1 part in c.2 million). See p.3 fn 2 & Rawlins 2003P §§E4&F5 & fn 6.]