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DIO

The International Journal of Scientific History

The Great Pyramid: Star-Fixed When?

The Ancients' Grandest Eclipse Cycle? 1325 Years

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News Notes: Greater Pyramid Misses Old Kingdom's Polestar

In this issue, Hugh Thurston expertly explains (§‡1) the curious[1] newly-famous 2-star "precession" pyramid-orientation method. Reacting to such theories, *DIO* has noted (*Nature 412*:699; 2001/8/16) that, by the simplest 2-star *or* 1-star theories, the star 10i Draconis was central[2] to orienting Khufu's Great Pyramid. (See articles cited on back cover here.) Though it was ancient Egypt's best polestar, 10i Dra has evidently been hitherto-unmentioned throughout pyramidalists' by-now-Greater Pyramid of ever-accreting litterature on Khufu's tomb.

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[1] How did stars with rt asc α permitting *just* enough azimuthal speed (to explain pyramid orientations' "trend") get chosen? (Anciently or modernly?) After all, had Haack's stars been solstitial ($|\alpha| \approx 90°$ or $6^{\rm h}$), the speed would've been negligible (‡1 fn 5). (I.e., Alphard [α Hya] would've been as useful for E-W orienting as Haack's Acrab [β Sco], *and* [at $\alpha \approx 5^{\rm h}.6$] would've stayed virtually put [in declination δ] over centuries of precession. Other near-equator stars [with various α]: ϵ Peg, β Crv, η Oph.) Same constancy applies if the Spence star-pair's α had been equinoctial (e.g, α Dra & β Boo). Note confirmability-lack: Spence shopped afar for a star-pair whose line's speed fit Spence 2000 Fig.4 line a's pyramid-based $0'.28$/yr slope (Haack 1984 p.S122 made it $0'.33$/yr), later altered by her (to become the $0'.31$/yr Mizar-Kochab slope which *DIO*'s *Nature* paper established), needlessly, since *DIO* showed that 2 stars [11α & 10i Dra], each merely 1° from the 2627 BC celestial N.pole, fit [better than her own stars] her original $0'.28$/yr slope. Note: Spence 2001 Fig.1 exhibits only 1 line, not the [2000 Fig.4] original's two for slope-comparison [theoretical vs empirical]. What a fit! J.Belmonte later proposed yet another alternative (*Archaeoastronomy 26* [*JHA 32*]:S1-S20 [2001] Figs.3ff): γ&δ UMa, whose extended line was crossed by the pole in mid-26th cy BC. His line-speed (barred by Spence's empirical slopes): $40'$/cy in azimuth. (Actually less. Since annual azimuthal speed $dA/dt = p \sin\epsilon \sin\alpha / \cos\gamma = 0'.39 \sin\alpha$ [compare ‡1 fn 5], no speeds top $39'$/cy without high p.m.) [b] By unfortunate contrast to Spence (and to R&P), his stars' midpoint is nearly 4 times farther from the pole than the stars are from each other, which leverages observational error disadvantageously.

[2] As against the several two-star "precessional" methods (details: ‡1), DR prefers the one-star ***DIO*** alternate theory that the Great Pyramid was oriented via bisecting 10i Dra's tight circumpolar semi-arc (1° radius) during 1 winter-solstitial evening, when it was virtually symmetric in azimuth. (Impossible then for 11α Dra [Thuban], though Spence [*Nature 412*:699-700 (2001/8/16) p.700] can't see why.) Suggestion (Rawlins&Pickering 2001 p.699): could S.Haack-K.Spence's allegedly precessional orientational "trend" merely reflect the *rise&fall of Old Kingdom surveying science*, which obviously peaked in the time of Khufu, whose pyramid's sophistication is nicely consistent with such a theory? (By contrast: according to precessional theories [incl. R&P's], it is ***purely coincidental*** that the smallest orientation-error occurred for Khufu's pyramid.) Spence explains away pyramid-misorientations' apparently random signs by assuming the lined-up stars were precisely *inverted* for the 2 discrepant cases among her 8 pyramids. (It has been speculated that using Mizar&Kochab was a ritual. Hmm. Was it permissible for Egyptian rituals to be performed in reverse? And at different times of the year?) But does this just serve to explain-away weakness in the precession approach? (For its main strength, see ‡1 fn 2.) Note: [a] A 6-2 coin-flip split is asymmetric at less than 5-to-2 odds (twotailed), hardly significant. [b] Within ordmag $1'$, the Khafre pyramid's orientation equals Khufu's, in magnitude *and sign*, suggesting co-orientation (p.3 fn 4). But the Spence theory, faced with the need to explain the orientations of *these two best-oriented pyramids* (several decades apart) by using an other-pyramids-based ([1] Fig.4) speed of about $3'$/decade, cannot straightforwardly be reconciled with such a small orientation-difference; so she must save the situation by bringing in special assumption-ex-machina: [a] her Mizar-Kochab line had swept past the pole during the years between Khufu&Khafre; so (*as absolutely required*, to compensate for [a]) [b] her method was used in opposite ways (M-atop-K for Khufu vs K-atop-M for Khafre). Thus, two conveniently-cancelling $0°.1$-sized errors are adduced to convert the actual difference (ordmag $1'$) into the required theoretical difference (ordmag $10'$).

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H Sparse-ReMotes vs Truckload-Beamers [Note added 2003/12/30]

By automatically rejecting the discoveries of the present paper — as well as Rawlins 2002B & Rawlins 2002H — purportedly on the basis that there are no remote 13th century astronomical records directly surviving, our glowingly self-satisfied Muffiosi invite the following ghastly mote-beam (Matt. 7.3-5, Luke 6.41-42) observation, which DR put forcefully to the world's top Babylonianists (2003/6/22), at the latest University of Notre Dame biennial history-of-astronomy conference: while a blank in 13th century BC records is perfectly understandable (given the rarity[10] of extremely early astronomical observations, as exemplified by the uniqueness of the even-earlier Venus tables of Ammizaduga),[11] no such excuse is at all possible to explain away the *total* absence (in extant Seleukid-era Babylonian cuneiform records) of any explanation of how "Babylonian" astronomical parameters & tables were arrived at, this for a period from which (unlike the 13th century BC) a truckload of astronomical-math cuneiform texts do survive. Such critical explanatory ancient texts we have in detail from our slim Greek astronomical-math heritage, where (by contrast to Babylonian) we occasionally can even discern theory-founding empirical methods in action (see ***especially*** Jones 1999A [or *DIO 9.1* p.2]) and can very precisely trace tabular parameters' empirical bases: e.g., Rawlins 2003J eq.31 & Rawlins 2002A eqs.6-13. Our utter blank in parallel Babylonian material is completely consistent with DR's long-loathéd position (Rawlins 1991W §§H3-H4 & fn 73) that *Babylonian astronomy is derivative*; but it is embarrassingly inconsistent with the sacred-central Muffia tenet that Babylonians were the true originators of serious ancient mathematical astronomy.

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[10] Ptolemy alleges (Rawlins 2002B fn 7) that full *continuous* Babylonian astronomical records surviving into his (or Hipparchos') time start at 747 BC; yet, from the first few post-747 BC centuries very, very few now exist. So how can we use our admitted blank in 13th century BC data, to found trustworthy conclusions about the survival rate (down to Hipparchos' time) of such early Babylonian eclipse records? [I.e., if Hipparchos had access to about 100 usable Babylonian eclipse records from the centuries soon after 747 BC, then their survival rate (from his day to ours) has been ordmag 1%. So, if he had just 10 (perhaps bunched) pre-1200 BC eclipse data (could this explain ancients' resort to odd [gappy or perigee-apogee] eclipse cycles?), then it's c.10-to-1 against any such surviving today.]

[11] Obvious question: if a 15th century BC Babylonian astronomical text (Ammizaduga-Venus) — entirely unmentioned in extant classical texts — can survive about 3500^y (incl. the Dark Ages) down to the present, then: by what divinely-bestowed wisdom do Muffiosi conclude that it is impossible that unmentioned (nontrivially *later* than Ammiz) 13th century BC Babylonian astronomical material cannot have lasted merely about 1/3 as long? — down to the time of high ancient Greek astronomy. [Note added 2004/4/25. Mesopotamian gov't interest in astral prediction far precedes 747 BC: Eckart Frahm (Yale Univ) has kindly informed *DIO* that an 11th century BC astrologer's advice to his king is quoted by a 657 BC successor (*State Archives of Assyria 10* [1993 Simo Parpola Ed.] Letter #100).]

Giza Monumental Considerations

DIO has posted (HASTRO 2001/8/16) an obvious but perhaps novel thought: the intelligent & admirably imaginative (if shaky) precessional theories of Steven Haack 1984 (*Archaeoastronomy 7* [*JHA 15*]:S119-S125) & K.Spence (2000/11/16 cover story[1] *Nature 408*:320-324, plus Gingerich pp.297-298 promo) have neglected[2] a simple consideration: there was no need to celestially orient Khafre's pyramid independently, since its east side (casing) is (deliberately?) almost exactly twice as near the west side of Khufu's Great Pyramid as the Khufu pyramid's W&E sides are to each other.[3] Strikingly consistent with this theory: the Khafre pyramid E&W sides' orientation is nearer[4] the Khufu W side orientation than to true N-S.[5]

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[1] Rawlins-Pickering's amiable correcting paper was sent Spence 2000/11/28. She ducked, so it went to *Nature* 2000/12/4; she kept right on delaying&delaying, which helped paralyze embarrassed *Nature*'s ability to get out a correction. Spence damns the *DIO* theory as "extremely unconvincing". But Echo Gingerich (her referee, prefacer, and partner-in-bungling&sniping) says: "extremely unconvincing". So the botched version spread abroad unchecked for 3/4 year: *Discover* 2001 Jan; *Scientific American 284.2*:28 [2001 Feb]; *Mercury* July-Aug; *National Geographic 200.3*:98 [2001 Sept]). After 9 months of variously impeding publication of *Nature*'s admission of the mismath underlying her choice of stars, she alleged (*Nature 412*:699-700 [2001/8/16]) that the error she'd hidden for so long really didn't much matter after all: so-what if she'd miscomputed her original graph's slope by 4′/cy when fitting it within 1′/cy of her data? But *DIO*'s correct math finally emerged: *Nature 412*:699 [2001/8/16]; *NYTimes* 2001/8/28 (Science [John Wilford]); *Astronomy* 2001 Dec (p.34 [William Schomaker]). Similarly, after at 1st privately (mis)claiming 10i Dra was too dim, Spence 2001 p.700 just substituted a baseless-scoff at our unanswerable p.699 counter that 10i Dra is very accurately placed in both of history's high-precision naked-eye 1000-star catalogs: Tycho's & Hevelius'. (It's only just to add here: [a] OG was 1st to admit the Spence error, & [b] she courageously over-assumed full blame.)

[2] If it's strange for Spence implicitly to aver that ancients clung — within a very few arcmin — to the Mizar-Kochab method long after its accuracy had drifted off by tens of arcmin, then how much stranger is it to insist further that ancients would preternaturally adhere to this method *decades* ***before*** *it became accurate*. (Obviously, modern proponents of the two-star method should've rejected all pre-Khufu data right off the top.) Sneferu-era observers' comparison [Mizar-atop-Kochab's azimuth vs K's-atop-M's] would've revealed a huge discrepancy: roughly 1°/2, i.e., as big as the lunar diameter! *Twice* even that for the last of Spence's pyramids, Neferirkare's. Again: how can she propose that ancients oriented sacred monuments to within *arcmin* of the indications of a method, which exhibited blatant internal contradictions of ordmag a *degree* in the very test [M-over-K vs K-over-M] which (as Gingerich astutely notes) was the sole justification for choosing the M-K line in the 1st place??

[3] So, for an ancient Egyptian surveyor, orienting the Khafre pyramid by simple geometry (i.e., non-celestially) from the N-S line of the Khufu pyramid's west side was no harder than internally orienting a side of either pyramid from its own opposite — once one had been celestially established as N-S.

[4] According to J.Dorner (1981 p.80), Khafre's E&W sides have 6′.0 W misorientation. Dorner (*ibid* p.77) has Khufu's W side off by just 2′.8 W; but Petrie (1883 p.11) found the Khufu west-side cornerstones ("sockets") c.4′ more W-oriented than this; so Khafre's architects could've been misled westward by c.6′, just by working off the west cornerstones of Khufu's already-revered Pyramid.

[5] From the finest survey of the Great Pyramid's base (fn 4 & ‡1 fn 11): [a] The N&S sides are more consistent (& more accurate) than the E&W sides (which seems astronomically strange, at 1st). [b] The E side is the worst (of the 4 sides) by both criteria, while the W side's orientation-error is much closer (than the E side's) to the N&S sides'. Indication (contra M.Lehner [*loc cit* ‡1 fn 9]): the original star-fixed meridian was the W side. [Though (final internal topography permitting), assumption of a celestially-based meridian along the Pyramid's central axis offers a direct explanation of the N&S sides' superior consistency&accuracy.] The N&S sides were then squared to the W side. (Presumably via straight-angle bisection, perhaps with large metal-compass or the equivalent, plus [for straightness] lengthy cord and-or long-distance sighting. Pyramid-builders *required* a perpendicularity-method; it can create parallelness, though not vice-versa.) The E side was similarly squared to the N&S sides; and, to avoid a mere rectangle, as distant from the W side as the N&S sides are from each other (equality assurable from checking diagonals vs bisected rt.angle, and-or laying end-to-end a small set of extremely long metal bars), the least appreciated of Giza surveying feats. Random error-growth left greater E side-vs-W discrepancy than N-vs-S.

‡1 On the Orientation of Early Egyptian Pyramids

by Hugh Thurston[1]

A Introduction

A1 Eight early Egyptian pyramids are oriented amazingly accurately; their eastern and western sides are less than degree from a true north-to-south line (§A3). If we arrange them in order of date, we find that with two exceptions their orientations drift slowly but steadily clockwise.

A2 This suggests that the pyramids were aligned by a method vulnerable to precession. The two exceptions, the pyramids of Khafre and Sahure, are out by the amount expected[2] but in the opposite direction, suggesting that the method was reversible[3] and was used in reverse for these two pyramids.

A3 If we reverse the sign of the deviation for the two exceptions cited in §A2 (clockwise deviations here are positive), we have the following data, listing pyramids in chronological order:

Site	Pharaoh	east side	west side
Meidum	Sneferu	$-20'.6$	$-18'.1$
Dahshur, south	Sneferu	$-17'.3$	$-11'.8$
Dahshur, north	Sneferu	$-\ 8'.7$	
Giza	Khufu	$-\ 3'.4$	$-\ 2'.8$
Giza	Khafre	$+\ 6'.0$	$+\ 6'.0$
Giza	Menkaure	$+12'.4$	
Abusir	Sahure	$+23'$	
Abusir	Neferirkare	$+30'$	

The deviations of the east and west sides are quoted from a paper [1] by Kate Spence, who used a number of sources, primarily the work of J.Dorner — especially his 1981 dissertation [2].

B Prior Suggested Methods: Haack, Spence, Belmonte

B1 Steven Haack [3] suggested that the Egyptians searched for a star that appeared to rise precisely due east and then aligned their pyramids' north and south sides on it.

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[1] Professor Emeritus of Mathematics, University of British Columbia. Address: Unit 3 12951, 17th Avenue, South Surrey, BC, Canada V4A-8T7; phone 604-531-8716. See Thurston's other writings on the pyramids in his *Early Astronomy* (Springer 1994) and in *Griffith Observer* 2001 September. All footnotes here are by DR. *DIO* expresses thanks to Peter Dorman (Oriental Inst, Univ Chicago) for advice (he regards it as not firmly established that ancient Egyptian cattle-counts were biennial) and to Michaela Rossini (U.Innsbruck) for transmitting J.Dorner's thesis.

[2] In spite of alternate possibilities, etc, noted elsewhere here (p.2 fn 2 and p.3 fn 2), DR must in fairness take space to emphasize the prime strength of Spence's theory (& Haack's earlier one): for her eight-pyramid sample (where she has dropped one of Haack's data [Zoser]; added another [Dahshur-Red]; & altered others [Sahure & Dahshur-Bent], presumably for the better), the temporal trend of misorientations' absolute magnitudes is monotonic (in either direction from her theory's null-error time) — which is consistent with the precession theory's explanation. (Though, some of the standard deviations estimated by Spence [2000 Table 1] are comparable to [and in some cases exceed] the associated differences which establish this monotonicity.)

[3] See p.2 fn 2.

F6 For the information of our readers, we here supply the dates of gaps for all four of our long delicate cycles; these gaps[9] indeed appear about every 6 centuries (§F4):
690^{y}: −788/11/12 to −386/ 4/15 and −111/ 7/ 2 to 254/11/12.
795^{y}: −683/ 9/23 to −357/ 9/19 and − 36/12/ 7 to 254/11/12.
1010^{y}: −830/ 2/ 4 to −548/11/29 and −244/ 2/ 7 to 67/ 5/17.
1325^{y}: −262/ 1/26 to −193/ 5/11 and 331/ 3/10 to 393/ 5/12.

F7 It will also be useful to other ancient-astronomy investigators to provide A_{g}&A_{V} for all of the long cycles *DIO* has discussed (delicate or no), including the 345^{y} cycle (whose minuscule A values dramatically illustrate its superiority, thus explaining its adoption [Rawlins 2002A] as the best foundation for the mean month), its double (690^{y} cycle), plus the famous 18^{y} saros and 19^{y} Metonic (Easter) cycle. We list successively each cycle's duration in synodic months U, as well as its integrally-rounded number of solar anomalistic years G, solar remainder r_{g}, associated amplitude A_{g}; lunar anomalistic months V, lunar remainder r_{V}, associated amplitude A_{V}; draconitic months W, & draconitic remainder r_{W}:

U	G	r_{g}	A_{g}	V	r_{V}	A_{V}	W	r_{W}
223^{u}	18^{g}	$+10^\circ 1/2$	$0^{h}3/4$	239^{v}	-3°	$0^{h}1/2$	242^{w}	$-0^\circ 1/2$
235^{u}	19^{g}	$-0^\circ 1/4$	0^{h}	252^{v}	-53°	9^{h}	255^{w}	$+7^\circ 1/2$
4267^{u}	345^{g}	$-7^\circ 1/2$	$0^{h}1/2$	4573^{v}	$-0^\circ 5/6$	$0^{h}1/6$	$4630^{w}1/2$	$+11^\circ 1/3$
8534^{u}	690^{g}	-15°	1^{h}	9146^{v}	$-1^\circ 2/3$	$0^{h}1/3$	9261^{w}	$+22^\circ 2/3$
9660^{u}	781^{g}	$-2^\circ +$	$0^{h}1/6$	10353^{v}	-92°	14^{h}	10483^{w}	$-2^\circ 1/3$
9831^{u}	795^{g}	-65°	$4^{h}1/3$	10536^{v}	$+2^\circ 1/3$	$0^{h}1/2-$	$10668^{w}1/2$	$+22^\circ 1/3$
9895^{u}	800^{g}	$-2^\circ 1/2$	$0^{h}1/6$	10605^{v}	-145°	19^{h}	10738^{w}	$+5^\circ 1/4$
12494^{u}	1010^{g}	$+42^\circ$	$3^{h}-$	13390^{v}	-8°	$1^{h}1/2-$	$13558^{w}1/2$	-22°
13645^{u}	1103^{g}	$+62^\circ 1/2$	$4^{h}+$	14624^{v}	$-173^\circ 1/3$	20^{h}	$14807^{w}1/2$	-0°
16385^{u}	1325^{g}	-109°	$6^{h}1/2$	17560^{v}	$+4^\circ$	$0^{h}2/3$	17781^{w}	$-22^\circ 3/4$

G Reflections

I do not know with certainty whether others have previously explored all of the peculiarities and the variety of interacting periodicities & pulsations which we have here revealed, in connection with the peculiar class: delicate vast eclipse cycles. (The lengthy study van den Bergh 1955 anticipated none of our new results.) But I hope that these will be of interest both to astronomers and to historians — and that the latter will be assisted (by the foregoing scientific findings) in future analyses of ancient astronomers' methods.

[Note added 2008. *DIO 11.1* and *DIO 13.1* have found solutions to all three previously unsolved ancient lunar period-relations, adducing five eclipse-pairs. The condition that all ten eclipses be above the horizon for at least some portion of the umbral phase relates to ΔT researches. (Of the 5 older eclipses, 4 were near the horizon, possibly helping later astronomers know their hour.) ΔT for the 13th century BC has heretofore been exclusively based on an *extrapolative* leap. (Across a 1/2 millennium gulf between then & the earliest extant eclipse records, data elicited and analysed by dedicated experts, for whom *DIO* has high admiration.) By contrast, *DIO*'s triple-solution, a "mere" computational speculation, represents a *mathematical* leap, to an isolated, non-extrapolated 13th century BC snapshot. As noted, our method is Greek-standard-attested (and easily explains all 3 period-relations' integrality & high accuracy); but if firm incompatibilities between the two leaps develop, it will be up to *DIO* to snipelessly publish the other side and up to others to choose.]

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[9] Some gaps are not entirely empty. The 1st of §F6's two 1325^{y} gaps contains two grazing eclipses: −233/7/2 & −215/7/12. The 690^{y} gaps contain grazing eclipses (over 100^{y} apart): −668/6/11 & −527/5/14 and 12/5/24 & 153/4/26.

which, again, could cause an overestimate of the synodic month's length (after dividing the pair's separation d by 16385) by about a part in 2 million.

F Long Delicate Eclipse Cycles' Patterns

In recent years, *DIO* has examined four huge delicate eclipse cycles (citations at §E5): $690^{\rm y}$, $795^{\rm y}$, $1010^{\rm y}$, & $1325^{\rm y}$. In doing so, we have discovered certain features common to all four, and these have physical as well as historical interest.

F1 Several saros-strings (ss) are always simultaneously active, and long delicate cycles are woven of ss ends (grazing-eclipses). Cycle-pairs' lunar anomalies v are usually spaced about 120° apart. When ss successively appear, disappear, & are replaced by new ss, delicate-cycle eclipse-pairs' anomalies tend (except during occasional transition periods) to be either very nearby or about 120° distant — an effect resembling a cinema of a variably-diffuse equilateral triangle.

F2 For any given epoch, the v at which a cycle's eclipse-pairs occur are near[7] the three points of this slowly precessing equilateral triangle, which we have already dubbed the "PBT"[8] while analysing it in Rawlins 1996C. Within each ss, the successive anomalies flow in retrograde at a mean speed of a little under 3°/saros or nearly a degree/$6^{\rm y}$.

F3 However, as each ss is replaced by succeeding ones (in the PBT cinema), the anomalistic triangle moves not backward at §F2's degree-per-$6^{\rm y}$ pace — but rather forward at about a degree per $17^{\rm y}$, a speed controlled by the lunar anomalistic precession of the highly accurate $441^{\rm y}$ Hipparchan draconitic equation (*Almajest* 4.2; Rawlins 1996C §F10 & eq.19 [or Rawlins 2002H eqs.1&3]).

F4 We have mentioned previously (§B1, Rawlins 1996C §F, Rawlins 2002B §B3) that our delicate long cycles are so fragile that (for all four) whole decades or even centuries will pass without a single umbral pair occurring. That is, during such a temporal gap, if an eclipse has a prior cycle-mate, it's penumbral & thus effectively invisible. For each of our four cycles, the mean periodicity of the occurrence of these gaps is roughly 6 centuries, an effect influenced by the near-integralities of a nest of secular lunisolar returns of about that length (inter-related by the saros & the $65^{\rm g}$ cycle): $6667^{\rm u}$ ($539^{\rm g}$), $7248^{\rm u}$ ($586^{\rm g}$), $7471^{\rm u}$ ($604^{\rm g}$), $8052^{\rm u}$ ($651^{\rm g}$). E.g.,

$$7248^{\rm u} = 7768^{\rm v} - 82^\circ = 7865^{\rm w}1/2 + 0^\circ = 586^{\rm g} - 4^\circ = 214037^{\rm d}18^{\rm h} \qquad (11)$$

F5 Of course, as noted in §F4, the period of disappearance (of umbral eclipses having earlier umbral matches, according to a cycle) does not occur with discrete suddenness. During fertile (i.e., non-gap) periods, the rate of pair-occurrence rises and reaches a maximum at about the halfway-point between gaps — and the average length of the intervals between the pairs themselves also reaches an extremum there. (And in the time just before&after a gap, the number of umbral pairs noticeably ebbs in comparison to the maximal density occurring halfway between gaps.) These extremal effects will combine to bias-corrupt any estimate of monthlength based upon naïve averages of data (as remarked at §E8 and Rawlins 2002B §B8), since pair-intervals' sizes reach the opposite extremum during a gap (whose intervals would of course be missing from the average's input, by the gap's very definition).

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eq.3-mate eclipse will almost always have been visible (i.e., above-the-horizon) in Babylon, one hour to the east. This is one of the reasons why visible eq.3 pairs were more frequent than visible eq.2 pairs.

[7] The density of eclipse-occurrence is greatest for the anomaly-point (of the equilateral PBT's three points) which is nearest perigee, and it is least (indeed often virtually or exactly nil) for that which is nearest apogee.

[8] Precessing ss-Bound anomalistic-Triangle (Rawlins 1996C §F). (Some cycles [e.g., $690^{\rm y}$] exhibit more than one PBT.) The PBT's advancement/gap is c.1/10 of the zodiac (a rate which increases very slightly over the centuries), so that after roughly 2 millennia, the three PBT points are advanced near enough 1/3 of a circle that the PBT is approximately as it was.

B2 The azimuth where the star appeared to rise would rotate clockwise. The azimuth where it appeared to set would rotate in the opposite direction at the same rate, and Haack suggested using this to orient Khafre's pyramid.

B3 However, the Egyptians cannot have done this.[4] As I saw on a recent visit (having been forewarned by Kate Spence), the plateau at Giza slopes upward to the west and when the Egyptians levelled the ground for Khafre's pyramid they left a cliff some 10 metres high a little way to the west. This would form a high local horizon and cause a star that rises due east to set much too far south (of due west) to account for the slight deviation of the pyramid.

B4 Haack suggested that the ancient surveyors used the star α Arietis for the two pyramids at Abusir, β Scorpii for the others.[5]

C Suggested Methods: Spence

C1 Kate Spence suggested that the Egyptians thought that the pole was directly between the stars Mizar and Kochab, and indeed in 2467 BC it was [1]. For Mizar above Kochab, the azimuth would drift clockwise, so most pyramids were aligned with Mizar above Kochab. For the two exceptions, Kochab was above Mizar. Both drifts were at $0'.31$ per year [4].

C2 2467 BC is very late in, or later than, the dates given by various authorities for the fourth dynasty. Dates for the start of the dynasty vary from 2640 BC (quoted in D.Arnold, *Building in Egypt* [1991] as from "R.Krauss, 1985") to 2575 BC (Baines & Malek, [5]). Dates for the end of the dynasty vary from 2504 BC (the earliest accepted by Beckerath [6]) to 2454 BC (the latest accepted by him).

D Suggested Methods: R&P

D1 Dennis Rawlins and Keith Pickering suggested [4] that the Egyptians thought that the pole was equally far from Thuban and 10i Draconis,[6] and indeed in 2627 BC it was. This would imply that when these stars were aligned horizontally, the point midway between them was due north.

D2 Most pyramids would be aligned with the two stars above the pole; the two exceptions, under the pole. This alignment would drift at $0'.274$ per year. The suggested date 2627 BC is very early in, or earlier than, the dates for the fourth dynasty given in §C2.

E Observations

E1 According to Haack's method, the Egyptians simply watched[7] for a star that rose due east or set due west and they aligned each pyramid on the rising or setting of this star. The observations had to be made at the time of year when the star rose or set at night.

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[4] See [4]'s last paragraph.

[5] These stars had two different speeds in azimuth: $0'.34$ per year for α Arietis, $-0'.39$ per year for β Scorpii. Both values for azimuthal speed provided here are for 2600 BC, when α was 332° for Hamal (α Ari) and 180° for Acrab (β Sco). (During the period of our interest, Hamal's speed increased merely $0'.004$/yr/cy, while Acrab's was virtually constant.) In general: $dA/dt = p \sin\epsilon \cos\alpha \sec\gamma \csc A$ (on horizon), which equals $0'.39\cos\alpha$ for due E or W at geographical latitude $\gamma = 30^\circ$ and (for mid-3rd millennium BC) annual precession $p = 0'.82$ & obliquity $\epsilon = 24^\circ$. If dA/dt is positive the rising and setting points rotate northward; otherwise, southward.

[6] The star 10i Dra is of variable magnitude (4 1/2-to-5). In almost exactly 2800 BC, adjacent Thuban became the most proximate pole star in history (brighter than 4th magnitude): less than $0^\circ.1$ from the exact pole. But, starting in 2627 BC, no brighter star was nearer the pole than 10i Dra, for the next 11 centuries. (And no brighter-than-5th-magn star nearer for the next 9.)

[7] A direct method would be: just looking for stars that appeared to set 180° from where they rose.

E2 For the other two methods the Egyptians needed to find the pole: not only its direction but also its altitude (which is $29°58'$-$59'$N true[8] at Giza, a little less at the other sites). If they didn't know where the pole was, they would not know that it was collinear with Mizar and Kochab or equidistant from Thuban and 10i Draconis. These two methods are most easily carried out by using a plumb-line. Stand at a corner[9] of the pyramid-to-be and watch the two chosen stars. When they are aligned, vertically or horizontally as the case may be, they are north. Set up a plumb-line north of the position of observation, high enough to cover the stars and very nearly touching the ground.

E3 A little earlier the next night, go to the same spot, taking with you a small ring fixed to the top of a tripod formed by three sticks lashed together. (If you want to test the practicability of this for yourself, you will find that a camera tripod serves very well.)

E4 For the vertical alignment method, watch one of the stars through the ring, moving the tripod to a position where the plumb-line covers the star. Move the tripod sideways to keep the star covered (the movement needed will be quite slow) and when the plumb-line covers the other star, drop a plumb-line from the ring. The line between the two plumb-lines gives the orientation sought.

E5 The horizontal alignment method is similar. Keep the plumb-line covering the midpoint between the two stars and drop the plumb-line when they are judged to be horizontally aligned. Because the two stars are close together (merely $1°1/2$ apart), a midpoint judged by eye will not be far out, and because the midpoint itself is so near the pole (barely 3/5 of a degree away), a slight misjudgement in the time of horizontal alignment will not move the midpoint very far sideways.

F Precedents

There is precedent for using two stars collinear with the pole to find north: today we (or at least Boy Scouts & Girl Guides) use the Pointers. And there is a precedent for using a vertical alignment: Polynesian and Micronesian navigators knew that when the Southern Cross was upright it was pretty well due south.[10] There is no known precedent for either of the other methods.

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[8] The apparent (not true) altitude of the pole is $30°00'$ (1/12 of a circle) as seen from all three Giza pyramids. (Latitudes: Khufu $29°58'.7$ N, Khafre $29°58'.5$ N, Menkaure $29°58'.3$ N.) Whether this is accidental is discussed in D.Rawlins, "Ancient Geodesy: Achievement & Corruption" (*Vistas in Astronomy 28*:255-268 [1985] pp.255-256).

[9] See Mark Lehner *The Complete Pyramids* London 1997 pp.212-213, 220 (& p.109).

[10] If based upon verticality of Crux's mast ($\alpha\&\gamma$ Cru), the Pacific peoples' S.Cross-method would have provided exact south just before 1000 BC; or just after 3000 BC, if horizontality of the crossbar ($\beta\&\delta$ Cru) was used instead. The UMa Pointers' line hasn't pierced the pole since the mid-13th century AD, but the Pointers are still popular anyway — primarily for finding Polaris (α UMi), a star which provided north by being about $1°$ from the true pole, as α Dra & 10i Dra were, back in the 27th century BC. (The gyroscopically-precessing celestial pole was $71'$ from Polaris in 1908 [when the Scouts were founded]; now, $43'$ away; in 2102, the pole will pass within $28'$ of Polaris and then recede from it.)

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accessible data-collection), going back to the same 13th century BC. [B] If eq.1 was based upon Ptolemy-era discovery of eq.3, then Babylon cannot have been the place of discovery, since the city of Babylon died out during the 1st century AD.

E5 Eq.3 is the longest of all the eclipse-cycles (incl. on §F7's list) which *DIO* has proposed may have have been used by ancient astronomers for gauging celestial motions. (We here itemize those which exceed 2/3 of a millennium: 690^y [Rawlins 1996C §§D&E]; 781^y [Rawlins 1996C eq.31]; $795^{\rm y}$ [eq.2 & §D]; 800^y [Rawlins 1996C eq.20]; $1010^{\rm y}$ [Rawlins 2002B eq.2]; $1103^{\rm y}$ [Rawlins 2002H eq.3]; & now $1325^{\rm y}$ [eq.3]. These are included in the list at §F7.) Eq.3 therefore especially possesses the attractive long-period-relation advantage that errors of imprecise-return and-or of timing at either end (of the cycle's time-interval) will not have a devastating effect on a monthlength value based (in standard fashion) upon dividing the interval's length by the cycle's number of months, since the latter is, after all, $16385^{\rm u}$. For, after division by 16385, a one hour error at either end of the span will contribute an error (in estimating the moon's synodic motion) of less than one part in 10 million. I have previously (Rawlins 2002B fn 22) doubted whether the hour of an eclipse at so remote an era would be likely to survive, an attitude which may need re-evaluation (if we accept that eq.3 underlay eq.1) given the excellent choice Ptolemy made in selecting eq.1 as his ultimate ratio of the synodic & anomalistic months. [Note added 2004/3/20. If a remote eclipse's hour survived without exact date, ancients' previously-established tables may've fixed the latter.]

E6 Another corruptor of the accuracy of any anciently cycle-induced celestial motion would have been anomaly-remainders r, which reflect imperfect integralities in a cycle and so cause variations in returns which ought ideally to be constant. An example is the $109°$ size of eq.3's solar-anomaly remainder $r_{\rm g}$. From Rawlins 1996C fn 56, we have the amplitude $A_{\rm g}$ of the wave of variation (in the length of ancient eclipse-pairs) caused by $r_{\rm g}$:

$$A_{\rm g} \approx 2\cdot 2° \sin(|r_{\rm g}|/2)/m \approx 8^{\rm h}\sin(|r_{\rm g}|/2) \tag{7}$$

(where $2° \sin v$ is the solar equation of center, and $m = 0°.508/{\rm hr}$, the mean lunar synodic motion). So, for $|r_{\rm g}| = 109°$ (eq.3), we have amplitude

$$A_{\rm g} \approx 8^{\rm h}\sin(109°/2) \approx 6^{\rm h}1/2 \tag{8}$$

E7 There is a similar (if ordmag smaller) effect from the lunar-anomaly remainder $r_{\rm V}$:

$$A_{\rm V} \approx 2\cdot 5° \sin(|r_{\rm V}|/2)/m \approx 20^{\rm h}\sin(|r_{\rm V}|/2) \tag{9}$$

(where $5° \sin v$ is the syzygial lunar equation of center). For $|r_{\rm V}| = 4°$ (fn 3), this is:

$$A_{\rm V} \approx 20^{\rm h}\sin(4°/2) = 1^{\rm h}- \tag{10}$$

not a major factor. For the $1325^{\rm y}$ cycle, the in-phase sum of $A_{\rm g}\&A_{\rm V}$ (eqs.8&10) can be no worse than a little over $7^{\rm h}$. Such an effect might cause an error of a part in 1-2 million if one (extremal) $1325^{\rm y}$ pair were used — i.e., without the caution of averaging several pairs.

E8 But even averaging is insufficient protection here, because an unrandom portion of pairs go missing (see likewise at Rawlins 2002B §B7). In fact, the average of eq.3-pairs' length for the five visible-visible pairs listed in §E2 was too high[5] by nearly $1^{\rm d}/4$ day[6] —

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[5] By contrast, those $795^{\rm y}$-pairs uneaten by gaps tend to be *lower* (than eq.2's mean $d = 290315^{\rm d}07^{\rm h}$). During the last 1 1/4 centuries (before the long gap after -36), the dozen umbral eclipses with prior umbral eq.2-mates ($9831^{\rm u}$ ago) tended to have hour-remainders in d (above the integral value $290315^{\rm d}$) which were about $3^{\rm h}$-$4^{\rm h}$ or $6^{\rm h}$-$7^{\rm h}$. The latter remainder will obviously much lower the odds of both eclipses (of a pair) being above the horizon. The unfavorable Hipparchan eq.2-pair cited at §D2 fell victim to a $7^{\rm h}$ remainder in d. For such and other bad-luck reasons: of the six umbral eq.2-pairs preceding Hipparchos' last known observation ($-126/7/7$), none were surely visible at both ends of the $9831^{\rm u}$ interval. Thus, the $-921/12/12$ & $-126/10/15$ firmly visible-visible eq.2-pair was the first for decades, which Hipparchos might have used, if: [a] he were still active, and [b] clear weather favored both observations.

[6] But this effect carries an important benefit (much favoring eq.3): on average, it enhances eq.3's $d = 483858^{\rm d}20^{\rm h}$ to a little over the integral value $483859^{\rm d}$. Thus, an Alexandria-visible eclipse's prior

D But What of the 795 Year Cycle?

D1 So does the case for eq.3 now overturn Rawlins 1996C's belief (esp. §E) that eq.2 underlay eq.1? It very well may. However, one's view of this question depends in part upon whether one accepts that eq.1 had to be new for Ptolemy to cite it. (Not the firmest basis of argumentation.) After all, it is possible (if not probable) that eq.1 arose from both eqs.2&3.
D2 Though obviously we now have a strong case that eq.1 originated in Ptolemy's era, the foregoing is not a rigid bar to Hipparchos having known of eq.1, too. Several eq.2-eclipse-pairs were available to him; and, for using eq.2, he would require eclipse data only back in the 10th (not 13th) century BC. (In eq.2's favor, see also Rawlins 2002B §G & Rawlins 1996C §I15 item [d].) Further, paralleling Ptolemy's coincidence (§C1): the only Hipparchos partial lunar eclipse (−140/1/27) happens to have a prior eq.2-mate (−935/3/26) — though (see fn 5), if current estimates of Earth-acceleration are correct, it ended before moonrise in Babylon (while partly visible ordmag 10° of longitude to the east thereof). (No Hipparchos eclipse has a previous eq.3-match, which is consistent with our general theory that classical-era astronomers were not using data older than the 13th century BC.) So all 3 extant small eclipses of Hipparchos & Ptolemy are connectible to the 3277^{u} cycle (eq.1), an impressively big-stretch coincidence, given the eclipse-pairs' infrequency.

E Implications

E1 *In toto*, the foregoing obviously favors eq.3 as the source of eq.1, but it also requires Greek access to such early Babylonian data that the genetic-conservatives (who've long ruled ancient-astronomy-history's petrified landscape) may find eq.3 even less palatable a progenitor than eq.2. But, if one responds receptively to the pro-eq.3 evidence of §B, the implications are more unsettling and important than those of Rawlins 1996C's theories (which assumed eq.2-ancestry). [See below at §H.]
E2 As noted at §B2, eq.1 appears to be based upon eclipse-pairs ending in the 2nd century AD — and (according to eq.3) with separation 1325^{y}. Thus, since eq.1 could not have been later than c.165 AD (§B1), simple subtraction tells us that eq.1 was based upon eclipse-pairs whose early eq.3-mates were no later than:

$$165 - 1325^{\mathrm{y}} = -1160 \tag{6}$$

— an era far earlier than the 'til-now conventionally-assumed limit (747 BC: §A1) for eclipse records that could have survived into classical antiquity. We've already seen (§C1) that Ptolemy's small eclipses both have eq.3-matches that occurred shortly before eq.6's date: Babylon-visible eclipses in −1200 & −1189. Moreover, several other eclipses that were recent to Ptolemy were matched by Babylon-visible eclipses 16385^{u} earlier. (Note: all 16385^{u}-cycle eclipse pairs are from ss whose Meeus & Mucke 1992 numbers differ by 14.) We will list a few such pairs, also appending the two Ptolemy-eclipse matches:

−1247/7/22	&	78/4/16
−1236/6/20	&	89/3/15
−1207/5/31	&	118/2/23
−1200/7/11	&	125/4/5
−1189/6/12	&	136/3/6

E3 Adding to Rawlins 2002B & Rawlins 2002H, the foregoing considerations represent the 3rd piece of *DIO*-produced evidence indicating that the astronomers of classical antiquity had access to 13th century BC (§C1) Babylonian eclipse records.
E4 Note two revealing implications here: [A] The possibility considered in Rawlins 2002H (§D4 vs fn 14), that Hipparchos may have had *special* access to 13th century BC Babylonian data, is hardly compatible with the indication here (§E2) that astronomers of Ptolemy's time may have used a different set of eclipses (presumably from a publicly

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G The Intervals of Time Between the Pyramids

G1 Except for Sneferu's 2nd & 3rd pyramids, the dates at which the pyramids were started will be (fn 12) within a year or two of the lengths of the pharaohs' reigns; a pharaoh will obviously start his pyramid early in his reign.
G2 The lengths of the reigns are fundamental to Egyptian chronology. Early chronology depends on a Sothic date in the 12th dynasty, which pins the 7th year of Senosret III at 1872 BC. (Most Egyptologists regard Sothic dating as valid. Those who don't have no anchor point.)
G3 Dates before Senosret III depend entirely on estimating the lengths of individual reigns and working back step by step.
G4 The fourth and fifth dynasties, together with the sixth constitute the Old Kingdom. Then came a period of chaos known as the first intermediate period (FIP), after which the eleventh dynasty started the Middle Kingdom.
G5 Our main source for the lengths of the reigns is the Turin canon, a papyrus document which unfortunately is far from complete. It can be supplemented by a late compilation in Greek by Manetho, by the Palermo stone, and by inscriptions at Abydos & Saqqara.
G6 We can put a lower limit on the length of a reign if we find a record of an event such as the 24th cattle-count in the reign of Sneferu. Cattle-counts probably took place every 2 years; if so, Sneferu must have reigned for at least 48 years. (See R.Stadelmann, "Beiträge zur Geschichte des Alten Reiches," *MDAIK 43*:229-240 [1986].)
G7 The records are complete enough to take us back from Senosret III to the beginning of his dynasty, the twelfth (Baines & Malek describe this date as "known with precision", [5], page 36), and with fair confidence to the beginning of the eleventh.
G8 We then have to deal with the FIP, about which there is practically no information. Cyril Aldred [8] makes it 149 years. Beckerath gives it a maximum of 50 years and a minimum of zero [6].
G9 Obviously for the Old Kingdom we should not put much faith in absolute dates, However, we can look at the lengths of the reigns. In the Turin canon, most of the names of the pharaohs are missing; but all except two can be supplied from tablets at Saqqara and Abydos. The two in parentheses below are from Manetho: we do not know the Egyptian forms of these names. This gives the table below.

Pharaoh	Length of Reign
Sneferu	24^{y}
Khufu	23^{y}
Djedefre	8^{y}
Khafre	$2[]^{\mathrm{y}}$
(Bicheris)	$[]^{\mathrm{y}}$
Menkaure	$[]8^{\mathrm{y}}$
Shepseskaf	4^{y}
(Thamphthis)	2^{y}

G10 Manetho had two pharaohs named Suphis (presumably Khufu and Khafre) with reigns of 63 and 66 years. Beckerath assumed that he obtained the 63 by adding 40 to the correct number, to agree with a remark by Herodotus that the pharaohs who built the enormous pyramids reigned over 60 years. Beckerath assumed that Manetho did the same for Khafre, and that therefore his missing digit is 6. He altered Sneferu's reign to 35 years. (We saw in §G6 that this is still too low.) At one point Beckerath stated that Menkaure's reign was 18, 28, or 38 years, presumably because there was not room on the missing fragment for more than three of the hieroglyphs for 10. He settled for 28 in his final list.

He allotted 7 years to Bicheris from the 7 allotted to Sebercheres by Manetho (who allotted 22 to Bicheris). He altered Djedefre to 9 and Shepseskaf to 5.

G11 This, together with Beckerath's dates for the fifth dynasty gives column Bk in the table below. For comparison, columns B&M and CAH give the corresponding intervals from Baines & Malek [5] and the Cambridge Ancient History [9], respectively. Columns Sp and R&P give the time in years to produce the changes in orientation of the eastern sides of successive pyramids, as shown in §A3, using the methods of Spence (Sp) and Rawlins & Pickering (R&P), respectively.

	Bk	B&M	CAH	Sp	R&P
Sneferu to Khufu	35^{y}	24^{y}	24^{y}	55^{y}	63^{y}
Khufu to Khafre	32^{y}	31^{y}	31^{y}	30^{y}	34^{y}
Khafre to Menkaure	33^{y}	30^{y}	$25^{y}+$	21^{y}	23^{y}
Menkaure to Sahure	43^{y}	32^{y}	41^{y}	34^{y}	39^{y}
Sahure to Neferirk.	13^{y}	12^{y}	14^{y}	22^{y}	25^{y}

When reading this table bear in mind that the best historical estimate for Sneferu-to-Khufu is none of those listed. It is 48 (§G6).

G12 Spence's method[11] makes Khufu's reign start about 2480 BC.[12] R&P make it about 2638 BC. Haack made the fourth dynasty start about 2640 BC.

G13 I don't know what chronologists will make of this. So far they seem to have ignored it. But at least it confirms that they were right to reject Manetho's long reigns for the pharaohs who built the Giza pyramids and backs Stadelmann's case [cited §G6] that estimates for the length of Sneferu's reign cited in §G11 are substantially too short.

H Appendix: Some Mathematics

H1 Spence's paper [1] gave rise to some trigonometry which, though not relevant to Egyptology, is of interest in itself. And it sparked the interest of Rawlins & Pickering.

H2 Let us denote by ϕ the angular distance between the celestial pole (as seen from Giza) and the plane through Giza, Mizar, & Kochab at instants when these stars are aligned vertically. Spence interpreted ϕ as the deviation from north given by the vertical line. It is not. (As pointed out by Rawlins & Pickering in [4] and agreed to in Spence's reply.)

H3 Figure 1 shows the situation. G is Giza. GP is in the direction of the pole. GN is horizontal, and PN is vertical, so GN points horizontally north.

H4 How do we find the angle between GP and the vertical plane through Giza and the stars? Answer: drop a perpendicular PQ to this plane; then the angle PGQ is the angle required (namely ϕ).

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[11] The misorientations of Khufu's sides (Dorner 1981 p.77), all W: N $2'28''$, S $2'31''$, W $2'47''$, E $3'26''$. (Note: parallel Khufu W&E sides' azimuths would disagree by $4''$ from Earth-sphericity; not negligible if we display $0'.1$ precision.) Taking the W side (above: p.3) as the closest approximation to the ancient surveyor's original orientation-error, R&P's Thuban-10i Dra method produces 2636 BC for the Khufu pyramid's start. [Proceeding as in fn 12: $-2626 - 2'47''/0'.274 = -2636 = 2637$ BC.] The R&P dates for Khufu are nearer conventional ones than Spence's (as noted in [4]).

[12] Taking the mean $3'.1$ W misorientation (fn 11 or §A3) of the Great Pyramid's E&W sides, one can divide by Spence's original (flawed: §H2) Mizar-Kochab $0'.28$ speed ([1] Fig.4 line a's slope) and subtract this from -2466 (2467 BC), the date of null error (when the pole was exactly on the Mizar-Kochab line): $-2466 - 3'.1/0'.28 = -2477 = 2478$ BC. She further corrected for a presumed 2^{y} gap (§G1) between the pharaoh's ascension and his pyramid's start, to find 2480 BC for the ascension date. (Repeating her calculation with correct slope [$0'.31$/yr] yields 2479 BC, instead; it's a tiny difference in this case, but date-corrections for some of the other pyramids are ordmag 10^{y}.)

A4 This 1325 yr cycle is comparable in delicacy to the 690 yr cycle (§E5), the 795 yr cycle (eq.2) and the 1010 yr cycle (Rawlins 2002B eq.2) for a reason common to them: in each of these four cases, the absolute magnitude of the [mean] draconitic remainder r_{W} ($23°$, $23°$, $22°$, & $22°$, resp) is so near the [true] $25°$– outer limit for eclipse-pairs occurring at all, that for each of these cycles the odds are merely ordmag 1/10 that a given eclipse will have a previous cycle-mate.

A5 Either eq.2 or eq.3 — or possibly both (§D2) — could have launched eq.1. Already in Rawlins 1996C §§D-H, the case for eq.2 has been presented; now, we will proceed below to investigate the pro-eq.3 evidence, e.g., §§B, C, E3, fnn 5&6.

B Which Cycle Was Ptolemy's Final Lunar Equation Based Upon?

B1 There is a remarkable feature common to all four of §A4's long&delicate cycles (690^{y}, 795^{y}, 1010^{y}, 1325^{y}): each exhibits gaps during which no eclipse-pairs occur. (Note: such unfragile relations as the key 345^{y} cycle [§F7] have no gaps at all.) We will discuss details later (§F4). But the immediate connexion is this: the 795^{y} cycle's classical-era gap [zero eclipses with mates 795^{y} earlier] extends from -36 to $+254$, while Ptolemy's writing of the *PlanHyp* (containing our sole attestation [§A1] of eq.1) was about 160-170 AD. By contrast, for our newly-proposed 1325^{y} cycle: the classical periods when no eclipses occur (which have prior umbral eq.3-mates) are (§F6) $-262/1/26$ to $-193/5/11$ and $+331/3/10$ to $+393/5/12$.

B2 Now, it is generally believed (and Rawlins 2002B §L4 bolsters the conventional view) that *PlanHyp* (source of eq.1) improves the theories of the *Almajest* by using data from Ptolemy's own time. If this theory is on the right track, it starts us in the direction of favoring eq.3 as eq.1's (prime) basis — for the obvious reason that no eq.2-separated eclipse-pairs ended during Ptolemy's career, or indeed for well over 100^{y} before he was even born (§B1). By contrast, eq.3-pairs repeatedly occurred (§E2) during his century.

C Ptolemy's Era Connectable to 3277 Month Cycle After All

C1 And it gets only better when we check in detail. An eclipse which has a mate (another umbral eclipse) 16385^{u}-distant must be a partial eclipse of magnitude less than 10 digits. Of Ptolemy's four eclipses (*Almajest* 4.6&9), the only ones of sub-10-digit magnitude are those of 125/4/5 & 136/3/6. By a striking coincidence, each has a mate 16385^{u} before (and visible in Babylon): $-1200/7/11$ & $-1189/6/12$, respectively. How *a priori*-unlikely is this? We will evaluate the odds in two ways.

C2 Approach 1: During the period of Ptolemy's contemporary observational data (125-141), sixteen lunar eclipses were at least partially[4] visible in Egypt, of which Ptolemy used four. The probability p_1, that these four eclipses should include the only two that had prior eq.3-mates, is:

$$p_1 = C_2^4/C_2^{16} = 6/120 = 1/20 = 5\% \qquad (4)$$

C3 Approach 2: During the years 125-141, seven sub-10-digit eclipses were visible in Egypt, only two of which had previous eq.3-mates. The probability p_2 that both happen to be the very two which Ptolemy preserved is:

$$p_2 = 1/C_2^7 = 1/21 \approx 5\% \qquad (5)$$

C4 Either way, the odds (though not huge) are statistically significant, and this in a pure probability case — i.e., where the gauging of degree-of-significance is not muddied by the nonGaussian vagaries which typically attend observational error.

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[4] See Rawlins 2002B fn 10.

‡2 Vast Eclipse Cycles: Stabilities & Gaps

Delicate Huge Eclipse Cycles' Six-Century Pulsations

Ancients' Longest Period-Relation? — 1325 Years

Ptolemy Now Connectable to His 3277 Month Cycle

Greek Use of 13th Century BC Data: Yet Another Hint

by Dennis Rawlins

A The 3277 Month Cycle & Our Expanding Temporal Horizon

A1 *DIO 6* ‡1 §E investigated Ptolemy's last lunar equation (*Planetary Hypotheses* 1.1.6 or Rawlins 1996C [www.dioi.org] eq.10):

$$3277^{\rm u} = 3512^{\rm v} \qquad (1)$$

(Superscripts here & below: u = synodic months, v = anomalistic months, w = draconitic months; g = anomalistic years, y = tropical [Metonic][1] years, y = sidereal years, K = Kallippic years; d = days, h = hours, m = timeminutes. Degree-remainders merely signify 360ths.)

A2 In Rawlins 1996C (eq.11), we found that tripling eq.1 produced an eclipse cycle:

$$9831^{\rm u} = 10536^{\rm v} = 10668^{\rm w}1/2 + 22^\circ = 795^{\rm g} - 65^\circ = 290315^{\rm d}07^{\rm h} \qquad (2)$$

It was there discovered that this would require eclipse data from no later than the 9th century BC, $84^{\rm y}$ before Nabonassar 1 (the long overconfidently[2] assumed 747 BC limit for Babylonian astronomical observations later used by the Greeks). But DR did not then go beyond, since the very idea of Greek access to pre-1000 BC data seemed just too outré.

A3 But since *DIO 11.1* (Rawlins 2002B & Rawlins 2002H), we have consistent indications (esp. Rawlins 2002H §§C9&D1) of Greek use of 13th century BC eclipses. So, on 2003/1/26, while walking along Baltimore's University Parkway, the *DIO 11*-triggered afterthought (finally. . .) arrived: I'd never checked multiples of eq.1 beyond three; so I swiftly tried out higher ones — and immediately (17:20 EST) found that five times eq.1 hands us the following eclipse-cycle:[3]

$$16385^{\rm u} = 17560^{\rm v} = 17781^{\rm w} - 23^\circ = 1325^{\rm g} - 109^\circ = 483858^{\rm d}19^{\rm h} \qquad (3)$$

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[1] See Rawlins 1996C fn 13.

[2] See Rawlins 2002B fn 7.

[3] In reality, eq.3's 2nd term was $17560^{\rm v} + 4^\circ$. (See eq.10.) Eq.3 adds yet another eclipse-cycle to *DIO*'s collection thereof (fully listed at §F7), which we have reconstructed just out of simple multiples of anciently-attested lunar period-relations. Rawlins 2002H §E3 (or, better, fn 17's large parenthesis) found it improbable — at a moderately significant level — that so many eclipse-cycles could thus arise merely by accident. However, the fact that eq.1 leads us to more than one eclipse cycle (eqs.2&3) does not increase these odds, since the computation of each cycle's likelihood (of having possible-ancestor-eclipse-cycles) was found by asking merely for the probability of non-zero potential ancestors.

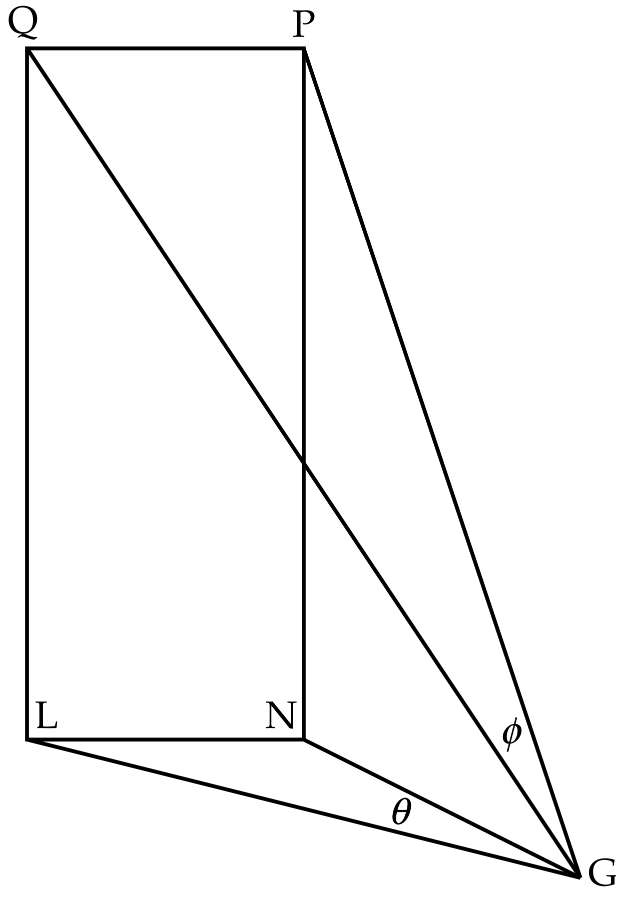

Figure 1: Showing the relation between θ and ϕ. [See §H3.]

H5 Drop a perpendicular QL to the horizontal plane through Giza. Then GL is the ground-level orientation given by the stars. The angle LGN is the deviation from north; let us call it θ.

H6 Because PQ = LN while PG and QG are greater than NG and LG, the angle PGQ is clearly smaller than the angle NGL. That is to say, $\phi < \theta$. By elementary trigonometry, we can find the exact relation between θ and ϕ. The angle PGN is the altitude of the pole, which is Giza's latitude γ. Then (in Figure 1):

$$\sin\phi = \frac{QP}{GP} = \frac{LN}{GP} = \frac{LN}{GN}\cdot\frac{GN}{GP} = \sin\theta\cos\gamma \tag{1}$$

H7 An indirect calculation can be done on the celestial sphere (e.g., Figure 2), which is a mathematical fiction devised by the ancient Greeks who had no device like the vector for dealing with directions. It is a large imaginary sphere with its centre at the centre of the Earth. Any direction in space is represented by the point on the sphere in that direction, and angles are represented by arcs of great circles on the sphere. The Greeks could then use spherical trigonometry in their calculations.

H8 Figure 2 illustrates the celestial sphere centred at Giza. G is Giza. GZ is vertically upwards. P is the celestial north pole. K and M are Kochab and Mizar when they are aligned vertically, so the great circle through them goes through Z. Both L and N are on the horizontal plane through Giza. PQ is an arc of the great circle through P perpendicular to plane ZMKL. Then ϕ is represented by the arc PQ and this in fact is how Spence quoted it. θ is the angle LZN.

H9 If we denote the interior angles of the spherical triangle ZQP by Z, Q, and P, and the sides by z, q, and p, we have (by the law of sines): $\sin z/\sin Z = \sin q/\sin Q$. But here $Z = \theta$, $z = \phi$, $Q = 90^\circ$, and $q = 90^\circ - \gamma$. So we have:

$$\sin\phi = \sin\theta\cos\gamma \tag{2}$$

just as in eq.1.

H10 Since ϕ and θ are tiny, the horizontal arc ϕ' from P to ZMKL is very close to the arc PQ.

$$\phi' = \theta\cos\gamma \tag{3}$$

Rawlins & Pickering used ϕ' instead of ϕ. The difference, if θ and ϕ are small, is negligible. (In case you are interested, if $\gamma = 29^\circ 59'$ and $\theta = 12'$, then the exact formula eq.1 gives $\phi = 0^\circ.1732341$, while the simpler relation eq.3 gives $\phi' = 0^\circ.1732342$.)

H11 As a result, the intervals between pyramids used by Spence [1] have to be divided by $\sec\gamma$ (which varies from 1.155 at Giza to 1.149 at Meidum).

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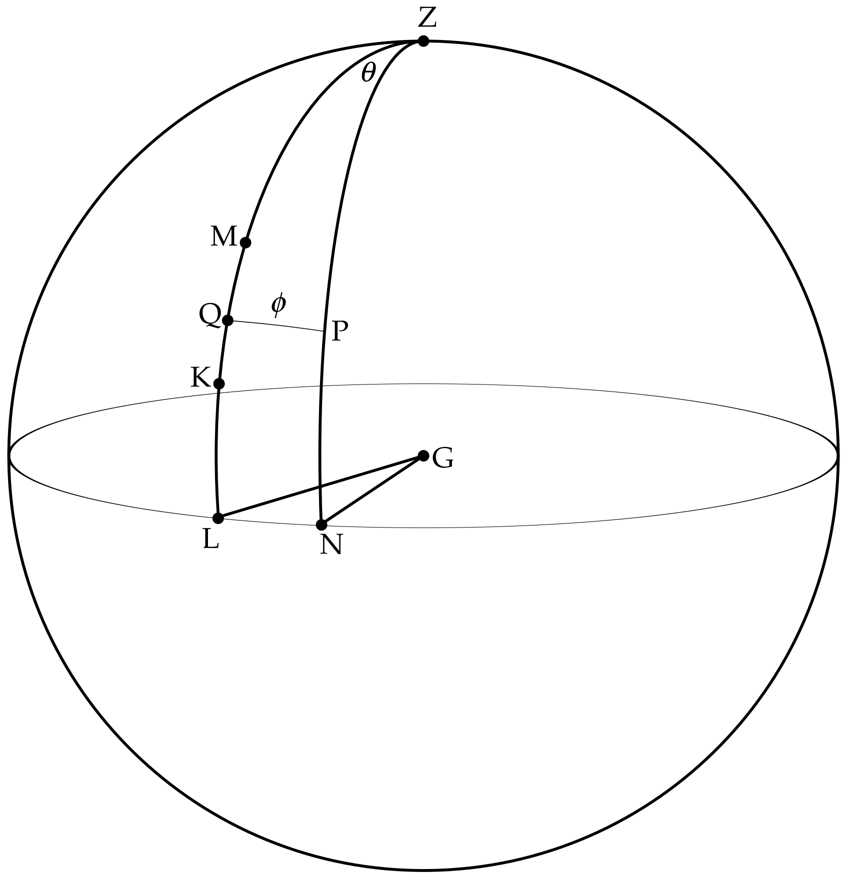

Figure 2: The relation between θ and ϕ using the celestial sphere. [See §§H7&H8.]