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B. L. van der Waerden (world-renowned University of Zürich mathematician), on DIO’s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: “marvellous.” (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)

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

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

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

British Society for the History of Mathematics (Newsletter 1993 Spring): “fearless . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth.”
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**NOTE Added 2013:**
The neat discoveries contained here at **DIO 1.3** are expanded in **DIO 20** [2012] §3 §§F&G, with novel, surprising results.

**DoublyGlacial Ethical Evolution in History of astronomy Ice Age**

[Note added to 1998 printing.] The central Mufia-circle 1991 misconceptions pointed out here in **DIO 1.2** were finally (if reluctantly — and belatedly) acknowledged by the **Journal for the History of Astronomy** at p.164 of its 1995 May issue (vol.26, #2). (The full, sinuous tale behind the retraction — plus our thanks to [author] Alex Jones, Hugh Thurston, Curtis Wilson, and Science’s Eliot Marshall — may be found at **DIO 6** §3.) This dawn-glimmer has unfortunately not been discernably exalted by subsequent ethical-sunrise for certain invincibly ineducable and coldly amoral archons.

**And DR Doesn’t Even Believe in Miracles**

[Note added to 2016 printing.] In 2005, Anne Tihon’s probe of the newly recognized 130 AD papyrus P.Fouad 267A, revealed an unexpected shock to **Journal for the History of Astronomy**—H.A.D. hatred of DIO’s discovery (within at §§K8-M4) of Hipparchos’ search for a ~157 Summer Solstice and contemporary adoption of Kallippic solar motion. Both were explicitly confirmed on the papyrus. Of course, rejection continues (JHA 39:293-294 vs DIO 20 §2 §§N2-N3), since JHAthinks has never had much relation to evidence.

* See DIO 4.1 §4 (“Casting Pearls Before Pyglets”); DIO 4.3 §15 fn 41 (& sources there cited — including here [DIO 1.2] at fn 129) and §12; DIO 6 §1 fn 1, §3 §§B1, D9, H; DIO 7 §15 §84.
Given the strong nature of some comments in the foregoing material, regarding classicist Alexander Jones, the publisher (DR) wishes to note that though Jones (like DR: www.dioi.org/err.htm) makes occasional reconstruction-misjudgements, he is an excellent & productive scholar in key areas of the field. Indeed, only a few years after DIO 1.2, he went on to make a shocking and uniquely valuable Hist.sci discovery (the sole surviving ancient ms record [103-104 AD] of an astronomer’s observational verification of a long period-relationship of a planet (Jupiter).

See our too-brief comments on this 1999 triumph in DIO 9.1 News Notes. Unfortunately, our gratitude to the Mufia’s positives remains unilateral, and Jones has in the new millennium (no longer under the late Curtis Wilson’s watch) increasingly reverted to cultist stolidity even in the face of the most unambiguous counter-Muffia evidence, degenerating into tactics of non-citation & shameless data-alteration, putting one in mind of the Jonestown 1991 crackpottery detailed in the preceding pages. (See DIO 16 p.2 & [3 §5E, F, esp. incredible G4; and do not miss Jones&Duke’s unprecedently persistent 6-fold destructive crusade, cataloged at DIO 2012 fn 10, http://www.dioi.org/vols/wk0.pdf.) For this reason, our longago pacifist gesture (of suppressing Jones’ name at 99% of the critical references to him in later editions of this issue) has been abandoned in 2015, with all original 1991 citations restored.

[Note added to 2003/10/31 printing.]

Nonetheless, enjoy Jones’ magnificent 2003 success regarding the Almajest planet mean motions — a find which additionally overturned a dumb DR misjudgement (even while confirming — better than DR had — a major theory of [an undeservedly lucky] DR): for specifics (and ironies!), see DIO 11.2 p.30 (& §G1). Also: above at G9.

More on DR’s admiration for Jones’ sometimes invaluable work, as well as his originality, can be found in these sources and thereabouts.

[DR must also here gratefully point to Jones’ crucial & expert assists in DR’s own work: see, e.g., DIO 11.1 §2 [§G3 & §3 D1] (though Jones appears unwilling to realize the benefit cited) — and don’t miss DIO 11.2’s front cover.]

[Notes added to 2012/5/18 printing. Augmented 2013/8/23.]

[1] The foregoing article analysing Hipparchos’ lunar researches has been expanded in DIO Vol.20 [www.dioi.org/vols/wk0.pdf], finding that the Hipparchian 1° discrepancy in Triao (above §M3) was due to a deliberately forged datum. Details at DIO 20 §F3G1.

[2] D.Duke (Centaurus 47:163-177; 2005) tries displacing the foregoing spot-on hits by trying to force Toomer’s method (above §D1&P1) to work, by fiddling the data (DIO 20 §K). A.Jones (JHA 33:15-19; 2002) plays a like displacement-attempt rôle vis-à-vis our Diller-DR discovery (DIO 5 & DIO 16 §3) that all 14 Strabo Hipparchan klimata fit sph trig calculations using accurate obliquity 23°3/2/3. Both authors assume ancient computational unreliability as conveniently needed (since they get no hits at all without such desperate speculation), while implying nothing is proved by DIO’s straightforward multiple perfect fits for ALL data: 4-for-4 (above eqs.19-20&23-24); 14-for-14 for Diller-DR (DIO 16 §3 Tables 1&2.) Neither author even cites the DIO analysis he is attempting thus to devalue! — despite (because of?) knowing both were set for highly prominent re-publication at Isis 93 1:58-69 (2002 March) pp.60&67.

[Note added 2015. We are obliged & glad to acknowledge that Isis’ 2002 publication of Thurston’s review of DIO’s work partially refuted our disbelief in Hist.sci’s ever moving past Muffia klaniness, though the Isis Cumulative Bibliography’s cessation of listing DIO papers looks suspiciously like joining herd-ormenta. As for whether Hist.sci can keep stably handling DIO’s discoveries and independence, the future will tell.]

§9 Muffia Orbituary

Hipparchos’ Early & Frankensteain Solar Orbits Discovered Thanks to: Topbilled JHA-Isis Proof They Couldn’t Exist!

The JHA’s Nonrefing, Winter Equinox, and Queer Year Aristarchos’ AU and His Orbital Elements of the Moon Early Trig & 2 Hipparchan Math-Astronomy Hoaxes

A Let Us Now Braise Famous Men

A1 The paper that follows here presents numerous serious scholarly discoveries, throwing surprising new light on the realities of ancient astronomy, e.g., [a] the hitherto-unknown heliocentrist basis of Hipparchos’ long-mysterious 2nd century BC lunar math (e.g. 23 & 24, below), [b] the existence of “Ptolemy’s Theorem” (and of resultant high-precision trig tables, fn 234), nearly 3 centuries before Ptolemy. However, this paper also reveals so many creatively-choreographed scholarship-pratfalls, by the currently prominent Neugebauer-Muffia cult, that it was impossible to choose between putting the article in DIO vs. the Journal for Hysterical Astronomy (J.HA). Thus the paper’s split header. Those scholars, who are primarily interested in startling & entirely novel evidence of heliocentrist’s primary rôle in high ancient astronomy, are encouraged to skip ahead and start reading at §K below (DIO 1.3). On the other hand, readers daunted by severe danger of involuntary up-chucking, can plunge directly (next page) into an extended Tragicomedy-of-Errors, starring the usual cast of eminent Mufosi. This (initial) half of the current paper will be

[Notes added to 2012/5/18 printing. Augmented 2013/8/23.]

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E.g., eminent BrownU scholars have discovered the previously unsuspected existence of the Winter Equinox (Journal for the History of Astronomy 22:2:101 [1991] p.119) and Autumn Solstice (Dictionary of Scientific Biography 13:321 [1976]). Both triumphs are praised at J.Hystorical Astron 1.2 §B4. (Who can fail to repress perfect trust in said discriminating genii’s judgement, as they condemn nonmembers of their clique as “disreputable”, “incompetent”, and “crank”?) These Ivy League fists will forever rank with W.T.Poor’s equally epochal 1926 discovery of the East Pole of the Earth. An understandably puzzled C. Robin has lamented such envy-submerged scientific breakthroughs: “people don’t like talking about them” (Winnie-the-Pooh Chap.9). But DIO & J.HA are consecrated to dispelling this heartwrenchingly unjust neglect — concurrently revealing the proportional shares of competence vs. brainkissing requisite for Ivy League professorship, at least in certain disciplines. (So that there will be no charge of suppression against DIO: physicist DR must confess to being a Harvard graduate.) Granted, criticizing these frantic magnates’ attempts at mat and apologia is oftimes about as challenging as shooting fisheshists in a barrel of monkeys. But, the targets insist: the showbiz must go on.

Above all, this remarkably long-running Hist.sci farce has the social utility of illustrating (with the sort of precociously stark nonambiguity which laymen and even children can follow) just how seriously we should take top academe’s aggressively-advertised Deep-Concern for maintaining open discourse and for ensuring reliable, non-crank expertise and refereeing. E.g., in the ongoing ancient astronomy controversy, the Princeton Institute for Advanced Study has profoundly invested its reputation into the hyperglorification of the uniquely clumsy fakist-astrographer C.Ptolemy, boldly and devotedly promoting this notorious occultist as “The Greatest Astronomer of Antiquity” — a mission so special that the Institute has itself faked several calculations in support of it.10 These math pretenses have, with exquisite irony, simply back-enhanced the Institute-resented notion that Ptolemy would behave likewise. Those eminent journals and scholars (e.g., the extremely handsome Journal for the History of Astronomy [JHA] and its esteemed Editor-for-Life) whose prestige has been hurled against skepticism of Ptolemy have also emulated the scholarly National Geographic Society’s longstanding example of: [1] courageously hiding from open debate or mutual cross-exam situations, during 20 of behind-the-back slander of dissenters, and [2] graduating from mere judge of an issue to championing-advocate of one side of that issue — ultimately becoming about as open to conversion-by-evidence as astrologers or parapsychologists.

Finally: both as justice and as demonstration of what certain handsome journals are incapable of doing, it will be the consistent policy of DIO to praise and utilize the occasional genuine accomplishments of our self-appointed Enemies — including even those of the brave Neugebauer clan, ever honored in these reverent pages as “the Muffa.” To quote11 from our premier issue: “The Muffa’s essential attitude is that [hate-objects Robert Newton] long hoped for an amicable outcome of this controversy — and a pooling of all parties’ respective talents, to assist a better understanding of ancient science. (See, e.g., DIO 1.1 § fn 7.) The former aim has been reached by irrevocable mistrust. [Spectator 1713/3/27]: “There is nothing that more betrays a base ungenerous Spirit, than the giving of secret Stabs to a Man’s Reputation.” See O.Gingerich’s schiz private libels of DR as DIO 1.1.11 fn 20 — circulated behind-the-back even, in his direct dealings with DR. OG was pretending to genial neutrality. See also DIO 2.1 § fn 33.) But that has prevented DR’s unilateral pursuit of the 2nd ideal. DR’s inept critics’ determination to eliminate such a bad-for-business whistleblower (in 268) is palpably stronger than their scholarly ability to accomplish that aim. (Which is precisely why Muffosi braves flee face-to-face dealings, leaving instead on the crutch of whispered libels.) They want me bad? Well, they’ve got me bad.

3 quoting the intro of JHA Adv. Ed. AND Isis Adv. Ed. A Van Helden, the noted scholar Isis deputed to review the JHA (& its wise & quality-insistent Editor for-Life) in the “Review of Journals & Serials” at Isis 52.12:280 (1990) p.298. No incest here. (For a summary of the outstanding qualities of the JHA & Isis papers under review in the present J. Hyster Astron paper, see below at §§C.6 & fn 92.) [Note added 1993: See DIO 2.3 § fn 18.]

2 Certain exposed Hist.sci archives have tried to portray DR as pure puceps horribilis. A key consideration in evaluating DR’s pungent recent evaluations of archonal misbehavior: despite occasional sharp private reactive criticisms (generally correct, though subject to change wherever evidence warranted) of Muffa pre-DR hide&slander tactics, DR’s original submissions for publication were pretty mild. E.g., to Science 1976/10/20: “Those few U.S. scientists who have been aware of the [Ptolemy Controversy] must thank Owen Gingerich for his Aug.6 review [Gingerich 1976] of . . . [Neugebauer 1975] . . . a commendable departure from the years of silence and systematic non-citation of [Robert] Newton’s findings. . . .” To Science 1977/10/10: “U.S. professional historians of astronomy, long deeply committed to the position that Ptolemy was the ultimate personification of ancient astronomical wisdom, have affected a Beneath Reply freeze toward [R.Newton’s mounting new] evidence . . . . I hope Science readers will consult the knowledgeable defenses of Ptolemy in: [Neugebauer 1975] pp. (2) 101-118, 283, 836, 894; [Pedersen 1974] (pp.131, 204-206, 248-258); [Gingerich 1976]; [Toomer 1975] (pp.189, 201: Ptolemy’s “method was to [improve] existing theory . . . to get good agreement with observed facts.”) and will compare their force with that of Newton’s papers (R.Newton 1973-4; Observatory 96:1666) & new book (R.Newton [1976] p.41 & Chaps.5 & 11).” But OG&co disapproved. Zero space was granted R.Newton’s side. Fifteen years of similarly admirable manipulations have earned Hist.sci archons the degree of respect DIO & co are showing them. I realize that: telling the truth in the corrupt milieu will gain one little but enemies, who will of course blame the truth-teller for all friction. Another truth, which Hist.sci prefers to suppress: though at first frankly & pointedly critical of Muffa treatment of R.Newton, DR (recognizing some merit in Muffa output) long hoped for an amicable outcome of this controversy — and a pooling of all parties’ respective talents, to assist a better understanding of ancient science. (See, e.g., DIO 1.1 § fn 7.) The former aim has been reached by irrevocable mistrust. [Spectator 1713/3/27]: “There is nothing that more betrays a base ungenerous Spirit, than the giving of secret Stabs to a Man’s Reputation.” See O.Gingerich’s schiz private libels of DR as DIO 1.1.11 fn 20 — circulated behind-the-back even, in his direct dealings with DR. OG was pretending to genial neutrality. See also DIO 2.1 § fn 33.) But that has prevented DR’s unilateral pursuit of the 2nd ideal. DR’s inept critics’ determination to eliminate such a bad-for-business whistleblower (in 268) is palpably stronger than their scholarly ability to accomplish that aim. (Which is precisely why Muffosi braves flee face-to-face dealings, leaving instead on the crutch of whispered libels.) They want me bad? Well, they’ve got me bad.

DIO is, among other things, an ongoing experiment: attempting to discover whether there is any limit whatsoever to the ethical, comedic, censorial, appropriational, and gangster extremes which will find acceptance by the Neugebauer Muffa — and silent assent to, or vocal promotion by, the larger Hist.sci community’s archon-angels.

The search continues.
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1991 December Journal for Hysterical Astronomy 1.2 §9

Such was hardly the 1st alert issued. DR regards such warnings merely as well-intended advice. Hist.sci archons see them as: Backtalk-Sass — a far more serious offense than mere plagiarism, fraud, & suppression. Lord Hoskin, Editor-for-Life (EfL) of the Journal for the History of Astronomy, has (with mysterious imprecision about the details) told inquirers for years that DR is “impossible to deal with”. The following excerpts from our correspondence will indicate how typically honest Lord H’s slander is.

B2 DR to Lord H, 1983/3/14, assertively responding to the EfL’s 1983/3/3 threats of legal action & future JHA nonpublication of DR work (EfL’s clumsy reaction to DR’s merely pointing out that the central results of a recent pseudo-referenced JHA paper were entirely founded upon mismathematics):

. . . What most disappoints me about your [1983/3/3] reply is that it indicates you’ve learned nothing from this [editorial] disaster. . . . I have pleaded with you privately for years to improve your process of evaluating and filtering incoming mss (specifically, to replace an obsession with superficialities, which has ensconced itself at several high-prestige institutions, including even a few Ivy League universities. This generously-budgeted, extravagantly produced perpetual-opéra-bouffe stars a gaggle of outwardly arrogant, privately insecure businessmen-professors. The costuming is lavish: bumbling, idée-fixe zanies, and intuitional mystics are painstakingly dressed up as centrists, technically-expert authorities. Admittedly, the story line is oftimes improbable; but, it never fails to entertain, with its ever-cornucopic menu of inspirational behavior: unscrupulous promotion of ludicrous cult theories over competitors’ patent-better-fitting solutions — garnished with [a] cohesive slander and-[or] noncitation-freezeouts of Unapproved scholars, [b] convenient switching (or conjuring-up) criteria in midstream, [c] ritual apologia or coverup for data fabrication, fudging, and plagiarism (including the cult’s own). [d] gang-up bullying and blanket denial of all discovery-credit to those who publicly dissent from orthodoxy’s sacred-unfalsifiable prescribed historical vision.

It should be clearly understood that none of the above-cited behavior is wrong or deserving of the slightest censure. Indeed, leading History-of-science (Hist.sci) specialists stand ever unsleepingly at-the-ready to reprove reproof — alleging that ethical disapproval of academic hoaxery reveals nought but the commentator’s own amateurishly “moralising” perspective.” In these gentlemen’s learned opinion: faking, fudging, and plagiarism can be excused as trifling or even defended as reflecting a “progressive” or Nobelist intellect! 6 Condemning a Hall-of-Fame scientist for fraudulent scholarship is an inexcusable, even mortal sin in the Hist.sci profession.

Thus, our perpetually-Koperetta’s castmember-careerists rightly continue to be highly honored. Their ethics, prejudices, shunnings, and especially their vasty amusing attempts at math, science, and logic (which are generously contributing an unending supply of cackle fodder to our Journal for Hysterical Astronomy [JHA]) have for 2 decades been woefully underappreciated — protected in their handsome captive journals from the slightest public larfing.

6 On 1980/3/2, JHA-loathe’d DR simultaneously sent 3 papers to the JHA for publication; however, these were of course submitted for prompt refereeing, not for immediate printing. (All 3 have since been published by far superior professional journals.) But the Editor-for-Life rightly feared that he couldn’t positively count on the papers (especially all 3) being rejected by referees; thus, the EfL got panicked, while thrashing about for style-nipick excuses (to explain why we couldn’t ‘possibly apply the same option’ to the 3 papers), and so carelessly blurted out whatever alibi-concoction came swiftly to mind. The result of these combined elements was an invaluable miracle-of-chemistry creation: a Lord H statement that is almost honest. His Lordship’s 1980/4/11 letter to DR claimed that, for the JHA, “any thing considered for publication must be, prima facie, be in a state ready-for-the-printer copy upon rst submission may give the impression that they are more publishers than editors.”

8 On another point, this same DR letter notes: “I asked in both my letters [1980/3/22 & 4/18] whether [if we get that far] I will see referee reports; and you in both your replies [1980/4/11 & 4/28] did not answer this question.” (DR was much enjoy your personal company. (And I feel the same about Owen [Gingerich], despite our differences.) But I cannot refrain from telling you as bluntly as I can that this is no way to run a journal. . . . I suspect that the root problem is just the pressure of time. . . . Regardless, the upshot is — in effect — a fixation on the superficial & the swiftly-gauged, as against the substantial. For obvious reasons, not very many scholars are going to say these things straight at you. But they need to be said. And I think it does you more good to say them than to keep silent. . . . Best wishes to you and David [Dewhirst] & Simon [Mitton].”
In fairness to the refs: both told Lord H the erring paper’s conclusion was “incredible”; but His Lordship was so anxious to replace a pending DR paper with this one that the EFL overruled the JHA’s own referees! Lord H’s 1983/321 reply severely communicated, thereby killing the already refereed, accepted, & advertised9 paper Rawlins 1999,10 then in the editing process. A statement in Rawlins 1999 was regarded as intolerable,11 since it did not meet with the approval of the omniscient “Mufa”, Ptolemy’s modern protectors & showbiz9 agents (introduced to our readers in DIO 1.1 §5 [C5–C13 etc.]). Anyone with a sense of humor will enjoy comparing the banned DR sentence’s temperate treatment

9 See Isis 73:158 (1982/3). (The original title of the paper was: “Aristarchos’ Tropical & Sidereal Years & His pre-Hipparchos Knowledge of Precession”. But the JHA pushed DR toward its own preferred title, “The Babylonian Ancestry of Ptolemy’s Year”. Obsessive. Like fn 15.) And see DIO 1.1 §1 fn 25. The main reason the JHA initially wanted to suppress: it contained what was then my sole anti-R.Newton finding (which vindicated a criticism of RN made by Moesgaard, Swerdlow, & van der Waerden). The privilege of being published in the JHA was seen as an effective attraction, to start prying DR (R.Newton’s most forceful supporter) away from RN’s heretical view of Ptolemy and thus to isolate RN (a neat plan, lately applied to DR by the same volk). (This technique usually achieves its object in no time; its failure in this instance must have been an awful shock — so great, indeed, that failure was ascribed to insanity. DIO 2.1 §13 [C9]). But JHA then still further refined its censorial filtration by cutely waiting until the very last pre-publication minute to suddenly insist upon deleting even the sole small portion of the paper that backed Ptolemy’s thesis. DR would not assent to this ploy; thus, Rawlins 1999 remains in acceptance-limbo: publicly accepted, but not withdrawn, and not published. (The paper is crucial to the tracing of eq. 6 here to Aristarchos: fn 81, §08.) Perhaps the reason Lord H threatened DR with legal action is that the JHA was well aware that it could itself be sued for its breach of publication agreement. When, on 1983/6/6, the JHA’s O.Gingerich re-dangled before DR the lure of attaining the galactic “prestige” (§81) of being published in the awesome JHA (DIO 1.1 §1 fn 11), he was still unsuitedly hoping to woo DR away from sympathy for R.Newton’s troublesome heresy. (Of course, if the JHA continues in its habits, it will have little prestige left, to bestow upon anyone.) On 1946/2/8, in the unexpected presence of myself & my wife, I tried the same publication-conference-offer ploy with van der Waerden (additionally tossing in travel expenses). No wonder the Mufa doesn’t want 2-sided public discourse. Why risk a clash of ideas, so long as there’s hope that the Wrong Side can be subdued by more traditional & reliable means?

10 Rawlins 1999 showed (DIO 1.1 §16 fn 1) that Hipparchos’ lunar period (MA; eq. 6 here) was from predecessors. This paper now finds that 2 other lunar elements (e & g; eq. 8 & 9) are also from predecessors. I.e., of Hipparchos’ 4 lunar elements, only e (or r) is original: eq. 19 (or eq. 20).

11 Describing it as “Forbidden fruit”, I read it aloud anyway, at a small Hist.sci Symposium, 1983/6/4 at Univ Aarhus. As a result, Lord H’s admirer O.Pedersen (U.Aarhus, Editor Centaurus) was so furious, that one observer told me he’d never seen him that angry. The responsive tactic was standard: all audience members that mattered were herded into a nearby room, believed to be just out of DR’s earshot, and then told by O.Gingerich that DR’s description of JHA as censorial was untrue, etc. Scholars attendant at similar archival confabs, who lack the intelligence to question why such discussions (and why, e.g., all archival outrage at DIO 5 so far) must be held under behind-the-back circumstances where 2-sided crossexamination is not possible, fully deserve the degree of enlightenment they will uncritically absorb.

12 Ptolemy’s superboy-billing (in, e.g., AAA’s Science, QJ Royal Astr Soc, & Springer-Verlag: Gingerich 1976 & §16) as “The Greatest Astronomer of Antiquity” is worthy of Greatest-Show-on-Earth P.T.Barnum — as is the fraudulent subject of such puffery. But the kilobucks&kilobooks-gross may seem worth it: even aside from some ordmag $100,000 Ivy League salaries, Ptolemy’s promoters are raking in handsome royalties on ordmag $100 books which sell ordmag 1000 copies worldwide, since they are dutifully & lovingly review-advertised (even previewed similarly: fn 239) by loyal members of the same incestuous fraternity, in such seemingly reliable forums as Nature — who evidently lack the initiative to occasionally go outside the Mufa’s p.t. team when choosing reviewers of ancient astronomy material.
was a geocentracist (& astrologer). It is DR’s contention (a point so self-evident that it would hardly even be controversial, except in a Hist.sc community that has become obsessed with “whiggism” & “paradigms”) that: there is a correlation between [i] the sort of intelligence required to discern that heliocentrism is preferable to geocentrism, and [ii] that which is required for contributing new insights to a science. (Likewise, I suggest that, in modern academe, one might detect a correlation between [a] competent, talented scholarship and [b] insistence on honesty, incorruptibility, & rejection of unethical compromise with a gang — or ring — of Nibelungs.)

S2 Hipparchos’ historical roles as propagandist, preserver, & calendarist will of course always be remembered (with varying degrees of gratitude). But it is as a dedicated observer that he will shine forever bright in the history of astronomy. The quality of Hipparchos’ massive (astrolabe-based) Ancient Star Catalog (1025 objects) — which Pliny 2.95 rightly calls a bold inheritance left to all mankind — was not improved upon until 17 centuries later (Tycho 1598: see DIO 2.1 §4). Hipparchos’ most admirable trait, which Mufiosis regards as showing his mental inferiority to Ptolemy (see [Mufia 1990] pp.204f), will ensure his rank among history’s premier early scientists: he refused to fake observations to accord with theory: §N15 & §R14.

S3 We are by now well beyond the firm inductions that culminated in §N-§O, and are swimming in fun but obviously speculative areas. So, rather regrettably, I leave off analysis at this point. But, it’s been grand. And my thanks again to the Mufia for handing me the Almajest 4.11 problem, which I’d foolishly skimmed past for so long.

References


291 The title of Wagner’s immortal cycle obviously has at least 2 meanings. Have we here stumbled upon a 3rd?

292 My gratitude is but the merest trifle, beside that which Hist.sc archons will (very privately) feel towards Lord Hoskin for directly causing DR to become the discoverer of the solutions to the Hipparchos math (§N & §O) of Almajest 4.11 and (fn 288) Almajest 3.1. I will only comment that: if one asents to the use of missmen as hitmen, the responsibility for disaster is one’s own, and no one else’s.

of the Muffia, vs. the extensive published abuse heaped upon the John Hopkins Univ Applied Physics Lab’s late Space Sciences Division Supervisor, R.Newton (Ptolemy skeptic) by JHA & its various Editors such as O.Gingerich (Muffia satellite) & N.C.Swerdlow (Muffioso): see sampling at DIO 1.1 §C7. E.g., the editorial gang at Lord H’s usually efete-British Journal for the History of Astronomy has publicly branded RN’s work “intelligence-insulting” & “garbage.”13 By contrast, Lord H suppressed Rawlins 1999’s brief, mild appraisal. Though the paper had been accepted because it contained certain important DR discoveries,14 it remains unpublished. [Later appeared at DIO 9.1 §3.] (The Editor-for-Life’s ban made an incalculable contribution to peace in the ancient astronomy history field, since it led straight to the starting of DIO.) The one-sentence statement of Rawlins 1999 which was anathema15 to the JHA (& its referee K.Moesgaard):

Newton’s conclusion [that Ptolemy deceived]16 has been attacked with such passionate disbelief in a variety of journals . . . that many onlookers may not be aware that a number of scholars agree that Ptolemy has indeed been shown to have been a liar.

DR’s main aim was to tell the hitherto protected JHA readership that there was a live scholarly controversy over Ptolemy’s integrity. JHA’s aim was to suppress that truth long enough to make it obsolete. Without DIO, this neatly circular plan would certainly have succeeded. Which tells us worlds about the honesty & worth of the Hist.sc community.

B3 Lord H was delighted to find a pretext for (keeping his readers just as uninformed as I’d noted, by) not publishing the above simple factual sentence (§B2); so his 1983/3/21 response to my letter was death to the paper & exile for DR.

I think we shall both benefit if we agree to refrain from writing to each other, both now and for the indefinite future.

13 Sources: HillSwedrow 1981 p.62 (JHA, published almost simultaneously with suppression of DR’s intolerable-statement! & Swedrow 1979 p.330 [American Scholar, whose Editorial Board was blessed at the time by the presence of the JHA’s O.Gingerich]. Swedrow is now on the JHA Board.

14 Some of the central material of Rawlins 1999 was cited by the paper’s JHA referee (Moesgaard 1983 p.57).

15 One notes that, a few years prior to the incident noted here, Mufiosis Asger Aabo & Bernard Goldstein both disappeared from the JHA’s windowdressing Board of Advisory Editors. They were, no doubt, merely objecting to the JHA’s stationery’s mish­spelling of “Asger” as “Asgar”. It was purely coincidental that, around this time, the DR in 1982, Toomer left the JHA board. These coincidences led to a decision that is not coincident: DIO decided from the outset to forego the usual formality of listing an “Editorial Board”. Such boards may look good, but they [a] add nothing to handsome journals’ actual quality (see O.Gingerich 1990 JHA 21 & fn 13). Indeed, the DR’s main aim was to tell the hitherto protected JHA readership that there was a live scholarly controversy over Ptolemy’s integrity. JHA’s aim was to suppress that truth long enough to make it obsolete. Without DIO, this neatly circular plan would certainly have succeeded. Which tells us worlds about the honesty & worth of the Hist.sc community.

16 Virtually every scholar on both sides of the Ptolemy Controversy now agrees that Ptolemy deceived — even the leading Neugebauer-Mufia capos (Toomer 1984 p.672, Swedrow 1989 p.54). But the Muffia denies this is lying. Which is a semantic play that we lost this controversy to the hated R.Newton & DR (who said right along that Ptolemy deceived) — but haven’t the integrity to admit it. See §H2 (options [e]&[f]). Indeed, the slimness of a certain volk partially accounts for the detail required in this paper when pointing out these bad­losers’ tantrums. (What priorities could lead a publisher to invite that sort of tradeoff?) The JHA learned its lesson and so went to serpentine legal extremes to avoid repeating the mistake of publishing RN: see DIO 2.1 §3 §B. Happy ending: the JHA Board now includes enough Muffiosi to ensure that the JHA will never do anything stupid again . . .
As regards benefits, His Lordship was half right. But, just exactly how much the Editor-for-Life of the Journal for the History of Astronomy has benefitted (by self-imposed insulation from DR’s helpful advice, e.g., [§B2], the reader may judge from what follows here and in future issues of the Journal for Hysterical Astronomy (http://www.dioi.org)); also DIO 1.1. And: a reminder. While reading the J.HA, understand that our appreciations here are of no ordinary genii & ethical paragons. These comedians pose as the cream of academe: professors at Harvard, BrownU, Yale, Cambridge Univ, promoted by Phl Beta Kappa & the MacArthur Foundation, highly admired at the Princeton Institute intellectual retirement home (which has somehow become disproportionately blessed with Hist.sci archons). (One thing O.Gingerich & DR can agree on & fervently pray for: 0 positively belongs at the Princeton Institute. DIO hereby nominates OG for permanent Fellowship there.) All to the good. If one is going to butcher math, science, logic, & free speech: let these deeds be staged where we can enjoy some basso echoes. And let those echoes ring down the history of Hist.sci: enshrining the Ptolemy Controversy as a classic case study of a community gone wrong, as convincingly demonstrated by Hist.sci’s persistent 22nd-long failure to handle a prominent conflict central to its own field. (The unsuitable techniques, used by archons to fix this fight from the outset, will be apparent from §§13 and DIO 2.1 §3 §B. The price for DR’s offered publication in the JHA was: going along with this fix. This he refused to do: fn 9.) If the Hist.sci community can’t perform a function so basic to its reason for existence, then: why do universities have Hist.sci departments? (DR’s increasingly asking this question for the last 15² has not exactly endeared him to the field’s archons, whose typically bright reaction has now resulted in the question’s wide circulation here.) An analogy would be the early 20th-century world of physics, unable²³ to arrange a fair encounter between advocates & doubters, in the disputes over quantum mechanics and relativity. A community so disabled has made itself the farce DIO honors it as.

B4 An especially cute feature of the most pompous Hist.sci journals is their elaborate pretense that they have “Editors”. (Those familiar with the reality are all too aware that Editors’ prominence depends more on socializing than editing.) It is easy to spot Hist.sci neophytes by their amusing innocence on this point. As an example of the sort of slip that gives away so many 1st year grad students’ youth: many — even those with incipient doubts about the Easter Bunny — actually suppose that being an influential “Editor” requires that one read the material one publishes. This curiously widespread myth already came up (relative to JHA) in DIO 1.1’s Journal for Hysterical Astronomy (§3 §G7). Yet another example, at Hist.sci journaldom’s Reputability-pinnacle: shouting Hi-There! from the extremely handsome pages of the 1991/§ issue of Cantab Lord Hoskin’s Journal for the History of Astronomy (which costs institutions merely $126/year [note added 1993: now $140/year]), we find sober discussion of Hipparchos’ alleged use of the 146 BC “date of the WINTER equinox”.²¹ I haven’t had the pleasure of encountering such calendrical creativity since the Muffin’s klan prince, Gerald Toomer, placed into the eminent Dictionary of

17 DR’s unwisdom may be gauged from his vain 1983/4/8 response: “. . . if I were refereeing your 3/21 letter, I would just restrict myself to saying: transparent and maishocratic . . . . You’re not a bad person. Why act like one? Best wishes, in spite of all —”.  
18 For J.HA’s putative brains at work, see DIO 2.1 §3 §B8.  
19 The same question is implicitly re-emphasized every time DR achieves a major historical result, since he’s a living proof of a ghastly truth, namely, that one doesn’t need Hist.sci training to contribute to scientific history. DR is a self-described amateur (see his self-composed bio in the 1982/6 p.329), who confidently renounced going the standard Hist.sci grad-school route, and who breaks virtually all the Hist.sci rules (e.g., [§C2, §G1], especially the ones requiring: [1] socratic writing, [2] innocence of the mathematical sciences, [3] encrusting papers with layers of superfluous archon-kissing citations (fn 179), & [4] careerist-lawyering for old-guard power-operators’ pet views instead of seeking new truths in unapproved directions.) For a similar situation, see D.Rawlins Peary . . . Fiction 1973 p.291 item #2. And see, at DIO 2.1 §3 fn 8, Lord Hoskin’s magnificently inventive scheme for killing off the airing of controversial observations. By comparison, Isis’ approach (exhibited here at fn 121) is childishly clumsy. The blue ribbon for this category unquestionably goes to Lord H.  
20 Jones 1991H p.119. (Caps added. With dentistic pride in assisting the creation of a surer, brighter smile.)
The sine of a slim angle is virtually equal to that angle in radians, so we use eq. 49 to rewrite eq. 48. Now, 207.29/60 = 6030/60 = 100.05 ≈ 100; so we have:

$$\theta_1 = 180^\circ / (207^\circ) = (360^\circ / 13) \cdot 60 / (207 \cdot 29) = 360^\circ / 1300$$

(50)

which accounts perfectly for Hipparchos’ otherwise inexplicable (& relatively inaccurate: $§7$) value, $\theta = 360^\circ / 1300$ (eq. 37). (Note that the odd factor 13 in eq. 37 is now explained as merely a byproduct$^{285}$ of the proper use of a sexagesimal expression for $\pi$; eq. 49.)

**R13**

Assuming that Hipparchos did indeed adopt Swerdlov’s proposed $R_0 = 490^6$, we are left with the discordant Hipparchan value, $R_0 = 10^6 / 16$ (Kleomedes: our eq. 42). Toomer 1974D has shown (though see fn 277) that if we also use $R_M = 1^3$, and thus (from eq. 26) get $\gamma = 2/61$, and then use these data along with eqs. 36 & eq. 37 in eq. 27, we have $r_M = 61^3$, which is close to the truth (60.27). (For the Theon-based $R_2 = 12^3/13$ of fn 277, Toomer similarly finds about the same $r_M$.) It would be nice to suppose that this was Hipparchos’ final value, but [a] we don’t know so, and [b] it seems unlikely that he would have been back so soon from low solar parallax to high ones of late. Since we will have to look toward the proper direction of the Sun’s parallax soon, the proper use of a sexagesimal expression for $\pi$ is hereby deprecated.

**R14**

A conjecture: Hipparchos finally settled on his 7° solar parallax — because it made the Sun the same as small as scholars could possibly accept. Obvious oculai evidence was against it (as noted at DIO 1.1 [5 in 7 item [a]], since it entailed (from eqs. 26, 35, & 38) half-Moons at elongation c.82° ($\gamma = 8^\circ$ Sunward of quadrature) — while in fact (as I know from repeated outdoor experiment) the Moon is definitely crescent-looking at this elongation (or even for $\gamma = 5^\circ$). However, perhaps desirous ($^{285}$) (assisted by aging eyesight) pushed ever-upward Hipparchos’ estimate of $\gamma$: as a geocentrist, it is surprising that even Ptolemy (at his career’s end) didn’t note, that the Moon’s angular diameter to vary by nearly a factor of two! — which anyone who looked at the real outdoor sky (a class obviously not including Ptolemy) knew did not happen. In this connection, with respect to the Ptolemy Controversy: for a quick & simple measure of which side knows what’s talking about, see the hilariously innocent remarks of Mufa-Sodogba Neugebauer 1957 p.196 (even while, only 10 pp later, he savagely Dahmen for promoting “flagrant nonsense” about Hipparchos’ solar theory!) vs. the truth, at R.Newton 1977 pp.182 — which shows that [a] Ptolemy not only believed his theory’s absurd quadrature-distance (a fact haughtily denied by Neugebauer loc cit), but [b] Ptolemy even faked a grossly (fn 288) erroneous “observation” in perfect agreement with $\ldots$

283 The best compact expression for $\pi$ is 355/113. Adding components of the familiar fraction 22/7 to this produces eq. 49, which is the most accurate brief sexagesimal expression for $\pi$. I see that the first 4 entries in Ptolemy’s trig table (Almagest 1.11) are precisely based upon $\pi = 377/120$. (But the first 3 entries are simply correct, so this is not a unique explanation.) Thus, the appearance of the factor 13 in Hipparchos’ lunar diameter expression (eq. 37) appears to be a precise thread, leading us to the realization that accurate $\pi$ and accurate trig tables existed already in the 2nd century BC. (However, note that if 227 is multiplied by 120, the rounded result is 377. Did ancient sexagesimal rounding accidentally produce the best ancient $\pi$?)

284 We have here noted, perhaps for the first time in explicit terms, something ignored by all textbooks: Aristarchos’ Experiment (the half-Moon argument) is pro-geocentrist. It placed a damper on near (and thus small) geocentrist prejudice could make the Sun — without requiring ludicrous visible effects. It is remarkable that Ptolemy’s crank mechanism (by bringing the Moon hugely closer to Earth near half-Moons) considerably eases the above-mentioned problem with visibly-silly $\gamma$. Which suggests the selection that geocentrism could have been part of the pre-Ptolemy impetus that produced Ptolemy’s well-named crank theory of the Moon. If so, the gain was hardly worth the trouble: while (unknown to Ptolemy) implicitly defating the solar volume, this theory simultaneously required the Moon’s angular diameter to vary by nearly a factor of two! — which anyone who looked at the real outdoor sky (a class obviously not including Ptolemy) knew did not happen. In this connection, with respect to the Ptolemy Controversy: for a quick & simple measure of which side knows what’s talking about, see the hilariously innocent remarks of Mufa-Sodogba Neugebauer 1957 p.196 (even while, only 10 pp later, he savages Dahmen for promoting “flagrant nonsense” about Hipparchos’ solar theory!) vs. the truth, at R.Newton 1977 pp.182 — which shows that [a] Ptolemy not only believed his theory’s absurd quadrature-distance (a fact haughtily denied by Neugebauer loc cit), but [b] Ptolemy even faked a grossly (fn 288) erroneous “observation” in perfect agreement with $\ldots$

285 If Hipparchos’ only extant quadrature observation (Almagest 5.3) was for checking Aristarchos’ Experiment, he got $\gamma = 3^\circ 34^\prime$ (not 8°). Against this suggestion is Hipparchos’ arrangement for null longitudinal parallax: that is irrelevant for measuring $\gamma$, while it is useful for checking the lunar longitudinal position, which is what Almagest 5.3 suggests was its purpose.

286 The DSB’s high quality may be gauged from its apparent nonbesmirchment by the name of R.Newton. DSB’s 1978 near-bucksliding (e.g., 1978/7/6 promise to DR that R.Newton’s work would be cited in vol.16: “We will do that”) evidently was reconsidered. Whew.

287 To borrow the unequivocally polite language of no less a correctness & competence-authority than The Mahattan I: see Toomer 1974D n.13 on R.Newton.

288 Theon of Alexandria horoscope 360/5/19 (Neugebauer 1975 p.966 n.16). Toomer dates it to 360/6/15 (a ~96 day error) due to his confusion of Alexandrian Thoth 22 with Egyptian Thoth 22. (The two calendars had been diverging by 1 day/4 yrs for nearly 4 centuries — thanks to Little Augie Caesar’s 30 BC “modernization” of the simple old Egyptian 365d calendar, incorporating the 365d/4 yearlength of Big Julie’s now-famous calendar.) The DSB was informed of this error 1978/5/18, I do not believe correction has ever been made in the more than 13 years that have passed since. (For DR’s helpful hint on how to spare other unmathematical minds similar embarrassment & strain, see DIO 2.1 [4 in 5] 1977.) From DSB 16:504, 508-510, we learn that Toomer’s articles on Ptolemy, Theon of Alexandria, & Hipparchos make no correction at all.

289 Admittedly, my optimism wasn’t very warm. Rehab is largely a chimera — as Hist.sci’s Jessontown folly has proved all too clearly. My initial attitude toward Mufa was, though critical, more optimistic and volunteering; but years of Mufa arrogance have effected a complete cure from such uneasiness. As recounted in Rawlins 1991H (fn 35), DR in 1986 wrote Mufa satellite N.Hamilton, a voluntary acknowledgement that a Mufa interpretation was superior to one of DR’s. A mob that can’t even reply to that, leaves no room for doubt regarding its character & priorities.

290 See fn 266.

291 E.g., Rawlins 1982G & DR to Isis 1980/10/16 item #6. And see inside front cover of each Archive Hist Exact Sci issue: AHES “nourishes historical research meeting the standards of the mathematical sciences.”
fertility and §B1 “impossible” behavior (i.e., nonincorporability into Hist.sci’s burnout-mill) will simply produce increasingly repulsive Hist.sci institutional evasions & pusillanimity, which will be fully reported in the J.H.A.

C3 I have unintentionally lived by F.Nansen’s epitaph: “What would life be worth without its dreams?” The Muffia has bestowed an extra source of personal uplift, for, after all, what would life be worth without its jokes?28

C4 A parenthetical anticipation of criticism: those scholars, who may be offended by the J.H.A.’s frivolous style, are urged to consider the subjects. Stiffing gagglers can be painful, e.g., for one observing a pack of arm-flapping, lordly-snob inebriates trying to fly — when they can’t even stand. And does one (can one?) maintain a serious face in the middle of a piethrowing contest?29

D1.3: We now speculate that, at a later date, an Aristarchos follower revised eq. 45 by deciding that the angular length of the tiny midperpendicular of §R9 would be the amount to be visually detected, using the original 0.4 visual-limit criterion. Eq. 45 thus becomes:

$$\sin \gamma_T = 0.4$$

— and, from that result, a heliocentrist member of the Aristarchos school could derive the Sun/Moon distance ratio in terms of the milli-AU (discovered above at §O2) substituting eqs. 22 & 46 into eq. 26, which gives:

$$r_M = r_5 \cdot \sin \gamma_T = 1000^a \cdot 2/75 = 27^a \approx r_S/37$$

The Moon’s distance $r_M$ is thus deduced as $27^a$ — or 1/37 of $r_S$. That completes the tentative tracing of the ratio adopted previously at eq. 39, on our way to finding the attested value $r_M = 671/3$ (eq. 41) — which we now return to, for the final member of our (speculative) reconstructions here.

R12 We have already seen (e.g., §N7) that Hipparchos was capable of attempting (mathematically unjustifiable) piecemeal improvement of his orbital elements. This suggests the next step in the evolution of his lunisolar theory: adopting a solar parallax of 7° (Swerdlow’s discovery),300 he changed his $r_S$ to the value of eq. 38, but did not wish (perhaps because301 his tables had been reargued) to change either his $r_M$ (eq. 41 or eq. 44) or $\theta_1$. This is the (hypothetical) sequence that has not been previously suspected. Merging eqs. 25, 36, & 38:

$$\sin \theta_1 = (1/6720 + 1/4900)/\{1 + 2 1/2 \} \approx 1/207^a$$

We now evaluate $\pi$ in ancient coinage (with $1^p \equiv 1/60$, as in §G10), using the following simple expression (which is accurate to better than 1 part in 40,000):

$$\pi = \frac{377^p}{2}$$

or

$$\pi = 13 \cdot 29/120$$

28 But, please, go lighter on my ribs, fellas. I’ve got stacks of educational past Hist.sci hiliarities yet to publish in J.H.A. (As they almost say at the auto-races: “Gentlemen, start your retronchocks.”) Enough that I’ve even made legal arrangements for posthumous serial publication. But you keep so inundating me with new tomfoolery that I can’t find time to get the vintage stuff into print. Ease up.

29 See DIO 2.1 §[H16]–[H17]. The wellfed Muffia is akin to a pristine-clean Ollie Hardy, who — though having virtually subsumed Stan Laurel in still-dripping meringue pie (fn 31, fn 269, etc.) — exudes lordly confidence that Laurel can’t or won’t fire back. The stunning smugness itself (preferably a brandnew tuxedo) is what elevates such cinema episodes to the pinnacle of good slapstick. But not even Laurel & Hardy were able to refine comedy to the point where Hardy could: [a] wipe the pastry & meringue off his Macbethian hands (fn 90, b) turn to watch piecased Laurel go into his windup, and then [c] prissily admonish Laurel that piethrowing is (§J7) “disreputable”.

30 In my absence I have observed the behavior §J7 is observing more akin to what the parapsychological looks more akin to as “psi-missing” — or a mass version of what their Freudian spiritual brethren call the suicidal “death wish”. (And I don’t even believe in those fads!) It’s systematic. And has been so for decades. In reaction to Hist.sci’s current lounges incident, no Hist.sci institution will effect any changes beyond the cosmetic. If that. (Numinous academic societies are not in the least upset by trivia such as unethical behavior. They reserve disapproval for the truly grave offense of: public exposure of unethical behavior. The reporter, not the perpetrator, is correctly identified as the culprit. Similarly, the much-lamented “unhistoricity” of R.Newton’s & DR’s reports is what really enrages Mullisio, but Pol Emmy’s fakes and thefts don’t cause them an eyebat [fn 96, §J1] — as they wish skeptical ogre DR would just emulate their own unexceptionable sense of priority.) Thus, the only possible educational benefit of this paper’s critiques will be: further encouragement for the wider scholarly community to give appropriate credence to the effusions of an academic cult whose institutions have, through decades of effort, refined & isolated such perfect rectitude that praise of it never ceases in these pages (e.g., DIO 1.1 §[G4]).

300 Despite my dismay (DIO 1.1 §[F7] fn 7) at some of Swerdlow’s procedures (when he tried to overforce desired agreement), I have long been sympathetic to Swerdlow 1969 (even though its implicit γ is remarkably outsized — a problem I attempt to overcome here at R14). In a phone chat of 1981/4/5 (DIO 1.1 §[F7]), I told Swerdlow that both the sign & size of the mean errors in Hipparchos’ equations (errors which degraded his PHI theory’s equation of center, making it worse than Kallippos’) were pretty consistent with Hipparchos’ having reduced his observations using a 7° solar parallax correction. Swerdlow said he already knew about that. [See DR to Cenauras 1977/39 p.B11.]

301 If different computers at his school were involved at different stages of his alterations of his lunar model, then coherence will be hard to come by. Which is one more reason why I regard most of the theories discussed in this section as speculative — except for Swerdlow 1969, which deserves better ranking than that, by the coherence criterion. See fn 275.

302 I see that Hartner accused Polomy of very similar inverse procedure at Almagest 5.14: Van Helden 1985 p.19.
by Swerdlow 1969. From eqs. 40 & 26, one finds:

$$r_M = r_S \cdot \sin \gamma = 2490^\circ \cdot \sin 1^\circ 33' = 67^\circ 21' = 67^\circ 1/3$$  \hspace{1cm} (41)

By contrast, the development of Swerdlow 1969 p.298 (without some forced procedures: fn 274) produces not 67^\circ 1/3 but 67^\circ 1/6 or 67^\circ 1/5 (as acknowledged by Van Helden 1985 p.13). This discrepancy led to the nimble Capt. Captious manipulations which are displayed & admired at DIO 1.1 \& fn 7.

R8 An alternate route to 67°1/3: starting with the attested Hipparchos solar volume 1050 Eaths (Klessmedes 83), we take the cube root and have

$$R_S = 10^\circ 1/6$$  \hspace{1cm} (42)

Thus, applying eq. 30,

$$r_S = R_S / \sin \theta = 2330^\circ$$  \hspace{1cm} (43)

Now, bringing in eq. 25 \& eq. 36, we find

$$r_M = 67^\circ 22'$$  \hspace{1cm} (44)

which ancients would most likely round to 67\°1/3, the value (eq. 35) attested by Pappos. (Of course, it is possible that eq. 42 was not the source of eq. 35. Perhaps eq. 35 led to eq. 42.)

R9 We now pause to ask: whence came the crucial (eq. 39) factor, 37? (It presumably appeared before the development set forth here in §R7. Keep in mind that we really don’t have to justify the finding that eq. \(\gamma = 37\), since it is effectively attested by Theron.) A guess at its origin: Aristarchos’ Experiment depends critically upon the observer (of the half-Moon’s occurrence) detecting lunar terminator-curvevature. 278 Now, the mean distance between the human eye’s foveal cones is (Rawlins 1982G p.263) no less than 0.4, and this is very near the actual limit of naked-eye discriminatory ability, which is about 1/10,000 rad (§R5). If the middle of the terminator deviates by twice as much (that is, 0.8) from the straight line (really: great circle) connecting the Moon’s horns, then no part of the terminator will be more than 0.4 from a straight line parallel to the hornline & bisecting the tiny midperpendicular connecting the hornline & the terminator’s middle. Understand: this minuscule angular distance is the crucial basis of Aristarchos’ Experiment; i.e., the 0.4 is all that visually distinguishes Aristarchos’ \(r_S \approx 19^\text{M}\) from \(r_S \approx \infty\): §R10. For, if \(r_S = \infty\), the lunar elongation complement \(\gamma\) corresponding 279 to the slight terminator-curvevature just described is (for Aristarchos’ value of the lunar semidiameter, eq. 30):

$$\gamma_A = \arcsin 0.8 = \arcsin \frac{1}{19} \approx 3^\circ$$  \hspace{1cm} (45)

— which agrees with eq. 28. This suggests a possible source for the 3° figure, founded on the speculation of DIO 1.1 (7 fn 6 — discussed here at fn 220) which questions whether the extant “Aristarchos” ms was directly written by him.

278 Presuming the original work of Aristarchos contained a discussion of this key point: the confused, self-contradicting, \& rather pointless pseudo-Aristarchos justification of Proposition 4 may be a “garbled” (to quote Neugebauer 1975 p.639) remnant thereof. The fact that this Aristarchos-influenced ms speaks here of the limit of human vision’s fineness suggests that eq. 45 is surprisingly impure speculation.

279 Likewise, if \(r_S / r_M \approx 19\) (the traditional Aristarchos-Ptolemy ratio), then the quadrature Moon will be slightly gibbous, with a terminator-curvevature bulge of 0.8 (beyond the straight line connecting the lunar horns).

Like teaching sponges to sing & dance. (Deepest apologies for that odious comparison, which I am swift to retract: sponges are scrupulous at filtering worthwhile nutritious matter from the mix submitted to them.) As will be demonstrated below, the JHA article is (even aside from its magnificent 366\° yearlength & Winter Equinox) Fourways Funny: [1] arithmetically miscomputed (corrections at §G9), [2] charmingly innocent of the elementary math & astronomy with which it purports to deal (§C11, §J2), [3] philosophically incoherent (§F4), & [4] founded upon the Mufia’s all-consuming conviction that indoor Babylonian astrologers secretly influenced high empirical Greek astronomy (§E4) — until the ultimo genius & “radical reformer” C.Ptolemy “ruthlessly expunged” (Jones 1991H p.122) all traces of this dependence (an obliteration as conveniently thorough as Collective Amnesia: §F1).

C7 Item [4] reminds me: it would be unjust not to isolate, highlight, and preserve the 2 most brilliant Muffia perceptions of ancient science. (As a museum might co-display a pair of equally too-perfect vases. Or crocks.)

[a] Babylevel Babylonian astrology underlay much of great Greek astronomy.

[b] Faker Ptolemy was the pinnacle of the latter.

31 Temperamentally, DR prefers jesting as lightly as possible (within the constraint that educational points are transmitted not over-obscurely). But, with certain academic archons, this is whispering to the deaf. If the above-cited already-published warnings (§C5, §F1) haven’t made a dent, then let’s have no打死ly opposition from the discussions here get a shave less than coy. I’m reminded of one of the great pre­modern comedians, Red Skelton, who, when his audience was slow to pick up on a joke, \[32\] would play­detail its meaning and then ritualistically shake his head: “Boy, when you gotta explain ‘em. . . .”

C9 Multiple warnings go unheeded. “Prestige” journals commit Jonestown Twice.

C10 Well, if nobody’s learning anything from semi-cute parody, it’s time to be a lot more direct (on occasion) about what’s happening. (DIO hopes to return to more oblique & soft humor in the future. But, anyway, between the dissections and novel scholarly discoveries laid out below, we’ll have plenty of comic relief from our favorite Muffia shmoons. If you know the cast, you know for sure: it won’t be dull.)

C11 The full, gory details are contained in the analyses that follow in the main body of the current paper (largely in J.H.A. 2.2). But I will attempt (to outline here, at the outset, Hist.sci. archondium’s sensational 1991 achievement (carried out, I repeat, in frontpage papers — and frontpage in the most prestigious & self-important Hist.sci.journals): Thus: [a] It is “proven” that the famous Greek astronomer Hipparchos (fl. c.130 BC) secretly used kindergarten-level Babylonian solar speeds — and temporarily adopted (just as secretly) a year three hundred sixty six D/S long days.

BRIEF INTERMISSION

[We pause here, while our dumbstruck astronomer-readers: re-hinge jaws & check funnybones for strain-fractures.]

31 See DIO 1.1 [1] §C7, fn 20, & §F3 §D3. Or here at fn 269 & fn 29, §J7, & fn 158. Not that one need be a heretic to get trashed by the Muffia. See, e.g., fn 211. Similarly: it is really necessary to refer to N.Halma’s uneven 1822-1825 edition of Ptolemy’s Handy Tables as “exceutable” (Toomer 1975 p.204)! Halma was the early pioneer in making Ptolemy available to modern scholars. Why is the Muffia so mercifully ready to excuse the fakir Ptolemy’s sins as due to the limitations of his primitive era [DIO 4.3 §15 [GB], but so mercilessly prone to criticize other, usually well-intentioned and honest scholars, who were also handicapped by crude means? The answer is: Mufosi have made a living selling Ptolemy and simultaneously selling their alleged ability to interpret him better than other scholars. (Some Mufosi are now gradually switching fringes, from Almajest to BabCycles: [S2 & fn 266].) So, the answer to the foregoing pseudo-paradox is simple: demeaning other commentators’ work helps generate Muffia income.

32 My 1973 book, Peary at the North Pole: Fact or Fiction?, noted (p.62) the remarkable case of the world-renowned Scott Polar Research Institute (Cambridge Univ) reviewing, as a serious article, Guy Potter’s (very) thinly veiled 1970 satire on Peary’s N.Pole hoax.
As above (§C7), I remind the reader that: the Mufa calls other scholars “crank” (fn 31) and “Velikovskian”. (See also §F1, §G3, fn 191, fn 192, §M7.)

[b] Trig-function orbit-fits are declared impossible for each of 3 solar-position data-trios, where (in all instances) it is immediately obvious that solutions must exist. All 3 solutions will be set out below.33 For all 3 cases, the central problem is simply finding 2 unknowns from 2 equations. Gee, didn’t we learn in high school that 2 is the number of equations required to find 2 unknowns? Or was it back in junior high? (The “parapsychology”-peddling magician’s easiest victim is the ESP-brained chap who arrogantly assumes that, if brilliant-he can’t explain an illusion according to mundane laws of nature, it must be impossible to do so.)

[c] By an irony which we might have supposed was impossible, one of these 3 orbit-fits — which the 1991/9 Isis prominently classified as non-existent — HAD ALREADY BEEN ACHIEVED & PUBLISHED, right in the very DIO issue34 which had just (§G7) been brought to scholars’ attention in Isis’ own sister publication! (This orbit’s elements are repeated below at §G10. The other two “impossible” solutions are as easily attained by anyone with the slightest facility in such matters: naturally, both are provided here, below, at §K9 & §M4.) It had also been published in 1990 by the American Astronomical Society (Bulletin AAS 22.4:1232). Does all this sound incredible? Well, shocks, let’s not be modest about Mufa talent — why, just the previous year, proto-Mufa 1990) had pulled off a similarly impervious feat. (We’ll honor that achievement below at §L4.)

[d] As usual, when skepticism on Ptolemy is mentioned, the accrued works of skeptics (especially DR) remain uncited. (Rawlin 1991H fn 6, describing 15° of the Ptolemy Controversy: “not a single inner member of [the Mufa] has ever35 cited any work by DR.” Even lower-level citations invariably acknowledge no contribution (e.g., §I5, fn 288). Historically, leading periodicals have kicked up to, honor, & prominently push such scholarship, while attempting to starve, ostracize, or low-rank those who provide correct mathematics and two-sided bibliographies (§II4). (Contrast R.Newton’s citation-integrity with Muffois’s: §E1. Examples of DR’s citation-policy are provided at fn 16 & fn 174.) What an inspiring model6 of academic behavior for young historians to look up to: cite the nonciters, and noncite the citers.7 The asymmetry’s as poetic as the justice isn’t.

33 §G10, §K9, §M4.
35 [Note added 1992: Mufa capo N.C.Swerdlow’s 1992/10 JHA paper at last cites a DR work. However, the essential Mufa trade tradition continues, as NCS of course concludes, that the paper has contributed nothing whatever to the field. The competency & integrity of NCS’s criticisms are displayed at DIO 2.1 13 [A.].] In case it is objected that the “tone” of the present paper makes it unpalatable, keep in mind that Muffois have for decades [a] used the vilest language against dissenters, and [b] have systematically sanctioned gentle DR papers and gentle scholars’ papers, if the findings are considered dangerous to Muffia hegemony or fundraising. (Having created no encouragement — or precedent — for respectful treatment, the Muffia has no ground for complaint in this regard.)

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Almajest 4.9 also says that Hipparchos used a curious mean-distance lunar semidiameter:

\[ \theta_1 = 360^\circ / 1300 = 18^\circ / 65 \]

(37)

Previous investigators (most recently Swerdlow 1969) have proposed ingenious theories relating eq. 35 to other assumed numbers (e.g., eqs. 36-38). Swerdlow 1969 makes a strong case that Hipparchos adopted (using 3438 = 1 radian) a solar parallax of 7', thus Hipparchos’ solar distance was:

\[ r_S = 3438' / 7' \approx 491' \approx 490' \]

(38)

But no prior scholar has yet been able to: [a] explain the origin of the weird expression in eq. 37 or [b] recover exactly 67°1/3 from normal43 computation. I will now hypothesize an explicitly speculative route which can explain how these parameters came into being.

R7 Whereas Aristarchos’ value for the lunar semidiameter \( \theta \) (eq. 30) is about two times more accurate than eq. 37 (Hipparchos), his alleged shadow-ratio estimate \( (\nu_A' \text{ eq. 29}) \) is crude — far less accurate than Hipparchos’ \( \nu_1 \) (eq. 36). So, though Almajest 4.9 mentions eqs. 36 & 37 together,7 the empirical impetus to revise \( \nu \) would be much greater than to mess with \( \theta \). Suppose, then, that Hipparchos at first adopted just eq. 36 (his best move, accuracy-wise, as just noted). A Hipparchan choice of \( \gamma \) which was more accurate than that of either Aristarchos or Archimedes (“Sandreckoner” p.223: \( \gamma = c.2^\circ \)) was first highlighted by F.Hultsch (Toomer 1974D p.140): Theon of Smyrna said Hipparchos had the Sun’s volume = 1880 Earths, while the Moon’s volume = 1/27 Earths. Refining the argument a bit, I will first compute (from eq. 26):

\[ \csc \gamma = r_S / r_M = \sqrt{1880 \cdot 27 \div 37} \approx \csc 1^33' \]

(39)

Next, combining eqs. 26, 27, 30, 36, we have:

\[ r_S = \csc \gamma + 1 \]

\[ = \csc 1^33' + 1 \]

\[ = (1 + v) \sin \theta = (1 + 5/2) \sin 0^15' \approx 248^0 \approx 249^0 \]

(40)

[Note: Without eq. 39’s rounding of \( \gamma \) to whole arcmin, eq. 40 yields \( r_S = 2489^09' \).] This is the very \( r_S \) Hultsch proposed was originally stated by Pappos, a value later rejected277.

274 The plod of Swerdlow’s p.298 bottom (use of “490” in numerator & denominator instead of cancelling it: 87’ is artful but somewhat abnormal). However, it is not at all impressive, since the steps displayed could have occurred in several successive stages of math development. And see fn 277 which (Swerdlow loc cit anticipates in a general way).

275 This important connection is the strongest point in favor of Swerdlow 1969 — overcomning the negative items noted at DIO 1.7 15.

276 Hipparchos’ Commentary (Manitius ed, pp.90-91, quoted by Jones 1991M p.449) states that already-existing eclipse predictions were only off by 2 digits at worst, which is better than one would get by using eqs. 29 & 30.

277 Swerdlow n.4 is commendably clear that there is a discrepancy in mss between “490” and “[space] 90”), adding: “This disagreement doubtless strengthened the suspicion of textual corruption.” (Unfortunately, this Swerdlow note consistently uses the Greek symbol for 6 where he intends that for 90.) I must emphasize that there is no direct attestation of 2490...90 in any work by Rawlins 1991H, Swerdlow 1969, etc.) Analogously: the clique who was proved wrong throughout the Ptolemy Controversy has emerged politically important. Considering the similarity of the Ptolemy eclipse predictions, one might be tempted to conclude that this is a neat coincidence. (Note: the resurrection of the same number by the Ptolemy School in the same context, DIO 2.1 13 [A.], is also a neat coincidence. It seems to me, however, that the coincidence is accidental, and that the Ptolemy School did not use the number in any meaningful way.)

Another example: §II4 item [a]. (On the personality-type that behaves so, see independent appraisal at DIO 2.1 13 [A.].) In case it is objected that the “tone” of the present paper makes it unpalatable, keep in mind that Muffois have for decades [a] used the vilest language against dissenters, and [b] have systematically sanctioned gentle DR papers and gentle scholars’ papers, if the findings are considered dangerous to Muffia hegemony or fundraising. (Having created no encouragement — or precedent — for respectful treatment, the Muffia has no ground for complaint in this regard.)
This entire paper was essentially accomplished in a brief but memorable period of nearly 4 weeks, the first breakthrough being that of 1991/10/27—29. Before I hit on eq. 24, I was using an analysis of it by another astronomer, such as Timocharis or Aristarchos. Vision, in [DIO 14 (2) (2008)]. It is a matter of record that Aristarchos wrote on vision & light: Heath [1931 p. 300.]. If so, then Poseidonios presumably agreed. The 10,000 AU estimate is far greater than the geocentrists’, but it is still much too small: the distance to the nearest star outside the Solar System is over a quarter million AU.

Aristarchos evidently chose his lower-limit 10,000 AU distance to the sphere of the fixed stars by just the same uniform 1/10000 rad vision-limit underlying all ancient heliocentrists’ cosmic-scale estimates: [a] the foregoing (10,000 AU), [b] the half-Moon experiment (eq. 45 & perhaps eq. 46), and [c] eq. 21. (See “Ancient Visions”, in [DIO 14 (2) (2008)]. It is a matter of record that Aristarchos wrote on vision & light: Heath [1931 p. 300.].) If so, then Poseidonios presumably agreed. The 10,000 AU estimate is far greater than the geocentrists’, but it is still much too small: the distance to the nearest star outside the Solar System is over a quarter million AU.

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D1 Aside from DR’s customary cleanups after the Mufa’s customary messes, the following paper also provides the actual, highly revealing solutions of the very same ancient material (Almajest 4.11) that the Mufa has consistently bungled for the last quarter-century: [a] Between 158 BC & 146 BC, possibly before he had yet made any of the astronomical observations which are his greatest legacy, Hipparchos originated a solar theory (called here the “EH” orbit: [fn 273]) which he adopted (for no more than c.10’’) and used in his eclipse calculations — until switching (c.146 BC) to the famous PH orbit preserved in the Almajest. The EH orbit is based on attested Hipparchan material ([fn 4]); it & the PH orbit neatly solve, to c.1’ (§3.3 & §10), all six of the hitherto “inexplicable” (fn 63) solar longitudes of Almajest 4.11. On 1991/8/31 & 9/16, spinning off of another paper I was working at — to be published in a later DIO — I wasted some time approaching these solar data through an inappropriate hypothesis. The results might, perhaps, be made to look OK by someone committed to the theory I was exploring, but: there was no gelling, no striking confirmation, no fruitfulness (vs. fn 85 & §O3). So, right after completing the other paper, I started dabbling (1991/10/27) with the problem of fitting orbits to the Almajest 4.11 data: 2 Hipparchos eclipse-trios. (Below, we will follow the chronological convention of Jones 1991H by distinguishing these as “trio A” & “trio B”.) Within 2 days, I had broken through on this front and was dabling confidently into territory previously unknown to historians ([fK]. [b] Almajest 4.11 contains four long-mysterious lunar orbit parameters, left to us by the legendary “father of astronomy” (fn 97), Hipparchos. These numbers have defied explanation for 2 millennia, at least since Ptolemy (c.150 AD) criticized & recomputed this Hipparchos material; the orbital-element numbers in question are: 3144 & 327 2/3 (trio A) and 3122 1/2 & 247 1/2 (trio B) — pairs of lunar mean distances & eccentric-motion amplitudes, respectively (all in unspecified units). For the last 24 years, these numbers’ origin has been researched by Mufa don & eminent Springer-Verlag Hist.sci “Editor” G.Toomer (BrownU, formerly Oxford Univ), to the extent of dozens of admirably erudite published Hist.sci journal pages. It seems to have been the dominant, pet math-astronomy research-puzzle project of his academic life. (See, e.g., Toomer 1967 & Toomer 1973; the hypothetical chord table underlying Toomer’s thesis is altered at Neugebauer 1975 p. 1132, without explanation — though stated to be identical at ibid p.1129 n.1. And Toomer 1984 p.215 n.75 speaks of more to come. See also Toomer 1988 n.44 and here at §D3 & §O1.) He has doubtless expended scores of pages of tedious handwritten analysis on this problem, naively attempting to fit it to the claimed methods of the geocentrist astrologers Hipparchos & Ptolemy. The exact solution of both the larger numbers (the first, eq. 23 below, discovered by DR in ordmug an hour,59. once Toomer’s approach had been cast aside) turns out to be expressible in 2 lines of highschool math (below, eq. 23 & eq. 24) — based on the hypothesis (verboten to all obedient little Muffllosi) that competent heliocentric astronomers’ work underlay that of the geocentrist astrologers (just as today).60
admirers if I empathetically quote a happy passage from the 1940 diary of Panzer-General Erwin Rommel. (In the interests of accuracy, it must immediately be acknowledged that Muffiss are far more adept than DR, at massing troops to crush Enemies.) And, to refine one’s sense of fairness, it helps to try discerning what can be admired and what can be sympathized-with, even in atrocity-perpetrators (whether brownshirt panzers or BrownU pansies). In the entry below, Rommel is reveling in the amazing, seemingly-miraculous moment when the “impenetrable” Maginot line was pierced — and he found himself speeding across France toward the Atlantic and victory. Toomer will see that he is not alone in quarter-century-frustration (in Rommel’s case: 1914-1940). From The Rommel Papers (ed. B.Hart pp.18-20, 1940/5/16-17 entry, with the blitzkrieg in the West less than a week old (launched 1940/5/10): after plunging through fierce fire (tak­ing a face wound), and stifling nearby Maginot forts, Rommel floored it & knifed dozens of km behind enemy lines, in one unprecedented 24 hr tear, much of it nocturnal.

Slowly the sky darkened and it became night. The way to the west was now open. The moon was up and for the time being we could expect no darkness. . . .

Gradually the speed increased. Before long we were 500 — 1,000 — 2,000 — 3,000 [meters] into the fortified zone [Maginot Line west extension]. . . . still no resistance. The flat countryside lay spread out around us under the cold light of the moon. We were through the Maginot line! It was historically cre­ative. Twenty-two years before, we had stood for four and a half long years before this self-same enemy and had won victory after victory and yet now we had fal­len.34 and we had broken through the renowned Maginot Line and were driving deep44 into enemy territory. It was not just a beautiful dream. It was reality.

42. See fn 16 & 31. Forty-fifth birthday. It helps to try discerning what can be admired and what can be sympathized-with, even in atrocity-perpetrators (whether brownshirt panzers or BrownU pansies). In the entry below, Rommel is reveling in the amazing, seemingly-miraculous moment when the “impenetrable” Maginot line was pierced — and he found himself speeding across France toward the Atlantic and victory. Toomer will see that he is not alone in quarter-century-frustration (in Rommel’s case: 1914-1940). From The Rommel Papers (ed. B.Hart pp.18-20, 1940/5/16-17 entry, with the blitzkrieg in the West less than a week old (launched 1940/5/10): after plunging through fierce fire (taking a face wound), and stifling nearby Maginot forts, Rommel floored it & knifed dozens of km behind enemy lines, in one unprecedented 24 hr tear, much of it nocturnal.

266. It’s delightful to see Springer’s current series of amazingly expensive books, “Studies in the History of Mathematics and Physical Sciences”, in which discerning hands as those of “Editor” Toomer — who is in truth editing this series with as much care as Editor-for-Life Lord Hoskin applies to the JHA. And what a lit­tle to realize that, unless one accedes to such geni­us’s tentacular-held creeds, one will be shut out of ancient astronomy conferences, without an above the horizon. (The — 216 Alexandria mid-eclipse was probably below the horizon.) Only the Muffiss could resort to merid­ian under these conditions. — all the while quarreling with astronomers’ linguistic usage . . . .

43. According to my calculations, the 3 eclipses that merit examination (i.e., those that were virtually total near the Hellespont) had linear magnitudes88 at Alexandria of: 76% ± 1% (~309/8/15), 90% ± 4% (~189/3/14), 77% ± 3% (~128/11/20). Thus, for his purposes, Toomer’s selection, (~189, is probably the worst of the 3 candidates. (His choice is still routinely promoted by Muffiss; see, e.g., Neugebauer 1975 p.316 n.9, Toomer 1984 p.244 n.38, & Van Helden 1985 p.11.) Facts:

[1] The —189 eclipse’s actual magnitude was probably the farthest (of the 3 candidates) from the observed value (80%).

[2] The direction of the O–C error would (if the erroneous magnitude 80% were used in an otherwise accurate Hipparchos calculation) lead to a too-small Hipparchos value for rm, not too-large — as is the case here: 77° (Hipparchos) vs. 56-57° (real lunar distance during these eclipses).

My favorite Toomer pronouncement69 has always been the Toomer 1975 p.196 appraisal

than to the meridian. For the most popular contenders rejected in this fashion by Toomer: the — 309 mid-eclipse was virtually due east (azimuth 100° ± 4°), and the — 128 mid-eclipse was only about 8° ± 4° above the horizon. (The —216 Alexandria mid-eclipse was probably below the horizon.) Only the Muffiss could resort to meridian math under these conditions — all the while quarreling with astronomers’ linguistic usage . . . .

R4

According to my calculations, the 3 eclipses that merit examination (i.e., those that were virtually total near the Hellespont) had linear magnitudes88 at Alexandria of: 76% ± 1% (~309/8/15), 90% ± 4% (~189/3/14), 77% ± 3% (~128/11/20). Thus, for his purposes, Toomer’s selection, (~189, is probably the worst of the 3 candidates. (His choice is still routinely promoted by Muffiss; see, e.g., Neugebauer 1975 p.316 n.9, Toomer 1984 p.244 n.38, & Van Helden 1985 p.11.) Facts:

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R2 All these data ([R1]) are given by Pappos. (See Rome 1931-43 p.68, Swerdlow 1969 p.289, & Toomer 1974D p.127.) The same material is discussed (without data) at *Almajest* 5.11. Hipparcos evidently indicated that he derived the lunar parameters for his part 1 by analysing an undated eclipse that was total in the Hellespont but only 4/5 total (Kleomedes 95) in Alexandria. Toomer 1974D attempts to show that various able scientists’ previous identifications of the eclipse are just the usual “worthless” effusions of inferior non-Muffia figures: G.Celoria & P.Neugebauer (no relation to ON) opted for the “Agathocles” eclipse of -309/8/15, while J.Fotheringham, S.Newcomb, & C.Schoch went for -128/11/20. Most of us have thought (even though none was a Hist.sci professional) that these men were among the finest astronomers & other experts to examine the question; but Toomer 1974D shows that they were morons compared to himself. (His suspicions of this went back at least to Toomer 1967.) Toomer gets even hitchen toward an appraisal by Robert Newton, who had just recently outraged the Muffia by showing that Ptolemy’s data were largely faked. Thus, Toomer 1974D sneers at Newton’s grammar — in a swipe whose triviality self-nominates it for recognition as Prissiest-Criticism-of-the-Century. (We can almost565 match it. See p.iv of [Muffia 1990], “Edited” by Toomer & published by Springer-Verlag, where the TeX-based Leslie Lampport system, which both [Muffia 1990] & DR use, has its trademark

\LaTeX

rendered by [Muffia 1990] as

“LaTeX”

— like sumsorta newfangled Frencie high-fashion, high-midnight cow-poke.) Toomer’s same grammar-fussy footnote also scorns a helpful RN observation (fn 264). Compare all this to Toomer’s simultaneous Autumn-Solstice calendric brilliance ([B4]).

R3 Thank heavens for Toomer’s elimination of all those astronomers’ unacceptable scholarship. But now (Win’Tell Overture, please): Muffia to the rescue! Toomer 1974D masterfully analyses 6 eclipses (in order to show how a nonworthless scholar will choose the right eclipse): -309/8/15, 281/8/6, -216/2/11, -189/3/14, -173/10/20, -128/11/20. Hmmmm. I find that the eclipses of -281, -216, -173 were not even close to total at the Hexapod (which I plot at 40°N, 26° W); so one may draw these rather than the saner but once. (Indeed, the -216 eclipse was probably nearer totality in Alexandria!) In order to deduce which eclipse best fits Hipparcos’ likely deductions, Toomer 1974D [a] computers with geocentric (not topocentric) latitudes & declinations (p.134), [b] tabulates no altitudes or magnitudes, and [c] uses a meridian diagram to choose between eclipses! ([pp.131-135]) — this despite the minor inconvenience that, for all 3 of the serious candidates (-309, -189, -128), mideclipse at Alexandria occurred nearer (in azimuth) to the prime vertical 264

Evidently following the lead of (unacted) Gingerich 1972, Toomer 1974D n.13 treats an intelligent suggestion by R.Newton (pp.106-107) as idiotic. (It’s revealing that both reviewers quote the same solitary passage from R.Newton 1970, when looking for a means to denigrate the book. Neither reviewer even explains his objection. Perhaps Toomer figured that O.G must have some reason for scoffing at the passage — and so just signed on, to share the credit for abusing The Hated One.) But Toomer improves on O.G by attacking even the RN statement’s English usage: “Hipparcos worked stop [sic] a year and a half after the eclipse of -128.” Now, now. Our little fangs are showing. If Toomer is out to stabilize alleged typos, let’s sic His Incumbuggian on the Auguean task of cleaning out the missprints all over [Muffia 1990], for which he was himself “Editor”: see fn 149 & 111. If one did not know better, one would assume that Toomer didn’t read the [Muffia 1990] final text before it went to press. However: [a] the book’s camera-ready copy was a TeX file; [b] the TeX Users Group is Toomer’s Providence, RI neighbor; [c] TeX is out of the Amer Math Soc; and [d] Toomer is an eminent Ivy League mathematician. He must therefore be completely at ease with the TeX system. (And, from Toomer’s 1984 vii & App.C, one is deeply — ray profusely — impressed with Toomer’s computer facility: why, of the 20 computer-generated calculations in Appendix C of Toomer’s world-famous edition of the *Almajest*, fully 55% of the results are absolutely correct! See DIO 2.1 fn 3 in 1.) Thus, we know he had [Muffia 1990] right up there on his monitor. To borrow a fn 63 word from Toomer: “inexplicable.”

265 See also DIO 2.1 I.7.

Granted all the more-than-obvious differences45 of academic-induction adventure vs. the military-exploration brand (e.g., the infantryman is frequently cold & wet, and enemy fire is usually from his front),46 still: the analog is inspiring. The common threads are the sensations that infuse one who is purposefully plunging into long-sought new regions: adventure, disbelief, contribution, flukish luck, victory, privilege, surprise, pride, possessiveness, & a mix, of the inevitable transience of thrill, with confidence in mutual (even if perhaps anonymous) immortalization. And, above all: grateful, stable-perspective humility demands recognition of the good fortune that has to play a part in finding oneself at the right place, at the right time, with the right equipment.52 Given the rarity of such exalted moments, one must wonder: how often in life will one attain, intellectually, the high of the invader? — and, not by burning homes55 and mass murder,52 but rather in the refined cause of doing justice to the now-powerless longago dead geniuses — themselves the boldest of adventurers — who rank among the greatest of our history’s pioneers in predictivity, knowledge-condensation, and universal perspective. (See W.Allen at fn 42.) Finally: the time-travel experience of intimate (if inevitably unilateral) communication with the minds of these ancient scholars — legendary brains which have been dust for over 2000 years — is a privilege beyond comparison.

D3 For *Almajest* 4.11, Toomer’s rickety trio A&B solutions — which he intermittently ([O1]) imagines to be a precious window into the history of the invention of trig53 — have

45 As a near-pacifist & anti-nationalist (whose father died in WW2), DR is an odd admirer of anything at all about the obsequiously Nazified Rommel (Hart p.501). But, even aside from his wellknown military intellect — and courage (he was lucky even to survive his 24hr spurt) — Rommel was a genuinely gifted writer. (As was Grant. Or his ghost.) His book is astoundingly composed but astoundingly filled set down during his years of occasional other responsibilities & diversions, such as commanding rapid-armor warfare, often under fire.

46 See gusy footsoldier O.Gingerich’s idea of combat at DIO 1.1 fn 20.

47 Romanticizing intellectual exploration appeals to DR, who holds that concentrating upon the pure & unpragmatic quest, after the grail of truth-for-its-own-sake (while consciously, systematically rejecting corrupting influences), strengthens not only one’s ethics but (perforce) one’s skills. Moreover: careerism is boring, while discovery is

50

48 For instances of the last four threads: see, similarly, the admirably unrestrained joy of the greatest US Arctic explorer, R.Peary ([Nearst the Pole 1907 pp.190, 192], exulting at his excurtivatingly hardwons (if modest) genuine 1906 Summer discoveries in northwest Ellesmere Land. Also the (ironically premature) let-down following: p.203. Like Rommel, Peary was an unusually able writer and thinker. (Writing specialist & skeptic H.Ward, perceptive on set.) Not leaving all the bloodletting to stoners (e.g., DIO 1.1 fn 3), Rommel could also kill face-to-face: in the latter part of the rapid 1940/5/16-17 thrust, when a French officer refused collaboration, Rommel personally murdered him on the spot (Hart op cit p.22). But most commanders are, like Muffa capos, unwilling so to dirty their hands — and thus depute live hatchetry to underlings. (My fellow-semipacifist Red Fox* reaction to frontline warfare: “I backed up so far, I bumped into a general.”) This delusion encouraged by Jones 1991M n.5. This note also uncritically pushes the persistent misimpression ([a] the book’s camera-ready copy was a TeX le; [b] the TEX Users Group is Toomer’s Providence, RI neighbor; [c] TEX is out of the Amer Math Soc; and [d] Toomer is an eminent Ivy League mathematician. He must therefore be completely at ease with the TeX system. (And, from Toomer’s 1984 vii & App.C, one is deeply — ray profusely — impressed with Toomer’s computer facility: why, of the 20 computer-generated calculations in Appendix C of Toomer’s world-famous edition of the *Almajest*, fully 55% of the results are absolutely correct! See DIO 2.1 fn 3 in 1.) Thus, we know he had [Muffia 1990] right up there on his monitor. To borrow a fn 63 word from Toomer: “inexplicable.”

52

53 A delusion encouraged by Jones 1991M n.5. This note also uncritically pushes the persistent misimpression that arc-accuracies did not exist in the 19th C BC Greek astronomy, contra the (differently rounded) degree-format star declinations (*Almajest* 7.3) of Timocharis (c.300 BC) vs. Arístylllos (c.260 BC) — a point noted (obviously vainly) by DR in the 1983/12 lit. (See item [c] below.) The misimpression arises from math-historians’ familiarity with the geometrically-written production of pseudo-Aristarchos & Archimedes — oblivious to the simple point noted at fn 262. Hardcover types pass off the *Almajest* 7.3 declinations by conveniently speculating that Hipparcos or Ptolemy must later have transformed hypothetical pre-degree data into degrees. Comments: [a] A scholar ought to be able to sense when his prejudice is forcing disconrmational data to fit a cherished theory. [b] In numerous cases, Ptolemy...
suffered repeated excruciating tinkering & revisions (§D1), including the embarrassing collapse (§P1) of the empirical underpinning of (what had been the more convincing) half of the work. After all this effort, the results still refuse to match the Hipparchos numbers of Almajest 4.11 — the very numbers which the simple DR solutions (below §P2) reproduce precisely in all 4 cases. One may securely predict that the foregoings will have no (visible) effect on the Muffia, who will simply continue lockstep-pretending that Toomer's development is the only valid one.

D4 Alternate possibility: act as if the Muffia thought of DR's permissible solutions first. (See options [b] & [c] at §H2.) E.g., publish a Muffia paper, containing these DR findings, a few months hence — with a preface signed 1989 or 1990. Too coquettish to consider? Hardly. Indeed, something remarkably similar has already happened. The Preface of Toomer 1984 is dated 2 years earlier (1982). Its special App.C (tacked onto very end of book) contains, without the slightest citation, DR's (entirely original) 1980 solutions for the mean motions of Mercury, Venus, & Saturn (published for DR by R.Newton 1982 pp.103-109). All 3 solutions are based on attested numbers drawn right from Almajest 9.3 (sample data: below at §H3). Each of the 3 solutions fits precisely, down to the last sexagesimal place: that is, to a 50 billionth of a degree/day. Numbers provided at DIO 2.1 §3 §C3. All three are so obviously correct that their Unouchable origins have established a thurible or logjam (§P3) in treating the mean motions issue. As DIO 1.1 §1 fn 9 noted: DR found these solutions in 1980, and mailed them to Toomer's correspondent-colleague, hence O.Gingerich, on 1980/4/13 & 9/2. (Given that OG's gossip circulates more widely than most journals, I claim this as a kind of publication.) Despite my pointed American Journal of Physics remark (Rawlins 1987 n.30) on Toomer's noncitation, Toomer remains silent (as does OG) — and so appears prepared to semi-pretend indefinitely that these discoveries are his own. Hist. Sci. archondum also remains silent in the face of such behavior. What kind of purported historians cannot show an interest in honest attribution

(§D1: Consistency: fn 78) gives non-degree observational data, and then explicitly transforms them into degrees for us. He does not do this for Timocaris & Aristyllos. (c) Unlike Timocaris' data, Aristyllos' solar declination stars are all rounded to 1°/4, an amazing coincidence if the data were originally not in degrees. And all Aristyllos' declinations are correct (within his precision); his mean single-date error, 6° (pre-rounding, it was 4°), is at least as good as that of the ancient declination observers who indisputably used degrees, e.g., Hipparchos. How could such accuracy occur by likely pre-transit-circle methods of recording altitudes? — and additionally survive an hypothesized subsequent transformation? How else but in degrees were early transit-circles graduated, yielding such high precision? (d) Third century BC non-meridian planet-star observations are probably not expressed in degrees merely due to lack of armillary astrolabe (which suggests that perhaps this instrument debuted a little later).

5.4 Eqs. 23 & 24 are quite safe, being much more heliocentric-heretical even to admit, much less grab. But I don't believe that the developments of eqs. 12-20 (despite fn 99) & eq. 34 are based upon any hypotheses permanently engraven in the Muffia Index Conceptions patronum: It may well transpire that Muffia's response to and/or evasion of this paper's solutions will become the subject of yet another J.H.A paper.

5.5 The Toomer 1984 App.C's patent loathing of these solutions makes the accurness of their source all too plain. Incraddly, despite years of gibberish (e.g., Neugebauer 1975 pp.151-152 vs. n.25, p.157 vs. n.6: see here at fn 56) on this issue, the Muffia had never actually carried out the simple divisions of the period relations numbers provided in Almajest 9.3 §1 — until DR did so and showed thereby that (contrary to the Muffia's long-time repeated Ptolemy-trusting insistences: partial list at Rawlins 1987 n.30 & DIO 2.1 §3 fn 38) the period relation quotients yielded the precise tabular mean motions for Mercury, Venus, & Saturn.

5.6 Solutions also sent R.Newton, e.g., 1980/9/2-4. Copies sent K.Meesgaard 1980/11/15. (Inexplicably uncited at Moesgaard 1987 p.45 — though, in a letter of 1983/3/2, he had offered his co-authorship to assist publication-chances of DR's 1983/1/10 ms, which had by then traced these solutions to a further stage in their ancient evolution.) All Almajest planet mean motion equations sent Isis 1983/3/3 & 1983/8/12. (First submission rejected without cause, 1983/3/20. Printed table of equations handed out at 1984/4/12 Astron Soc-HAD meeting. Later appeared in excellent science journal: Rawlins 1987.) I recently asked (DIO 2 §2 fn 15: 1991/8/23) Mufessor P.Huber to request from O.Gingerich a xerox of DR's original 1980/4/13 letter. Huber's reply (1991/8/9) did not acknowledge the request. Have Mufosi been hopeing these transmisions are unprovable? In fact, my tiles contain detailed replies, from R.Newton (e.g., 1980/9/14 & 11/7), and O.Gingerich (e.g., 1980/11/3), all showing that the solutions were new to them. The solutions were also unknown to Mufosi, whose longterm persistent upside-down misconstruing of the same data was exposed in n.30 of Rawlins 1987 (§3 & see fn 55 above). See also the more arrogant but equally misguided comments of Toomer 1977 pp.144-145, while he was, as usual, showing how inferior another scholar (O.Pedersen) was, in his incomparable self.

1991 December DIO 1.3 ¶9

Combining equations eq. 25 & eq. 26, we have (again in Earth-radii):

\[ r_M = \frac{1 + \sin \gamma}{1 + v_A} \sin \theta \]  

(27)

Q4 Aristarchos' estimate of \( \gamma \), implicitly providing (via eq. 26) the ratio of the lunar distance to the solar, is reported as (Heath 1913 p.353):

\[ \gamma_A = 3^\circ. \]  

(28)

(See below: eq. 45.) And pseudo-Aristarchos' (poor) value of the Earth-shadow/Moon ratio is (idem):

\[ v_A = 2 \]  

(29)

Finally, the corrected261 Aristarchos value of the lunar semidiameter:

\[ \theta_A = 1^\circ/4 \]  

(30)

Q5 Substituting the data of §Q4 into eq. 27, we have:

\[ r_M = \frac{1 + \sin \gamma_A}{1 + v_A} \sin \theta_A \]  

\[ \leq 80^\circ \]  

(31)

where, again, \( 1^\circ = 1 \) Earth-radius. This entails (via eqs. 26 & 28)

\[ r_S = 1536^\circ \]  

(32)

Since \( r_M \) is actually 60°/27, eq. 31’s result is about 1/3 high.262 But it approximates263 the earliest reconstructable empirically-based estimate of a celestial distance. (Ptolemy’s later 59° value is far, far better: Almajest 5.15.) Note: all of the data producing eqs. 31 & 32 are attested.

R Impure Speculations, Pseudo-Aristarchos’ Fatal Contradiction, & The Muffia’s Haute Courture

R1 This next section will not be pure speculation. But it’ll be near enough to justify the sectional title’s lead here. To anyone familiar with our few scraps of information about Hipparchos’ lunisolar work, the foregoing (eq. 31) should trigger recollection that Hipparchos’ initial lunar distances in his own (now lost) work “On Sizes & Distances” were in the same range. Pappos says these distances were, for part 1 of the work: 77° (mean distance) and 83° (greatest distance). For part 2: 67°1/3 (mean distance) and 72°2/3 (greatest distance). It has long been known (Toomer 1967) that each ratio (greatest/mean) corresponds almost exactly to the r of trio B (§N10).

261 See DIO 1.1 ¶7 fn 6.

262 Note, however, the enormous improvement of this estimate (80°) vs. the terrible value (20°) resulting if no correction is made to Hypothesis #6: Neugebauer 1975 p.637 eq.19. (Said improvement’s approximation to Aristarchos’ probable actual \( r_M \) was earlier pointed out by R.Newton 1977 p.392, whose analysis is otherwise not much like DR’s.) Needless to add, if Aristarchos lacked trig, the calculation of eq. 31 would have been carried out less precisely, and the result would perhaps have been expressed as belonging between 2 geometrically-derived limits. However, the evidence against his having trig (e.g., Neugebauer 1975 p.638) is not much different from what could be pseudo-induced from much of I.Newton’s published work, to prove the nonavailability of calculus for him. See fn 53 & fn 249. (P.Huber has made a similar comment to defend Ptolemy: fn 224.)

263 [Note added 1992: An incompletely attested but more reasonable case can be made that Aristarchos’ \( r_M \) = 60°. See “Ancient Vision”, to appear in a future DIO.]
to insist on the validity of its own patently inferior solution to the same mystery. Another way of putting it: I am, in effect, effortlessly compelling Mufiosis to insist upon going in the opposite direction from the truth — i.e., betraying their very profession. Not every scholar’s detractors are so obligingly cooperative in thus destroying their own intrinsic credibility.)

The same sort of thrombus has occurred with respect to, e.g., the Almagest 5.3k5 trio of Hipparchos lunar positions (§G7 & §I2), the Almagest mean motions (§D4 & fn 129), and Eratosthenes’ precision (Rawlins 1982G). When correct solutions are blocked by political cholesterol-clog, then, inevitably, the flow of progress halts in the stricken areas.

P4 During the foregoing, I’ve tried to appreciate Toomer’s erudition. And I’m tempted to sympathize with him, even though he’s about as merciful as257 his Funny-Forum mentor. But sympathy for Malignant T. Gloriosus would be wasted here, for an obvious reason: he will never know disappointment in this matter — because [a] his colleagues & hangers-on will emit no (audible) snickers, & [b] he will himself never accept the obvious preferability of the DR quadruple-success perfect-fit solution. Even if direct ancient attestation of the truth surface, he would spurn enlightenment. (DIO is not speculating. Indeed, two instances of Toomer’s stubborn rejection of the plainest possible ancient testimony, have already been presented above: §I1 & §M7.)258 You see, there is a positive side to possessing a robust hallucinatory capacity. Forum’s Miles Gloriosus: “I am my ideal.”

Q Improved Estimates of Aristarchos’ Distances to Sun & Moon

Q1 Because the eventual suppression of (public) ancient heliocentrism (DIO 1.1 §7 §G2) was ultimately so successful, numerous scholars are unaware of the prominence of Aristarchos enjoyed in antiquity. The incomparable Archimedes (in “Sandreckoner”) speaks of him as if he is the most respected of astronomers. He and/or his followers are also cited by Hipparchos, Vitruvius, Plutarch, & Ptolemy. (Ptolemy’s solar distance is obviously based on Aristarchos: §O2. For Pappos’ late comments, see fn 220.)

Q2 Yet, despite the fact that modern freshman astronomy textbooks customly illustrate Aristarchos’ method of finding the relative distances of the Sun & Moon, convincing absolute distances have never even been approximated. (See, e.g., fn 262: 20’) Until now.

Q3 DR’s emendation (fn 261) of pseudo-Aristarchos’ infamous 6th hypothesis, in accord with Archimedes’ explicit testimony, permits a vast improvement in our estimate of Aristarchos’ actual rM. The key equation (based on the standard ancient eclipse-diagram, with the traditional assumption that Sun & Moon have the same angular semidiameter) is well known.260 For lunar distance rM, solar distance rS (both in Earth-radii), solar and lunar semidiameter θ, Earth-shadow semidiameter v times larger than θ:

\[ \frac{1}{rM} + \frac{1}{rS} = (1 + v) \sin \theta \]  

If the half-Moon occurs γ short of quadrature, then:

\[ rM/rS = \sin \gamma \]

257 His called nonempathy with & sneering tactics toward seemingly powerless dissenters, reveal a less than attractive character. Both as regards ethics & simple sportsmanship. A cult, whose ancient hero stole others’ labors at will (Rawlins 1987), naturally cannot be greatly disturbed at seeing a modern scholar’s original work suppressed or misattributed for a trifle like half his career. However, that cult can hardly expect the robbed party to be entirely respectful under the circumstances.

258 DR’s approach is: give higher weight to attestation when it leads in unzany directions; lower weight, when it leads to incredible results. Mufiosis have a precious talent for inverting these rules. For several further examples of our respective viewpoints in action, see: fn 66, fn 162, fn 192, & DIO 1.1 §7 fn 6.

259 Ancients disagreed regarding whether to adopt this key condition for mean or greatest lunar distance from the Earth. See fn 273.


58 DR has been informed from the inside, in so many words, that “blackballing” is the deliberate policy here. (See DIO 1.1 §1A8.) I might add that a very able scientist & author (formerly connected to a famous Ivy League university) recently told me that a different academic clique had (for an alleged offense against its archons) decreed his ostracism from his field, and that a lower-echelon member of the clique had privately told him that this member had specifically ordered never to cite any of the ejectee’s papers, in any field, on any subject. So there’s nothing unique about such behavior. What’s special here is the naivete of those who trust these zoos’ effusions.

Statistics-wise, this reminds one of Rose Bird, the environmentally-sensitive (thus business-embracing) judge whom anti-death-penalty Gov. Jerry Brown appointed to head ‘Toma’s Supreme Court — until her career was executed by ballot-recall (ostensibly triggered by her court’s going off cyanide). Proving in advance that C.Thomas didn’t invent judicial evasiveness, she alleged that her court’s unblemished record, of blocking every one of more than 50 consecutive capital convictions, had nothing whatever to do with an anti-capital-punishment bias. She claimed it just-so happened that: all 50+ cases were contaminated with technical flaws.

57 of major scholarly discoveries? Is this subject not, after all: history? Is there no Hist.sci concern for accuracy or ethics? To put it yet more plainly: is effectively grabbing credit (for major discoveries) of no account? The Mufia & Lord Hoskin determined years ago to exile uppity DR from ancient astronomy: DIO 1.1 §1A8. (Even Lord H’s JHA now admits that fellow-rebel R.Newton was for years similarly treated as a “pariah”, by the very same people: fn 90.) But, with customary wisdom, this clique did not anticipate or assess an implicit risk: what if DR continued a series of original & compelling solutions to important ancient astronomical mysteries? How, then, could leaders maintain pride & power by continuing the blackballing under such ghastly unforeseen circumstances? Simple. Having gotten this deeply into slime, the responsible archons’ only possible recourse: deny the originator credit. (As was done to R.Newton while he lived.) Again & again & again . . . . As many times as prove necessary, in order to maintain the proper pecking order. (Examples & methods partially cataloged at §H2. For details of the open&shut planet-mean-motions case, see DIO 2.1 §3 §C. The predictability of the credit-denial pattern accounts for this paper’s heavy annotation, as I feebly attempt to anticipate, aloud, its findings’ probable Old-Man-&-the-Sea fate, even while I acknowledge my relative limitations at imagining new ways to cheat scholars outside one’s cult.) I repeat: no one anticipated this cycle. But, once a clique locks itself into the pattern, there’s no way out. Except honest admission of massive error and decades of false defamation of worthwhile scholarship. (But too many other scholars have heard Mufia slanders of it, so such retraction — or indeed any perceived success by a Mufia-dammed party — would be ruinous to Mufiosis’ long-polished image of reliability & expertise.) The cumulative transparency of the disingenuousness such a policy entailed is just the grotesque antithesis of the original mistake. A further mistake: when a discovery is stolen or suppressed in order to lower a scholar’s recognition, the archons, who have no excuse whatever for being taken in by Mufia&0 pretenses to reliable scholarship, will emit no (audible) snickers, & [b] he will himself never accept the obvious preferability of the DR quadruple-success perfect-fit solution. Even if direct ancient attestation of the truth surface, he would spurn enlightenment. (DIO is not speculating. Indeed, two instances of Toomer’s stubborn rejection of the plainest possible ancient testimony, have already been presented above: §I1 & §M7.)258 You see, there is a positive side to possessing a robust hallucinatory capacity. Forum’s Miles Gloriosus: “I am my ideal.”

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If the half-Moon occurs γ short of quadrature, then:

\[ rM/rS = \sin \gamma \]
have been no surprise.) So, the message here to Hist.sci archondum is: don’t blame other subcoelenterates for your own inertia & deafness.

E DeToya Party: Lead Paper, Lead Balloon

E1 The primary purpose of the extremely handsome Journal for the History of Astronomy is too simple to be written on its inside cover. The JHA consciously aims at being the most prestigious journal in the astronomy-history field. It is purportedly edited by U Cambridge’s Lord Hoskin (Cambridge Univ, Churchill College) and O Gingerich (Harvard), with hawkeye expert overseeing by “Advisory Editors” such as Muffioso Noel C. Swerdlow (Univ Chicago’s Dept of Astronomy & Astrophysics) and Albert Van Helden (Rice Univ). (Swerdlow’s friend and promoter, Van Helden is also an “Advisory Editor” of Isis.) In this magnificent magazine’s 1991/5 issue, the long-anticipated LEAD paper, “Hipparchus’ Computations of Solar Longitudes” (Jones 1991H), announces an astonishing discovery by [a] Neugebauer-Muffia product & Toomer protégé, Alexander Jones (Institute for the History & Philosophy of Science, Univ Tokyo), masterfully proving that Hipparchus was virtually a closet Babylonian in Greek drag. Jones deftly de-togas Hipparchos by fitting a Babylonian-style solar scheme to the “inexplicably”362 discrepant Hipparchan longitudes at Almajest 4.11 — and thus Jones 1991H actualizes the persistent dream of Muffia capo & Isis darling Bernard Goldstein (U Pitts) and of JHA co-Editor O.Gingerich (§F1) by establishing at last, through mathematical “proof” (fn 107, Jones 1991H pp.104, 110), the long-sought Babylonian influence lurking invisibly behind Greek astronomer Hipparchus’ solar orbit. This grand discovery was swiftly re-trumpetted in Isis’ 1991/9 LEAD paper, “The Adaptation of Babylonian Methods in Greek Numerical Astronomy” (Jones 1991M). And the Jones Pb-papers’ revelation is joyous news for Muffiosis, since it initially appears to weaken the simplest argument against Tolemo’s integrity. After all, it is by now generally acknowledged that Tolemo just took his Greek-trig orbit of the Sun from Hipparchus and faked allegedly outdoor solar “observations”, in almost

659 If it is protested that such “Advisory Editors” don’t oversee work that is published in JHA, then: why list them proudly in each JHA issue? (Of course, see DIO 2 1/1 309!) One of the (not very) implicit messages of DIO’s JHA: what is “prestige” or “reputable” publication worth? — if evidence for scrupulous editing is undetectable. Advertised as forthcoming for months, on the inside covers of JHA issues. As noted here (fn 114, §K, & §L), the actual method of solution to the 3 solar longitude problems addressed by Jones 1991H is that of Rawlins 1991H. But Jones 1991H p.117 claims (partly due to a critical miscomputation on the same page: §G7) that Rawlins’ 1991H’s solution could not happen — even though Rawlins 1991H was published 4 months before Jones 1991H (1991S) appeared! [When DIO 1 1 appeared in 1991, Jones said he wouldn’t look at it. But later he agreed with it.] The Multimobility is as pestifer as Hegel’s 1800/01 Univ Jena denial of the existence of Ceres, which had already been discovered on 1801/01/31 and publicly announced in Jena on 1801/05/6. [Note added 1992: DIO readers are urged to consult the Editor-for-Life’s hilarious & typically well-refereded 1992/8 JHA attempt to deny Hegel his rightful goat’s horns. Unable to translate Hegel’s messy Latin for four-thirds-power, Lord Hoskin simply OMITS the Hegel 1800 analysis’ essential final math paragraphs on the planets, where he states the very distance formula which is the subject of Hoskin’s paper! Our most sincere thanks to His Lordship for so promptly & convincingly exemplifying the earlier disaster’s prime lesson, which follows immediately here.] As unperturbed by mere facts as the Muffia, Hegel went on to the heights of professorial power, teaching nothing else so clearly as the lesson: you don’t have to be correct, able, or sane to be an exceedingly influential academic. 660 See, e.g., proud patronization & strong praise at Toomer 1988 n.25 (fn 271 below) & n.43. Jones is, of course, from the same Brownie troop as Muffia capo G.Toomer, the vaunted History of Math Dep’t at BrownU. (See p.36 of Jones 1983; paper recommended by Asger Aaboeh, also Muffia.) 661 See fn 15, also Toomer 1988, e.g., pp.360&361. 662 Toomer 1967 n.2: “How Hipparchos made errors of such magnitude . . . is to me quite inexplicable.” Toomer 1973 n.10 (quoted at Jones 1991H n.20): “his errors in the longitude intervals are completely inexplicable to me.” (One might suppose that Toomer will be grateful that DR has here alleviated his longstanding puzzlement. Don’t.) Britton 1967 (p.64) tried to explain Hipparchos’ peculiar trio A&B solar longitudes, but concluded (p.65): “Unfortunately, I can find no plausible scheme which would account for the discrepancies which appear.” [Note added 1993: References here are to pp.38-39 of the 1992 edition of Britton. This work — fiscally supported (p.vii) by the Princeton Institute — unqualifiedly recommends (p.39) the Jesustown 1991 gradeschool mistranslitchwhich Britton vetted (§24) and which is the subject of our present Journal for Hysterical Astronomy rom. . . .]

self-satisfaction (see DIO 2 1 309) is complete, and he — Muffiosi’s the expert on Tolemo (fn 240) — back in the glow of his genius (Toomer 1973 p.15): “our calculations have proven” their theses. (See also §O1.) From Funny Thing Happened on the Toom (1966 Plautus-based cinematic musical) one recalls the equally-modest Miles Glorious’ appraisal, upon observing himself: “Even I am impressed.” But Toomer’s intricate & learned geometrical development, despite various arbitrary steps & nudgings, has never quite recovered any of Hipparchos’ four numbers. His trio A investigation wasn’t very convincing: e = 338’, & r2m = 3134’, vs. the attested (Almajest 4.11) numbers, 327’2/3 & 3144’. But the trio B search seemed to get within an ace of the Almajest 4.11 value r = 247’1/2, when Toomer 1973 pp.10-11 came up with r = 246’1/3. (His associated value for r_M was not so lucky: 3082’2/3 vs. 3122 1/2 attested.) But then, the entire trio B analysis of Toomer 1973 turned out to be founded upon a scribal error for the 2nd time-interval. (See Toomer 1984 p.215 n.75. This correction is one of numerous useful contributions Toomer 1984 has made to our knowledge of the Almajest. It is highly probable that without this fruit of the massive labors that went into Toomer 1984, I would never have pushed away from the Toomer 1973 theory and fallen into eqs. 23 & 24.) I have recomputed Toomer’s values, based upon the correction. The results: r = 231’ & r_M = 3021’. These are a long way from the numbers in Almajest 4.11: 247’1/2 & 3122’1/2. (For trio B, Toomer arbitrarily assumes an unlikely Hipparchos blunder, in order to get even that close; without this convenient step, his trio B’s corrected value would be r_M = 2916’.) As noted above (§G2, §O1, fn 252): despite these flabbergiments & crumblings, Muffia scholars — including Jones & Toomer himself — continue to act as if this analysis has proved Hipparchos’ use of the chord trig table that Toomer hypothesizes for him throughout the attempted reconstruction. Yet it is now, all too plain that Toomer’s entire intricate structure has been a castle in the air: a lovely, well-crafted, admirably imaginative fiction. But fiction, nonetheless.

As students of cult behavior have already realized: the Muffia has painted itself into a very tall corner here, since it MUST continue to pretend that “Editor”-potentate Toomer’s theory is correct. (This in spite of the fact that his solutions’ agreements, with the four attested numbers, are worse than DR’s by factors up to 1000 — and even ∞ in the final case.) But optimistic DR now lodges the confident prediction that Muffia sinuosity will prove equal to the seemingly insurmountable hurdle DR has here set before it.

P3 The main outcome may be: another (§D4) thrombus. Though Muffiosi will (at least privately) eternally try to chip away at the findings of this paper, they will (as also for the mean motions & Star Catalog logjams) never find genetically different solutions that gel as neatly & are as precise in fit. (This for the simple reasons that: [a] the DR solutions accord with reality, so [b] Muffia complaints will be mere lawyering.) Thus, junk solutions (§H2 option [g]) must be invented to haze the void into a permanent fogjam. (Among fund-violations of ungrabbable DR solutions to mysteries in this field: each forces the Muffia

256 Lyrics excerpted here are by S.Sondheim. The “ancient” 1966 screenplay contains an unintentional anachronism, when a character worries about rubbing elbows with someone allegedly infected with a supposed Cretan plague. He asks: Is it contagious? Reply: Ever see a plague that wasn’t? This line was a bad joke in 1966. Now, it’s AIDS-lobby-approve govt’ policy. In other words: it’s still a bad joke.
for geocentrists to express the Moon’s distance in Earth-radii, it was just as natural for heliocentrics to express the Moon’s distance in AUs. A nal point: since [a] Babylonians preferred sexagesimal expression for numbers over 59 (van der Waerden 1963 p.38, van der Waerden 1978 p.667), and [b] the base distance used for Hipparchos’ lunar work was 1004 (a choice of round number which denotes a decimal system for large values), we know that the work behind the Hipparchan Almagest 4.11 material was not Babylonian (as suggested by Bostrom) but Greek.

O6 An overview refutation: there is a striking analogy between [a] the foregoing lunar findings and [b] the matter of the Almagest planet mean motions. For both problems, DR has found perfect fits to Greek data via Greek methods, as against Mufia and Moesgaard Babylonian solutions that don’t fit the same Greek data. See tabular comparisons at §P2 for the lunar numbers, and DIO 2.1 §3 §C3 (also §H3 here) for the planet mean motions. Notice also that Greek heliocentrism provides part of the solutions for both problems: eqs. 23 & 24 (distance of the Moon), and fn 129 (Mars mean motion; see also DIO 2.1 §3 §C3).

O7 Besides the foregoing solutions, there is an implicit total-weakness about Mufia&0 forcing a Babylonian step-function onto the Almagest 4.11 solar data — a contradiction that should immediately have impressed itself upon such proprietary experts, namely: the undeniable context. Question: What is Hipparchos’ purpose in the work reported at Almagest 4.11? Answer: he was effectively trying to nd the amplitude of the trig (Greek) szyzial equation of center for the lunar motion. Have any of the geniels selling the solar-step-function Babylonian unicycle ever stopped for a moment to take in the implicit lunar-vs-solar incongruity? — i.e., is it credible that Hipparchos would use a crude Babylonian-arithmetical solar scheme for the mid-eclipse solar longitudes on which he bases the highly error-sensitive parameters of his Greek-trig lunar model? How did JHA & Isis end up accepting (and so readily publishing) a hypothesis that is about as credible as a painting that mingles subtle Rembrandt ﬁgures in with stilted-prole Egyptian ones?

O8 Historical note: when the JHA Editor-for-Life suppressed Rawlins 1999, His Lordship killed one of the key links (fn 9, fn 81) revealing Hipparchos’ intimate debt to Aristarchos. Now that this debt has independently veriﬁed here (eqs. 23-24), thus adding further credibility to the suppressed paper, I look forward to enjoying the JHA’s updated alibis for the original censorship (DIO 1.1 §1 fn 25). They will be as honest as the rest of the JHA’s act.

P Basking Case

P1 From §N14 & eqs. 23-24, we have found the long-sought solutions to the four mysterious parameters set forth at the outset of our explorations here (§D1 item [b]): 3144’ & 327’2/3, 312’1/2 & 247’1/2. The above solutions agree with the attested values, to the precision displayed, in all four cases. (Note that, even if a Mufia anti-helio-centric-inuence fanatic rejects the solutions for 3144’ & 312’1/2, he must face the fact that the other 2 solutions stand on their own merits, being based merely upon entirely attested Greek astronomical math: §N12-§N14. Indeed, of the current paper’s discoveries, this solution-pair and §N10’s matching integral hits will present Mufia evasiveness its sternest challenge.)255

Before proceeding further here, let us examine, for contrast, how closely Mufia capo Toomer’s quarter-century of labors (§D1 & fn 116) has brought him to the same four numbers. His quasi-agreements are so compellingly semi-good that Toomer’s typical exact agreement with Hipparchos’ solar theory — and then brought these fakes forth at Almagest 3.1 as “empirical” support for the correctness of the very same theory. (If such behavior isn’t science fraud, what is? See fn 99.) This realization is due to J.Delambre, the 19th century’s finest astronomer-historian, who in 1817 broached several suspicions about Ptolemy. However, the full, ghastly truth (explaining all 4 of Ptolemy’s solar “observations”, on the nose) was rst revealed in J.Delambre Histoire de l’Astronomie du Moyen Age 1819 (pp.Ixviii-lixv, a source never cited by the Mufia): Claudius Indoor Ptolemy faked all his solar data by simple arithmetic operations, and [b] the matter of the Almajest 4.11 must be based upon the Mufia’s beloved kindergarten fast-arc-slow-arc scheme (Babylonian System A step-function velocity, as against the superior trig-based “sophisticated” astronomy of the Babylonians. (Toomer 1988 p.361 [& p.299 of the Journal for the History of Astronomy’s obit for Neugebauer, Swerdlow 1993]. I fail to see how anyone past the 9th grade could apply the term “sophisticated” to astrologers

255 Mufiosi will predictably resort to a chronology-argument (fn 234) for rejection (since there isn’t any other ground for their foregone conclusion). But this will, typically, be mere opinion, not demonstration — though it will just as predictably masquerade as the latter.

64 See fn 166 & fn 168. Over a decade ago, DR added to this argument the ironic oddity that these arithmetical fakes show that Ptolemy consistently built upon 6h-precision Hipparchan data — to create solar “observations” rounded to 1h precision! Like RN’s argument at §33.

65 Jones 1991M (especially given the information in its n.28) appears to reject less dogmatically (than Jones 1991H p.122) the general pre-Mufia perception that Hipparchos constructed a solar theory (the PH orbit: §K10) like that of the Almagest. Jones 1991M p.440 believes that this could only have been at the very end of Hipparchos’ career. (Actually, it seems obvious that the full PH solar orbit existed from about 146 BC, the [year] of the [Vernal] Equinox that ts perfectly with the PH theory. See Rawlins 1991H §E5.) And, even so, Jones 1991M believes (p.449; & see n.28) it could have been of Babylonian zone format.

66 Mufia: “all we know” (Neugebauer 1975 p.280) & “All the evidence” (Toomer 1984 p.330 n.56, comments plainly depend upon uncited Neugebauer 1975 p.280 for more than this familiar wording — echoes also noted at fn 100 here) tell us that Hipparchos, the reputed discoverer of ecliptical precession (and thus the messy inconstancy of equatorial coordinates), whose extant solar & lunar coordinates were entirely ecliptical, would not record his stars ecliptical!, Classic Mufia logic. (The remarkable stupidity of this reasoning was pointed out in a DSB 1976/18 document sent to Toomer by the DSB Editor C.Gillispie 1978/6/16: p.14, commenting on Toomer 1978H p.217.) That such elementary considerations just might argue for Hipparchos having rendered his star catalog ecliptical is nal recognized at [Mufia 1990] p.216; but he & his Mufia patrons will still not frankly admit the obvious. (When the options are face vs. truth, the Mufia always chooses wisely.) On the basis of Toomer’s forged Almagest 7.3 translation (§I1), accepted and quoted verbatim at [Mufia 1990] p.215, the work concludes . . . by saying (p.216) that we cannot “on the basis of presently available data” know whether Hipparchos compiled an ecliptical star catalog. Had Toomer translated Almagest 7.3 without Mufia bias, [Mufia 1990] could not say that. (For one likely cost of this amazing exercise in Mufia tenacity, see DIO 2.1 §4 fn 29.)
who preferred a step-function arithmetical approximation to a continuous trig function: in 87. But no hype is beyond Mufiosis when they are hugging Babylonian astronomy for a very handsome professorial living.) Barely a year ago, in the JHA, Gingerich 1990 denigrated his superficial-if-nonetheless-inadvertently-stimulating arch-rival R.Newton for “missing” the marvellous Mufia insight that Babylonian step-function math underlay the solar calculations used when Hipparchos analysed (as reported at Almajest 4.1.11) the lunar eclipse trios of 383-382 BC (trio A) & 201-200 BC (trio B).

E3 Jones 1991H (p.118): “The solar theory has always [until the Mufia’s Jonestown triumph] appeared to be one area where Babylonian data did not enter into Hipparchus’ calculations.” The author’s use here of the word “data” (in reference to elements, not outward observations) reminds us of a simple reality which ought to have served as a brake on their Mufia’s mass-suicidal plunge into its Jones 1991H chisteer. That reality: not a single empirical solstice or equinox from Babylon is known to us. Neugebauer 1975 (p.366): “The insight that the solstice-equinox-Sirius dates were based exclusively on the cycle [19° = 235°] without any further consideration shatters the traditional belief — inherited from late antiquity — in extensive Babylonian observational activities.” (Jones avoids this point. L.Taub’s 1987 thesis doesn’t. Nor does P.Huber: DOI 2.1 §2 [HI4. The picture (fn 129) is entirely consistent with Seleukid-era Babylonian “astronomy” being mostly indoor astrology, as DR has contended for years. (See, e.g., Rawlins 1984A p.985. See F.Rochberg-Halton in Leichty, Ellis, Gerhardi 1988 pp.523f, on Babylonians’ very order of the planets being astrological, not physical; “good”-to-“bad”: Jup-Ven-Mer-Sat-Mar.) E.g., there is no record of transit circle observations (standard among the best Greek “scientists”) anywhere in the Babylonian record. It is remiss of him to find their description “impressive” (Jones 1991H p.118) applied in relation to the allegedly original output of Babylonian astronomers, considering that (while Greek astronomers knew their latitude to ordmag 1°) the Babylonian standard figure for the latitude of Babylon (actually at 32°32’N) was effectively: 35°N (Neugebauer 1975 pp.366-367, 726) — off by 148’ or 148 naut mi.

Usual Mufia self-delusional alibing at Neugebauer 1975 p.367, almost verbatim repeat of his pp.667 & 938.) No matter whose fault this massive error was, it’s a devastating disproof of the Mufia’s entertaining key tenet, that “sophisticated” astronomical science was being communicated from Babylon to Greece during the Seleukid period.

E4 Oblivious to the plain implications of the foregoing, the refined Muffia nose smells a Babylonian lurking beneath every incompletely understood Greek achievement. (It’s the same familiar, pretternaturally penetrating brilliance by which other fundamentalists254 find God in geological strata, by which astrologers discern messages in planetary configurations, by which the New Left spies plots behind all its failures, and by which L. LaRouche induces that Bertrand Russell & Henry S. Kissinger were brother secret-Commie agents.)

Contextual background: as noted at the outset here (§E1), the Mufia is frantic to establish Hipparchos’ use of the simple arithmetical methods (designed for the feebleminded) by which “Babylonian mathematical astronomy is characterized” (Toomer 1988 p.356). Jones

degree-fraction; e.g., Swerdlow 1979 (pp.527-528) savages252 R. Newton 1973-4 (pp.112-113) for being misled by just such an error (a longago misreading of 344°1/12 as 34°12’). [Neugebauer 1975 p.166.5.3 requires such an ancient scribal error. Same error: ibid p.729 n.15.] Now suppose a member of Hipparchos’ school, deputed to calculate r from eclipse trio B (for which he would need |rm| for use in eq. 18), started by misreading the eq. 23 distance |rm| = 52°24’ as: 52°24’. This would transform distance |rm| into:

\[ |rm| (trio B) = 52°1/24 = 52°20′2/12 = 3122′1/2 \] (24)

Which recovers the Almajest 4.11 trio B value (§D1, §N10 for |rm|, on the nose.

O4 Note that, without the good luck (for us, anyway) of Hipparchos’ computer having made his fateful scribal slip, the above heliocentrist explanation (§20) of the origin of 3144° would not be utterly evenable. Even though true. Fellow explorers will better empathize with my pleasure at discovering (1991/11/28) the confirmatory scribal miscue, if they realize that I had already (since 1991/11/9) come upon eq. 23 by theorizing that Hipparchos was (at this point in his career) drawing his astronomical scale (eq. 22) from real contemporary astronomers, who were, naturally, heliocentrists. Though I was confident of heliocentrist eq. 23 anyway, the moment of confirmation — the finding of eq. 24 — was exquisite. I then knew positively that the hypothesis behind eq. 23, namely that heliocentrist

Given the contrast between eq. 23 & eq. 24 (even accounting for the likelihood that they were produced at different times), it is unlikely that the same person performed the lunar calculations leading to eq. 19 as well as eq. 20. (The Mufa also regards the trio A & trio B calculations as well separated events, e.g., “first” by which other fundamentalists254 find God in geological strata, by which astrologers discern messages in planetary configurations, by which the New Left spies plots behind all its failures, and by which L. LaRouche induces that Bertrand Russell & Henry S. Kissinger were brother secret-Commie agents.)

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252 Fn 169. To the open & detailed retraction at R.Newton 1977 p.130, Capt. Captions NCSclawswell responds, by attempting to portray such refreshing behavior as dishonest! [Note added 1993: Britton 1992 p.xvi repeats this hilariously inverted slander. As a historical & probably verifiable fact, I can report that (contra Britton’s unsubstantiated claim of “errors scattered throughout Newton’s work” allegedly caused by his use of the Halma & Talieno editions of the Almajest): R. Newton had both the Manitius & Heiberg editions of the Almajest out of the JHU library for years on end. (At the start of his researches, RN indeed openly used primarily the Halma edition; but he henceforth consulted all available versions of the text.) Britton loc cit offers the usual Muffia capo gotts-go-now excuses for not providing “extensive references” to RN’s allegedly copious resultant errors. The same Mufia pretense to apprehension of scores of serious RN mistakes has been going on for over 20’. Swerdlow 1979 p.530 says the very few RN errors he alleges “could be multiplied twentyfold”. DR’s 1979/10/26 letter forcefully challenged NCS to prove it. NCS instead just repeated himself unquantitatively at HamSclawswell 1981 p.61, still refusing to substantiate the big lie. See DOI 4.1.15 fn 6; also DOI 3.2.3 fn 61, which directly charges that “the Muffia klan has simply been bluffing in this regard”, especially with respect to its capsos’ (unwritten) slanders against DR’s accuracy. But Hist.sci’s brave & just leadership still hasn’t required that 2 decades of Muffia libels be made good.] Compare Newton’s frankness to Toomer’s (G2 behavior when upset by the very same problem: altered number-basis.) Thank heavens for NCS’ alertness. Without the Muffia on guard, why, academia might become corrupt.

253 Given the contrast between eq. 23 & eq. 24 (even accounting for the likelihood that they were produced at different times for different works: [N3]: it is unlikely that the same person performed the lunar calculations leading to eq. 19 as well as eq. 20. (The Mufia also regards the trio A & trio B calculations as well separated events, e.g., fn 23.) So it is not surprising that the identity of their production place is in doubt (eq. 23 vs. 24), then could not have innocently published two different versions of the exact same output! [Had the bungled eq. 24 been published first, we could suppose that eq. 23 was set forth later to quietly correct the RM given. But it is generally agreed that the reverse was the case: [N3]. And trio B not trio A became the basis of Hipparchos’ later work: [W1].] So the blundered disparity, between eqs. 23 & 24, provides unexpectedly strong evidence for some historically intriguing realizations: [a] Hipparchos headed a stable of talent. (None of whose names are known to us. Did a now-anonymous Hipparchos-circle Kepler go on to produce work now glamming in the fragments we have of Posidonios’ corpus? Was Poseidonios himself a Hipparchos protégé?) [b] The alertness & comprehension of Hipparchos’ editing of his computers’ work were about on the order of “Editor” Toomer’s when he vetted [Mufia 1990].

254 Nonetheless, in the 1991/11/14 first draft of this paper (laserprinted 11/25), DR felt obliged (before finding eq. 24 conservatively to call eq. 23 speculative. (DR’s attitude in such matters is best gauged from his remarks at Rawlins 1985G p.253 last sentence, & here at JR10)
mistake for the real scientists of antiquity. I should add that the import of heliocentrism in serious ancient astronomy has, from the outset (1976), been a theme of DR’s studies in this area. The idea of heliocentrist’s rôles in 3rd century BC astronomy (establishing a tradition that carried down through Seleukos the Chaldaean and into eastern astronomy) was earlier broached by van der Waerden 1970 (whose proposals were attacked in Isis, with classic Muffia surety & haughtiness, by Swerdlov 1973: see Rawlins 1991H fn 6 & 36). So the following surprise developments (eq. 23 & eq. 24) represent a remarkable — and wholly novel — double-success for this Muffia-resented, flagrantly whiggist viewpoint. 

O2 We know that the Poseidonios school’s distance to the Sun was 10,000 Earth-radii, or:

\[ r_S = 10,000 \text{f} \]  

(21)

So, suppose heliocentists such as Aristarchos scaled the universe (perhaps very much the way modern use the Astronomical Unit) similarly, using 1000 units (not necessarily Earth-radii) = 1000 for the solar distance:

Astronomical Unit (Sun to Earth distance) = \( r_S = 1000 \text{f} \)  

(22)

We will call 1° a milli-AU. Now, the traditional Aristarchan ratio of the Sun/Moon distance (also adopted by the geocentrist tradition: Delambre Histoire de l’Astronomie Ancienne 1817 vol.2 p.207, R.Newton 1977 p.199) is based on the half-Moon being 3° from quadrature — i.e., the transverse Earth-Moon line subtends 3° as seen from the Sun. So, if the Solar System was based on this scale (eq. 22), then in the same milli-AU units, the Moon’s distance \( r_M \) will be

\[ r_M = 1000 \tan 3^\circ = 52^924' = 314' \]  

(23)

This recovers \(^{250}\) (to a precision of ordmag \(10^{-4} \)) the precise Hipparchan number (3144 for trio A’s e) cited at Almagest 4.11 (§D1, §N10) for the Moon’s distance from the Earth.

O3 A hypothetical Mufioso, secretly reading DIO, is now about to (indeed is required to) protest that the eq. 23 shocker just unveiled: [a] is mere-coincidence (albeit a very long shot) and [b] anyway doesn’t fit-trio B. So, let’s next unleash the remarkable clincher, as we now use the same theory to solve also trio B’s superficially discordant Hipparchan \( r_M \). Compare the Triang Thomasi success, by the fruitfulness criterion of fn 85, to the earlier failures of DR, Toomer, & Jones: §D1, §D3, §F4.) One of the best known & peskiest types of scribal errors in Greek astronomy & geography is the confusion of arcmins with...
In the following section, I will attempt to inject a smidgeon of sanity & perspective into these proceedings — by offering a few brief looks at the a priori credibility of Muffissio's classically cultish monomania for tracing virtually all pre-Ptolemy Greek astronomy back to Babylonian work. (Note: no one denies some Babylonian influence. E.g., the Almajest cites numerous Babylonian eclipse observations. And see Rawlins 1987 n.28 and Rawlins 1991H §A, §D10, & §G5. The main issue is rather: whether major Greek astronomers depended primarily upon Babylonian mathematical methods & orbits.)

F

Newton’s Ghost Flattens Babylonian Unicycle

F1

If Hipparchos’ solar orbit was crude Babylonian & not Greek, why does Ptolemy not say so when discussing it at Almajest 3.4, where he instead speaks of the Greek-style solar theory (Toomer’s transl): “the eccentricity [e] . . . is approximately 1/24 . . . . the apogee [A] is approximately [65°1/2] . . . . We too, for our own time, find approximately the same values”. Where’s that part about the Babylonians? (Collective Amnesia strikes again. As at §G6 & fn 191.) The transparently feebly explanation (Jones 1991H p.103): it “is obvious that Ptolemy is at pains to emphasise the points of agreement between his own results and Hipparchus’s, a motive that might have led him to gloss over embarrassing inconsistencies in Hipparchus’s opinions.” Pure fantasy. In truth, Ptolemy notes Hipparchus’ every slip in detail and points out his own allegedly superior results. (As Jones 1991H p.105 is well aware.) Indeed, another Muffia work ([Muffia 1990] p.207) comments on Almajest 3.1 (the very book of the Almajest in which Jones 1991H p.103 suggests Ptolemy avoids exposing Hipparchos’ inconsistencies): “Here Ptolemy criticizes Hipparchos as inconsistent.” (See fn 78 here on Ptolemy’s alleged consistency-fetish. And see Almajest 4.1 & 3.1.) Jones 1991H p.103 goes on to suggest wishfully that, even if Hipparchos did use a Greek model “at some stage of his life”, that doesn’t prove he didn’t act Babylonian in some way or at some point (or still prime Muffia basis for scepticing Babylonian influence behind Hipparchos’ solar theory is the close coincidental agreement of his Springlength with a proposed (unattested & inexplicably hybrid) Babylonian Springlength: injecting the System B lunisolar month into the Babylon-level System A solar scheme, as shown at Jones 1991H p.118. (The reconstructed value is 94°11′57″, only a trifle over 3°mirth of Hipparchos’ Spring, 94°12′.) Question: what sort of “Editors” would buy this used kiddiecar, without ever reading the fine print? I.e., if the computed Babylonian Springlength agrees with Hipparchos’ value, then the natural question one would expect a multicelled animal to ask is: what about the other three season lengths we may compute from the same scheme? Welllllllllll. Muffissio 80

80 See, e.g., Neugebauer 1975 pp.347f and van der Waerden at DSB 15:667 (1978). And, regarding admirable early Babylonian math expertise (far preceding the Greeks), see van der Waerden below at fn 234. For Babylonian solutions of cubic equations, see idem.

81 A point never faced by the Muffia: if the “Babylonian” month $M_A$ (eq. 6) was not taken from Greek astronomy (as DR claims), then how was it determined? (It is accurate to a fraction of a timesec, yet nothing in our records of Babylonian work indicates an ability to perform the sort of precise math that might be needed to make such an accurate determination of the month’s length as $M_A$.) In DIO 1.1 [fn 1] (using Rawlins 1999), DR has shown that, while the monthlengths of Meton & Kallippos (who had access to Babylonian astronomy) were off by ordmag 1$^\text{st}$ (though getting better), the Aristarchos tropical yearlength is consistent with a monthlength which is within 1$^\text{st}$ of $M_A$ and of reality. (So, whatever month he actually used had to be near or — as I believe — equal to $M_A$. See fn 5) I.e., we can trace a chronology of steady Greek improvement here. (See also Neugebauer 1975 p.601: noted at Rawlins 1987 n.28.) If Aristarchos had similar information do we have for Babylonian astronomy?

82 When it suits him, O.Gingerich (the very JHA Editor who secured the Jonestown treasure for his JHA) pleads “our inadequate understanding of Ptolemy’s intentions in writing” the Almajest (DIO 2.1 [§3] §B6).

83 Which also suggests to Jones 1991H that even if Hipparchos was using his famous $e$ & $A$, he might have used “more correct” [trig] a simple schematic [Babylonian] function (Jones 1991H fn 101).

84 Obvious point in passing: Babylonian astronomers who adopted the System B lunisolar month $M_B$ (eq. 6) would be more likely to use a solar yearlength equal c.235$M_A$/19 ([§E3] rather than the two-yearlength-monstrosities of Jones 1991H.

85 From the viewpoint of same philosophy of science (see also §F4 & §O3), the best validation-measure is fruitfulness: a new theory that explains one scholarly mystery leads the investigator onward, to unanticipated

O

Ancient Heliocentrists’ Adoption of the Astronomical Unit

O1

It now remains only to explain the peculiar numbers which Hipparchos gave for the distance to the Moon, $r_M$ (Almajest 4.11 & §N10): 3144” (trio A) and 3122 1/2” (trio B). These values we took as already existing for Hipparchos when he computed eqs. 19 & 20. On the other hand, Toomer 1973 (p.16 & p.27 n.14) claimed that he had “conclusively” & “inevitably” established a very different explanation, from which (for each trio) these $r_M$ values emerge during the calculation of $e$ (or $r$). Toomer 1973 also proposes that, since his theory involves assumption of a relatively crude chord table (suggesting nascent trig), he has cast light into the murky origins of this wonderful mathematical device. His proposal is intelligent and intriguing — and sufficiently attractive that it took me a few days of stubborn testing before I abandoned it. (The chord table theory has a solid parallel, as convincingly shown by Toomer 1973 n.4 & p.24.) But one of this paper’s prime bases has utterly collapsed (§G2 & §P1). (That inconvenient trigle does not, however, prevent the paper’s unqualified citation — by Toomer 1988 n.44 & Jones 1991M p.443 n.5 — as evidence of Hipparchos’ alleged pioneering of primitive early trig tables! We will return to examine this shambles at §P1. Meantime, we simply note that P.Tannery & B.van der Waerden have already persuasively argued against Hipparchos’ invention of trig: In 287.) After naively hopeful effort to salvage the ingenious theory of Toomer 1973 (thinking such an outcome would give both of us a pleasant shock), I finally turned to search in other directions and almost instantly (same day: 1991/11/9) hit upon the solution, a crucial and amazingly fortunate relic of ancient heliocentrism’s vitality — outside the Hipparchos-Ptolemy succession of geocentrist-astrologers, those useful conduits (note innocent irony at Neugebauer 1975 p.943), whom the kneekjee anti-whooshists of modern Hist.sci consistently

243 See Rawlins 1991H fn 1, which (implicitly) somewhat overpressures the need for accurate $A$ when determining accurate $M$. Whether $A = 96^\circ$ is due to Aristarchos or Apollonios (or both or neither) is now no proveable. However, we know that Hipparchos used Aristarchan material, and trio B is from Apollonios’ era. Both Aristarchos & Apollonios worked on lunar topology, and the existence of the latter’s lunar tables is directly attested. In 242.

244 Only by stepping back from the math details can one see an inherent weakness of the Toomer 1973 theory (as against DR’s §G5 proposal of a small scribal error) for explaining Almajest 4.11’s large $r_M$ numbers, 3144 & 3122 1/2: these numbers differ by less than 1%, which Toomer 1973 implicitly regards as just another piece of evidence. After abandoning the theory of Toomer 1973, my first realization was this oddity, which led right to the watershed question: did these numbers perhaps precede Hipparchos’ math, as against being (as Toomer has supposed) a product thereof. Now that the answer is found, I expect we’ll all eventually realize the a priori improbability of the idea that both $e$ (or $r$) and $r_M$ were products of Hipparchos’ math. Obviously, normal procedure would be to set some value for $r_M$ at the outset (as Ptolemy himself does at Almajest 4.6, where $r_M \equiv 60^\circ$), and then express $e$ or $r$ in the units thus established.

245 Actually, eqs. 23 & 24 show that heliocentrism’s data were absorbed into Hipparchos’ work. For a similar situation with Ptolemy, see §H3 & §O2.
### Seasonal Predictions

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**F2** The Muffa’s Bladders shouldn’t hide such woes. If you want to sell this jalopy, then remember: good salesmen make virtues of what lesser minds see as debits. (Like [Mufa 1990](pp.215-216 on data-faking as “progress”). That’s the spirit.) After this manner therefore prey ye.

**What’s thatcha say?** Whaboot-the-other 3 tires, ya say? Son, ain’t a smartbuyer like yew seen the latest thing in hypothetical automobiles? why, this rooomy 4-door beauty rightheer’s the “Boooyooman Unicycle”; the world’s first ONE tire sedan. Engineer’ geen’us! Just imagine the savins in rubber alone. And, ah tellya, that little tire’s the best fit since Hoskin saw DIO. Dealer & repper outlets allorover: BrownU, Harvard, Yale, Wisconsin, the Pitts, London, Cambridge, Aarhus . . . And if (God’bid) deefex popup, them guys come together like in heat. Why, it’s better’n a lifetime garnette. Trustme. And all she costs is a few measly grants from here to eternity. Loo installments. Eeeeeeasy credit . . .

---

**Don’t exactly volunteer to talk publicly about that part of the deal.** [Though Jones 1991H n.41 pretends that Summer checks out OK.] The Muffa sales force just proudly&loudly kicks one of its used auto’s tires — the Spring one — and hopes the buyer won’t notice that the other 3 tires aren’t there at all. One might have expected the editors & referees & other legendary entities at JHA (that’s right, $126/year) & Isis to have had a pretty bumpy testdrive. But when a car’s occupant is in a very, VERY deep sleep, he doesn’t feel a thing. Fact: not one of the other 3 seasonlengths equals Hipparchos’. Even if one rounds the Babylonian values to the nearest quarter-day (or eighth of a day, as desired), still: all 3 disagree with Hipparchos’ values. (Nor do they agree particularly well with any Greek astronomer’s: see Neugebauer 1975 pp.627-628 for various Greeks’ seasonlengths.) The computed Babylonian seasonlengths are easy to compare to Hipparchos’ values (Almajest 3.4, R.Newton 1977 p.76):

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**Confimation in a 2nd, independent arena.** (Even for speculation, e.g., Rawlins 1985G [1]: the Pyramids & Karnak.) Some examples here that mark this paper’s solutions (of the Almajest 4.11 data) as plainly superior to the Mufa’s: eq. 23 produces a theory that neatly explains eq. 24 as well; our method (eq. 24) also solves trio B’s ve (eq. 20). See fn 209.

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By their cohesive harrassment of R.Newton's creative contributions, Neugebauer's clones hooded Newton to his grave. (The Muflia now has the equally genial Macbeth family's traditional residue problem on its sanguinary hands. The Muflia has also attempted to kill off DR in the same fashion, by the usual banishment & slander (fn 2, & DIO 1.1.11 §A8, §C7), permitting no right of reply (e.g. [§113 & DIO 1.1.11 §A9 & C10]). But it has instead merely scotched a Scot. So, there's [Marlovian] justice in R.Newton's intelligence now coming back from the dead to haunt the Muflia, by lodging here the simplest, most devastating point ever raised against the very foundation-stone of the Kugler-Gingerich-B.Goldstein-Jonestown fantasy that Seleukid Babylonian arithmetical astronomy underlay major pre-Almajest Greek work — an amazing notion, which has inexplicably been taken seriously for decades. Newton's observation is contained in an unpublished letter to DR, responding to Gingerich 1980 (p.255), in which OG (in a fashion which perfectly typifies the Muflia's amusing superiority-complex, as Newton has elsewhere pointed out) adopts Kugler's speculation (fn 68) as fact: "the summer solstice date given by Hipparchus derives from the parameter of the Babylonian System A solar theory." R.Newton's comment on this (to DR 1980/9/14, boldface added):

Is Gingerich trying to claim that Hipparchus fabricated his summer solstice on the basis that his [H's] value for the length of spring [94 1/2 days] agrees with the value calculated from Babylonian numerical astronomy? [DR: Precisely this claim is explicitly lodged by Jones 1991H p.118, from Bowen & Goldstein 1988 pp.68-69.] Has he tried calculating the statistical significance of this agreement? When we remember that Hippharchos' solar data [twenty extant in Almajest 3.1] are all rounded to the quarter day, there is no statistical significance to the agreement.

Hipparchos' part may only have been that he put his name on math work actually carried out by others. What is the significance of this agreement? When we remember that Hipparchus's solar data [twenty extant in Almajest 3.1] are all rounded to the quarter day, there is no statistical significance to the agreement.

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N14 For trio A, we have $\alpha$ & $\beta$ from eqs. 10 & 17 from eq. 13, so these are substituted into eqs. 16 & 17 to obtain $U = 5.4253$ & $V = 12.043$. Substituting $r_M = 314^\prime$ (Almajest 4.11, §D1, §N10) into eq. 18, along with the $U$ & $V$ just found, yields:

\[ e = 314^\prime/9.50554 = 327^\prime 39^\prime = 327^\prime 2/3 \] (19)

in accord with the attested $e$ of Almajest 4.11 (§D1, §N10). Relative to the needlessly-contradicted notion (e.g., [§M7, §N11]) as to whether Hipparchos possessed the Almajest tables for solar mean longitude & lunar mean elongation (and thus for lunar mean longitude; §N11), it should be pointed out here that exact — not tabular — calculation in eq. 12 would have produced $173^\circ 08^\prime 05^\prime$, which would have seriously degraded the agreement of the above-deduced $e$ with the attested trio A value $327/2/3$, since the outcome of eq. 19 would have been $327/36^\prime = 327/3/5$. For trio B, substituting $r_M = 3122^\prime$ (Almajest 4.11, eq. 24) into eq. 18, along with $U = 6.2169$ & $V = -2.30$ (computed by substituting eqs. 11 & 15 into eqs. 16 & 17), we have:

\[ r = 3122.30^\prime/12.6161 = 24730^\prime = 2471/2 \] (20)

which agrees perfectly with the Hipparchos value at Almajest 4.11 (§D1, §N10).

N15 Before publishing his results, Hipparchos evidently failed to review how well his deduced elements checked with his lunar positions, a check we will now perform. Starting with trio A, we use standard ancient Greek eqs. 3-5 — but with the appropriate lunar theory elements of eq. 19 as well as $e = 178^\circ$ (eq. 8), and with $g$ found from eqs. 6-7&9 (d values at §M9). The results (ranging to 1’ as we go) would have been for trio A:

\begin{align*}
A1: \phi &= 08^\circ 14^\prime + 4^\prime 30^\prime = 08^\circ 44^\prime = 08^\circ 3/4 \\
A2: \phi &= 263^\circ 59^\prime - 2^\prime 15^\prime = 261^\circ 44^\prime = 261^\circ 3/4 \\
A3: \phi &= 077^\circ 07^\prime - 0^\prime 16^\prime = 076^\circ 51^\prime = 076^\circ 7/8 \\
\end{align*}

(rounding $51^\circ 5$ – to $7^\circ 8$).

Computing trio B likewise, using eq. 20 ($&L2$; d values):

\begin{align*}
B1: \phi &= 352^\circ 00^\prime + 3^\circ 54^\prime = 355^\circ 54^\prime = 355^\circ 11/12 \\
B2: \phi &= 180^\prime 41^\prime - 4^\prime 27^\prime = 176^\circ 14^\prime = 176^\circ 1/4 \\
B3: \phi &= 340^\circ 28^\prime + 4^\prime 17^\prime = 344^\circ 45^\prime = 344^\circ 3/4 \\
\end{align*}

Most of these six opposition-matches to the corresponding solar positions (trio A §M10, trio B §L3) are as good (i.e., within a few arcmin of exact 180° elongation) as are comparable Sun-Moon longitude matches for the eclipses calculated in the Almajest. Except for the A1 lunar longitude (above vs. §M10). (And, of course, the 1° discrepancy in A3: §M3.) The A1 lunar longitude exhibits a serious disagreement with both the stated & computed A1 solar longitude — i.e., with one of the very longitudes which Hipparchos’ eclipse calculation was supposed to match (but didn’t, because A1 was not used in the calculation: eqs. 12 & 13). The original 1° slip for trio A (§M3, fn 162) probably occurred (from a needless borrowing) during the simple longitudinal subtraction. A3—A2. Note: had Hipparchos genuinely tried to extract (from trio A) all 3 parameters ($e$, $g$, $\phi$), which Tolemaic & Toomer have previously understood was the case. Hipparchos would probably have noticed the 1° error in this work, since some longitude-comparison is necessary to find $e$. (Almajest 4.6 finds the middle eclipse’s mean longitude for 2 other trios, of Tolemaic’s choice.) One presumes Hipparchos later noticed A3’s 1° discrepancy and A1’s 1°/7 mismatch. These problems (and the accidentally-not-bad match of unused B3’s computed solar & longitudinal data) may help explain why Hipparchos’ later work neglected the trio A-deduced $e$ and instead used (Toomer 1967) the $r$ gleaned from trio B. However, it appears (since Tolemaic mentions no explicit retraction by Hipparchos) that the trio A error was not publicly admitted, and both pretenses (to mathematical exploitation of all data of eclipse-trios A & B) were allowed to stand. This constitutes mathematical fraud. Ameliorating factors:
G TrigOut Orgy

G1 Now, it is essential also to realize that our Muffia-HHA triumph (§E1: Babylonian mathematical methods underlying Hipparcos’s solar orbit) requires acceptance of the puzzling notion — which, from a lesser source, would invert our brow-furrowed frown — that, though the Greeks used trig to describe the Sun’s motion, nonetheless, a famous Greek astronomer (sometimes regarded as the “father” of astronomy as well as of trigonometry) was drawn instead to the crude, infantile (non-trig) Babylonian step-function for solar speed.

G2 A few passing comments on Hipparcos & trig. (Note that in our later developments here, leading to eqs. 19 & 20, we will carry out precise reconstructions of Hipparchan math, which are consistent with the availability to Hipparcos of extremely accurate trig tables: §N14. This lowers the likelihood that trig was a novelty in his day.) Toomer 1988 makes the following assertions: [a] There is no trace of trig’s existence before Hipparcos. (See p.361; to support Hipparcos’s use of Toomer’s proposed chord table, n.4 unqualifiedly cites Toomer 1973, the key triumph of which — praised at Neugebauer 1975 pp.299&319 — has been gutted, as indicated at Toomer 1984 p.215 n.75. See also §O1 & fn 252.) [b] Hipparch was the importer of the (sub-trig) arithmetical methods and the predictivity (Toomer 1988 pp.360&361) of Babylonian astronomy into a Greek astronomy which was hitherto merely “theoretical” (p.361) and “explanatory”.100 While scouring those benighted non-Muffosi whose inferior feel for ancient science causes them to “misread the ancient evidence” (n.42 & here at §N16, Toomer seems unaware that his perception of Hipparch’s alleged pioneering role ([a]&[b] above and §N16 below) suffers from some self-evident problems, internal & external. (Internal: there’s no trig in Babylonian astronomy; Toomer 1988 p.361 calls this contradiction “confluence”. External: every Muffa try at finding its precious Babylonian simplistic-arithmetic methods in Hipparcos’s work has foundered: fn 73.) Final paradox: Jones 1991H p.113 asserts that his Babyling-Hipparcos’s scheme’s apogee agrees with the “longitude that Hipparcos found for the solar apogee”. But this value is based upon a trig calculation (Almajest 3.4; Jones 1991H p.101). Why would an astronomer (allegedly a trig pioneer), who found his solar theory’s apogee via trig, then (Jones 1991H p.103) abandon trig and graft that very trig-based apogee onto the less accurate preschool-level math of a Babylonian System A-style solar theory? (See §F1.)

G3 By the 4th & 3rd centuries BC, Greek astronomy was using transit circles (Timocharos at c.300 BC; Almajest 7.3), astrolabes, and trig — all of which Babylonians, non-Muffosi now praise as intelligent, admirable, or even progressive: e.g., Swerdlow 1989 pp.43, [Mufa 1990] p.215. [Note added 1993: Pedersen himself, ever politically-correct, now swears he believes this — read it for yourself at Isis 84.5.558 (1993) p.559. Compare to above & Pedersen 1974 p.258.]

100 Toomer 1988 p.360: “everything that we know” says so; n.42 scoffs at even attempting to show otherwise. (Whenever, as at fn 66 & Hipparcos 1975 p.688, Muffosi chant the “everything-we-know” mantra, one may translate: we can’t prove our assertions, so Muffa superstitum will be invoked instead.) At least I agree with Toomer 1988 p.362 that Greek astronomy was relatively untainted by astrology until Hipparcos’ century. See same appraisal at Rawlins 1984A p.979, which also comments that Ptolemy’s Intro to his Tetrabiblos betrays nonuniversal acceptance of astrology even 3 centuries later. (PHuber reasons similarly, from the Almajest preface: 1991/101/ to DIO.) See in fn 237.

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N13 Trios A’s attested time-interval is (Almajest 4.11, §M2): 177°01.2b 2.3. For entering the Almajest 4.4 lunar mean longitude table, this must be broken into 5 months (150°), 27°, 1°, plus 2/3 of 1°; adding the corresponding tabular entries, we have232 the computed mean longitude interval:

\[
\Delta f_A = 176° 27'26'' + 355° 45'44'' + 0° 32'56'' + 0° 21'58'' = 173° 08'04''
\]

(12)

Thus, since the corresponding attested true longitude interval (Almajest 4.11, §M2) is \(\Delta \phi_A = 175° 1/8\) or (as above)235 \(\Delta \phi_A = 175°07'\), we have:

\[
\delta_A = \Delta \phi_A - \Delta f_A = 175°07' - 173°08'04'' = 1°58'56''
\]

(13)

(Toomer 1973 p.13 has 1°59'1/2, for he uses the exact attested expression \(\Delta \phi_A = 175°1/8\), instead of our arcmin-rounded figure 175°07'.) We next establish the same groundwork for trio B. The attested time interval is (Almajest 4.11, §L1): 178°06'. We break this down into 5 months, 28°, & 6°, so (from the Almajest 4.4 tables):

\[
\Delta f_B = 176° 27'26'' + 8° 56'19'' + 3°17'39'' = 188°41'24''
\]

(14)

The corresponding attested true longitude interval (Almajest 4.11, §L1, §N10) is \(\Delta \phi_B = 180°20'\); thus:

\[
\delta_B = \Delta \phi_B - \Delta f_B = 180°20' - 188°41'24'' = -8°21'24''
\]

(15)

(Toomer 1973 p.9 has –8°22.’) At this point, instead of finding 2 more \(\delta\) values for each trio (as in Almajest 4.6 & Toomer 1973), we veer off into the Hipparchos shortcut234 proposed at §N7. Taking

\[
U = -[(\cos \alpha + \cos \beta) + \cot \delta (\sin \alpha - \sin \beta)]/2
\]

and

\[
V = \cos (\alpha - \beta) + \cot \delta (\sin \alpha - \beta)
\]

we can find \(e \text{ or } r\) immediately from:

\[
e = rsg/(U + \sqrt{U^2 - V})
\]

(18)

232 That \(\Delta f\) is expressed to greater precision than \(\Delta \phi\), in both eq. 13 & eq. 15, is partly due to the fact that, by the time the lunar investigations were made, the solar longitudes of the eclipses (on which the true longitude intervals \(\Delta \phi\) were based) had been computed previously (fn 214), and probably already published in Hipparcos’s famous 600° eclipse catalog, presumably already rounded in the fashion indicated at §M10 for trio A & §L3 for trio B.

233 The 07’ ending here is, of course, precisely that of §M10 (reused) or §N10 for the A–32 interval.

234 Mathematicians will quickly see that eqs. 16–18 are based upon [a] the standard angle-sum trig equation of “Ptolemy’s Theorem” (Almajest 1.30, [b] our eq. 3, and [c] the ability to solve quadratic equations. (Though never sophisticated in math astronomy, even the early Babylonians could handle quadratic — and cubic! — equations: van der Waerden 1963 pp.69–71 or van der Waerden 1978 pp.668–670; van der Waerden 1963 p.71 also shows incidentally that these old Babylonian mathematicians were more adept at such math than O. Neugebauer.) Toomer’s lifelong promotion of a wobbly fantasized Hipparchan trig table (§D1), built up merely from the half-angle theorem (thus using a 71/2 interval), has been based upon Toomer’s disbelief that Ptolemy’s Theorem existed as early as Hipparcos. (See Toomer 1973 pp.8, 18, Toomer 1984 p.50 n.59.) The high-precision success of the present reconstruction suggests (as does fn 283) that both the theorem and resultant highly accurate trig tables predated Hipparcos. Of course, one may safely predict that an enraged Toomer will attempt to discredit this DR reconstruction’s support for the theorem’s existence in Hipparcos’ day by pretending that everything-we-know (which may be translated as: nothing; fn 100) tells us the theorem didn’t exist then. It’s hard to draw a circle that’s rounder than Multiologic. (See fn 99.)
For trio A, we use the 3 eqs. just cited, following the tabular method of ancient calculation. (The tabular format was obviously that of Almagest 4.4, where the anomalistic tables are almost exactly equal those of Hipparchos, which were based upon eqs. 6 & 7. We again adopt the notation $1^\circ = 365^d = 1$ Eq.yr. For finding $g_1$ (using the absolute time $t_1$ of $\text{Mar} 9$, the tabular addition is as follows: $g_1 = 82^\circ (g_0) + 92^\circ 14^\prime (t_2) + 73^\circ 21^\prime (13^\circ) + 326^\circ 37^\prime (25^\circ) + 9^\circ 48^\prime (18^\circ) + 0^\circ 20^\prime (3^\circ/5) = 224^\circ 20^\prime$.

Next, from the same tabular method, the anomalistic motion is found for the interval $t_2 - t_1 = (177^\circ 13^\prime 3^\prime 4^\prime)$.

For trio B, we compute very similarly but not quite identically. (This calculation is generally regarded as having been derived separately from that of trio A: $\text{Mar} 3$. In this case, [i] we won’t need $g_1$, and [ii] we express our sums (given the hint at $\text{Mar} 3$, regarding the trio B computer’s predilections) in standard-anomalistic rounded fractions of degrees instead of arcmin. Thus (using the $-1.2-3.3$ absolute time $t_1 = 12.5^\circ 345^\circ 00^\prime$), we have $g_1 = 82^\circ (g_0) + 221^\circ 40^\prime (108^\circ) + 162^\circ 04^\prime (14^\circ) + 351^\circ 27^\prime (33^\circ) + 195^\circ 58^\prime (15^\circ) + 3^\circ 49^\prime (7^\prime) = 296^\circ 58^\prime$ which nearly equals $297^\circ$. For the interval $t_2 - t_1 = (178^\circ 14^\prime 1^\prime 1^\prime)$, the anomalistic motion $g_2 - g_1 = 159^\circ 45^\prime (150^\circ) + 5^\circ 49^\prime (28^\circ) + 3^\circ 16^\prime (6^\circ) = 165^\circ 50^\prime$ or $165^\circ 56^\prime$.

Next, for each trio, to enhance notational facility, we will set the two consecutive $g_i$ used by Hipparchos equal to, respectively, $\alpha$ & $\beta$. For trio A:

$$\beta_1 = g_1 = 224^\circ 20^\prime$$

$$\alpha_1 = g_2 = 177^\circ 44^\prime$$

For trio B:

$$\beta_2 = g_1 = 297^\circ 00^\prime$$

$$\alpha_2 = g_2 = 105^\circ 50^\prime$$

The anomalous differences, $\alpha - \beta$, are: $153^\circ 24^\prime$ (trio A) & $168^\circ 50^\prime$ (trio B). (Toomer 1973 pp.9 & 13 has: $153^\circ 25^\prime$ & $168^\circ 50^\prime$.) Now each subsequent calculation (of $e$ & $r$) will be exceedingly sensitive to tiny errors in $\delta$. (This is especially so for trio A, where even a lapse of $1^\circ$ is critical: $\text{Mar} 14$.) Thus, we will compute the mean longitudinal motion precisely — to the arcsec — from the lunar longitude tables of Almagest 4.4. (These tables are merely the precise sum of: the solar tables of Almagest 3.1 added to the much older lunar elongation tables of Almagest 4.4. The former are precisely based upon Hipparchos’ mean solar motion $F_1$; $\text{G}10$. The latter tables are precisely based upon $366^{d/2}$ divided by the standard eq. 6 ancient monthlength used by Hipparchos, $M_A$ — which indisputably goes way back (to Aristarchos, DR asserts: $\text{SN} 2$). It would thus seem reasonable — to anyone outside the Muffia — to suppose the both tables were available to Hipparchos. A byproduct of the development below is: new confirmatory evidence at $\text{Mar} 14$ that this idea is not only unshocking but, better yet, true. See contrary arguments at Jones 1991H pp.103, 113: here at $\text{F}1$ & $\text{G}2$.)

In conclusion, it is not our concern to dispute the fact that Hipparchos, one of the most famous astronomers in history (drawing upon the wisdom of such calendric-pioneer predecessors as Meton, Kallippos, & Aristarchos), must have used, during his work on eclipse-trio A, a civil year that was made up of not 365 but 366 solar days! Jones 1991H is (p.112) “unavoidably” stuck with this epochal claim, due to his insistence that Hipparchos was using an altered Babylonian System A theory. The author is canny enough to avoid making the 366th beaut explicit in his Isis paper (Jones 1991M), though in fact Jones 1991M (pp.447, 449, & n.28) unqualifiedly continues his insistence on precisely the Babylonian scheme that contains this precious 366th yearlength discovery. (Muffioi slander nemesis R.Newton as “Velikovskian”: $\text{M}7$. $\text{JHA}$ #2 Editor O.Gingerich describes DR similarly: e.g., 1983/11/14. But envy works in mysterious ways. Velikovsky’s WC. Chap.8 proposes a 366th yearlength — which must rank as the most formidable competition Jones 1991H has, in the rarified field of Queer Years.) I am mortified to confess that, for sheer originality, Jones’ astounding discovery, of Hipparchos’ 366th solar theory, leaves me dazzled & meagre findings here quite in the shade. The Jones 1991H theory’s net error (Sun slower than reality by 22° 1/2 per year) would accumulate to about 1/3 of a year in the Aristarchos–Hipparchos interval — and would affect trio B (c.200 BC) solar longitudes calculated from Hipparchos-era (c.150 BC) tables by 3 days or 3°. I will have to leave it to Muffia fundamentalists to explain why Hipparchos would adopt such grossly erroneous solar speeds as Jones 1991H theorizes. Note also: the arc-speeds he proposed for trio A (Jones 1991H p.112) and trio B (op cit p.114) have no relation to each other. (See fn 299.) Of course, from Neugebauer 1975 (p601 & Rawlins 1999), we know that all

102 In selecting theories, assistants, or publishable articles, the Muffia often, I grant, exhibits a special affinity for the inferior. (See fn 30, under “psi-missing.”) But why project this dementia onto the Greeks? — all the while complaining (§G2) of others’ alleged mis-projections!

103 E.g., fn 84.

104 Of course, the trio A 366 day-year solution is conventionally abandoned (for the year of E9K here) by MacOccam Jones when trio A’s first longitude is reconstructed over the long interval between Hipparchos & trio A: Jones 1991H p.119.

105 A pre-example for our upcoming review of Hist.sci.’s Bureau of Double Standards ($\text{HIJ} 3$ & fn 183, and see $\text{DIO} 2.13$ fn 8): When DR found evidence (Rawlins 1982C) for Hipparchos’ adoption of 2 successive different obliques: the first was 198° 276-277 (which now goes under the JHA & is now 227°), the second was 231° 55/6. So does that prove Ptolemy attests for him (see fn 195), while the unattested 2nd value (23° 40’) has been independently elicited by 4 different scholars from 4 separate ancient data sources (Rawlins 1991H fn 21): Hipparchos’ Comet, Strabo, Pliny, & the Almagest Ancient Star Catalog’s north portion. It may be that (in $\text{DIO} 1.1$ $\text{fn} 7$) DR has unfairly applied the same double standard against Swerdlow 1969, whose general theory is attractive & original. (This despite an indefensible manipulation displayed at $\text{iden}$. Note: Swerdlow would gladly use a step even 1/10th as gross, to try killing a Muffia-proscribed party: e.g., $\text{DIO} 1.1$ $\text{fn} 33$, $\text{D} 13$ & 12.) Hipparchan parameter shifts are right at Almagest 4.11.
Greeks and Babylonian astronomers, from a time long before Hipparchos, had the yearlength pinned down within a fraction of an hour.\textsuperscript{105} I presume no reader remains who by now cannot understand why DR has become such a devotee of Muffa & JHA output. It’s the best entertainment since the Gong Show & Benny Hill.

But how, you ask, has our learned JHA-leader-author managed to dispense with the idea (so attractive to limited, sub-Muffa, non-JHA-worthy minds) that a prominent Greek astronomer used (I blush at the presumption): Greek methods? Jones 1991H enlightens us by examining the 1\textsuperscript{st} & 3\textsuperscript{rd} eclipses of trio B, which occurred at very nearly the same day of the year (creating a potentially drastic problem in orbit-fitting): the 1\textsuperscript{st} at −200/9/22 and (says Jones 1991H p.106) the 3\textsuperscript{rd} at −199/9/11 (though the latter event actually\textsuperscript{106} occurred early on −199/9/12). Finding that calculations from Ptolemy’s trig-based Almajest 3.2\&6 table (Hipparchos’ prime solar orbit = “PH” orbit) produce highly discrepant results for these 2 eclipses, Jones 1991H fatefully concluded that Greek trig-founded solar theory is hopelessly irreconcilable with these two Hipparchos eclipse reports. The underlying “proof”\textsuperscript{107} of this alleged irreconcilability: “the rate of change of the solar equation [DR: what astronomers call the equation-of-center] cannot have tended to zero between the apogee and perigee, which means that the solar velocity according to the scheme [used by Hipparchos] was discontinuous. This conclusion rules out consideration tables based on trigonometric functions,” like Ptolemy’s equation table or the Indian sinusoidal equations . . . . And we may be sure of this conclusion (the article’s groundrock-premise), since Jones 1991H was published upfront in the extremely handsome JHA, after the inimitable Editor-for-Life’s invariably even-handed, rigorously intensive referring there, and the paper received customary well- deserved & co: A.Bowen, J.Britton, C.Haines, & B.Goldstein. (BG is the perfect-choice scholar who hatched R.Newton 1977 in the AAAS’s Science\textsuperscript{111} — and whom Isis regards as highly expert in ancient astronomical matters.) When the Muffia & JHA Pb-paper buried the idea of a Greek

\textsuperscript{105} In another context (the origin of the Kallippic year), Neugebauer 1975 p.602 delivers a warning (directly applicable to Jones 1991H): “I see no justification for assuming Babylonian influence in the choice of a parameter which itself is attested nowhere in Babylonian astronomy.” Since we will find below (§5b) that the Kallippic year is embedded in Hipparchos’ Early (EH) solar orbit, the late Neugebauer’s wisdom here turns out to be directly related to the current case.

\textsuperscript{106} Same slip perhaps occurs in date given (−199/3/19) for eclipse B2. (Actual eclipse start 3/19, midrise 3/20. I say “slip” since neither error affects the nonexistent, purely indoor Ptolemaic math of Jones 1991H or Toomer 1984.) These dates are (Jones 1991H n.17) copied — unchecked, of course — from Toomer 1984 p.214-215, who himself evidently copied them from a pre-1925 study (though not uncritically: Toomer 1984 p.215 n.74). Before 1925, almanacs & most scholars (exception: T.v.Oppolzer) used noon-epoch, post-midnight-epoch. (The slips’ source was not Maniusi 1912-3.) Note: all six of the dates of the starts of the Almajest 4.11 eclipses are correctly rendered at p.126 of the Muffa’s least favorite book, R.Newton 1977. It’s curious that a cult, which doesn’t even reliably know what day it is — or even month (in 24 [note added 1993 & see fn 170]) — should damn (as cranks) scholars who do. (The date-confusion difficulty here reveals an obvious & slightly relevant fact: neither Jowett nor the Malignant 1 have ever computed real eclipses. Tiros &AB or any others. The limit of their experience with eclipses is computation by Ptolemy’s highschool-level-math methods.)

\textsuperscript{107} Jones 1991H p.104, speaking of the reasoning of p.110 (here quoted), claims that this “proof” raises his Babylonian-arithmetic solutions (of Hipparchos’ solar data) above mere “conjecture.”

\textsuperscript{108} Jones 1991H p.110, emph added. The reasoning leading to this fateful conclusion is prefaced at p.108 thusly: the author’s System A explanation of Hipparchos’ solar theory “could only be regarded as conjecture so long as there remains the possibility of a simpler explanation of Hipparchos’ figures. The first part of my argument will therefore be to show that any figures cannot be derived from tables or rules for solar longitude plausible in this period except the kind explained by the Babylonian System A.”


\textsuperscript{110} In support of the claim (Jones 1991H p.106) that “modern historians [have not] put forward a satisfactory explanation” of the Almajest 4.11 solar positions, Jones says the upcoming republication (fn 170) of Britton 1967 contents that (quoting Jones 1991H n.20) “the discrepancies imply a systematic difference between the ways that Hipparchus and Ptolemy computed solar longitudes.” (See the young Britton’s prescient speculation: DOI 1.1 16 [§H2].) This sounds allot like the perplexed discussion at Britton 1967 pp.47-48. See §E1. [Note added 1993: References here are to p.39 of the 1992 edition of Britton.]

\textsuperscript{111} B.Goldstein 1978.

N9 Another note on ancient procedure: if Ptolemy’s approach (Almajest 4.2 f) is any guide, Hipparchos’ math formally used anomalistic motion (eq. 7) rather than apogee motion (fn 217). Thus, recalling that all text on \( e = 178^\circ \) (eq. 8) produced \( A \) nearly equal to 96\(^\circ\) (§N5), we see that the corresponding Phil I epoch value for mean anomaly \( g \) would have been:

\[ g \approx e - A = 178^\circ - 96^\circ = 82^\circ \] (9) — just as integral a parameter as input \( e \) & \( A \), of course. We will now find out if this was indeed Hipparchos’ adopted value.

N10 To test the intriguing hypothesis of §N7, we need only set \( e \) or \( r \) equal to the value which Hipparchos calculated (reported at Almajest 4.11) and then work backwards to solve for \( g \). The results for \( g \) are given below, along with the input data:

<table>
<thead>
<tr>
<th>Reported Interval</th>
<th>Assumed</th>
<th>Deduced</th>
</tr>
</thead>
<tbody>
<tr>
<td>A3−2A ≈ 175°07'</td>
<td>( e = (327°2'/3)/(314.4') )</td>
<td>( g = 81°59' )</td>
</tr>
<tr>
<td>B2−B1 = 180°20'</td>
<td>( r = (247°1'/2)/(3122'1/2) )</td>
<td>( g = 81°58' )</td>
</tr>
</tbody>
</table>

N11 The \( g \) found here are strikingly close to each other and to the already-suspected (eq. 9) integral value, \( g = 82^\circ \). (The odds are ordmag 1000-to-1 that both \( g \) would hit by chance so close to the same integral value.) As just noted, the associated Aristarchos-EH \( e \approx 178^\circ \) (eq. 8) is also integral. So I am proposing (§N7) that Hipparchos adopted these two integral values for \( e \) & \( g \) at the outset — and thus he solved only for a single unknown (or \( r \)) from a single interval, for each eclipse trio. Since it is clear\textsuperscript{225} that he pretended otherwise, this is a double mathematical hoax.\textsuperscript{226} (Comment: Though I have sometimes been critical of Hipparchos — whose scientific acumen was rather overrated by later ancients — neither I nor any other scholar ever previously suspected him of any sort of dishonesty.) As noted, the single-interval math would be relatively easy. Two true longitudes form the given interval. And we could find the corresponding two mean anomalies by computing from eqs. 6, 7, & 9; however, the first step (§N12) of my reconstruction is simpler than this. That reconstruction’s ultimate success (§N14) therefore evidences, on the ancient computer’s part, a subtle feel for the problem. He understood 2 related points: [a] the anomaly-differences needed to be more accurate than the absolute anomalies; [b] after computing the 1\textsuperscript{st} eclipse’s anomaly from the epoch value (\( g_e \), eq. 9) and his anomaly tables (found upon eqs. 6 & 7), the remaining required eclipses’ anomalies are most easily found differentially (much as at §L3).
N7 Suppose the 2 peculiarities noted at §N5 are related. Realization of the implications of overdetermination here suggests a simple hypothesis which solves both oddities simultaneously: Hipparchos did not use the full trio to solve for 3 elements; instead, he merely, in each case (trio A & trio B), used one interval (between one pair of eclipses) to solve for one element: \( e \) for trio A and \( r \) for trio B. The 3 unknown calculation for a trio is extremely laborious (see Almajest 4.6-8 or, e.g., Toomer 1973 or Pedersen 1974 pp.172f), so Hipparchos took a shortcut, which we will now examine. (If this ploy was that of hirelings, did Hipparchos even know of it? See fn 253.) For reconstructing his shortcut, we assume that he used the intervals A3-A2 and B2-B1. The four retained eclipses\(^{223}\) can A2, A3, B1, B2 — will (§N10 & eq. 9) prove to be consistent with the same value of \( A \) (or \( r \)).

G5 To appreciate the simultaneous: Hipparchos did not use the full trio to solve for 3 elements; instead, he

N8 Pedersen 1974 says (p.174 n.5) that O. Neugebauer suggested (as does Toomer 1973 p.8) Hipparchos as inventor of the eclipse trio method of Almajest 4.6. The results of the current paper show otherwise — and leave us with no direct evidence of the method’s use before Ptolemy. (Same for equation of time: Jones 1991H n.25. Ptolemy’s math at Almajest 4.6 is far superior to what we are here revealing to be behind the Hipparchan work preserved at Almajest 4.11.) Comments: [a] There is no evidence that Ptolemy originated it (though he seems admirably adept at it). In fact, Hipparchos’ mention of trio of G5 in both cases A&B suggests that the triad technique (Almajest 4.6) predates him (thus his pretense to former’s superiority. After all, throughout, Jones 1991H does not say “trig”). The paper always says, very properly: “trigonometry”. A journal must have standards.

G6 Now, when so impressive an array (§G4 of Mufficio decrees — in a journal so inappropriate to them as the unique finding — that a theory is impossible (in this case, explaining eclipse trio B by a trig-based solar theory), it may prove a heady exercise in foolishness & heresy to explore the condemned hypothesis’ consequences. (After all, Lord H calls DR “impossible”, too: §B1.) The results of said exploration appear below, starting at §K. However, before describing these exhilarating adventures, I will offer: a revealing computational check (§G7), an observation on expert perception (§I10), and 2 predictions regarding Mufficio integrity (§I11 & §J7).

H7 Having buried forever (§G4) the Mufficio-condemned notion that one can fit a Greek trig-based orbit through the Greek Hipparchos’ eclipse intervals (Almajest 4.11), Jones 1991H then (p.117) proceeds to “prove” that the same orbituary applies to the 3 Hipparchos solar observations\(^{112}\) of 128&127 BC (Almajest 5.3&5) — establishing yet another “impossible feat.” This Journal for the History of Astronomy judgment was so typically smart that: The Impossible Solution had (in Rawlins 1991H) already been accomplished & published!\(^{113}\) (No excuse for unawareness of this. As noted at §C11, DR’s solution was printed by the American Astronomical Society in 1990. And the DIO issue containing Rawlins 1991H was cited in the 1991/17 History of Science Society Newsletter p.35, noting that “seven members of the Society had received” it. And, thanks to Ruth Freitag, the specific paper Rawlins 1991H was cited in the Amer Astron Soc’s 1991/3 HAD Newsletter #18 p.19, by title: “Hipparchos’ Ultimate Solar Orbit . . .”). Nonetheless, Jones 1991H (p.117) denies that a plausible scheme assuming continuously varying [trig-based] solar speed [can] explain Hipparchos’ numbers. According to Hipparchos’ solar model, the Sun reaches its farthest (65°30’) approximately 6/2/3 days after the vernal equinox . . . we know both the [anomalies], and the intervals separating \( t_1 \), \( t_2 \), and \( t_3 \) from the date when the Sun was at apogee:

\[
\frac{[(}\lambda_1 - 65;30')}{(t_1 - 67;40\text{ days})} \approx \frac{65;5'}{66;50\text{ days}} \approx \frac{0;56;29'}{63;24\text{ days}}
\]

\[
\frac{[(}\lambda_2 - 65;30')}{(t_2 - 67;40\text{ days})} \approx \frac{27;45'}{28;24\text{ days}} \approx \frac{0;58;38'}{63;24\text{ days}}
\]

\[
\frac{[(}\lambda_3 - 65;30')}{(t_3 - 67;40\text{ days})} \approx \frac{35;24'}{38;38\text{ days}} \approx \frac{0;55;54'}{63;24\text{ days}}
\]

The quotients, which should represent the mean solar daily motion between the apogee and the date of observation, obviously do not behave as they

\(^{223}\) [Revised 1997.] The unreduced reports (seasonal hours, apparent time) of the trio B eclipses (Alexandria) are curiously poor, tenuous, trio A observed in Babylon), curiously poor. (The eye can discern a total eclipse’s mid-time to ordmag 1 timemin; so the main sources of ancient eclipse times’ errors may be:

[a] lunar br-angle via sundial for time, & [b] reporters’ roundings.] Comparing to modern calculations, we find equinoctial-time O – C error for each observed eclipse bound: –46° (A1), –40° (A2), +10° (A3), –7° (B1), +10° (B2), –10° (B3). (Trio A rms error = 0h.6; trio B merely 0h.2. So, I do not agree with R. Newton 1977 pp.122f & 345 that the trio B raw observational data were fabricated.) Note, at Almajest 4.11: most trio A eclipse-times are given in whole hrs; trio B, thirds of hrs. (Halving these figures produces roughly the above cited rms errors — which should be the case if the data are genuine.) This presumably reflects the (real) greater precision-nets of 200 BC Greek theorists vs. 328 BC Babylonian theorists. And Greek borrowing of the trio A Babylonian observations hints that 328 BC Greek astronomical observers were inferior to Babylon’s. Ptolemy’s deduced reductions (to Alexandria mean time of mid-epoch) infected the data with serious additional errors: +11° (A1), +24° (A2), –13° (A3), –6° (B1), +22° (B2), –21° (B3). Thus, Prol–C errors of his Almajest 4.11 intervals: +20° (A2–A1), +13° (A3–A2), +45° (B2–B1), –63° (B3–B2). Ptolemy’s lunar A and r were well chosen, but his acceptance of the Hipparchos PH lunisolar theory introduced a large annual periodic error (amplitude c.20 days) for trio A (Mar–Sep) than for trio B (Jun–Dec). Thus, his intervals’ errors (above) had to be larger for trio B than for trio A. Such conveniently small errors are typical of “The Greatest Astronomer of Antiquity”. I believe that the first clear enunciation of this crucial point (in another Almajest context) is due to Gingrich 1980 p.262 & Fig.3. (Ptolemy was fudging towards a theory that happened to give correctly times for trio A, but for trio B, where his theory happens to be very wrong, the same habits led to disaster. I.e., his improvement of trio A times’ accuracy doesn’t undo RN’s correct conclusion that both trios are fudged.) Bottom lines: [a] Ptolemy pretended that his theories fit outdoor eclipses to ordmag a timemin, though his adopted trio B eclipse-time intervals were off by ordmag an hour. [b] Trio B’s 3 observed times were (rms) thrice as accurate as trio A’s; but, after The-Greatest’s masses, trio B’s 2 intervals were (rms) thrice as accurate as trio A’s.\(^{224}\) This elementary point was never understood by Ptolemy, nor is it grasped by most modern commentators on him — few of whom have experience at fitting orbits to data (a point which needs no further defense after the current paper’s revelations). An exception is Mufficio-friend PJHuber (1991/010 to DIO, quoted mostly fully at DIO 2.1 §H22): “I am pretty sure that the ancient astronomers . . . must have derived their parameters by trial and error from rather inadequate sets of observations.” Compare to DR’s comments (1986/22/8 to van der Waerden): “A . . . general thought about our remnants of ancient astronomy. Real evolution of empirical astronomical theories has nothing to do with the sort of artificial math we find in the [Almajest]. The orbit of a planet is not based on a handful of observations which are then treated by Euclidean postulates in a neat formal manner. No, ancient orbits obviously evolved like orbit explaining Almajest 4.11, it was: a well-attended funeral.

\(^{112}\) Jones 1991M p.448 inadvertently cites Almajest 4.3&5, when Almajest 5.3&5 is meant. An alert referee, familiar with the material, would have known that.

\(^{113}\) Rawlins 1991H eqs.13, 17-18, 28-31. See also here at fn 60.
G8 However, the “abrupt” speed drop between 27°45' and 35°24' is simply another addition to the Muffia’s ever-waxing fantasy-catalog. It is base totally upon several Jones 1991H miscomputations in simple arithmetic — not to mention turning a blind eye to the obvious possibility (explicitly suggested115 by Britton 1969 p.47, fn 215) that the computer’s apology and eccentricity might differ from the PH values! By contrast, Jones has no reluctance about altering, at will, the parameters of the Babylonian solar scheme, plunging right into that job, attempting solutions for all three Hipparchos solar-observation trios. But, mathematically, this task requires nothing beyond gradeschool-level arithmetic (with some junior-high-level arithmetical algebra) — i.e., nothing that would tax the talents of a Babylonian astrologer. On the other hand, solving for the elements of a Greek eccentric-model orbit involves more complex116 math, including trig. A remarkable feature of the Jones papers: they both argue against trig-based orbits, yet the author at no point actually performs a trig calculation (in either paper). Did’t this striking oddity alert anyone to JHA (where such incongruity is nothing new) or at Isis? (The very approach reflected in the superficial equations at §G7 are obviously those of a scholar who lacks the math background to analyse the Almajest 4.11 problem.)

G9 To illustrate the reliability-quotient of the work so prominently published by Michael (Univ Cambridge) Hoskin’s extremely handsome Journal for the History of Astronomy (S126/year to institutions) — and so cooperatively puffed by the History of Science Society’s Isis — I will here recompute the gradeschool arithmetic of the 3 bungled equations of Jones 1991H p.119 (reproduced above at §G7). An obvious glitch in the data going into these equations was Jones 1991H’s reading (§G7) of 67°23' for the (correct) interval, 67°23' — which corresponds to 68°23'. [A Gonnegggggeggggg formerly at this place has been moved to DIO 11.2 fn 21, in honor of A. Jones’ correctness (vs DR’s error) on two Almajest planet mean motions.] The correct equations are:

\[
\frac{\Delta t}{t_1 - 68; 40} \approx \frac{63; 05'}{65; 50'} = 0; 57, 30' \text{/day}
\]

should: the speed ought to increase gradually with increasing elongation, and here it appears to drop abruptly between 27°45' and 35°24' from apogee. All in all, it seems most probable that at least one of Hipparchos’s solar longitudes was observed rather than predicted.114

114 Jones 1991H n.37 adds that Toomer 1978H p.219 “has already suggested that at least the longitude for 126 July was observed.” Use of the astrolabe as an analog computer for placing the Sun (Pappos’ method, R.Newton’s mistaken preference) collides at the solstices (same for finding longitude from a measure of solar altitude, whether by plinth, parallactic rulers, or transit circle). See Rawlins 1982C p.372. So Hipparchos’ 1267/7 solar longitude (closest of the three Almajest 5.3.6 data to a solstice) is obviously the least likely (of the 3) to have been observed. In any case, as is self-evident from Almajest 5.1 (Toomer 1984 p.219 n.4, & see Wlodarczyk 1987 pp.177), all 3 of these Hipparchos solar longitudes were computed from his solar theory and were then used in setting his armillary astrolabe for the 3 co-reported lunar observations. All 3 data are consistent (vs DR’s error) on two Almajest planet mean motions.]


116 Her sci. folk traditionally console themselves (when caught in technical footsies) by falling back into a pose of superior Feel (vs. those “unhistorical” scientists) for the broad-historical-perspective. Yet, the truth is that Muffia’s Big-Picture of ancient science is even worse than their computational limitations. (See §C7 & fn 92.) E.g., banning from his mind (and all the journals he can possibly influence) the import of heliocentricity in ancient astronomy is precisely why Toomer wasted a quarter-century (3D1 & 3P5) looking vainly for the solution to the numbers so swiftly solved here in eqs. 23 & 24. The professionally-convenient Hist.sci pseudo-surety, that ability to do science is somehow correlated with inability to understand its history, is as durable a myth as the notion that lightning calculators are all “idiot-savants”. (Like Gauss?) Such misperceptions (which have an obvious resemblance to homeopathy, & are about as true) thrive for a common reason: limited talents crave solace.

N2 We will use here the intervals of [M2, not §M3.] In order to arrange210 that lunar & solar positions will be 180° apart at the given mid-eclipse times, I have, using §K10, set (§N10 & fn 237): \[
\epsilon = 178° \quad \Delta \epsilon = \epsilon - \phi = 178° - 227°2/3 = 310°1/3 \quad (8)
\]

(Note that both 178° and 310°1/3 are merely the Almajest 4.2 values transposed to Phil 1.) I believe mean-e-longation-at-eclipse (or mean-synodic-longitude-at-eclipse) \(\Delta \epsilon = 310°1/3\) was Aristarchos’. (See Rawlins 1985K. Since eqs. 23&24 and Almajest 3.1 all establish a connection of Hipparchos to Aristarchos’ work, I will use Aristarchos’ name for the pre-Hipparchos epicycle values, from c.300 BC, which we are recovering here; however, one cannot really be sure who was responsible for much of this work: Kalippios 330 BC. Timocles c.300 BC. Aristarchos 280 BC, Aristyllos c.260 BC — perhaps Apollonios c.200 BC, who is actually credited in antiquity with lunar tables: §M7 & fn 242. Or anonymous222.) Any value for \(\epsilon\) will suffice at this point, though it turns out that, here and below, eq. 8 will provide the \(\epsilon\) which Hipparchus used. From the intervals (§M2 & §L1), I have solved for the solar elements, with the following results:221

The trio A solution: \(A = 96°.136\) & \(\epsilon = 6°.403 = (335 1/2)/(3144).\)

The trio B solution: \(A = 95°.410\) & \(\epsilon = 4°.773 = (248 2/5)/(3122 1/2).\)

N5 Now, the most striking aspects of these solutions are: [a] The \(\epsilon\) & \(r\) results are not equal to the values cited in Almajest 4.11, namely, \(6°.253 = (327 2/3)/(3144) & 4°.756 = (247 1/2)/(3122 1/2),\) respectively. [b] Both values for \(A\) are around212 96°, which is far from the correct value (91°.4) or the Almajest value (92°.43°) for epoch Phil 1.

N6 In our earlier investigations of Hipparchos’ solar work, we were seeking unattested orbital elements that would produce his given intervals within standard ancient rounding precision. But here in the lunar case, we are examining a more precise (and inverse) situation: certain given intervals (§M2 & §L1) are the basis of Hipparchos’ 4.11 calculations, which will produce partly attested elements (§D1 item [b]). So our demands on the precision can be higher, which should make our findings more revealing. 210 This is easy to accomplish, since, when using eclipse intervals (differential data) for searching out Hipparchos’ A & (or r), \(\epsilon\) cancels out of the problem. (See fn 217.) It may thus be adjusted later, at one’s leisure, to ensure 180° arguments at mid-eclipse times.

212 Against Kalippios’ rôle: his rough monthlength was supplanted by Aristarchos’ later-canonical accurate value (eq. 6). But this improvement (fn 81) of the lunar speed’s accuracy would not prevent Kalippios (contemporary with Phil 1) from being the source of the 310°1/3 Phil 1 epoch value in eq. 8. Note that Timocles made observations both before and after the decade during which were taken the Aristarchos-era observations proposed by Rawlins 1985A as underlying various attested ancient solar, lunar, & planetary period relations. (Perhaps Timocles played Flameldeus to Aristarchos’ I.Newton. If so, one hopes the relationship was more amicable than the later rendition.) I must note that some of the DIO 1.17 fn 7 problems with Aristarchos’ sole alleged extant work (“On the Sizes & Distances of the Sun & Moon”) were anticipated by Neugebauer 1975 p.642. Moreover, Neugebauer 1975 (p.636 — especially n.4 — & p.643) makes the telling point that the grossly false distances of the Sun & Moon (which would result from the work’s terribly erroneous 2° lunar diameter) are never presented — despite the fact that these distances are part of the work’s title. Thus, “Sizes & Distances” gives the Sun’s & Moon’s sizes but not distances; the former are virtually unaffected by the infamous semiidiameter-error, while the latter are much affected. This leads Neugebauer to doubt the work’s empirical seriousness or sincerity (p.643), while DR takes it merely as further (see also [R10]) indication that the author was not Aristarchos but was just a talented developer of Aristarchos’ six hypotheses (who somehow, when confronted with his distances — which disagreed violently with those presumably well known to be Aristarchos’). Another possibility: the author was a pure mathematician, a posthumous devotee, whose innocence of the outdoor sky caused his inadvertent mangeling of hypothesis y6, as explained in DIO 1.1 (loc. cit.). In any case, the question (on which I remain flexible) of the botcher’s identity should not divert one from the main point of idem: we now have the explanation of the long-mysterious “Aristarchan” lunar diameter error (by a factor of 4). There is no doubt that some of “Sizes & Distances” (not the part involving the factor-of-4 error) was cited by Archimedes only a few generations later (Archimedes p.223). And the peculiarity of pseudo-Aristarchos’ 2° value was noted by Pappos, so we know that the whole pseudo-A work was (in some form) already accepted as genuine by c.300 AD at the latest.

The lunar anomaly configuration of the eclipses is far more advantageous for solution than the solar anomaly configuration. Thus, the results of analysis are fortunately tight; e.g., one has no long groove (as in fn 205) of possible solutions for the lunar longitudes.
The times for trio B will be those of §L2, and the times for trio A will be those of §M9. (While computing the foregoing solar places, we ignored fractions of hours. This is not advisable for the Moon, whose mean motion is rapid: 33'hr sidereal, 30'hr synodic.)

N2 Each trio establishes 3 equations of condition, from which one may solve for 3 lunar elements: [1] mean-longitude-at-epoch $\epsilon$, [2] apogee-at-epoch $A$ or mean-anomaly-at-epoch $g_0$, and [3] the lunar epicyle-radius $r$ (for trio B) or eccentricity $e$ (for trio A). Also required are the mean motions$^{N4}$ of the Moon (& Sun) and the lunar apogee. The standard ancient length of the synodic month is the admirably accurate “Babylonian” value,$^N5$

$$M_A = 29^d31'50''08''''02'''''''' = 29^d5305941 \quad (6)$$

(Almajest 4.2-4), which DR has mathematically traced to Aristarchos (Rawlins 1985S, Rawlins 1991H §B10 & fn 1; see also here at fn 81 & §N11). This and the relation (also Aristarchan: Rawlins 1985S)

269 anomalous months = 251 synodic months

are both associated with Hipparchos (Almajest 4.2). (His adopted anomaly epoch value will be induced below: eq. 9.) Thus, at the outset of our search for the 3 unknowns noted above ($\epsilon$, $A$, & $r$ or $e$), we may tentatively assume that these 2 motions (eqs. 6 & 7) were adopted by him.

N3 Finally: since the Moon’s motion is the sum of its synodic motion and the Sun’s motion, choice of yearlength will affect $\epsilon$ — which allows us to test for the yearlength. Hipparchos adopted the time of the lunar calculations. This turns out to be his traditional PH orbit value ($\xi$K10). Which tells us what is already selfevident (since the solar longitudes were used in the lunar deductions): both lunar calculations were performed after the latter of the two solar calculations ($\xi$L3 & $\xi$M4), both of which used Kallippos’ yearlength. However (as noted at fn 214), none of this tells us which lunar calculation came first. But, as already noted by Jones 1991H p.113 (which via n.31 refers to Toomer 1967), the Pappos & Ptolemy (Almajest 4.11) accounts both indicate that the trio A lunar deduction preceded that of trio B.

N4 Since the Almajest 4.11 data are actually just (for each eclipse trio) 2 intervals instead of 3 independent data, we may simplify (& make sure) our search by just using these 2 intervals. Moreover, one uses just the intervals, $\epsilon$ cancels$^{N6}$ out of the equations of condition; thus, we have merely 2 equations for finding 2 unknowns: $A$ and $e$ (or $r$). (Note: the $e^2$ correction to the solar intervals has no effect here, since the $e^2$ error occurred between$^{N7}$ the solar and lunar calculations. Thus, to reconstruct Hipparchos’ trio A work, we

216 Different likely (non-Jonestown) ancient values for the necessary mean motions (2 lunar, 1 solar) will have little effect on a 3-unknown solution for the time of the chosen trio — though of course they will affect the solutions for $\epsilon$ & $A$ at Hipparchos’ remote tabular epoch (Phil 1).

217 See fn 161. An alteration in $\epsilon$ will alter the deduced $A$ by the same amount. However, in effect, the 2 unknowns here are actually [a] the lunar anomaly for any one of the 3 observations (the other 2 anomalies then follow, once even a crude anomalistic motion is adopted), and [b] the lunar $e$ (or $r$). Using $A$ for the 1st unknown (instead of anomaly) introduces a dependence upon $\epsilon$ and the precise anomalistic motion; but (in this case) these be turned out to be merely traditional constants, so: treating $A$ as an unknown causes no real loss of flexibility in the solution process here. However, my modern preference for using the (more slowly varying) apogee is probably unhistorical. (See §N16.) I am converted at eq. 9.

218 As a matter of interest: had the $e^2$ slip not occurred in trio A, Hipparchos would have found $e = 8^\circ1/2$ & $A = 89^\circ$, by treating both as unknowns (which he didn’t): $\xi$(N7). Both elements are more accurate than those he actually got (Almajest 4.11) for either trio A or trio B. (Averaging these with trio B’s results produces elements not far from Ptolemy’s.) With some justice, the Muflia has criticized R.Newton (e.g., Swerdlow 1979) for his shaky calculation that the Almajest lunar epicyle radius $r = 5^\circ1/4$ came from Hipparchos. Since both Ptolemy’s proofs of this value are the usual supernate fabrications, we ought to look elsewhere for its origin. Note: Ptolemy scorns Hipparchos’ two discrepant values, but fairminded scholars ought at least to credit Hipparchos with nonfudgery, while instead scorning Ptolemy’s own laughably overprecise agreements: $r = 5^\circ13'$ & $5^\circ14'$, both at Almajest 4.6. See R.Newton 1977 pp.122-123, which finds that an error of merely $15^\circ$ in 1 eclipse-time would affect deduced $r$ by $9'$, and $g_0$ by $43''$. See fn 237.

Further, since $\lambda_1 = 123^d7/12$ (Almajest 5.3, Rawlins 1991H eq.29), the first numerator should be $63^d05'$, not Jones’ ($\xi$G7) $65^d05'$. Gonnegggggggg . . . . (NCSwerdlow, the Muflia’s Capt.Captious, snidely attacks politically-disliked E.Rosen $^{117}$ thusly in Isis 72/73, p.79: “Even addition and subtraction pose problems.”) One sees (as noted at §G8) that the Muflia’s fantasized standard spread-$\langle \xi$G7 $\rangle$ melts, once correct computations have deflated these entertaining JHA proceedings. Not to worry. The JHA attempted to — even boasted DIO 1.1 §G6 it intended to — ignore its 1982/10 Editorial disaster, too. (Only the decent author’s insistence on printing correct work caused eventual tardy public JHA retraction.) And the JHA has not acknowledged (publicly or privately) the 1984 JHA calendric foulups displayed at DIO 1.1 §G5. So it’ll presumably likewise refuse to correct the $3^2$-grade-arithmetic errors in the Jones 1991H article it gave top billing to. (And, in case correction ever occurs, DIO will not likely be quoted.)$^{118}$ Evidently, an image-obscessed dearth of editorial integrity has its compensations. See DIO 2 [1].

G10 For those without access to DIO 1.1 (Rawlins 1991H eqs.13, 17-18, 28), I will here provide the UH elements, which neatly satisfy (to about 1’/60) choice of yearlength will affect the solutions for

$$\frac{(\lambda_A - 65^d30^\circ)}{(t_2 - 68; 40 \pm 25 \pm 25)} \approx \frac{27; 45^\circ}{39; 57; 00} \approx 0; 56, 36^\circ / \text{day}$$

(117) Whatever Rosen’s academic & temperamental shortcomings, he cited Swerdlow frequently & acknowledged that he owed several enlightenments to: see JHA 21:206; 1990. (This despite Swerdlow’s repeated juridical assaults upon Rosen.) DR’s response to Swerdlow’s slanderous attacks on RNR & DR has been similar. (When Rosen earlier attacked T.Africa’s 1961 Isis paper on Copernicus, Africa’s temperate reply concluded simply: “Professor Rosen does not have to accept my interpretation of Copernicus … if it is erroneous, surely the good sense of the scholarly community will reject it.” See Isis 53:509. I suspect that, when young, Swerdlow suffered from Rosen’s sometimous arrogance. It is curious that Swerdlow fails to discern certain subsequent analogies.) Capt.Captious’ Muflia has yet to acknowledge that DR has ever contributed anything to the ancient astronomy field. The Muflia is proud of that pristine record. And the Hist.sci community’s top journal (is) has prominently taken part (§14) in the effectively censorial and explicitly vindictive (DIO 2.1 §2) strategy it’s part of.

118 However, until the underlying $e$ & $A$ are corrected to equal those of the UH orbit, the speeds will still appear not to gel fully with the DIO orbit. This tells us what is already selfevident (since the solar longitudes were used in the lunar deductions): both lunar calculations were performed after the latter of the two solar calculations ($\xi$L3 & $\xi$M4), both of which used Kallippos’ yearlength. However (as noted at fn 214), none of this tells us which lunar calculation came first. But, as already noted by Jones 1991H p.113 (which via n.31 refers to Toomer 1967), the Pappos & Ptolemy (Almajest 4.11) accounts both indicate that the trio A lunar deduction preceded that of trio B.

The 3 Hipparchos solar positions, which this orbit fits (and which Jones 1991H called unfittable), are (Almajest 5.5&5): $123^d7/12$ (-127/8/5 1/4), $37^d3/4$ (-126/5/2 1/4), $100^d9/10$ (-126/7/7 2/3). The UH orbit calculations are given at Rawlins 1991H §D9. The match is to within $1'$ in all 3 cases — though, before DR published the UH orbit, the discrepancies were mostly about $1'/4$. I

119 An honest journal would draw extensively from the relevant articles (DIO 1.1-3), following the procedure set out in the DIO publication statement, inside back-cover of this & subsequent issues.

120 Indian tables used $e = 2^\circ15'$ (Toomer 1973 p.149, Neugebauer 1975 p.317 n.11), which might be a traditional (in 1975) rounding of the UH value. The improved accuracy may also suggest an empirical (not necessarily Greek) source.
H Browning Squared

H1 To sum up the Isis-Jonestown contremtemps over Hipparchos’ – 127&– 126 data: the History of Science Society put at the back of its small Newsletter a brief mention of the publication containing the correct & accurate solution to these Almajest 5.3&5 data (& drew no attention to this solution: fn 176) — while almost simultaneously running at the front of its Important Journal (Isis — from which DR is effectively banned) an article which [a] denied THE VERY EXISTENCE of these ALREADY-published solutions, and [b] promoted a misconceived and mathematically-botched “hazmat” solution and got away with it (THE SAME DATA-TRIO — all of this topped off with: [c] the fantasizing author’s deliberate-smub condemnation (§114) of the prominently-published corpus of the correct author (DR). (Once upon a time, the Mufa stood proudly on-guard against citation-failure, implying dishonest scholarship on the part of those hapless scholars who failed to measure up to exacting Mufa standards in this critical25 department.) The deed is diamond-like in the multiplicity of facets utilized to flash its brilliance to the world. How am I dazzled? In the tradition of a poetperson whose name escapes me, let me count the ways: timing, egregiousness, irony, prominent-expert evaluation, rubberstamp pseudo-refereeing of archon-Browning pseudo-research, political arrogance, technical innocence, ostracism of dissent.32 One involuntarily marvels — as if at a satiric satire on a religious miracle — and, aww,ewstruck, asks: how can Hist.sci archons ever top this one? Well, believe me, I’ve asked that question before — and thus can offer some well-foundaced voice-of-experience advice: don’t bet they can’t.

H2 Indeed, the topper could materialize quickly, due to the Mufia’s incurable insistence upon its own genius & DR’s anathematization. After the present unambiguous exposures, Muffosi must choose one of several typically slippery options. (Muffies & DR are as one in our confidence that: Hist.sci archons’ policing of gangup-misbehavior will have even the camcordered L.A. cops begging for lessons.) These options cannot include frank admission along the lines of “Rawlins-is-right.” (Described vaunted Mufia linguistic facility, rumored-but-still-classified testing is said to have found that Mufia lips, attempting to master this excruciatingly painful 14-letter tongue-twister, automatically lock in a mysterious involuntary eternal-stammar paralytic-freezeframe — leaving a facial expression reported resembling that of one whose fingernails & heart are being ripped out simultaneously.) DR cannot be right on any fact. In any sphere. After all, such admission might confer a hint of Reputability upon a heretic already deacred otherwise (DIO 1.1 §1, §7, §8) by Infallible

121 In a 1983/1/28 letter to DR, Isis reacted, to the dreary news that both its own referees had recommended publication of a DR submission, by stating that Isis wished indefinitely to receive no further DR ancient astronomy contributions and that, if the current one were (published & then) attacked, DR would get no reply space: §13. Question: what are the odds that Jones was treated likewise?

122 See [H2 item c] on class, snobbery, & academic ethics.

123 See, e.g., §15, Toomer 1980 p.108, & DIO 1.1 f6 fn 6. The last item exhibits Swerdlow’s sarcastic attack on van der Waerden for allegedly shady citation-practice. Curious. The Swerdlow 1989 discussion — e.g., p.30 on planet mean motions (also p.32 on Ptolemy’s inferior planet/eclipse radii being based on whole-degree greatest elongations from Piny 2.38-39) — of the extent to which Ptolemy’s parameters pre-existed his “observations”, was positively obliged to cite R.Newton 1982 and Rawlins 1987 p.236 (especially item [5]). Naturally, it didn’t. (See also fn 166. Incidentally, the above-noted whole-degree Piny connections were discovered by DR 1985/5/30-31; but, unlike the equations of Swerdlow 1989 p.41, I do not believe that these were mailed to Isis.) As long as Swerdlow continues to be honored by archons regardless of his citation-practices, then: why indeed should he bother to start behaving in a way that may be distinguished from what he himself has publicly ridiculed as sleazy scholarship?

124 The History of Science Newsletter’s 1991/17 p.35 bluff on DIO concludes by noting (at my telephone request) DIO’s refusal to publish that my case against Poynting’s dispute as central to the Polemy controversy lacked due to a lack of space to create a brief exposition on the neglect of setting up such a debate.) Who are these Hist.sci people, Martians? Have they no terrestrial potency? Guys, you don’t let a remark like that just sit there. You have the capacity to arrange such a debate, don’t you? So stop talking and start scheduling it. While not inclined to set rigid conditions, I do now propose (given my long experience with Hist.sci archons’ capacity for welshing) that prominent, contiguous publication of position papers by both sides should precede the debate by several months (see DIO 2.2 [L1-§4, L8]). A few years ago, Isis-HHA’s A.Van Helden was talking of a possible debate, to astronomer Sam Goldstein. But when I then phoned Van Helden about it, he backed off and said that maybe there could be a debate 15 hence! Van Helden’s Swerdlow-promoting book (DIO 1.7 §5 fn 7) was published at this time by Swerdlow’s Univ Chicago.

125 Hipparchos performed his lunar computations later than his solar work (§N3), so we cannot tell their chronological order from that of the corresponding solar calculations. (Indeed, the reverse-order turns out to be the case: [N3].) But I expect that Hipparchos’ 600 years of computations (mostly of past lunar eclipses’ solar longitudes, naturally) occurred roughly backwards during his career, not forwards. This is our finding here for trios A&B, and it is obviously the most likely order if his eclipse-catalog project wasn’t initially so ambitious as it eventually became.

126 A helpful clue (to the exact original times) is provided by the disparate endings of the trio A intervals (§M3). Those familiar with ancient rounding practice will quickly see what I mean. (DR determined the absolute times for all 3 mideclipses, of both trios A&B, before proceeding with his analysis. I.e., eventual agreements of hypothesis with attested data were not effected by post-hoc re-adjustments of these times.) Curiously, the absolute values of times and longitudes have not previously been induced; e.g., Britton 1967 p.47: “Since the actual longitudes of the sun are impossible to deduce the values of the solar equation [eq:ctr] which would account for the observed discrepancy.” See §G8. [Note added 1993: Reference here is to pp.38-39 of the 1992 edition of Britton.]

127 Hipparchos’ Sham Emerges: Aristarchos’ Lunar Apoage

N1 Thus far, we have dealt almost entirely with Hipparchos’ solar model. We now turn to his syzygial lunar model. As Toomer 1973 (n.10) & Jones 1991HN (nn.20&25) have helpfully & correctly pointed out: the Almajest 4.11, cited times, counted as those used by Hipparchos (in his lunar deductions), lack correction for equation-of-time. (By contrast, Ptolemy rightly uses eq.time for the Moon, though not for other bodies, where the effect is less critical.)
(and the precise confirmatory implication of §114), it is delightful to encounter, in a Pb
paper of the History of Science Society’s Isis, the straightforward, mind-blowing Muffia fantasy that Hipparchos & prior Greek astronomers never computed predictive astronomical tables. (Jones 1991M p.446; see here at §E4. So Hipparchos’ 600-year eclipse table was entirely backwards? He wanted to predict only past eclipses? Well, most of the eclipses in the canons of moderns T.v.Oppolzer and J.Meeus are past ones, but neither stops at his present. Have Muffiosi forgotten that astronomy’s historical pre-eminence has been built upon its predictive power?! See Schmaler at §N16.) Jonestown’s (anti-tabular) conception of ancient astronomy is presumably the sort of special Hist.sci insight which uncomprehending modern scientists are forever denied. Do not lose sight of what is certain: if a frontpage paper in Isis proposes this idea, it’s got to be sober scholarship. Now, a naïve scholar, unfamiliar with Muffia wisdom & determination, might imagine in his innocence that Jones’ revolutionization notion founded upon ancient astrologer Vettius Valens’ explicit attestation to [predictive] Hipparchan solar tables & lunar tables by Hipparchos’ near-contemporary, Apollonios. But these 2 impediments are brushed aside with ease: Jones just implies that Valens’ testimonies may be based on a fake and an identity-confusion. This speculation is assisted by the fact that only one of a vast catalog of Hipparchos works is extant. (Guess who else takes advantage of paucity of early records — to let fly his psychoanalytic speculations on colliding worlds?) So Jones 1991M (p.446, emph added) points to: “the apparent absence of predictable tables in Hipparcuss’s [surviving] work. Only one author, the astrologer Vettius Valens (late second century [AD]), speaks of Hipparchan tables for the sun . . . . [possibly] a later fabrication based perhaps on Hipparchus’s solar theory . . . .” Same suggestion at Jones 1991M (p.102). Note, too, the related & equally-advanced (contra §N16) Jones-Muffia argument (fn 242) that the also-affected lunar tables of Apollonios (c.260 BC) never existed and that the ancient (Valens) reference must be to Apollinarios (an Apollinarios who lived long after Hipparchos), since the name is similar to Apollonios. Just drop the “onios” and add “inarios”. Well, the Muffia may be right. I can’t prove otherwise. 212 But, since Muffiosi routinely slander dissenters from Muffia wisdom (like §G3) as Velikovsky’s mental kin (see idem and DIO 1.1 §7, §C7, §3 §D2), I cannot help recalling Johns Hopkins religion prof W.Albright’s defiant claim, cited by M.Gardner, who expresses it thusly: 213 “Velikovsky’s historical method . . . is on a level with that of the professor who identified ‘Moses’ with ‘Middlebury’ by dropping the ‘-oses’ and adding ‘-idletbury’.” Is it not inspirational to see similarly creative anagrammatism given top billing as bigleague scholarship, in the JHA and in the History of Science Society’s Isis? 214

Presumably, Hipparchos used hired calculators. (Which for the lunar analyses — performed long after the solar calculations — suggests consultation of his own school’s §M7-cited 600’ catalog of mideclipse solar longitudes. This, without availability of — or concern to doublecheck — the data earlier used for the original computations, over several years of work on this catalog. See fn 214.) When these men were originally churning out the trio A solar data, they used the most up-to-date constants (ep & Ap) but found it most convenient to use (§M6) the older EH tables to compute mean longitude motion (since epoch) and equation of center. It was noted above at §M6 that the orbital elements used to

not to 600’ into the future but to the interval from Nabonassar (747 BC) to about the time of Hipparchos. However, the reader is not reminded that gentle Toomer 1967 p.146 called “absurd” P.Tannery’s acceptance that Hipparchos had calculated 600’ eclipses (in either direction). Perhaps we may salvage Toomer’s judgement a bit here by suggesting (as DR has done, throughout this paper) that Hipparchos’ past catalog involved, in many or all cases, merely computing for each eclipse the solar position at an empirically reported time. (Calculating future eclipses required at least a syzygial lunar theory and involved much more work.) 212 However, a common-sense point here: Valens refers to the Apollonios lunar tables along with those of Sinudes & Kidnies (both Babylonian), who were much nearer chronologically to Apollonios & Hipparchos than to the particular late Apollinarios whom Jones&Toomer refer to: Neugebauer 1975 pp.262-263, 306, 574, 601 & n.2, 610-612, 666. [Note added 2018. Doubt that Hipparchos’ authored celestial tables vanished by 2005: Rawlins 2018C fn 86 item [i].] 213 Original from NY Herald Tribune Book Review 1952/4/20, cited & relayed in M.Gardner Fads & Fallacies NYC 1957 ed. p.327.

125 See §D1, §D3, fn 116, §O1, fn 252, §P1.
126 Why worry about chronology, if the prior author is untachable? E.g., on 1982/6/14, DR gave Centaurus ancient astronomy. See K.Moesgaard’s detailed least-squares study of ancient star declinations, the main novel result of which was that Aristyllos — who had until then usually been misdated to c.300 BC, making his declination data supposing (as DR has done, throughout this paper) that Hipparchos’ past catalog involved, in many or all cases, merely computing for each eclipse the solar position at an empirically reported time. (Calculating future eclipses required at least a syzygial lunar theory and involved much more work.) 212 However, a common-sense point here: Valens refers to the Apollonios lunar tables along with those of Sinudes & Kidnies (both Babylonian), who were much nearer chronologically to Apollonios & Hipparchos than to the particular late Apollinarios whom Jones&Toomer refer to: Neugebauer 1975 pp.262-263, 306, 574, 601 & n.2, 610-612, 666. [Note added 2018. Doubt that Hipparchos’ authored celestial tables vanished by 2005: Rawlins 2018C fn 86 item [i].] 213 Original from NY Herald Tribune Book Review 1952/4/20, cited & relayed in M.Gardner Fads & Fallacies NYC 1957 ed. p.327.

125 See §D1, §D3, fn 116, §O1, fn 252, §P1.
126 Why worry about chronology, if the prior author is untachable? E.g., on 1982/6/14, DR gave Centaurus ancient astronomy. See K.Moesgaard’s detailed least-squares study of ancient star declinations, the main novel result of which was that Aristyllos — who had until then usually been misdated to c.300 BC, making his declination data
for the Almajest Mars synodic mean motion. (This solution depends upon an unheard-of monthlength & uses an equally unattested & lunar-parallax-degraded method for finding planet-motions.) Gingerich 1988 passes off as merely “idiosyncratic” DR’s solution for the very same Mars mean motion, but does not make clear to the reader that, while the solution OG prefers is a nonfit, DR’s fits precisely [though false: fn 129] and is based on a simple period relation similar (except heliocentrism in format) that underlying the other Almajest planet mean motions. DR’s period-relation solutions (using mostly Almajest-attested numbers) fit all planets’ synodic mean motions (degrees/day) on the nose (§D4 & fn 78). For 3 of the 5 planets, we find in each case that the degrees (numerator) & days (denominator) whose ratio yields DR’s perfect fit are attested (right in Almajest 9.3) by Ptolemy himself! (Saturn, Venus, & Mercury.) E.g., for Venus, the Almajest 9.3 mean motion = 0.365.25,53.1128 degrees/day. DR’s solution (§D4), previously specifically denied by the math-befuddled Mufa (e.g., Neugebauer 1975 p.157 vs. n.6) is: 180°2/3 (291°2/3 = 540°0/3759/3 = 0.365.25,53,11.28 degrees/day. Moesgaard’s solution: 3265292000/5297443919 = 0.365,25,52,07,12 degrees/day. (Similarly for all other planets: DIO 2.1 §3.C.1. I urge readers to investigate these matters in detail. You will learn much about Ptolemy’s integrity & judgement — and the Mufa’s. I must add that it is a credit to Moesgaard’s ingenuity that his fits are as remarkably good as they are.) Yet Toomer 1984 pp.671-672 actually proposes that not only this perfect match but the same 50billionth-of-a-degree/day precision for all three of these planets (Saturn, Mercury) could be mere coincidences!! Such an instinct for statistics. This from a Springer-Verlag “Editor,” atop BrownU’s Hist.of Math Dep’t. And Harvard’s expert OG backs him: fn 129. New-Frontiers-in-Plasticity Dep’t. The Mufa used to argue: 123 perfect 6-place fits showed Ptolemy got his planet mean motions from observed data, as he consistently stated.124 But then RN showed that all the Mufia-alleged fits were false. This finding, and DR’s flock of perfect 6-place fits, showed that Ptolemy had lied about all the planet mean motions’ origins: DIO 2.1 §3.C. So the new Mufa position of Gingerich 1988 and NSF-funded Swerdlow 1989 p.30 is: perfect 6-place fits are meaninglessly overexact! Which explains why environmentalist DR urges gov’t support for Mufiosso. Sympathy-wise, it’s no different from preserving any other pathetically species of clumsy-but-rare&precious wildlife. Why, if longago gov’ts had looked after the care and feeding of the dodo, it might still be with us.

129 OG has a deep unstated stake in continuing a coverup here (DIO 2.1 §3.C.), [a] to hide his own prominently published, mathematically-misbegotten solution (Gingerich 1981) of the same material [i.e., his difficulty with simple arithmetic], a dent then compounded by [b] attempting to justify his fatal 1983/7/23 suppression of correctly-computed DR planet mean motion solutions. (See full math details at DIO 2.1 §3.C.) Almajest 9.3 Mars motion (degrees/day) = 0.27.41.40.19.20.58. DR’s solution (see Rawlins 1987 p.237 for simple ancestor period-relation): 152145°/329621 days = 0.27.41.40.19.20.58,58 degrees/day. (For all 5 planets’ ancestor period-relations [whose validity is unaffected by the 2003 discoveries], see DIO 2.1 §3.F.) Gingerich’s preferred solution (Moesgaard 1987 pp.46-47) is: 349920000/758089897 days = 0.27.41.40.19.51.55. [NB: After common-factor cancellation, this solution contains numerator & denominator thousands of times larger than DR’s, in order to fit the attested Almajest mean motion thousands of times worse than DR’s! Yet DR’s solution was also historically false: fn 24]. While tabulating no less than four (6- sexuality) versions of this Mars motion, Gingerich nowhere opposes either DR or Moesgaard. As for Moesgaard’s suppression of DR’s time-complex DR’s three completely new & not unquestionably valid solutions (Mercury, Venus, & Saturn), especially since OG’s Harvard colleague Toomer was at this very time preparing to publish them without credit (§D4).)

130 Numerator & denominator given explicitly at Almajest 9.3.


132 DIO 2.1 §3 fn 16.

1991 December DIO 1.3 §9

M4 Solving analytically207 finds that, for the traditional (§K10) H-P value, $c_2 = 227^2/3$ (choice justified by matching lunar positions later on here: §N15), and $y_K = 365^{1/4} (§K9)$, the elements of the precise-fit orbit solution are (fn 205): $c_2 = 3^20^6$, $A_3 = 6^41^3$. Thus, taking into account [i] the trio B solution, [ii] Hipparchos’ known PH orbit elements, [iii] ancient rounding practice, & [iv] the lunar solution to come (eq. 8, also epoch Phil 1), we induce (after some testing & checking) the solar orbit Hipparchos used for trio A:

$$c_2 = 227^2/3$$

mean motion $K_4 = 360^0 (365^{1/4})$, adopted from Kalippos apogee $A_p = 65^0$

$$c_2 = 3^20^6$$

M5 This presents us with an odd but (as we shall see presently, in §M6) surprisingly informative Franklinian208 orbit: half209 of the elements are from EH (§K9); the other half, from PH (§K10)! M6 But the split is not random. We note a revealing correlation: those elements that are associated with tables (namely, $F$ and $c_2$) are EH. Those which are just epoch-constants ($A$ & $A_p$) are PH. Simple explanation: the Hipparchan school’s calculation of the solar positions of eclipse trio A was done during the transition period when Hipparchos was in the process of adopting his PH orbit.210 (Contemporary PH epoch − 145/9/29 noon = Nab 603 or Pot 1 Thoth 1 noon; Rawlins 1985K.) At this point, he had determined the PH elements ($§K10$), but had not yet compiled the necessary associated tables. This has to have been around the PH epoch: 146 BC. Thus, by the luck of happening upon transition-period work, we’ve learned the date of a famous H eclipse calculation. Later (§N3), we’ll find that, by the time he performed the associated Almajest 4,11 lunar calculations, he’d fully adopted (including contingent, prepared lunisolar tables) the PH solar orbit: §N2. This gives us the essential order of the Hipparchos work described at Almajest 4.11.

M7 OK, comic-relief-time again. Thanks to Pliny,211 it is wellknown that Hipparchos computed 6000 of eclipses. In the context of considering this famous tabular monument

207 Some details are provided at fn 205. The math of orbit-fitting (see, e.g., Rawlins & Hammerton 1973A or lunar 1991H1) reminds me of a common alibi for Ptolemy, which is as beloved of the Mufa as it is irrelevant to Ptolemy’s pretensions. Mufa loyalist Peter Huber to DIO: Ptolemy never heard of Gauss. (See DIO 2.1 §2 [§H & §J14].) Well, has the Mufa? It was not DR talent but Mufa innocence — as to how even to compute the EH (or UL) problem — that left DR alone at the portals of the ancient intellectual temples we are now discovering & exploring here: the EH & Franklinian orbits, Aristarchus’ lunisolar elements, and ancient use of the 10000 Astronomical Unit. (My op-ed-expressed gratitude to the Mufa has never been entirely facetious.)

208 The pieced-together Franklinian orbit & A’s integrity were discovered 1991/10/31-11/4. Realization that Hipparchos had (for both trios) taken all but 1 of his lunar elements from previous work occurred 1991/11/3-6. This may at last look ad hoc, but I recommend 3 counter-considerations: [a] See the chronological-correlation discussions at §M6 & §M8. [b] The trio B monthly speeds of Jones 1991H (pp.114-115, 121) are unattested and ludicrously over-precise — down to the arcsec. [c] Jones’ hypothetical Babylonian schemes both require “modified” parameters (p.104; §F4): two unrelated sets of parameters for all 3 trios discussed (see fn 203), and the EH orbit elements are based on foundation-data which may be easily deduced, independently, from Hipparchos’ attested output: §K4-§K5. [d] Jones’ hypothetical Babylonian schemes both require “modified” parameters (p.104; §F4): two unrelated sets of speeds. (See fn 104. The common factors are merely apogee $A$ & the model: half the circle at one consolence, another half for the other.) I.e., the trio A & trio B predictions of Jones 1991H not only lack Babylonian attenuation (other than trio A’s speeds, which lead to 124.4 $M_A = 366$ day hilarity): they have only one common parameter. (Jones 1991H p.113: “There are reasons for doubting whether Hipparchos’s [determinations of trio A & trio B] could have been carried in a single work.”) For contrast: half the 4 elements of DR’s trio A solution are shared with DR’s trio B solution (§M6), and the other half match the attested PH orbit.

210 Jones 1991M p.446: “Hipparchos could scarcely have composed [solar tables] when he wrote his treatises on the lunar heliocentric; for on this stage he had no known value for any of the fundamental periods . . . of the solar model.”

211 Pliny 2.53 called these calculations “predictions”. There is a reasonable interpretation by Neugebauer 1975 pp.319-321 & Toomer 1988 p.355, namely, that the Pliny report of Hipparchos’ 6000 of calculated eclipses referred
data which are the most reasonable choices we could induce for the young Hipparchos (§K).

I.4 But: hold on there! The Editor-for-Life’s extremely infallible JHA, on the advice of a gaggle of Mufa-circle archons, has decreed (Jones 1991H) that no such trig-based Greek-orbit solution is even possible for eclipse trio B; see the irreftutable Mufa analysis quoted above at [G4]. (And note: the JHA co-Editor most directly responsible for publishing Jones 1991H is one whose name is printed as senior author of the extensive orbit computations published in 1983 as vol.59S of the Memoirs of the American Philosophical Society.) So, we must be imagining the foregoing. If Mufosi, the greatest ancient astronomy experts in the history of the universe (just ask them), say it isn’t so, believe them: it ain’t so.

M Frankensteinorbit Meets Trio A

M1 Applying to trio A the same unerringly insightful Mufa strategy we have (§G4) alreadyiggled our way through for trio B, Jones 1991H of course concludes that trio A must also be solved by Babylonian methods. But, I’m such a hard case that I’ll try, yet again, the Greek approach to the Greek, Hipparchos. (No wonder Mufosi snub DR.)

M2 Hipparchos’ given intervals for trio A (Almajest 4.11):

\[
\begin{align*}
A2-1 & : 173^3 - 1^3/8, 177^4 13^3/4 \\
A2-2 & : 175^3 + 1^3/8, 177^0 1^3/2
\end{align*}
\]

M3 Now, whether we’re going Greek or Babylonian here, there is a 1° or 4° discrepancy in the foregoing (vis à vis a 365°/4 calendar), as the slightest testing will show. (See fn 162!) It is precisely by refusing to face this (thus taking the intervals literally) that Jones 1991H (p.112) ends up boxed into the risible conclusion that Hipparchos used a 366° calendar (§G3). Understand, Jones 1991H is a gov’t-funded paper, by a protégé of gov’t-funded Mufosi, experts whom we trust to discriminate between their Received orthodoxy vs. “garbage” (fn 13) produced by “cranks” who extract “money from the government on false pretenses” (DIO 1.1 §3 §D3). To a low thing like DR, who has not yet attained to the lofty plane of Mufa enlightenment, it would instead seem that there is just a 1° error here in the Hipparchos report of one of the intervals, and thus with either eclipse A1 or A3. After some testing, I have opted for the assumption that the problem is with eclipse A3, thus the A3–A2 interval (§M2) must be as (R.Newton already discovered)139 enhanced by 1°. Adopting this correction, our revised solar interval-data are:

\[
\begin{align*}
A2-1 & : 173^3 - 1^3/8, 177^4 13^3/4 \\
A3-2 & : 176^3 + 1^3/8, 177^0 1^3/2
\end{align*}
\]


134 [Note added 1993: consult Dave Barry’s equally-explicit (& equally-ignored) rats at DIO 2.1 §8 §C25.]

135 Numerous graphs in [Mufa 1990] bear two curious errors: [a] inversion of axes, [b] scale-error by a factor of two. (We at failings: Toomer’s fuzziness about others’ editorial’s fn.26c.)

136 The pages fall out of $70$ [Mufa 1990] more easily than from any reputable-firm science book I ever recall encountering. (Who was responsible for this “Springer-Verlag” German-imprint book getting cheaply bound in rural Virginia, USA?)

137 The observations’ purpose has always been obvious: even to Gingerich 1980 p.256. (And see Rawlins 1991H §E6.) The sole evidence Jones adduces to support [the] star-placing hypothesis is Hipparchos’ reference (for 1 of the 3 sights) to the “course” of the Moon in a 248 day table of anomalistic results (see Jones 1983), as if it established that Hipparchos’ prime interest is in lunar speed (as against absolute position). But there is no confirmation of this Mufa speculation.

sole evening observation, the Sun (c. 37° high) was not near the horizon and would not set for c. 31/4 — a delay that would cause needlessly inflated uncertainty in estimating the (nearby 4°) shift between the daytime & nighttime lunar positions (due to longitudinal & parallactic errors in a lunar theory Hipparchos clearly knew to be flawed: §R14). Note that Ptolemy’s illustration of the method (Almajest 7.2) correctly keeps the elapsed time to a minimum: 11/2. (See fn 139.) d) R. Newton 1982 (pp. 646) appears to be the first to note the significance (§R14) of the fact that 2 of these 3 observations (Almajest 5.3 & 5) were performed when the Moon had virtually null longitudinal parallax — a feature which would have (less than) no value for stellar work (where the Moon is used at 2 different positions: fn 139) but would be ideal for correcting the lunar theory — the very purpose jettisoned by Jones 1991M (p. 448).

13 From wherever did Jones get the refreshingly original but distinctly bizarre idea that Hipparchos would use the half-Moon (Almajest 5.3) for stellar observations? (There are several instantly-obvious objections to such procedure.) The lunar mean elongation for the −127 observation (Almajest 5.3)140 is 264°. And [Mufa 1990] (p. 153, in his section 5.4)141 claims that the average lunar elongation for the observations underlying the Ancient Star Catalog is 250° — promoting (also p. 152) the weird idea that Hipparchos cataloged his stars using the Moon when they were far from the Sun. This Springer—Verlag book ([Mufa 1990]) was turned out under the impressive “Editorship” of Mufa cap. G. Toomer, BrownU’s History of Mathematics Dep’t. Despite that exalted advantage, [Mufa 1990]’s 250° result is wildly false, being based upon 3 serious snafus: [a] The curve [Mufa 1990] fit to Catalog star longitudinal errors (Δλ) is so misplaced (to the right) that the bungle is obvious from the remotest glance at the figure. ([Mufa 1990] p. 152–154, fn 155.) Note: the abscissa scale on this figure is a factor of 3. [b] Same for other figures hereabouts.) If the curve to be fitted is taken to be of the form (Rawlins 1982C p. 366, effectively equivalent to [Mufa 1990] p. 152)

\[ \Delta \lambda = z - G \sin(\lambda - \theta) \]  

(1)

(205) Had we proceeded double-differentially here, as at §M10, f3 would have come out = 167° 27′ but this would not have affected the final result at §L3, since φ3 = 164° 47′ approximately rounds to 164° 45′.

(203) For consistency, same rounding procedure is adopted for these calculations as at Rawlins 1991H §D9 — & later here for trio A (§M10). The 3 perfect matches are based on the tiny ancient-convention roundings adopted. (Compare to both of Jones’ extremes: fn 209 item [b]! But, even forgetting the last column, the greatest discrepancy is 1′, which is about the uncertainty attendant to interpolation from an ancient table for the equation-of-center (e.g., that of Almajest 3.6). i.e., all considered, the agreements found in this paper are a good deal closer than are really necessary to establish the propositions broached. See also fn 205. And I would point out the long odds against the neat (ordinmag 1.1) explanation of trio B’s radiant by an orbit (EH) founded upon 6h-rounded data! The same remarkable fit occurred (Rawlins 1991H) for the trio of Almajest 5.3&5. (The a priori odds against these fits remain one of the peculiarities that Ptolemy’s 4h-precision solar loxodromes are founded upon 6h-precision Hipparchos & Meton data: fn 64.) If one alters merely by an eighth of a day (3h) any one of the EH foundations times (~157 VE, SS, AE), then the orbit deduced from these will fail to satisfy the trio B intervals by ordmag 0.1, which would utterly destroy the match.

139 Even Ptolemy, for his alleged Sun-Moon-star astral observation-pair (fn 146, Almajest 7.2), knew enough to allow them to pass down the horizon at odd hours. For this purpose, the seeming advantage of having low lunar parallax (Moon near meridian & zenith) is illusory: all that matters is the magnitude of parallax-shift between the 2 observations — which is generally maximized (but not minimized) by this situation.

140 As clever as Hipparchos’ choosing null parallax: Ptolemy’s lunisolar report (also zero parallax) at Almajest 5.3 is additionally for the rare moment when the eq.ctr for Sun&Moon are at an additive maximum by his simple models.

141 This very section of [Mufa 1990] is cited during discussion of the three Almajest 5.3&5 data: Jones 1991M n.23, which is [a] not original, & [b] already obsolete, as noted at Rawlins 1991H fn 32.

142 [Note added 1993: References here are to pp.41f of the 1992 edition of Britain.]

143 This may also be computed from the numbers supplied at [Mufa 1990] pp. 150-151, so long as the sign error is not repeated.

L3 Next, we compute the solar longitude for each of these times. (Note: all times for computing the Sun’s place may be rounded to the nearest hour — though that’s not necessary for d1 here — because the worst possible error caused thereby will be barely 1°.) The process is, effectively: substitute the time-data just given, along with the elements of §K9, into eqs 2-5. We will start by finding trio B’s f1 from figuring the time d1 elapsed since epoch Phil 1 = 323/11/12 Alex noon (Toomer 1984 p.11, using 1° = 365.25 days = 1 Egyptian year): d1 = 4487557/24 = 122°345°07′. However, when using eq. 5 to find mean longitude, we may easily forget that ancient computation was tabular, not electronic. And ancient tables (e.g., Almajest 3.2) were formed in units of 15°, 1°, 0°, 14°, and 1°. Thus, d1 must be broken into: 108°, 14°, 330°, 15°, 2°. Tables founded upon mean motion Fk (§K9) will provide the corresponding arcs, which we add to (§K9) in order to find trio B’s f3, f2, & eq. of §K9. Thus, for f3 we have: f3 = “333.23” (108°) + “332.15” (14°) + “333.23” (30°) + “1447” (15°) + “017” (2°) + “228” (0°) = “178” 15°. An experienced ancient mathematician would then save labor (see also §N12) by proceeding henceforth merely differentially: just find the tabular mean-motion arc for the time-interval elapsed from eclipse B1 to eclipse B2, stated (§L1, Almajest 4.11) to be 178°06° or 150° + 28° + 6°. Similarly for the interval between eclipses B2&B3. Thus, summing Fk-based tabular entries, we have: f2 = f1 = 147°51′ (150°) + 27°36′ (28°) + 0°15′ (6°) = 175°42′; and f3 = f2 = 147°51′ (150°) + 25°38′ (26°) + 0°02′ (1°) = 173°31′. Collecting & summing our results,202 we have: f1 = 178°15′; f2 = f1 + 175°42′ = 353°57′; f3 = f2 + 173°31′ = 167°28′. From this point, the computation is standard (eq. 4): mean longitude f plus eq-ctr Ek (eq. 3) = true longitude φ, we round to the arcmin as we go, and then express the sum rounded according to ancient custom:203

B1: φ1 = 178°15′ − 2°19′ = 175°56′ = 175°11/12
B2: φ2 = 353°57′ + 2°18′ = 356°15′ = 176°14/4
B3: φ3 = 167°28′ − 2°40′ = 164°48′ = 164°45/4

The longitude intervals B2—B1 and B3—B2 are, then: 180°20′ and 168°33′ — which precisely accord with the stated intervals (Almajest 4.11, Jones 1991H p.106 Table 1 last column, or above at §L1). A gratifying outcome, since the orbit used has been founded independently of the Almajest 4.11 Hipparchan intervals205 — from eqinolost-solair soli.
The ancients took mean solar anomaly \( g \) with respect to the apogee \( A \):

\[
g = f - A \quad \text{so} \quad g_E = f_E - 44^{\circ} \tag{2}
\]

where \( f \) = mean longitude. (Throughout the remainder of this paper, \( A \) will refer to apogee-at-epoch.)

The EH orbit’s equation-of-center \( E_q \) is now determined (see Rawlins 1991H eq. 22):

\[
E = - \arctan \left( \frac{e \cdot \sin g}{\cos g + 1} \right) \quad \text{so} \quad E_E = - \arctan \left( \frac{\sin g_E}{\cos g_E + 240/13} \right) \tag{3}
\]

and the true longitude \( \phi \) is:

\[
\phi = f + E \quad \text{so} \quad \phi_E = f_E - \arctan \left( \frac{\sin g_E}{\cos g_E + 240/13} \right) \tag{4}
\]

where

\[
f = e + F + d \quad \text{so} \quad f_E = 228^{\circ} + 360^{\circ} \cdot (d/365.5)^{1/4} \tag{5}
\]

\((e = \text{mean-longitude-at-epoch}; \ d = \text{days since epoch}). To a precision of about 1', the EH orbit fits the above three proposed foundation-solar-positions. (The appropriate dates are cited at §K6–§K8. The \( d \) are given at fn 201.) Longitudes computed\(^{140}\) from eqs. 2-5 (applied to the elements of §K9) are: \(0^{\circ}01'\) for VE (–157/3/24 1/2), \(90^{\circ}00'\) for SS (–157/6/28 1/4 or Kallippos’ –329/6/28 1/4), \(180^{\circ}01'\) for AE (–157/9/27 1/2). This is a more than satisfactory fit.

I Hipparchos’ Eclipse Trio B Reveals His Early Solar Orbit

L1 Armed with the EH orbit (trig), we now return to consider the supposedly-impossible (§G4) task of finding a Greek-style (trig) solar orbit that will explain the solar-position-intervals Hipparchos used for his analysis of eclipse trio B (Almajest 4.11). Will the EH orbit do the trick? The intervals to be satisfied are (Almajest 4.11):

\[
\begin{align*}
\text{B2} – \text{B1} & : 180^{\circ}20', 178^{\circ}06'' \\
\text{B3} – \text{B2} & : 168^{\circ}33', 176^{\circ}01''/13
\end{align*}
\]

First, we find from these two time-intervals (§L1) and from the Almajest 4.11 discussion, the absolute times Hipparchos probably used for the 3 eclipses: B1, B2, B3. These times are:

\[
\begin{align*}
\text{B1: Nab 547 Mesore [12] 16 07'^{h} & = -200/09/22-23 19^{h} \\
\text{B2: Nab 548 Mechir [06] 09 13'^{h} & = -199/03/19-20 01^{h} \\
\text{B3: Nab 548 Mesore [05] 14 13'^{h} & = -199/09/11-12 02^{1/3} \\
\end{align*}
\]

party who altered the original. \( A = 65^{\circ} \) to \( A = 65^{\circ}/2 \). Note: both here-reconstructed \( A \) values of Hipparchos (EH & UH) are in whole degrees, which is reasonable, given the looseness of \( A \)’s empirical determination. (Ptolemy’s sometimes amusing overprecision is also evident at Almajest 3.1 & 4.7.) I note that the math of Neugebauer 1975 pp.58, accini\(^{2d} 16\’ 20''/30' \), yields \( 65^{\circ} \), not \( 65^{\circ}/2 \) as Ptolemy gets from arccos \( (10^{d} 20' 30''/0') \). Without computationally checking a thing, Neugebauer 1975 p.58 simply forced his result to be the “right” answer, anyway — i.e., Ptolemy’s 65°1/2. For similar Mufffiosi ineptitude, see fn 38. Mufffiosi are unarguably the perfect cult to defend astronomy’s most notorious faker.

\( A \)’s mean-longitude at epoch is constant, while the lunar apogee moves at a constant speed.\(^{140}\)

\( A \)’s mean-longitude at epoch (see Ph½ 1 = –323/11/12 noon, the \( d \) are for the (–157 orbit-foundation dates): \( d_1 = 60398d \) (VE), \( d_2 = 60493d/34 \) (VS), \( d_3 = 60585d \) (AE).

\[\text{[Notes added 1992&1993: Just when you finally think Mufffiosi math can’t get any funnier, those lovable imps pop right up and restore confidence in their irrepressible creativity. In the 1992/2 number of the extremely handsome Journal for the History of Astronomy, there is a memorable review of [Mufa 1990] by James Evans (JHA 23.1-2). Evans &[Mufa 1990] are the Mufa’s top experts on the Ancient Star Catalog, which RN-DR have shown (1976-1979) was faked by Ptolemy. Evans 1987 attacks RN-DR by saying it wasn’t faked, while [Mufa 1990] attacks RN-DR by saying it was faked. Which creates a bit of a problem for reviewer Evans — but he manfully overcomes it by: [a] half-saying (p.67) he doesn’t agree with [Mufa 1990]’s charge, while [b] half-implying (p.66) he’s always somewhat agreed with it. Regarding the Catalog controversy, Evans manages to cite Peters, Knobel, Vogt, Nadal, & Brunet (who did not prove Hipparchos’ authorship) — but not RN or DR (who did). (The L Hipparchos’ Eclipse Trio B Reveals His Early Solar Orbit)

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14. The astrolabe is described at Almajest 5.1; its use for placing stars is explained at Almajest 7.2 for a 139/2/23 sunset lunisolar observation of Regulus, though as noted at Rawlins 1982C p.373 & Rawlins 1991H §F-5 (Sent to prominent scholars in 1977 and 1986, respectively — well before [Mufa 1990]. Each of the DR papers presents a reasonable and accurately-computed Sun & star error-curve phase-relation, while [Mufa 1990] and thus Evans 1992 both have this relation fantastically wrong. The discovery of this relation is completed by Rawlins 1991H, and the fit is a felicitous value.) Evans can hardly claim ignorance of Rawlins 1982C since, in the old-reliable JHA. Evans 1987 spent 64 pp in a wandering wander-attempt to denigrate other parts of Rawlins 1982C. And so we have yet another (weirdly ironic) case where Hist.sci has given some lucky chap credit for a DR discovery. Shame on DIO (Rawlins 1991H in 4) for calling the Mufffiosi ungenerous. . .

15. Wlodarczyk 1987 also provides his (eqs. 5-6) correctly-signed-formulas for the solar error-curve. [Mufa 1990] (p.336) cites Evans 1987, an article which is immediately followed by Wlodarczyk 1987 (on the same subject). If [Mufa 1990] read Evans 1987, how did they miss these formulas? [Notes added 1992&1993: Just when you finally think Mufffiosi math can’t get any funnier, those lovable imps pop right up and restore confidence in their irrepressible creativity. In the 1992/2 number of the extremely handsome Journal for the History of Astronomy, there is a memorable review of [Mufa 1990] by James Evans (JHA 23.1-2). Evans &[Mufa 1990] are the Mufa’s top experts on the Ancient Star Catalog, which RN-DR have shown (1976–1979) was faked by Ptolemy. Evans 1987 attacks RN-DR by saying it wasn’t faked, while [Mufa 1990] attacks RN-DR by saying it was faked. Which creates a bit of a problem for reviewer Evans — but he manfully overcomes it by: [a] half-saying (p.67) he doesn’t agree with [Mufa 1990]’s charge, while [b] half-implying (p.66) he’s always somewhat agreed with it. Regarding the Catalog controversy, Evans manages to cite Peters, Knobel, Vogt, Nadal, & Brunet (who did not prove Hipparchos’ authorship) — but not RN or DR (who did). (The L Hipparchos’ Eclipse Trio B Reveals His Early Solar Orbit)

I5 So, the question suggests itself: did Toomer—“Edited” [Muffia 1990] actually read Rawlins 1982C? — and, if not, how did the paper Rawlins 1982C end up: [a] having its conclusion rejected out of hand, and [b] being cited & listed in the bibliography of [Muffia 1990] (p.340)? Since we have (§I4) just been wondering how [Muffia 1990] read Rawlins 1982C without learning the correct solar-error phase, we may also ask how he managed to mis-speak the name of the journal in which Rawlins 1982C was published. In the footnote ([Muffia 1990] p.167 n.42, which instead mis-speells DR’s name)99 where [Muffia 1990] aschans the central new crucial experiment of Rawlins 1982C, it is curious that the only papers cited are secondary & n.publication (1979–1981) discussions of the paper’s argument, not Rawlins 1982C itself.150 But: are we to believe that a $70 Springer­Verlag book, “Edited” by no less than Mufa capo G.Toomer, would produce a partly faked bibliography? Unthinkable. Especially when we recall the immortal words of the Muffia’s very own bibliography ethics-monitor, Noel C. Swerdlow, who (faulsly)151 believed he had apprehended a far less serious bibliographical slip by R.Newton. Swerdlow 1979 (p.528): “it is best to be clear about one’s conduct, especially” when discussing matters of fraud.

I6 The test (of the Catalog’s authorship) invented by Rawlins 1982C is simple: a gross −1°1.1 mean longitudinal mis-set (which all parties agree infects Potlemy’s star catalog), of the Catalog-observer’s armillary astrolabe, would produce error-waves (amplitude 1°1/2) throughout the Catalog’s latitudes152 & northern longitudes. But least-squares investigations

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period from −157 to −146 (the early part of Hipparchos’ career, when his adopted solar orbit has previously been known to us), let us take the last (−157) of the 3 early AE data as Hipparchos’ AE standard. (The −158 & −157 AE’s agree.) According to Almajest 3.1, this AE (§K4 item [a]) was stated by Hipparchos to have been carefully observed as occurring at −157/9/27 1/2 (noon).

K7 The −145/3/24 Alexandria VE cited at Almajest 3.1 is reported to have occurred at 11 AM. The 12’ interval between this date and the −157/3/24 VE is divisible by 4, so (given that the year’s length is c.365½/4) the Alexandria krikos observation of the −157 VE would be made under similar geometric conditions, thus it should have been recorded at roughly the same time (likely c.1 PM). Hipparchos (who always rounded such data calendrically to the nearest 1°½) would express this as noon. Thus, we have reconstructed Hipparchos’ other equinox datum ( Hij: 365½): −157/3/24 1/2.

K8 Finally, the interval from −157 back to Kallippos’ = 329/6/28 1/4 epoch is 172’; this is easy to see that, if Hipparchos used the Kallippos calendric Summer Solstice (§K4 item [c]), it would have been −157/6/28 1/4, a quite inaccurate time. (See §K4 for Kallippos’ calendric year and epoch.)

K9 From these data, we have: Spring = 95°3/4, Summer = 91°1/4. It remains now merely to solve for the orbit which satisfies the 3 data of §K6–§K8. The math method is set forth in Almajest 3.4. (It is available, in modern dress, at Neugebauer 1975 p.58 or Rawlins 1991H §C6f.) The resulting orbital elements are given below (for Hipparchos’ chosen prepublication (1979–1981) discussions of the other equinox datum (§K4 item [b]):

\[
\begin{align*}
\text{mean-longitude-at-epoch} & = 228^\circ \\
\text{mean motion} & = 360^\circ/(365³/4) \\
\text{eccentricity} & = 3 \text{p} 1/4 \\
\end{align*}
\]

K10 These elements are rounded much as one expects of ancient work (Rawlins 1985K).


\[
\begin{align*}
\text{mean-longitude-at-epoch} & = 227°2/3 \\
\text{mean motion} & = 360^\circ/(365³/4 – 1/300) \\
\text{apoapse} & = 65^\circ \\
\text{eccentricity} & = 2p/1/2
\end{align*}
\]

gnomon for solar altitude, and adoption of a polestars-based georg latitude L, would explain most of these three AE errors — as well as how the gnomon-measured S.Solstice zenith distance was subtracted from this L to find the obliquity. (It appears that adopted L was low by a few arcmin.) Gnomon-use indicates that Hipparchos’ early empirical work was subprofessional. The suggestion here is that the first Hipparchan obliquity, 23°5⁵⁷, was measured by the same party (probably himself) who observed the 162-158 BC equinox data. DR has long proposed (Rawlins 1982C). Evans 1987 p.251 makes it 8½cosL, since his latitude error function \( \Delta L = 0° \pm 31 \cos(L + 63°) \) breaks down into \( \pm 17\sin(L) \). Nonetheless, Evans 1987 p.252 has the nerve to attempt applying −17½sinL against 29cosL, blithely noting oh-by-the-way that “the phase is not exactly right”. I.e., a phase difference of sixty-three degrees equals “not exactly” in phase! (See the JHA’s editorship’s near-fainting conniption at another journal’s “scandalously” low referring standards: in §F. The JHA show is a banner at $126/year.) For Hipparchos’
do not find these waves. A concurrent alibi-inspiration by O.Gingerich (to evade a different 
Newtown argument) caused OG to suggest that perhaps Ptolemy (insanely) observed the 
Catalog using 233 Hipparchos’ obsolete longitudes (mean 137 AD error: 3°34’ instead of 1°1.) 
for his principal stars — and then later Ptolemy computationally added 2°2’ of 
prediction onto all the thousand-plus stars! Not only inexplicably circuitous & bloop-
walking but: the error-waves produced this way would then be over 3 times larger (than for 
the conventional scenario of Rawlins 1982C). About 1°2’3/2 in amplitude! (Assented to at 
Evans 1987 p.251.) These waves’ entailment is demonstrated, with a clarity impossible for 
even a highschooler to misunderstand, by R.Newton 1979F pp.389-390. This is cited at 
[Mufa 1990] p.167 where he states that such an error (which [Mufa 1990] does not even 
quantify!) is “SO SMALL for both coordinates [longitude & latitude?] that it cannot be 
significantly tested” (caps added). The wellknown (so-called) Catalog rms errors σr, σl 
[Mufa 1990] p.80) are about 1°3/3; it’s in this context that [Mufa 1990] claims waves of 
amplitude 1°2’3/2 (five times larger than σr) are too “small” to detect! 
[Note added 1993: similar if less egregious NCS [slip] at J.HA 2.3 (f2 in 3.1.)] 
[Mufa 1990] adds (n.42) the comment: “We cannot follow contrary claims by Rawlings [ sic].” 
Well, when such a master of least squares analysis (§13) junks a least squares 
entailment, the authority-approvability deprivation, the involuntary chortling. Little wonder Mufa “Editor” 
Toomer rushed this dandy to press. (The absent-error-waves test is so simple a disproof of 
Ptolemy’s authorship of the Catalog that it can only be evaded by deception. [Note 
added 1993: See DIO-3.1.] The Mufa seems unaware that continued 
resistance, when proof is certain, leads only to ethical self-evisceration, with long-term 
costs far more lethal than the short-term face-loss Mufosi so frantically fear.)

17 Disoriented readers, perhaps unfamiliar with Mufia fairytale, are urged to recall our 
earlier account (§1) of astonishingly elastic Mufiosi flips & springsaults over the Star 
Catalog issue. It may all come down to this: Mufia resistance to admitting Ptolemy’s Catalog 
theft (a theft asserted for years by R.Newton & DR) was becoming a laughingstock among 
knowledgeable scholars. So G.Toomer was relieved to escape from his predicament by: 
[a] finding a different argument for Ptolemy’s plagiarism ([Mufia 1990]), [b] publishing 
this, littered with attacks on R.Newton’s & DR’s proofs of the same proposition, & [c] now 
suddenly switching criteria and claiming plagiarism isn’t wrong! (Not even Ptolemy had 
the gall154 to try that one.) Mufia 1990 can’t attack R.Newton often enough. In just 

1989, Rawlins 1982C p.367 found \( \Delta \delta \approx 9°13’ \approx 13 \sin^2 \alpha \). The 9° cos-component (explained at Rawlins 1982C fn 16; 
see also Rawlins 1991H [G&K & Wlodarczyk 1987 p.183] is nowhere near the 29° 
needed to exculpate Ptolemy. The sin-component (which, as noted, Evans attempts to mixup with the cos-component!) is obviously due mostly to 
the young Hipparchos’ well-known obliquity error: Hipparchos 1:12 testifies to his adopted obliquity being near 23°51’20”, 
which was 9° high for that time. Subtracting 9° from 13° leaves a sin-component discrepancy of merely 4°. (DR nulls 
this by presenting a variety of evidence suggesting that Hipparchos’ 1st adopted obliquity was 23°55’. Rawlins 1982C p.368.) While Evans 1987 wanders all over town trying to explain away alleged mysteries of the Catalog, he fails to adopt the obliquity (single-bead) that Ptolemy plagiarized the whole Catalog leaves very little error that even needs explaining away. [Note added 1992/12: The Peters latitude error curve is 
not a pure sinusoid, as remarked by NCsWerdlow at J.HA 25.3:176. If one assumes that this is due to the periodic 
error of the observer’s adopted solar theory (with the usual thin waxing crescent stepping-stone Moon: in 138), then 
the best-fit periodic error curve would be far more likely to be truly virtual for that of Ptolemy’s solar orbit. (The effect is small but, formally 
at least, it is statistically significant.) (Kallippos’ was transmitted cryptographically — “call us up” — 
in our naturally-unmutual challenge to the Mufia’s mathematicians, published at DIO 2.3 [f 8] C16. To solve this orbit 
problem, I was hoping that Hipparchos might have been using an empirical orbit. (Velikovsky was also famously gifted in the 
Revolutionary-Singer Parodies [e.g., A.Lord’s book].) 153] Note the implicit OG admission that Ptolemy’s (undeniably ghastly) fundamental astronomy must be strictly 
derivative — though Ptolemy’s pretends (Almajest 3.1, 7-2.4) to have done firsthand fundamental astronomy. So this defense of Ptolemy against the charge of plagiarism is little more than: lawyerly pleading his client guilty to a different 
sin.

154 Letter to DIO from Muffia-associate P.Huber (DIO 2.1 [f 313].) “Customs can vary widely. Compare for example our current attitudes with regard to copyright and plagiarism to those prevailing among medieval authors 
and, more close to our days, among Singers of Tales [e.g., A.Lord’s book].”
There is much of the comic in the Toomer-[Mufa 1990] ploy’s clumsy transparency, its politically correct intolerances, its amazing mismatch. But there is also the less humorous question: what sort of scholars evade acknowledging the force of dissenters’ prior proofs of a proposition (in this case, Toлемy’s Catalog plagiarism) — while suddenly, belatedly, and only bestowing upon themselves ALL of the credit152 for proving that very proposition? (See §H2 option [f].) And [Mufa 1990] is additionally saying ([Mufa 1990] p.215), with the evident approval of Springer’s “Editor” Toomer, that Toлемy’s plagiarizing one of our oldest astronomical heritages (Hipparchos’ 1025-star catalog), shows Toлемy’s “methodological progress”19

Evolution in action (fn 72): on the Star Catalog, the original Mufa position #1 was denial (fn 66, §1) that plagiarism had occurred. Mufa-convincus-alibi #2 was that Toлемy’s forgery was normal ancient science: see Sci Amer 1979/3. When this was disproved at Rawlins 1987 n.12 (& see DIO 1.1 fn 24), the new tack was amnesiac: forget

H3, H42 at certain scholars’ (neatly selective) receptivity to the notion that condemnation of plagiarism is just a trivial modern affectation. (One doubts these alibi-fonts would yawn-awhite of their own works!) I have called this (unadventurous) false racism. (E.g., AA 1990/10/22.) For the actual ancient situation, see Pliny’s (77 AD) condemnation of plagiarism as “theft” (Rawlins 1982/4 n.4 or DIO 1.1 §1 [B1]); also, Synesios (to Herculan, c.400 AD; A.Fitzgerald 1926 ed, p.238), “it is much greater sacrilege to steal the verses of the dead than to steal their garments, a thing called grave-robbing." (And almost 4 centuries ago, Tycho called Toлемy’s plagiarism “usurpation”: DIO 2.1 §4 [C1].) As for alleged higher modern standards (we were discussing gaffe?): [a] The public speeches of every modern US President are read (usually off deliberately-invisible idiot-boards) from prepared written copy of “speechwriters.” [b] TV ‘news anchors operate similarly. (A practice betrayed by their eyes’ tiny horizontal oscillations.) [c] The only US person, to whom a US national holiday is dedicated, faked his PhD dissertation by systematic plagiarism. (Detailed textual comparisons: Wall Str J 1990/119, NYT & Wash Post 1990/11/10. Memory-Holed since. The degree & holiday still stand. Thus, I see no basis for ritualistically claiming that condemning Toлемy’s plagiarism betrays an anachronistic ethic (fn 36 & DIO 1.1 §7 [G3]). Unless the point is: modernity is about how we are, which is why they fake such ludicrously stupid and bases one key counterargument ([Mufa 1990] p.163 item ii) on this understanding of RN. But the truth is that R.Newton 1977 merely considers this option (hypothetically) for a moment (p.246), before presenting (p.247) evidence of whole-degree division, which he then adopts in all his discussions: R.Newton 1977 (pp.247, 252, 255) and (cited at [Mufa 1990] p 167 n.42) R.Newton 1979F. After composing his attacks on his own half-degree straw man, [Mufa 1990] had the shock of encountering the truth at R.Newton 1977 p.252; so, instead of dropping these attacks, he simply rejected what his eyes read, calling the disclaimer Toлемy’s “internal inconsistency” caused by a “slip”! (No other scholar, e.g., K.Meesgaard, C.Wilson, O.Gingerich, etc. has ever had the slightest difficulty in knowing what RN meant here. See, e.g., Thoren 1990 p.155.) And this $70 book is carefully “Edited” by G.Toomer (who agrees that R.Newton is a relapsively unreliable scholar). Springer-Verlag, these are your experts.

See also DIO 1.3 §9. Toomer corresponds on Velikovsky but has refused to communicate with the RN-DR axis (Gillispec to DR 1978/7/6). See also DIO 1.1 §3 in 7.155

155 In the Small-Understanding dept. [Mufa 1990] repeatedly speaks (pp.85, 88, & 162) of R.Newton’s critical fractional-degrees argument as presuming that the Catalog observer’s astrolabe ring was graduated in half-degrees — and bases one key counterargument ([Mufa 1990] p.163 item ii) on this understanding of RN. But the truth is that R.Newton 1977 merely considers this option (hypothetically) for a moment (p.246), before presenting (p.247) evidence of whole-degree division, which he then adopts in all his discussions: R.Newton 1977 (pp.247, 252, 255) and (cited at [Mufa 1990] p.167 n.42) R.Newton 1979F. After composing his attacks on his own half-degree straw man, [Mufa 1990] had the shock of encountering the truth at R.Newton 1977 p.252; so, instead of dropping these attacks, he simply rejected what his eyes read, calling the disclaimer Toлемy’s “internal inconsistency” caused by a “slip”? (No other scholar, e.g., K.Meesgaard, C.Wilson, O.Gingerich, etc. has ever had the slightest difficulty in knowing what RN meant here. See, e.g., Thoren 1990 p.155.) And this $70 book is carefully “Edited” by G.Toomer (who agrees that R.Newton is a relapsively unreliable scholar). Springer-Verlag, these are your experts.

K Old Turkey: the Mystery of Hipparchos’ Roots

K1 Since 1979, DR has proposed that Toлемy’s amazingly large +2° error in the L of Byzanzion (modern Istanbul) was initiated by Toлемy’s use of a primitive solstitial gnomon. (This instrument consistently misleads the observer, but only by about ±1/4 in deduced altitude.) The gnomon was most probably (fn 195) the instrument used to record the systematically late 162-158 BC Autumn Equinocs reported by Hipparchos. (Each of these equinocies was late by a fraction of a day. But an observer whose latitude was off by 2° would be expected to record an equinox wrong by c.5°.) Therefore: these 162-158 BC A.Equinoxes were not observed in Byzanzion. (Otherwise, Hipparchos would have gotten the L there correct within about 1/4°, just like the equinocies.)

K2 Since 1979, DR has proposed that Toлемy’s approximately large +2° error in the L of Byzanzion (modern Istanbul) was initiated by Toлемy’s use of a primitive solstitial gnomon. This value is actually attested for Byzanzion (Neugebauer 1975 p.983); and, by ancient sph trig formulas,136 this would lead to longest-day M = 15°1/4 and L = 40°4/5. (Actual Byzanzion L = 41°1/0’.) Hipparchos’ near-native city, Nicaea (modern Iznik), is listed with M = 15°1/4 & L = 41°11/12 (Geogr Dir 8.17.7 & 5.1.14, resp; Rawlins 1985G p.262). (Actual Nicaea L = 40°26’; error = +0°29′.) These considerations suggest that Hipparchos never advanced beyond horizontal astronomical observations after leaving Byzanzion. And this almost has to be the case, since the slightly vertical instrument used by Hipparchos would have revealed his 2° error in L.

As for the 162-158 BC equinox trio (so inconsistent with this §K2 error): Muffosi doubt that this trio was observed by Hipparchos. However, [a] Hipparchos says (Almajest 3.1) that they were observed with great care.188

[b] They are uncited to another authority. So, I will suppose (very uncertainly) that they are his own observations, made with a gnomon — before he moved up to a transit circle (used for the equinox observations made from 147 BC to 128 BC: Almajest 3.1). If the 162-158 data are his, then ([K1] Hipparchos had left Byzanzion by 162 BC. Given the long, data-bare 11° break between 158 BC & 147 BC, one may speculate that the 162-158 BC trio was taken by Hipparchos (mainly for calendric 135

184 Neugebauer 1975 p.275 speaks of Hipparchos’ observations in Byzanzion (Heiberg 1907 p.67), but these are mere weather astrology. They are not evidence that Hipparchos made any serious astronomical observations in Byzanzion. See Neugebauer 1975 pp.227 & 298.

185 The standard ancient vertical gnomon will read the solar altitude high by c.16°, the solar semidiameter. This will affect an equinox time by about 2/3 of a day. See, e.g., Malmistris 1912-3:1-419-420 or Rawlins 1982G.

186 Neugebauer 1975 p.57 eqs. 4a, 5a, and-or 6. For probable Byzanzion interest in orthive amplitudes at about Hipparchos’ time, see Pliny’s (77 AD) condemnation of plagiarism as “theft” (Rawlins 1982C n.4 or Thoren 1990 p.155.) And this $70 book is carefully “Edited” by G.Toomer (who agrees that R.Newton is a relapsively unreliable scholar). Springer-Verlag, these are your experts.

187 Toomer 1980 p.103 fairly notes that there is no proof, but stresses that, besides the early AE trio, there are no Hipparchos observations known from that time. See also Toomer 1978H p.208, Toomer 1984 p.133 n.7, B.Goldstein & Bowen 1989 p.289 n.1, & Sarton 1959 p.285.

188 The remarkable classicist-turned-astronomer J.Fotheringham stresses this hint. And Neugebauer 1975 (p.276 n.20) sneers at him for doing so.
Carrying the foregoing NCS garbage-test corpus-rejection criterion (§15) to still further ironic heights: N.C. Swerdlow himself has made a false imputation of fraud against R. Newton, based not just upon error but upon the creative Swerdlow’s own error (fn 169). (We have elsewhere displayed NCS’ equally uplifting excursion into neatly-forced math: DIO 1.1 §5 fn 7.) So, do we yet again call back and now finally herniate Swerdlow’s frazzled garbageman — saddling him with NCS’ own entire hefty output? I emphasize that NCS is proud Hist.sci’s idea of Good News: its very finest ancient-astronomy-history scholar. (The Bad News? He probably is.)

A final comment on the Jonestown affair: Isis has published (§113) a lengthy, highly detailed (partly valid) criticism by another scholar on the accurately computed math of a single (noncentral) aspect of one paragraph of Rawlins 1982G. Is Isis thus obliged to publish a comparably extensive correction — by detector DR — of the (central) errors of Jones 1991M? (See inside back-cover DIO statement: this JHA 1.2-DIO 1.3 analysis is hereby submitted to Isis, with no editorial constraints whatever.) Somehow, I doubt Isis will so conclude. For the archon-angels above: double norms are the single norm.

JHA referee with 

Prediction: DR’s results here (§K-§O), though founded on valid math (replacing the Mufa’s own hilarious botches) will be automatically rejected by the Neugebauer-Mufa — simply & entirely because the findings constitute a discovery made by a scholar whom the Mufa loathes. Such behavior I have come to expect, for I know from long experience the cohesiveness & integrity of those who now dominate this Hist.sci area. But, let’s not permit an Ivy League buffoon-union’s antics to distract us from the part of this problem which cannot be answered (§112 items [1]&[2]) and will thus be most hysterically evaded.

Notice that there are really 4 separate questions here:

[1] Is DR correct in pointing out that the Mufa’s Jones 1991H was dead wrong in claiming (in the 1991/5 JHA’s Pb paper) that a trig-based solar theory could not be fitted to trio B?

[2] Is it not immediately obvious that the Jones 1991H claim of impossibility is false?

[3] Has DR provided a tightly-fitting and mathematically correct Greek-astronomy-style explanation (EH orbit) for the very Greek-astronomy problem which Mufa capo Toomer has twice (fn 63) publicly called “inexplicable”?

[4] Is DR correct in proposing that this tight-fit & trig-based EH theory (§K9) was actually used by Hipparchos?

Now, as with any controversial & novel historical discovery, there will of course be some disagreement (at least for awhile) about item [4]. Though there’ll be no dissent whatever in certain quarters: the Mufa’s genii will (as just noted: §111) unquestionably consign item [4] to the stake, sight unseen. But I ask that observers of the Ptolemy Controversy not be distracted (by that unconditionally pre-ordained roast) from keeping careful watch on the JHA-Mufa assertion of impossibility (upon which the entire JHA paper, Jones 1991H, is founded) is self-evidently false. In this situation, proper academic procedure

\[ e = 3 \]
requires a printed retraction-correction — regarding items [1] & [2], with credit to and published input from DR and DIO (see DIO inside back-cover), who first publicly pointed out the fallacy underlying the central contentions of the Jones papers, to which both JHA & Isis gave such upfront prominence — for political reasons, not because of fair & capable refereeing.

A note on Hist.sci's world-renowned Bureau of Double Standards (see also fn 104 & fn 183): when in 1983 DR found that an entire JHA paper was based on a mistake, the erring author's prompt retraction was (after much delay) published by JHA (see Rawlin's 1991H fn 15). But when critical comments (later appearing in Isis 74:556-561; 1983/12) upon one paragraph of DR's valid Isis paper (Rawlin's 1982G) were received by Isis, these were immediately approved for first-hand publication before DR was informed of anything — less much offered a chance to retract, had that been appropriate. (And the original DR paper had been secretly sent to the Mufia by Isis without DR's permission.) These critics (spun off from one DR paragraph) ran 6 pp (see fn 96) — about as long as the original DR article! And DR was asked by Isis to reply within 1 month, in more than 250 words — and was pointedly advised not to reply at all (Isis letter of 1983/1/28). The brass is monumental.

The next time Isis had the hideous misfortune (DIO 1.1 §1 §9) to have a DR paper (on Tolemy's 2 clumsily contradictory fake observations of Venus' 136 AD max elongation: see Rawlin 1987 p.236 item [4]) approved by eminent refs (C.Wilson, K.Moesgaard) for Isis publication, the journal now (Isis 1983/12/20 letter): [a] demanded excision of all substantial criticism of N.C.Swerdlow (note fn 269), [b] asked that I also not submit to Isis "in the immediate future" (whatever that means) any other papers on "specialized" subject of "ancient astronomy" (a ban that now fails to apply to both B.Goldstein & Bowen 1991 and Goldstein's friend Jones 1991M even though both have ready access to a flock of their own clique's captive journals, from which DR is banned), and [c] announced that DR would get no reply space if his proposed article were attacked164 (I don't know whether I'm just super-subtle or what; but, somehow, I got a glimpse of a hint of a vaaaague impression that Isis wasn't exactly breathless-anxious165 to print the DR paper. So, I opted to pass up this inviting publication-opportunity. Nonetheless, the paper is cited at R.Newton 1985 pp.10, 261.) Such is the state of Hist.sci's leading journals. But I cannot complain, since too of the central equations166 discovered by the 1983 DR

163 One takes it for granted that, if the Mufa ever alters its opinion on this matter, DIO will receive neither credit nor even mention. I would point out to the Mufa (what I have already impressed upon Sky&Telescope): there is no need for peace-feelers or whatever that 2 noncommunicating parties, as a precondition for each citing the other fairly. A scholar of integrity can treat other scholars honestly, even if he cannot abide them personally. I might add that my acquaintance (since c.1984) of involvement with Mufosis is not due to personal animosity or snobishness (I leave such degrading games entirely to the Mufa) — but, rather, to repeated experiences of the sort detailed in DIO 1.1 (at, e.g., §1 §11, C7, C12, §3 fn 7, §6 fn 35). Mufa-ridden journals are governed by the social notion that publishing original research is not an obligation but a favor, bestowed only upon “trustworthy” parties. (Pragmatists' inspirational moto: "A man who can’t be bribed, can’t be trusted.") One must sympathize with OG's frustration at fn 9.

164 Does it tell us something about the state of Hist.sci journalism that research demonstrably worthy of NSF & MacArthur Foundation awards would be greeted in such a get-lost manner? The 1983 incident has accidentally provided us a neat controlled test: the equations are those of the later, NSF&MacArthur-supported paper Swerdlow 1989 (equations DR already submitted to Isis in 1983) — but when we alter the name's authorship, the Mufa's name, to that of someone socially acceptable in Hist.sci, we find that the very same equations treated as an imposition-bother in 1983 become attractive and award-winning in 1989.

I had already just been through a Hist.sci journal double-cross (DIO 1.1 §1 fn 25) at JHA regarding Rawlin's 1999, which Lord Harington was loathe to publish (a point not only self-evident but directly verified by JHA co-Editor E.O. Muenzer, 1983/66), despite JHA referee-recommendation and even a published JHA statement of acceptance (Isis 73:158; 1982/3). When a journal of this stripe does not wish to publish a paper, it will delay, snip, and generate impedimenta at will. The wise author will not go down that road in the first place. (The DIO inside-back-cover's standing offer to JHA, Isis, et al., has been designed to assure that DR will never again become ensnared in editorial wrangling with actors.)

165 Compare Swerdlow 1989 p.43 to p.7 of the DR ms sent for publication to Isis 1983/8/12. These equations show that certain Venus fakes were not even out of the Almagest tables but were just from highschool-level trig. (The stellar fakes, like the solar foins of 6A, were more primitive yet: mere gradeschool-level arithmet; see R.Newton 1989 December Journal for Hysterical Astronomy 1.2 §9

J3 As if the foregoing weren't grand enough: Hist.sci's Isis is as proud as punchy to run under the History of Science Society's choice for the very FIRST Isis article EVER to run under the University of Chicago imprint? The perfect-pick Pb-paper: Jones 1991M.

J4 In fairness to Jones, we should, however, note that his recent work, “Ptolemy's First Commentator” (Jones 1990), establishes a valuable179 first full translation of an obscure early Greek text on Ptolemy. Jones 1990 also makes an erudite case (following A.Rome & O.Neugebauer) for early diffusion of Ptolemy's works (much earlier than that proposed, e.g., by Rawlin 1984A p.983). Jones 1990 has value regardless of the precise correctness of its title. And Jones must be correct on the date, as well. However, his early 3rd century AD dating of the ancient text depends almost entirely upon assuming (Jones 1990 p.3 n.7) that a 213/4/24 midnight horoscope computed therein is contemporary. Now, this horoscope may indeed have marked an event in Caracalla's reign (211-217). But most horoscopes are for birth dates; and this could well be a natal horoscope for a mature, even elderly person. So, the horoscope might originally have been computed as late180 as c.300 AD. Two other matters are worth note.

[a] Though skeptical, I am not rejecting outright the Mufalia date for this document; but I suggest the comparison-thought-experiment of imagining the Mufa's derisive reaction if “RN-DR titularly concluded for anything this soft."


J5 With item [b], we are again reminded of N.C.Swerdlow's attack upon R.Newton for (on a single occasion) reaching a dubious skeptical verdict — which also turned out to be based merely upon a scribal-slip (fn 169). NCS has (in a journal whose Ed.Board included Mr.Nice-Guy, archon-angel O.Gingerich) used such trifling material to suggest that all of RN's ancient work is "garbage": fn 13, DIO 1.1 §3 fn 3 (& §D3). So: do we now also turn over Mufa-don Neugebauer's lifetime of ancient analyses to Swerdlow's overworked garage collector? Perhaps the obvious analogy hereexplains why, when Jones 1990 p.51 n.13 corrects Neugebauer, no mention is made of ON's bungle-based slander of the ancient computer. Nor does Jones mention that Neugebauer 1975 p.949 n.6 miscomputes the solar mean longitude by 30° — a half-degree — and thereby forces his solar mean longitude to equal182 the scribal error within one arcmin! (See, under fudge-Mufa calculations, at DIO 2.1 §3 fn 38.)

179 When first encountering DR's policy of evenhanded citation, Mufosi presumably supposed that it was an attempt at buttering them up. (Of course, that theory does not jibe very well with DIO's general treatment of the Mufa — but Mufosis are nothing if not loyal to their worte authors. E.g., §D3 item [b].) As I happen to know from direct testimony, some very prominent Hist.sci.comers operate by a conscious policy of brainwashing archons. [Note added 1993: But none's technique is quite up to the earlier British prototypes quoted at DIO 2.3 fn 1 fn 18.] Thus, I interpret others' compliments by what psychologists call projection. (See fn 3 & fn 169.) I.e., Mufosis have never understood — and are incapable of believing — that DR praises their occasional valid work largely as just an expression of decency & proper scholarship. (Additionally: when a Mufoso is right about something, the event is, well, an occasion — it deserves some fuss, encouragement, & commemoration here.)

180 The — it deserves some fuss, encouragement, & commemoration here.
accurate orbital analysis (namely, Rawlins 1991H) underlying DR’s novel key discoveries regarding HIPPARCOS’ SOLAR ORBIT WORK, published in DIO 1.1. (And the current exposé is the direct result of that bit of snobbery.)

Instead, emulating Boss Tweed’s sneer (quoted at DIO 1.1 §1 [C13] & haughtily defying DIO 1.1’s criticisms of Hist.sci.’s journals & archons, Isis spat in the face of: [a] manifold (& explicitly documented) DIO 1.1 warnings regarding the Mufa’s special technical & psychological disabilities and NONCITATION policy; [b] multiple DIO demonstrations of Ptolemy’s scientific-criminal career; and [c] the announcement that a new magazine had launched a special supplement whose primary purpose was to admire Hist.sci.’s journals’ pseudo-referencing, NONCITATION, and ancient astronomy goofiness (see DIO 1.1 §1 [C]). Reaction? Having not broadcast much Muffia output for the last few years, Isis (as soon as DIO appeared) evidently hired out a jetpowered taxicab and vrrrooommed right to the printer, to rush the very skies the old faker never even looked at, [ii] NONCITING R.Newton & DR where simple honesty (and keeping Isis’ readers fully informed) plainly required it ([§14]), and [iii] basing its conclusions heavily on the unprecedented math of the Jostenesen analyses of HIPPARCOS’ SOLAR ORBIT WORK. (Yes, that’s a LOUD echo of §11.) And, understand, Jones 1991H is not just about orbit-fitting; rather, the paper is attempted orbit-fitting (to ancient positional data) — by a Muffia-promoted Hist.sci scholar who (though admirably well read in ancient literature) [is not experienced in] orbit-fitting, as the slightest expert refereeing [even young scholar’s right, for his own protection] would instantly have discerned. In the days of Eratosthenes, suicide was admired as “the philosopher’s death.” But the Au—P Balloon said-M-alchemy of those who turn Greeks into Babylonians has managed to make—comes — a mass self-suck spectacle — even of the once-noble art of self-destruction. A clique who’d trust [the Muffia] for orbit-fitting, would cast Lily Pons in Brühlmilde’s Immolation scene. (The more fastidious JHA would presumably insist upon Susan Alexander.)

Consider in review the impressive range & enormity of Hist.sci.’s perversity in its Jostenesen extravagana: [1] The author, whom Hist.sci archons were so frantically determined to place first in their 1991/5 JHA and 1991/9 Isis issue’s offerings, [was] the last scholar one would choose to perform orbit-fit math, on which both these prominent papers are based. His statements of impossibility for trig fit-solutions are invariably false. [2] Jones 1991H has moreover seriously misinterpreted data ([§7] vs. [§9]) which he alleges justify his central thesis. (Again: no referee-checking, at even a subprofessional math level — gradschool or gradschool. See similar JHA funnies at DIO 2.1 §4 [fn 65].) [3] Both articles’ main finding regarding Hipparchos is based upon acceptability of the wildest yearlength ([§3] ever to adorn modern academic journals.

Since I criticize archons for slowness/learnability, it is only fair that I acknowledge my own temporary belief that the note (about DIO 1.1), at p. 35 of the 1991/7 History of Science Newsletter, was “credible” (to quote my first reaction when phoned about it) and possibly even sincere. Since the 1991 Isis Current Bibliography cites (p.45) Jones 1991H & Jones 1991M but not Rawlins 1991H (or any other DIO paper), Isis has only itself to thank for the entertainment it is here providing us. (By contrast, Ruth Freitag has cited DIO in her admirably complete & comprehensive Isis Current Bibliography [as AAS HAD bibliographies].)

[Note added 1992: I see that the History of Science Society’s Editor, R.Numbers, has [following Isis’s recent linkup with the Univ Chicago Press) engaged in a bit of hype which may enhance DIO readers’ appreciation of the Hist.sci realities displayed in the present DIO article. Editor Numbers (Isis 83:1, 1992/3, emph added): “our publishing schedule [has] . . . picked up speed . . . . We are currently able to publish articles within about nine months of acceptance.”]

Orson Welles’ Citizen Kane 1941. (Music by Hollywood’s finest film composer, Bernard Herrmann, who [with 1 exception] never won an Academy Award, for the excellent reason that he was personally disliked.)

The contrast raises obvious questions:

[i] What sort of journal is Isis? — that it would knowingly permit citation only of an unpublished longago college thesis (Britton 1967), to the exclusion of such an array of subsequent world-forum publications? At the least, Jones ought to have been required to cite the most recent skeptical papers (Rawlins 1987 in the Amer J Physics or Rawlins 1991H in DIO). (Recall like JHA behavior above at fn 166.)

[ii] What sort of scholars fear Isis’ readers even seeing the reasoning of the other side’s arguments in the Polynomi Controversy? The Mufa’s usual tactic for 20 years has been: mention the “standard translation” by K.Manitius. E.g., both the 1969 paper and R.Newton 1979 17.10 & 19 refer to Almajest 3.2 for the equinox-solstice data, which are at Almajest 3.1 in every edition besides Halma’s, including Taliaferro’s: black&white proof that Newton’s original work with the Almajest was through Halma, and thus that the malicious remarks of Swerdlov 1979 were justified. (These were published in the journal of Phi Beta Kappa, Honorary Members.) [ii] Scholarship this low does not flourish unless the author knows he will be protected from exposure. (No need to speculate on this point: see fn 113 item [a].) [b] Expect no retraction. [c] The triviality of the original charge; these thin material the Muffa must reach for, to portray Enemies as dishonest. (RN’s use of varyingly considered, R.Newton 1985 p.53.) Fn 22 shows one: the degenerates in R.Newton’s attempt to portray Newton as dishonest. One might say that Swerdlov missed his calling as a lawyer, except: he is a lawyer, for the tight Muffa cartel of businessmen-scholars. In this respect, he & his is inspired by their ancient hero, who was a lawyer for geo-carticentricity: DOI 1.1.7. The Muffa & Polynomi share lawyers’ most characteristic proto-prosecution: they know their conclusion before investigating the facts of a case [fn 160]. Which presumably explains how Swerdlov came to co-author a prominent review (published by O.Gingerich in JHA) which did not understand the purpose or even the title of the book being reviewed. (See R.Newton’s astonished response in DOI 1.1.5, e.g., SA2.)

170 However, Britton 1967 is scheduled eventually to be re-published in revised form, according to Swerdlov 1989 p.59 & Jones 1991H1 p.123-124. See fn 110. [Note added 1993: Republication of Britton 1967 indeed finally came in 1992, under the usual Impressive auspices, “Sources & Studies in the History & Philosophy of Science”, monitored by no less than eight editors & Advisory Board members, including B.Goldstein, N.C.Swederlov, & K.P.Moesgaard. The author appears to have taken admirable care to right several numerical typos in the 1967 thesis. But, unfortunately, as usual (despite or because of the publication’s funding by the Princeton Institute), no one knowledgeable in the subject actually read the work through before re-publication; thus, obviously curious errors survive intact from the original edition. E.g., see the mis-rendering at p.48 n.1 of the wellknown Almajest 5.13 date of C.Polynomi’s notoriously faked lunar parallactic “observation”. Also, the self-evident sign blunder in the last angular argument on p.142 — a high school math miasma that might have spotted this error anytime during the last quarter-century by a moment’s comparison to the next-last argument (whose coefficient is, incidentally, mistaken by 10). Britton’s new 1967 work of citing every R.Newton work he can think of — no matter that Boardperson Moesgaard has since 1991 (see DOI 2.12) [D] been well aware that DOI 1.1.16 (Rawlins 1991H1) has solved the very orbit problem (fn 110) posed at Britton 1992 p.38 (1967 p.47); it is rather strange that Moesgaard co-sponsored the publication of Britton 1992 without urging a citation of DR’s success & its confirmation of Britton’s intelligent speculation.)

171 On 1970/11/15, O.Gingerich (close to S&T) informed me (as he told many others, including his adoring grad students, as well as Scientific American persons) that the R.Newton 1973-4 articles on Polynomi were cranky and were regarded as scandalous nonsense by all the Ivy League Hist.sci experts (e.g., O.Neugebauer of Brown, A.Aaboe of Yale). Implicitly making the classic statistical error of presuming data-independence, I stupidly assumed these “experts” couldn’t be wrong, and so was put off the Polynomi scent for over a year — until encountering Henderson’s S&T piece, which raised my suspicions by its ploy (which I did not yet know was standard for Muffosi) of mentioning doubts of Polynomi but not citing R.Newton’s prominent papers. After 1976 phone chats with Henderson, Aaboe, Newton, Gingerich, Neugebauer [DOI 1.2.5 & 49], I began seeing that Polynomi was not the only fakery in this affair. 172 These exchanges included Gillispie’s consultation with DSB Associate Editor Harry Woolf. And it was from that moment that Gillispie started leaning away from attempting to get at the truth. Gillispie has high scholarly standards & a gift for Patmos. [c] Gillispie’s Editor C.Gillispie (despite extensive pre-correspondence of which see fn 110, R.Newton 1976, R.Newton 1977, etc.), & The Greenwich Centenary’s Longitude Zero Symposium (Rawlins 1985G). Though useful & generally competent, Britton 1967 is, in any case: [a] by a Muffioso, & [b] unpublished. 170

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