9 Muffia Orbituary

Hipparchos’ Early & Frankenstein 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 (eqs. 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,1 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 undaunted by severe danger of involuntary up-chuckling, 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 J.HA 1.2 (to distinguish it from the concluding half, DIO 1.3). DIO hereby, humbly & gratefully, dedicates J.HA 1.2 to the politically-toppe astronomical lights of History-of-science (Hist.sci), Cambridge Univ’s Lord Hoskin & Harvard’s Chairman O.Gingerich (co-Editors of the extremely handsome Journal for the History of Astronomy), whose wellknown editorial acumen, care, & balance have inspired DIO with the confident eternal prospect of an inexhaustible stream of Hist.sci jollies for our Journal for Hysterical Astronomy.

A2 In the pages immediately following, as the Muffia’s Ivy League poseurs are solicitously, lovingly stewed in their own cranial blubber, the discerning reader will detect the self-isolating royal delusion which is the root cause of the cult’s spectacular muff-quotient. From DIO 1.1 (¶1 §C12 & “Black Affidavit” at ¶10 of this DIO): “The Muffia’s essential attitude is that [untouchables Robert Newton] & DR are not ever right . . . while DIO &] J.HA will merely show that Muffosi are not always right.”

A3 Given their records to date, it seems likely that JHA, Isis, & Muffosi will handle the disasters here displayed by simply hiding their very existence. And all History-of-science archondum will cooperate religiously in this desperate, purportedly-life-preserving censorial project.

1 E.g., highschool-math foulups, forcings, & fakings — at the hearts of sacred Muffia-propositions’ purported proofs — published by scholars and-journals with prominently-displayed imprimaturs such as Harvard, Yale, Princeton Institute, Univ Cambridge, etc. Brief compilation at DIO 2.1 ¶3 fn 38. And see here at §G9.
Old-proverb 2 I invented:
Archons who won’t tolerate mild criticism always get their way.

B  The Winter of Our Disreferencing

The editors of the Journal for the History of Astronomy, reviewing 3 its magnificent self:

The reputation of a journal rests, of course, on the quality of its articles.

B1  Our readers will recall that DIO 1.1 ([1 §C1]) inquired:

Who are the academic-businessmen-politicians that control Hist.sci [History of science] journals and thereby assume god-like prerogatives both as censors of information flow and as arbiter-bestowers (upon the Less Fortunate) of the “prestige” that is said 4 to attach to publication in their incestuous forums? These gentlemen allegedly evaluate incoming manuscripts. But: who evaluates the evaluators? Are these editors and/or their hypothetical referees 5 capable in the very disciplines where they pretend to measure others? Even in high school math? [See “Referees Referee”, DIO 2.1 §3 fn 38.]

2 Certain exposed Hist.sci archons have tried to portray DR as pure praeceans horribilis. A key consideration in evaluating DR’s pungent recent evaluations of archival misbehavior: despite occasional sharp private reactive criticisms (generally correct, though subject to change wherever evidence warranted) of Mufa 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 ventured to publish their observations in the Cotton manuscripts, among other incriminating documents, are now bearing responsibility for a truly epoch-making discovery.”

3 The Editors of the Journal for the History of Astronomy, reviewing 3 its magnificent self:

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4 On 1980/3/22, JHA-leafed DR simultaneously sent 3 papers to the JHA for publication; however, these were of course submitted for prompt referreeing, 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 possibly count on the papers (especially all 3) being rejected by referees; thus, the Editor got panicked, while thrashing about for style-nitpick excuses (to explain the referees would not even be asked to look at any of the manuscripts), and so promptly 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. Lord’sorphest’s 1980/4/11 letter to DR claimed that, for the JHA, “Any other paper was to be submitted, not for publication, but for a steady rewrite and finally prepared for publication.” Rarely has a journal so clumsily confessed to the pure-formality of the purported editorial oversight it applies to its articles. DR’s 1980/5/4 letter to Lord H included the passing comment, relative to unsightly jammed-in footnotes in first drafts (back in those primitive pre-wordprocessor days): “I have . . . well-thought-out reasons (explained in 1980/4/18 letter, which discussed the risk of introducing new typos at each rewrite) for believing it unwise to re-type whole pages every time (my incurable idea-fecundity tacks a new footnote onto one of my manuscripts)!” Should that add editors who ask for ready-for-the-printer copy upon publication first may submission may give the impression that they are more publishers than editors.” On another point, this same DR letter notes: “I asked in both my letters [1980/3/22 & 4/18] whether [if we get that paper] . . . (You don’t even realize that my opposition to [your editorial] policies 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 [Dewhurst] & Simon [Mitton]."

B2  DR to Lord H, 1983/3/14, asserting responding to the EfL’s 1983/3/3 threats of legal action & future JHA nonpublication of DR work (EfL’s chummy reaction to DR’s merely pointing out that the central results of a recent pseudo-refered 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 B1 . . . . Our readers will recall that DIO 1.1 ([1 §C1]) inquired: Who are the academic-businessmen-politicians that control Hist.sci journals and thereby assume god-like prerogatives both as censors of information flow and as arbiter-bestowers (upon the Less Fortunate) of the “prestige” that is said 4 to attach to publication in their incestuous forums? These gentlemen allegedly evaluate incoming manuscripts. But: who evaluates the evaluators? Are these editors and/or their hypothetical referees 5 capable in the very disciplines where they pretend to measure others? Even in high school math? [See “Referees Referee”, DIO 2.1 §3 fn 38.]

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7 Retracted by the honest author (1984/6 JHA) — a fine scholar who was simply let down by the JHA’s failure to insist on real refereering.

8 See fn 9.

9 See fn 9.

10 C6 & fn 92.) [Note added 1993: See DIO 2.3 fn 16.]
... a story in the current Washington Monthly: an illegally parked car was tagged [over several days] with 2 parking tickets by 2 different officers — who failed to note the minor item that there was dead body (sporting a prominent bullet hole) in plain view in the front seat. Now, if someone openly questioned whether ... “effective” policing was in force, would the police commissioner be justified in ... pointing to the 2 parking citations to prove that the cops were on guard? ... If you knew me at all, you would realize that threats neither intimidate nor rile me. Their only effect is the suggestion that you wish to suppress open discussion. ... I continue to wish you well.

(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 1983/3/21 reply severely communicated, thereby killing the already refereed, accepted, & advertised9 paper Rawlins 1999,9 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 §11 [§C5–C13] etc.). Anyone with a sense of humor will enjoy comparing the banned DR sentence’s temperate treatment of the Muffa, vs. the extensive published abuse heaped upon the Johns Hopkins Univ. Applied Physics Lab’s late Space Sciences Division Supervisor, R.Newton (Ptolemy skeptic9) by JHA & its various Editors such as O.Gingerich (Muffia satellite) & N.C.Swerdlow (Muffioso): see sampling at DIO 1.1 §3C7. E.g., the editorial gang at Lord H’s usually effete-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 anathema to the JHA (§13C9) & its referee K.Moesgaard]:

Newton’s conclusion [that Ptolemy deceived] 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.sci 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.

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 §11 fn 25. The main reason the JHA initially wanted to tag this paper as: 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 trying DR (R.Newton’s most forceful supporter) away from RN’s heretical view of Ptolemy and thus to isolate RN (a neat plan, later applied to DR by the same vol.). (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 censorship filter by curtly waiting until the very last pre-publication minute to suddenly insist upon deleting even the sole small portion of the paper that backed Newton’s thesis. DR would not assent to this ploy; thus, Rawlins 1999 remains in acceptance-limit: publicly accepted, but not withdrawn, and not published. (The paper is crucial to the tracing of eq. 6 here to Aristarchos: fn 81, §80.) 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” (§B1) of being published in the awesome JHA (DIO 1.1 §11 fn 11), he was still unsuitably 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 besow upon anyone.) On 1946/4/28, in the unexpected presence of myself & my wife, 0 tried the same publication-conference-offer ploy with van der Waerden (additional proffer on travel expenses). No wonder 2 sided publication discourse is such a rich & fickle 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 (M AH; eq. 6 here) was from predecessors. This paper now finds that 2 other lunar elements (ε & η eqs. 8 & 9) are also from predecessors. I.e., of Hipparchos’ lunar elements, only ε (or r) is original: eq. 19 (or eq. 20).

11 Describing it as “forbidden fruit”, I read it aloud anywhere, 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 response tactic was standard: all audience members that mattered were herded into a nearby room, believed to be just out of DR’s carhust, and then told by O.Gingerich that DR’s description of JHA as censorial was untrue, etc. Scholars attendant at similar archonal confabs, who lack the intelligence to question why such discussions (and why, e.g., all archival outrage at DIO so far) must be held under behind-the-back circumstances where 2-sided cross-examination is not possible, fully deserve the degree of enlightenment they will unfortunately lack.

12 Ptolemy’s superhybe-thing (in e.g., AAAA’s Science, QlRoyal Astr Soc, & Springer-Verlag: Gingerich 1976 & §16) is “The Greatest Astronomer of Antiquity” is worthy of Greatest-Show-on-Earth P.T.Barnum — is as the fraudulent subject of such puffy. But the kilobucks&kilobooks gross may seem worth it: even aside from some ordaging $100,000 Ivy League salaries, Ptolemy’s promoters are raking in handsome royalties on ordaging $100 books which sell ordaging 1000 copies worldwide, since they are dutifully & lovingly review-advertized (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 go outside the Muffia p.t. team when choosing reviewers of ancient astronomy material.

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

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

15 One notes that, a few years prior to the incident noted here, Mufosi Asger Aaboe & Bernard Goldstein both disappeared from the JHA’s windowdressing Board of Advisory Editors. They were, no doubt, merely objecting to JHA stationery’s mis-spelling of “Asgar” as “Asgar”. It was purely coincidental that, around this time, the JHA committed the heinous indiscretion of publishing a single short note (JHA 8:200-203: 1979) by R.Newton, even though the note’s conclusion was immediately followed (same page) by obligatory anti-thoughtcrime commentary by K.Moesgaard on “Hipparchus and his Babylonian [!] colleagues”. Another correlation: shortly after I revisited DR in 1982, Toomer left the Isis board. These coincidences led to a decision that is not coincidence: 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 handades journals’ actual quality (see DIO 2 §4 fn 65, & [b] render them hostage to censors’ tantrums. (What priorities could lead a scholar to insult art to publish a paper in the JHA?) The JHA learned its lesson and so went to serpentine logical extremes to avoid repeating the mistake of publishing RN: see DIO 2.1 §3B. Happy ending: the JHA board now includes enough Muffia to ensure enough to the JHA that the JHA will never do anything stupid again.

16 Virtually every scholar on both sides of the Ptolemy Controversy now agrees that Ptolemy deceived — even the leading Neugebauer-Mufa capos (Toomer 1984 p.672, Swerdlow 1989 p.54). But the Mufa denies this is lying. What’s the point of a long, fraudulent subject of such puffery. But the kilobucks&kilobooks gross may seem worth it: even aside from some ordaging $100,000 Ivy League salaries, Ptolemy’s promoters are raking in handsome royalties on ordaging $100 books which sell ordaging 1000 copies worldwide, since they are dutifully & lovingly review-advertized (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 go outside the Muffia p.t. team when choosing reviewers of ancient astronomy material.

99 1991 December Journal for Hysterical Astronomy 1.2 §9
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 Phi 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: 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: ensnaring the Potomac Controversy as a classic case study of a community gone wrong, as convincingly demonstrated by Hist.sci’s persistent 22-long failure to handle a prominent conflict central to its own field. (The unsubtle techniques, used by archons to fix this fight from the outset, will be apparent from §[113 and DIO 2.1 §[3]. The price for DR’s offered publication in the J.HA 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 155 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, unable 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 J.HA) in DIO 1.1’s Journal for Hysterical Astronomy (§§[G7]). Yet another example, at Hist.sci journaldom’s Reputability-panicle: shouting Hi-There! from the extremely handsomest pages of the 1991/5 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 calendric creativity since the Mufa’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 massochistic. ... You’re not a bad person. Why act like one?”

18 Best wishes, in spite of all ..."

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-declared amateur (see his self-composed bio in the 1982/6 Isis p.329, where he concurred that he went the standard Hist.sci grad-school route, and who breaks virtually all the Hist.sci rules [e.g., (C2), (3)], especially the ones requiring: (1) socratic writing, (2) innocence of the mathematical sciences, (3) encouraging 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.

20 For a similar situation, see D.Rawlins Peary ... Fiction 1973 p.291 item #2. And see, at DIO 2.1 §[13 fn 8, Lord Hoskin’s magnificently inventive scheme for killing off the airying of ongoing controversies. By comparison, Isis’ approach (exhibited at fn 121) is childishly clumsy. The blue ribbon for this category unquesionably goes to Lord H.

21 Jones 1991H p.119. (Caps added. With dentritic pride in assisting the creation of a surer, brighter smile.)

22 The DSB’s high quality may be gauged from its apparent nonobfuscation by the name of R. Newton. DSB’s 1978 near-backsliding (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.

23 To borrow the unexceptionably polite language of no less a correctness & competence-authority than The Malman ... 1975 n.13 on R. Newton.

24 Theon of Alexandria horoscope 360/9/19 (Neugebauer 1975 p.966 n.16). Toomer dates it to 360/6/15 (a 9-day error) due to his confusion of Athenian 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/14 yearlength of Big Jul’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 fn 5.)

25 Admittedly, my optimism wasn’t very warm. Rehah is largely a chimera — as Hist.sci’s Jonestown folly has proved all to 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 unreality. 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.

26 See fn 266.

27 E.g., Rawlins 1982G, & DR to Isis 1880/10/16 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 §11 ‘impossible’ behavior (i.e., nonincorporability into Hist.sci.scri.burnout-mill) will simply continue to produce increasingly repulsive Hist.sci.institutional evasions & pusillanimity, which will be fully reported in the J.HA.

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.HA.'s frivolous style, are urged to consider the subjects. Stigging giffes 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

C5 DIO 1.1 (§1 ff 12) spoke joyfully of “numerous Hist.sci professionals’ doubtless unbiased conviction that mere scientists are ill-equipped to contribute to the field. As we shall see [in DIO], some among these superior folk can indeed be class entertainers when attempting, e.g., astronomical calculations.” In order to awaken Hist.sci.journals, DIO 1.1 even specified (§1 §5 & §6 fn 4) examples of what to look out for: “For simplifications of truly epic [Neugebauer-Muffia] Muffa struggles with the mysteries of elementary arithmetic, see DR’s exposures in the American Journal of Physics: Rawlins 1987 nn 30 & 35, (…) undeniably accurate but highly embarrassing material which pathetic Isis had previously refused to publish.” See also fn 9 there, and here [DIO 1.1 §6 at fn 6, fn 21, fn 33; also, …] [§5 fn 7.] (The above reference is to the late Otto Neugebauer of the Princeton Institute. I owe Neugebauer much serious & refined knowledge — but his heritage also includes a Muffia which infects all-too-faithfully his rigidity of viewpoint.

C6 So anyone as DIO 1.1 appears, what is the immediate reaction of the two biggest relevant Hist.sci.journals? — these being: The Journal for the History of Astronomy (purportedly edited by Univ Cambridge’s Lord Hoskin & Harvard-Smithsonian’s O.Gingerich) and Isis (History of Science Society). Answer: just as soon as superhumanly possible, both journals go out and publish — in their lead Articles mind you — the most blatantly mis-computed and uncomprehending astronomical mathematics which (even) they have ever promoted. Moreover, in order positively to ensure that the totality of their imperviosity will not elude the most forgivingly-inclined observer (no matter how dim), both journals elected to have this double-disaster30 composed by a scholar from the very Neugebauer-Muffia klan which DIO 1.1 had explicitly warned of. Brown-U-product Alexander Jones. Pronounced as in. Jonestown. Enlighiten Hist.sciEditors? I’d prefer less formidable educational challenges.

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 Muffia’s all-consumming conviction that indoor Babylonian astrologers secretly influenced high empirical Greek astronomy (§E4) — until the ultimate 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 Bon §14 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.)

C8 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) haven’t awakened anyone, then: let’s have no mockshock when the discussions here get a shade less than coy. I’m reminded of one of the great pre-JHA comedians, Red Skelton, who, when his audience was slow to pick up on a joke,29 would play-detail 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 alot 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 showmen. 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.HA 1.2). But I will (attempt to) outline here, at the outset, Hist.sci.achondram’s sensational 1991 achievement (carried out, I repeat, in frontpage papers — and frontpage in the most prestigious & self-important Hist.sci.journals):

[a] It is “proved” 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 days long.

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, & §3 [D3. Or here at fn 269 & [fn 29, §J7, & fn 158. Not that one need be a heretic to get tangled 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 “exceivable” (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 faker Ptolemy’s sins as due to the limitations of his primitive era [DIO 1.1 §15 [G8], but so mercifully prone to criticize other, usually well-educated and honest scholars, who were also handicapped by crude means? The answer is: Mufflosi have made a living selling Ptolemy and simultaneously selling their alleged ability to interpret him better than other scholars. (Some Mufflosi are now gradually switching franchises, from Amalgam to BabCycles: [§F2 & fn 266]. So, the answer to the foregoing pseudo-paradox is simple: ‘dеменне 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 (§7), I remind the reader that: the Muffia calls other scholars “crank” (fn 31) and “Velikovskian”. (See also §[F], §[G], fn 191, fn 192, §M7.)

[b] Trig-function orbit-fits are declared impossible for one of 3 solar-position data-trios, where (in all instances) it is immediately obvious that solutions must exist. (All solutions will be set out below.) 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 Muffia talent — why, just the previous year, proto-Muffia 1990) had pulled off a similarly impervious feat. (We’ll honor that achievement below at §4.)

[d] As usual, when skepticism on Ptolemy is mentioned, the accursed works of skeptics (especially DR) remain unctured. (Rawlinns 1991H fn 6, describing 15 of the Ptolemy Controversy: “not a single inner member of [the Muffia] has ever35 cited any work by DR.”) Even lower-level citations invariably acknowledge no contribution (e.g., §I5, fn 288). Historia’s leading periodicals kiss up to, honor, & prominently push such scholarship, while attempting to starve, ostracize, or low-rank those who provide correct mathematics and two-sided bibliographies (§114). (Contrast R.Newton’s citation-integrity with Muffiosi’s: §E1. Examples of DR’s citation-policies are provided at fn 16 & fn 174.) What an inspiring model36 of academic behavior for young historians to look up to: cite the nonciters, and noncite the citers.37 The asymmetry’s as poetic as the justice isn’t.

33 §G10, §K9, §M4.
35 [Note added 1992: Muffia capo N.C.Swerdlow’s 1992/10 JHA paper at last cites a DR work. However, the essence of the tradition continues, as NFCs of course concludes that the papers cannot contradict anything whatever to the field. The competency & integrity of NFC’s criticisms are displayed at DIO 2.7.18 & C7.]
36 Analogously: the clique who was proved wrong throughout the Ptolemy Controversy has emerged politically & ideologically dominant, while those whose charges have been repeatedly vindicated are banished from the scene. (See Rommel’s reflections on WWI at §J2, and his na¨ve implicit conviction that initial WW2 good fortune augurs a different fate — even while fully aware that he will cede none to DR — the solution was 1st realized while I was working at JHA.)
37 §I14 item [a]. (On the personality-type that behaves so, see independent appraisal quoted at DIO 2.1.33 [A].) In any case it is objected that the “tone” of the present paper makes it unattractive, keep in mind that Muffos have for decades [a] used the vilest language against dissenters, and [b] have systematically noncited 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.)

D Even a Hun can Have Fun: Blitzkrieg in the ’Test

D1 Aside from DR’s customary cleanups after the Muffia’s customary messes, the following pages also provides the actual, highly revealing solutions of the very same ancient material (Almajest 4.11) that the Muffia 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” series, §K9), which he adopted (for no more than c.100) 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 (§K4); i.e. & the PH orbit neatly solve, to c.1 (§L3 & §M10), 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 conrmation, 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 from a breezily believing confidently into territory previously unknown to historians (§K).

[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 deed explanation pending a DR solution of both the larger numbers (the rst, eq. 23 below, discovered by DR in ordmag an process on WW1 at §J2, and his na¨ve implicit conviction that initial WW2 good fortune augurs a different fate — even while fully aware that he will cede none to DR — the solution was 1st realized while I was working at JHA.)

38 One can just imagine the invective which Capt.Captious Swerdlow would sling onto his JHA pages, had R.Newton done something like this.
39 A slight improvement over the dawdling DR pace described at Rawlinns 1991H fn 34. (And, to give Toomer his due — even while fully aware that he will cede none to DR — the solution was 1st realized while I was working at Toomer 1984 p.215 & fn 75. It is scribbled right there on my valued copy of his book.) Even so, it was another 19 days before I hit on eq. 24.
40 As to whether one may sometimes extrapolate from modern experience to ancient: compare fn 36 to fn 154.

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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 Mufosi 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 panzies). 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.42 Toozer 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.44-70, 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 stifting 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 came near. . . . 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 hardly conceivable. 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 finally lost the war.43 And now we had broken through the renowned Maginot Line and were driving deep into enemy territory. It was not just a beautiful dream. It was reality.

41 Fn 46 & SP1. Mufosi have that killer instinct for elimination of The Enemy and (Rawlins 19844 pt.972) all traces that he ever existed. (See fn 16 item [a], §C11 item [d], §H2 options [b]-[e], §D2.1 §f3 §A; & note contrasts at DIO 1.1 §f1 §C6 §C12.) Unusual passions for purported historians — but just right for political conquerors.

42 It need hardly be added that the norm in warfare is that all participating nations lose in the long run. DR to the Mulfi’s puppet-loyal O’G (1983/831), warning B Pooche not to hoop jump too hastily onto what might appear at the moment a (politically) winning bandwagon in the Poltrology Controversy: “Don’t you know that there will be no winning side?” To quote a figure slightly more popular (than DR) among Mufosi: even Hitler of all people publicly said the same thing (1939/106), while planning this very offensive (W.Sherer Rise . . . ppbk ed p.849). On the morning of the Rommel diary entry here quoted, Rommel got his orders from his superiors, General G von Kluge. Less than 5 years later, both these Nazi “victors” were forced to commit suicide by poison, at the order of Hitler & the Nazi gov’t (Hart op cit pp.17, 499-506), for whose cause both generals had repeatedly labored & risked their lives. There’s a Woody Allen question (extrapolating from & satirizing what is sometimes called “New York thinking”), intended for those who attempt great ventures for posterity’s sake: “What did posterity ever do for you?” The fates of Rommel & von Kluge provide a macabre short-term answer. I hope that the conclusion of §D2 will supply a more uplifting long-term response.

43 See fn 36.

44 Rommel stopped at dawn. Hart comments (p.23): “Rommel’s division had advanced nearly 50 miles since the previous morning . . . a daring act. Then and later, most commanders considered that, even in exploiting a victory, the continuation of a tank advance in the dark was too great a hazard.” (One is reminded of US Adm. M.Mitchers’ legendary — i.e., equals successfully, in the event — night-time carrier gamble in the Pacific.) Actually, it was never totally dark for Rommel. Presuming his timepieces were on then-standard CEDT, Rommel stopped the advance at 6:15, just after 5:57 sunrise, at about 50°06’N, 3°34’E. The waxing gibbous Moon had set at 3:45, after feeble dawn twilight’s onset. (Sun’s 3:45 altitude: -15°1/2.) (Two minor astronomical oddities: [a] Though 60° short of full, the Moon set barely 2h before sunrise. [b] For the following 4:00, the Moon, and all 8 planets were below the horizon.)

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Granted all the more-than-obvious differences 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.47 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.48 And, above all: grateful, stable-perspective humility49 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.50 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 homes51 and mass murder,52 but rather in the refined cause of doing justice to those 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, Toozer’s rickety trio A&B solutions — which he intermittently (§01) 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 Naziified Rommel (Hart p.501). But, even aside from his wellknown military intellect — and courage (he was lucky enough to survive his 24hr spurt) — Rommel was a genuinely gifted writer. (As was Grant. Or his ghost.) Haut, Brian. The Secret Life of Rommel, p.274. (JG) 54

46 For instances of the last two threads: see, similarly, the admirably unrestrained joy of the greatest US explorer, R.Peyre (Nearst the Pole 1907 pp.190, 192), exulting at his excruciatingly hardwon (if modest) genuine 1906 Summer discoveries in northwest Ellesmere Land. Also the (ironically premature) let-down following: p.203. Like Rommel, Peyre was an unusually able writer and thinker. (Writing specialist & skeptic H.Ward, perceptive on some else about Peyre, wrongly regarded the 1907 book to be romanticized; but, in fact, large & wonderful human sections such as these are straight out of the explorer’s diary, virtually verbatim. I speculate that my own willingness, to display personal reactions here, has a debt to Peyre.)

47 Carnavon (ed) op cit p.510). On DIO p.22. But most commanders are, like Mufa capos, unwilling so to dirty their hands — and thus depute live hatchetry to underlings. (My fellow-semipacist Redd Foxx’ reaction to frontline warfare: “I backed up the dead, then, into a gutter.”)

48 A delusion encouraged by Jones 19914 n.5. This note also uncritically pushes the persistent misapprehension that arc-de-ciels did not exist in 1936 BCE. For instance, see fn 18f. The only clear old homes DR is destroying are: hotair castles built of cozy cliques’ cozier prejudices.

49 Not leaving all the bloodsweating to stonies (e.g., DIO 1.1 §f3), 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 Muiffa capos, unwilling so to dirty their hands — and thus depute live hatchetry to underlings. (My fellow-semipacist Redd Fox’ reaction to frontline warfare: “I backed up the dead, then, into a gutter.”)

50 A delusion encouraged by Jones 19914 n.5. This note also uncritically pushes the persistent misapprehension that arc-de-ciels did not exist in 1936 BCE. For instance, see fn 18f. The only clear old homes DR is destroying are: hotair castles built of cozy cliques’ cozier prejudices.

51 As was done throughout the Nazi march into France. E.g., Hart pp.18f. The only dear old homes DR is destroying are: hotair castles built of cozy cliques’ cozier prejudices.

52 Not leaving all the bloodsweating to stonies (e.g., DIO 1.1 §f3); 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 Mufa capos, unwilling so to dirty their hands — and thus depute live hatchetry to underlings. (My fellow-semipacist Redd Fox’ reaction to frontline warfare: “I backed up the dead, then, into a gutter.”)
suffered repeated excruciating tinkerings & 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 foregoing will have no (visible) effect on the Mufa, who will simply continue lockstep-pretending that Toomer’s development is the only valid one.

**D4** Alternate possibility: act as if the Mufa thought of DR’s permissible solutions first. (See options [b] & [c] at §H2.) E.g., publish a Mufa paper, containing these DR finds, a few months hence — with a preface signed 1989 or 1990. Too cloddh ick 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, within 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 §C. All 3 are so obviously correct that their Untouchable origins have established a throbuss or logiam (§P3) in treating the mean motions issue.^[55] As DIO 1.1 §1 fn 9 noted: DR found these solutions in 1980, and mailed them to Toomer’s correspondent-colleague, fence 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.)\[^{55}\] Despite my pointed American Journal of Physics remark (Rawlins 1987 n.30) on Toomer’s noncitation, Toomer remained unmoved by this. Section 3.1 (OG) — and so impunity prepared the ground for these discoveries are his own.

J.HA archondom also remains silent in the face of such behavior. What kind of purported historians cannot show an interest in honest attribution (the Mufa’s Mr.Consistency: fn 78) gives non-degree observational data, and then explicitly transforms them into degrees for us. He does not do this for Timochars or Aristylos.^[c]\[^{1}\] Unlike Timochars’ data, Aristyllos’ six star declinations 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-datum error, 6’ (pre-rounding, it was 4’), is at best as good as that of the ancient declination observers who indisputably used degrees, e.g., Hipparcos. (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).\[^{54}\]

\[^{54}\] Eqs. 23 & 24 are quite safe, being much too heliocentrist-betical even to admit, much less grab. But I don’t believe that the developments of eqs. 12-20 (despite fn 99) & of eq. 34 are based upon any hypotheses permanently engraved in the J.Copernicum Prohibitorum. (It may well turn out that Mufa’s response to and/or evasion of this paper’s solutions will become the subject of yet another J.HA paper.)\[^{55}\]

**D5** Hist. sci.’s highest archon-angels may perhaps be tempted (privately) to blame their Jonesont staple upon the Mufa. Perhaps also upon servile Mufa satellite O.Gingerich, whose passionate faith, not to say bigotry, permits his acceptance of virtually any nonsense seeming to exculpate Ptolemy & thus save OG’s faces. (Rawlins 1982C n.1 naively accused OG of honesty, after OG had temporarily retracted Gingerich 1976’s reasoning and agreed that the Ancient Star Catalog had probably been swiped by Ptolemy from Hipparchos, after all. But OG has since re-re-reversed, so I withdraw the accusation.) E.g., OG’s J.HA pre-publication promotion (§E2) of Jones’ Babylonian fantasy (§E1) shows that 0 was the immediate cause of its special J.HA exaltation — which no doubt played a part in Isis’ taking it seriously. Well, far be it from DR to defend Mufa&co. but: crediting them for Jonesont would be unjust. No, the responsibility belongs to Hist. sci.’s own archons, who have no excuse whatever for being taken in by Mufa’s pretenses to reliable expertise: these archons have been warned in detail both in the Amer J Physics (Rawlins 1987 n.30) and in DIO 1.1 (which various archons received) of these parties’ difficulties with, e.g., simple arithmetic. (Thus, the gradeschool fumblings of Jones 1991H should have been no

[^54]: The Toomer 1984 App.C’s patent loathing of these solutions makes the accurness of their source all too plain. Incredibly, despite years of gibberish (e.g., Neugebauer 1975 pp.151-152 vs. n.25, p.157 vs. n.6: see here at fn 54). On this issue, the Mufa had never actually carried out the simple divisions of the period relation numbers provided in Almajest 9.3.1 — until DR did so and showed thereby that (contrary to the Mufa’s longterm repeated Ptolemy-trusting insistance: partial list at Rawlins 1987 n.30 & DIO 2.1 §3 §F 38) the period relation quotients yielded the precise tabular mean motions for Mercury, Venus, & Saturn.

\[^{54}\] Rawlins 1980/9/2-4. Copies sent K.Moesgaard 1980/11/15. (Incidentally 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 another stage in their ancient evolution.) All Almajest planet mean motion equations sent Isis 1983/3 & 1983/8/12. (First submission unacknowledged; 2nd submission rejected without cause, 1983/12/20. Printed table of equations handed out at 1984/6/12 Amer Astron Soc-HAD meeting. Later appeared in excellent science journal: Rawlins 1987.) I recently asked (DIO 2.2 §15: 1991/8/23) Mufa-assistant PHuber to request from O.Gingerich a xerox of DR’s original 1980/4/13 letter. Huber’s reply (1991/9/6) did not acknowledge the request. Have Mufa’s been hoping these transmissions are unprovable? In fact, my files contain detailed replies, from R.Newton (e.g., 1980/9/14 & 1/3/7), and O.Gingerich (e.g., 1980/11/3), all showing that the solutions were new to them. The solutions were also unknown to Mufa’s, whose longtime persistent upside-down misconstruing of the same data was exposed in a 30pp. of Rawlins 1987 (§ & 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, to his incomparable self.

\[^{55}\]: DR has been informed from the inside, in so many words, that “blackballing” is the deliberate policy here. (See DIO 1.1 §1 A8.) 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 been specifically ordered never to cite any of the eejit’s papers, in any field, on any subject. So there’s nothing unique about such behavior. What’s special here is the na¨vete of those who trust these zoos’ effusions. Statistics-wise, this reminds one of Rose Bird, the environmentally-sentitive (thus business-enraging) judge whom anti-death-penalty Gov. Jerry Brown appointed to head ‘Tornas’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.

\[^{56}\]: 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 massive scholars have heard Mufia slanders of it, so such retraction — or indeed any perceived success by a Mufia-damned party — would be ruinous to Mufiosi’s long-polished image of reliability & expertise.) The cumulative transparency of the disingenuousness such a policy entails is just another unanticipated outgrowth of the original mistake. A further mistake: when a discovery is stolen or suppressed in order to lower a scholar’s recognition, repeatedly denying credit is going to get progressively smellier. Hist.sci’s highest archon-angels may perhaps be tempted (privately) to blame their Jonesont staple upon the Mufa. Perhaps also upon servile Mufa satellite O.Gingerich, whose passionate faith, not to say bigotry, permits his acceptance of virtually any nonsense seeming to exculpate Ptolemy & thus save OG’s faces. (Rawlins 1982C n.1 naively accused OG of honesty, after OG had temporarily retracted Gingerich 1976’s reasoning and agreed that the Ancient Star Catalog had probably been swiped by Ptolemy from Hipparchos, after all. But OG has since re-re-reversed, so I withdraw the accusation.) E.g., OG’s J.HA pre-publication promotion (§E2) of Jones’ Babylonian fantasy (§E1) shows that 0 was the immediate cause of its special J.HA exaltation — which no doubt played a part in Isis’ taking it seriously. Well, far be it from DR to defend Mufa&co. but: crediting them for Jonesont would be unjust. No, the responsibility belongs to Hist. sci.’s own archons, who have no excuse whatever for being taken in by Mufa’s pretenses to reliable expertise: these archons have been warned in detail both in the Amer J Physics (Rawlins 1987 n.30) and in DIO 1.1 (which various archons received) of these parties’ difficulties with, e.g., simple arithmetic. (Thus, the gradeschool fumblings of Jones 1991H should have been no
surprise.) So, the message here to Hist.sci archondum is: don’t blame other subcoelenterates for your own inertia & deafness. (Let it never be said that DR compared Hist.sci Almajest 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, ghostly truth (explaining all of Ptolemy’s solar “observations”, on the nose) was first revealed in J.Delambre Histoire de l’Astronomie du Moyen Age 1819 (pp.Ixvii-lix, a source never cited by the Mufia): Claudius Indoor Ptolemy faked all his solar data by simple arithmetic from Hipparchos’ observations & yearlength — not (quite) from Hipparchos’ trig-based PH solar orbit.64 So, even if Jones 1991H’s anti-trig-orbit theory were true (which it isn’t), it could not exculpate Ptolemy in the slightest. After R.Newton found that his independent discovery of the same argument had been anticipated by Ptolemy — who in 1817, cited Britton 1967 (R.Newton 1976:113) & J.Delambre 1819, uncited by Britton 1967, had published the same argument much earlier, so R.Newton 1977 (p.93) gave explicit credit and precise page-citation for Delambre’s discovery. By contrast, the Mufia itself has, despite almost countless opportunities, not yet cited the same Delambre passage. (§114. Mufiosi prefer that all citations, of Ptolemy’s manifold embarrassments, be to Mufia-orthodox-and-thus-forgiving discussions: [§14 & DIO 1.1 §1 fn 5. Classic insular cultist thought-control.) Despite this unsuble comparative record, the Mufia’s most abusive mout’piece has repeatedly tried (fn 123, fn 169, & fn 170) to consult the Editor-for-Life’s hilarious & typically well-refereed 1992/8 JHA papers on that solar positions for the two Hipparchian eclipse-trios (A&B) preserved at 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 continuous function velocity preferred by the Greeks) — which allegedly originated in the “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

63 See fn 166 & fn 168. Over a decade ago, DR added to this argument the ironic oddity that these arithmetical fakes showed that Ptolemy could not even produce such a result! [When DIO 1.1 appeared in 1991, Jones said he wouldn’t look at it. Later he agreed with it.] The Muhummability is as peripatetic as Hegel’s 1803/04 Univ Jena denial of the existence of Ceres, which had already been discovered on 1801/01/1 and publicly announced in Jena on 1801/01/5. [Note added 1992: DIO readers are urged to consult the Editor-for-Life’s hilarious & typically well-refereed 1992/8 JHA attempt to deny Hegel his rightful goat’s horns. Unable to translate Hegel’s messy Latin for four-thirds-power, Lord Holmkin simply OMITS the Hegel 1801/01/3 analysis’ essential final math paragraph on the planets, where he states the very distance formula which is the subject of Jones’ paper! Our most sincere thanks to His Lordship for so promptly & convincingly exemplifying the earlier disaster’s prime lesson, which follows immediately here:] A unperturbed by mere facts as the Mufia. 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.

64 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 [§7]? 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.

65 Advertised as forthcoming for months, on the inside covers of JHA issues. As noted here (fn 114, §3, & §3.1, 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: [§7]) that Rawlins 1991H’s solution could not happen — even though Rawlins 1991H was published 4 months before Jones 1991H (1991/5) appeared! (When DIO 1.1 appeared in 1991, Jones said he wouldn’t look at it. Later he agreed with it.)

66 That such elementary considerations just might interest Hipparchos for having rendered his star catalog eclipsally (and thus the messy inconsistency of equatorial coordinates), whose extant solar & lunar coordinates were entirely eclipsical, would not record his stars from the same Brownie troop as Mufa capo G.Toomer, the vaunted History of Math Dep’t at BrownU. (See p.36 of Jones 1991M; paper recommended by Asger Aaboe, also Mufa.)

67 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 Jomestown 1991 graadschool’s arithmetic which Britton vetted (p.68) and which is the subject of our present Journal for Hysterical Astronomy romp . . .]
who preferred a step-function arithmetical approximation to a continuous trig function. fn 87. But no hype67 is beyond Muffia when they are hustling 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 Muffia insight68 that Babylonian step-function math underlay the solar calculations used when Hipparchos analysed (as reported at Almajest 4.11) 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 Muffia’s Jonestown triumph] appeared to be one area where Babylonian data did not enter into Hipparchos’ calculations.” The author’s use here of the word “data” (in reference to elements, not outdoor observations) reminds us of a simple reality which ought to have served as a brake on the Muffia’s mass-suicidal plunge into its Jonestown folly. The reality: not only is the empirical solstice or equinox from Babylon 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 quote. L.Taub’s 1987 thesis doesn’t. Nor does P.Huber: DIO 2.1 ff (H14). 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, Gerardi 1988 pp.323f, 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 astronomers) anywhere in the Babylonian record. It is remarkable to find the description “impossibly accurate” (Jones 1991H p.118) in relation to the allegedly original output of Babylonian astrologers, considering that (while Greek astronomers knew their latitude to ordmag 1°)69 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.70 (Usual Muffia 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 Muffia’s entertaining key tenet, that “sophisticated” astronomical science was being communicated from Babylon to Greece during the Seleukid period.

E4 Obvious 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 brilliance71 by which other fundamentalists72 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 Muffia 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 67 [Note added 1993: See Physics Today 46.7.61 (1993/7) p.64 for outgoing NAS Pres. Frank Press’ image—so comprehensively on grant-hustling exaggerations. He dismisses the trend, but “I don’t consider hype to be unethical in the sense of scientific dishonesty.” This ethical flexibility arises from understanding forgiveness: “in the past few years, with scientists in a state of stress, competing with each other, attacking one another in the news media and the courts, the image [of science as a reasonable enterprise] has been tarnished. . . . The only way to understand this phenomenon is that scientists are not themselves because of the crisis in finding the middle of the road.”]

68 Originally due to F.Kugler (1900): see §§F1, §F3, Bowen & Goldstein 1988 pp.66-69 (System A solar + System B lunar) & Jones 1991H n.41.


70 Hipparchos’ latitude for Babylon: 35°N (e.g., Geogr Dir 5:206). See also Geogr Dir 8:20-27 and Rawlins 1985G pp.260f & fn 13. [And see confusion at Strabo 2.1.23.30-31.]

71 It is only fair to point out that, as far as I know, N.Swerdlow has taken no irratinal part in the Muffia’s delicious search for Babylonians behind everything in pre-Ptolemy ancient astronomy.

72 Anyone who doesn’t believe in evolution should try tracing Muffia Poltemy-apologia. Especially §§H. See also, e.g., §§I & §§J item [c], DIO 1.11 in 9, & DR to OG 1983/11.25. [Note added 1993: see Pedersen at fn 99]
In the following section, I will attempt to inject a smidgen of sanity & perspective into these proceedings — by offering a few brief looks at the a priori credibility of Muffiosi’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.8, §D10, & §G5. The main issue is: whether major Greek astronomers depended primarily upon Babylonian mathematical methods & orbits.)

F R.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 §C6 & fn 191.) The transparently feeble 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 Hipparcuss’ 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 Hipparchus’ inconsistencies): “Here Ptolemy criticizes Hipparchus as inconsistent.” (See fn 78 here on Ptolemy’s alleged consistency-fetish. And see Almajest 4.11 & Almajest 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 used Greek astronomy in some way or at some time. The original (and still prime Muffia basis for suspecting 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 Bablum-level System A solar scheme, as shown at Jones 1991H p.118. (The reconstructed value is 94°41’57”, only a trifle over 3°27th of Hipparchos’ Spring, 94°41’2.”) Question: what sort of “Editors” would you use this kiddiecar, without ever reading the fine print? I.e., if the computed Babylonian Springlength agrees with Hipparchus’s 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? Wellllllllll, Muffiosi

86 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.

87 A point never faced by the Muffia: if the “Babylonian” month M₁ (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 M₁.) In DIO 1.1 [§6 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 1₆₄ (though getting better), the Aristarchos tropical yearlength is consistent with a monthlength which is within 1° of M₁ and of reality. (So, whatever month he actually used had to be near or — as I believe — equal to M₁. See fn 95) I.e., we can trace a chronology of steady Greek improvement here. (See also Neugebauer 1975 p.601: noted at Rawlins 1991H [B11.]). What similar information do we have for Babylonian astronomy?

88 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 §B].)

89 Which also suggests to Jones 1991H that even if Hipparchos was using his famous e & A, he might have used “improved trig [script] Babylonian function” (Jones 1991H p.103).

90 Obvious point in passing: Babylonian astrologers who adopted the System B lunisolar month M₁ (eq. 6) would be more likely to use a solar yearlength equal c.235M₁/19 (§E3) rather than the yearlength-monstrosities of Jones 1991H.

91 From the viewpoint of sane 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
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 Muffia 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 other Greek astronomer’s: see Neugebauer 1973 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):

F2 The Muffia’s Bablers 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? Whabout-the-other 3 tires, ya say? Son, ain’t a smaller buyer like yew seen the latest thing in hypothetical automobiles? Why this rosso-4-door beauty righterheese’s the “Booolumian Unicycle”: the world’s first ONE tire sedan. Engineern’ geennys! Just imagine the savins into the Bablum-level System A solar scheme, as shown at Jones 1991H p.118. (The three short of Hipparchos’ Spring, 94°41’57”, only a trie over 3°27th of Hipparchos’ Spring, 94°41’2.”) Question: what sort of “Editors” would you use this kiddiecar, without ever reading the fine print? I.e., if the computed Babylonian Springlength agrees with Hipparchus’s 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? Wellllllllll, Muffiosi
By their cohesive harrassment of R.Newton’s creative contributions, Neugebauer’s clones hounded Newton to his grave. (The Mufa now has the equally genial Macbeth family’s traditional residue90 problem on its sanguinary hands.)90 The Mufa has also attempted to kill off DR in the same fashion, by the usual banishment & slander (fn 2, & DIO 1.1 §48, §§7, permitting no right of reply (e.g., §§113 & DIO 1.1 §5 §§A9 & C10). But it has instead merely scotched a Scot.91 So, there’s [Marlowian] justice in R.Newton’s intelligence now coming back from the dead to haunt the Mufa, by lodging here the single 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 Mufa’s amusing superiority-complex, as Newton has elsewhere92 pointed out) adopts Kugler’s speculation (fn 68) as fact: “the summer solstice date given by Hipparcos derives from the traditional parameters 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 Hipparcos fabricated his summer solstice on the basis that his [HI’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.118, from Bowen & Goldstein 1988 pp.68-69.] Has he tried calculating the statistical significance of this agreement? When we remember that Hipparcos’s solar data [twenty extant in Almajest 3.1] are all rounded to the quarter day, there is no statistical significance to this agreement.

90 Macbeth 1.7.2.2.3.2. See in 157. (And fn 29 …)

91 Macbeth 5.1. As R.Newton’s health declined towards death, Gingerich 1990 tried washing up a bit, by owning that Newton had been treated as “something of a pariah in the history of science community.” (Rather like Hon. Joe.Cutler & one of another brilliant Johns-Hopkins scholar. Ok, I must warn intelligent editors that the proposed theories are not well-founded. (Such attempts to kill off DR in the same fashion, by the usual banishment & slander (fn 2, & DIO 1.1 §48, §§7, permitting no right of reply (e.g., §§113 & DIO 1.1 §5 §§A9 & C10). But it has instead merely scotched a Scot.91 So, there’s [Marlowian] justice in R.Newton’s intelligence now coming back from the dead to haunt the Mufa, by lodging here the single 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 Mufa’s amusing superiority-complex, as Newton has elsewhere92 pointed out) adopts Kugler’s speculation (fn 68) as fact: “the summer solstice date given by Hipparcos derives from the traditional parameters of the Babylonian System A solar theory.” R.Newton’s comment on this (to DR 1980/9/14, boldface added):

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92 Macbeth 3.2. DR is, aptly, almost as Scottish as Macduff. (“Mac” is rumored to be an obscure north Scotland dialect’s synonym for “kick.”)

93 R.Newton (DIO 1.1 §5 [D14]) notes that HamSwordlew 1981 (HS) presents certain examples which “concern matters of controversy, but HS do not mention this point. Instead, they choose one side without mentioning the other side, and then show my alleged lack of understanding by demonstrating my differences from their viewpoint, which they present as established fact.” DR adds: [a] The Mufa has always thus portrayed its Enemies as incompetent or nutty. (See fn 99. [Note added 1993: Also DIO 2.3 [6 §E2].] [b] Sometimes, there may be good reasons for calling another scholar such names. One may wish to call another incompetent, if he falsely pretends to math or science skills; perhaps it is appropriate to call a figure crazy or dishonest, if he (like Ptolemy) sells out to orthodoxy and pretends that inferior planets’ obvious heliocentric movements are illusory (see DIO 1.1 §8 §B). But these cases are a long way from mere difference of opinion on issues where reasonable persons can disagree. Another common basis of academic superiority-airs is an enemy’s mere ignorance or unファッションabilty [note added 1993: DIO 2.3 [6 §E2]; an opposition scholar perhaps does not know of (or does not care for the interpretations of) a cult monograph. There are those who would call this incompetence. Centrist journal articles routinely denigrate those who aren’t up with the latest fad. E.g., at Isis 82.1/12, a book reviewer sniffed: “If the generalizations were accurate and based on current research … one may see from §§13, OG & his fellow Muffies have since expended years of bizarre rationalizations & hundreds of outdated prejudices”. (Instead of current prejudices?) Likewise at Isis 83.116-117 (1992). Similarly, Gingerich 1972 rejected the scholarship of R.Newton 1970 by saying that it naively accepted “the controversial and now rather outdated hypothesis (adapted Hipparcos’ star catalogue for the earlier longitudes)”. Incidently, as one may see from §§13, OG & his fellow Muffies have since expended years of bizarre rationalizations & hundreds of outdated prejudices”. (Instead of current prejudices?) Likewise at Isis 83.116-117 (1992). Similarly, Gingerich 1972 rejected the scholarship of R.Newton 1970 by saying that it naively accepted “the controversial and now rather outdated hypothesis (adapted Hipparcos’ star catalogue for the earlier longitudes).")

A shame that the hypothetical referees at JHA & Isis never thought of this crucial point (assuming they even understand it!) — which tears the very heart out of the Jones 1991H & Jones 1991M Hipparcos solar arguments.93 (By contrast, this criticism does not at all affect DR’s EH orbit proposal, below at §99 — since the EH orbit is based upon Hipparcos’ data, not the reverse.)94 In brief: the entire argument for a Babylonian base to Hipparcos’ Springlength is preposterous. In the precise literal sense of that word. (Just as Ptolemy’s solar fakes’ overprecision is preposterous: fn 64.) The precision-situation reveals that cause&effect have been either inverted or (almost certainly the case here) imagined.

One other point in passing: what sort of philosophy-of-science criteria can have led JHA and Isis into promoting their Jonesenest papers? — papers whose mathematical deductions never come close to getting into a convincingly coherent picture. (The unsupported Jones 1991H Babylonian-cencil scheme remains for eclipse trio A must be scrapped — and, for trio B, replaced by a largely different unsupported solution. And no solution at all is achieved for the 3rd solar-position trio. See also fn 85.) The papers’ incoherence is obvious not only from their math details — but even more so from Jones’ own excuses & comments (pp.104 & here at §M5). So the most rudimentary Occamite instincts would have warned intelligent editors that the proposed theories are not well-founded. (Such aircastles thrive when genuine curiosity and flexible intelligence are replaced by dogmatic ad-hoc thinking, merely masquerading as the former. It is a disservice to an enthusiastic contributor such as Jones when the journal does not attempt to restore him to his senses.) I have impressed such considerations upon Isis in past correspondence.95 Wasted advice in that direction.

94 It is possible that, even after several readings of Newton’s point, Muff/session still won’t get it. If so, I shall here attempt again to disturb their invincible innocence. Of course, one might (ignoring unicitycility’s inherent importance) advance the Babylonian-Hipparchos relation by supposing a different causal arrangement. (We are about to see that such salvation is unlikely.) But Newton’s observation instantly vaporizes the Springlength-agreement “evidence” for the already questionable Mufa theory that Hipparcos’ Springlength had a Babylonian origin.

95 I emphasize this because, after the Mufa realizes — to its unspeakable joy — that its least favorite scholar has found the “impossible” Greek solutions to all nine of Hipparcos’ Almajest 4.11 and 5.3&S solar positions, this sinusoid lot will be ready to Memory-Hole its own previous denial of these solutions’ very existence — and then just fall back to the defensive position of claiming that: either Greek or Babylonian solutions could work, so it’s just a matter of which side has the superior Feel for ancient astronomy. Won’t wash. Newton’s argument proves positively that the Mufa’s long-beloved causal order Babylon-to-Hipparcos Hetet is merely false for the Sun but reflects hilariously revealing nonrealization of the plain implication of data-precision here. It also shows what the Mufa’s vaunted overview-wisdom is actually worth — i.e., how genuinely perceptive is the “expert” judgement, that underlies the entire Mufa con: that Seleukid-era Babylonian mathematical astronomy made vital intellectual contributions to high Greek astronomy. (The Almajest contains higher-level astronomy than anything from Babylon — partly because the selection-discrimination of the Mufa’s own chief hero, whatever his downsides, was far superior to no selectivity: the random rubble of Babylonian cuneiform astrology-test-cachets.)
G TrigOut Orgy

G1 Now, it is essential also to realize that our Mufia-JHA triumph (§E1: Babylonian arithmetical methods underlying Hipparchos’ 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 astronomy77 as well as of trigonometry)98 was drawn instead to the crude, infantile (non-trig) Babylonian step-function for solar speed. G2 A few passing comments on Hipparchos & trig. (Note that in our later developments here, leading to eqs. 19 & 20, we will carry out precise reconstructions of Hipparchos’ math, which are consistent with the availability to Hipparchos of extremely accurate trig tables: §N14. This lowers the likelihood99 that trig was a novelty in his day.) Toomer 1988 makes the following assertions: [a] There is no trace of trig’s existence before Hipparchos. (See p.361; to support Hipparchos’ use of Toomer’s proposed chord table, n.44 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] Hipparchos 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-Mufiosi 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 Hipparchos’ 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 Mufia try at finding its precious Babylonian simpleton-arithmetical methods in Hipparchos’ work has foundered: fn 73.) Final paradox: Jones 1991H p.113 asserts that his Babyl-Hipparchos scheme’s apoege agrees with the “longitude that Hipparchos 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 3rd & 2nd centuries BC, Greek astronomy was using transit circles (Timocharis at 305 BC: Almajest 7.3), astrolabes, and trig — all of which Babylon lacked. Greek astronomical math and observations were far more accurate than the Babylonians’ relatively childish efforts in this direction. (Even astrologers choose superiors not inferior to copy. E.g., today’s astrologers use the orbits published by the US Naval Observatory, not those of indoor occultists.) So, why would sophisticated scientists have a Mufiosa101 passion to depend upon primitive Babylonian astrologers? (See §G2.) Like trading Chartres for a shack. Moreover, in order to prove this alleged dependence, Jones 1991H soberly (in a JHA pageone paper, I remind the reader) proposes a yearlength revision that may constitute the most creative & epochal discovery ever published in a purportedly serious academic publication (the sort of Big discovery that fully justifies clipping libraries for $126/year): 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 not of 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 beat 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. (Mufiosa slander nemesis R.Newton as “Velikowskian”: §M7. JHA #2 Editor O.Gingerich describes DR similarly: e.g., 1983/11/14. But envy works in mysterious ways. Velikovsky’s W.C. Chap.8 proposes a 360th 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 JHA discovery, of Hipparchos’ 366th solar theory, leaves my dull & 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 and 3/4 of a year, respectively, in the intervals from Hipparchos back to Aristarchos’ & Meton’s famous S.Solstices! And, from the time of Hipparchos back to trio A is about 240, so the error of his computed mean longitude would be about 3/5 of a circle. (Merely during the brief trio A’s occurrence, the net error would be almost a full degree — this in a precision-context at least an ordmag higher. . . .) So the impervious author just blithely suggests that Hipparchos mightn’t have noticed or cared: perhaps (Jones 1991H p.110) “the yearlength was not a conspicuous element in Hipparchus’ solar scheme”? Jones 1991H finally comes floating down to Earth a bit at eclipse-trio B, supposing it was computed with an (almost) normal Hipparchos year nearer 365.5. However, even it has an error (Sun fast by 86 7/9 yr — far worse than any attested Greek yearlength that accumulates to 7 days or 7° over 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 Mufia 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 relation102 to each other. (See fn 290.) Of course, from Neugebauer 1975 (p.601 & Rawlins 1999), we know that all

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97 Tradition noted at Neugebauer 1975 p.319.
98 E.g., Sarton 1959 p.284.
99 It requires no crystal ball to predict exactly how Mufiosi will react to DR’s discovery: the Mufa will rule the entire development (culminating in §N14) invalid, because there could not have been accurate trig tables in Hipparchos’ time — and will probably declare that any suspicion otherwise merely betrays the theorist’s incompetence. Standard. (See fn 92 & fn 234.) I.e., Mufia hypothesis (nonexistence of trig in 2nd century BC) will be assumed in order to defend that very hypothesis from evidence against it. Little wonder these chaps praise Ptolemy’s equally phony & circular “methodology” (§E1; fn 78): faking data from theory & then using such computed data to “prove” that selfsame theory. Hmmm. Back when it was not believed that Ptolemy was guilty of this, an eminent Ptolastriest rightly called such behavior “swindling with the very method of science”: O.Pedersen 1974 p.23. But up-to-date orthodox Mufiosi now praise it as intelligent, admirable, or even progressive: e.g., Swerdlow 1989 pp.43, [Mufiosi] 1990 p.215. [Note added 1993: Pedersen himself, ever politically-correct, now swears (1997:276-277) at scoffed (fn 113) his own political correctness in such disparate solutions merely indicated unreliability of solution. No matter that DR’s deduced 1st Hipparchos obliquity (23°3’) is close to that Ptolemy attests for him (see fn 195), while the unattested 2nd value (23°4’) has been independently elicited by 4 different scholars from 4 separate ancient data sources (Rawlins 1991H fn 21): Hipparchos’ Comet, Strabo, Pliny, & the Almajest Ancient Star Catalog’s north portion. It may be that in (DIO 1.1 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 idem. Note: Swerdlow would gladly use a step 1/100 as gross, to try killing a Mufia-proscribed party: e.g., DIO 1.1 §3 [§13 & §12]. Hipparchian parameter shifts are attested right in Almajest 4.11.)
Greek and Babylonian astronomers, from a time long before Hipparchos, had the yearlength pinned down within a fraction of an hour. I presume no reader remains who by now cannot understand why DR has become such a devotee of Muffia & JHA output. It’s the best entertainment since the Gong Show & Benny Hill.

But how, you ask, has our learned JHA-lead-author managed to dispense with the idea (so attractive to limited, sub-Muffia, non-JHA-worthy minds) that a prominent Greek astronomer used (I blush at the presumption): Greek methods? Jones 1991H enlightens us by examining the 1st & 3rd eclipses of trio B, which occurred at very nearly the same day of the year (creating a potentially drastic problem in orbit-fitting): the 1st at −200/9/22 and (says Jones 1991H p.106) the 3rd at −199/9/11 (though the latter event actually occurred early on −199/9/12). Finding that calculations from Ptolemy’s trig-based Almajest 3.2&6 tables (Hipparchos’ prime solar orbit = “PI” orbit) produce highly discrepant results for these 2 eclipses, Jones 1991H fatefuly concludes that Greek trig-founded solar theory is hopelessly irreconcilable with these two Hipparchos eclipse reports. The underlying “proof” 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 of 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 refereeing there, and the paper received the Muffa’s customary well-deserved govt’ funding.

Additionally benefiting from the former’s superiority. After all, throughout, Jones 1991H does not say “trig”. The paper (says Jones 1991H p.106) the 3rd eclipse of 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: §§1B.) 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 (§10), and 2 predictions regarding Muffia integrity (§11 & §J7).

Having buried forever (§G4) the Muffia-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 of 128&127 BC (Almajest 5.3&5) — establishing yet another “impossible” feat. This Jupiter of the History of Astronomy judgement 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 1991/7 History of Science Society Newsletter p.35, noting that “several 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 [trigbased] solar speed [can] explain Hipparchus’ numbers. According to Hipparchus’s solar model, the Sun reaches its apogee (057°30’1’) approximately 67 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:

\[
\begin{align*}
\frac{\lambda_1 + 65;30'}{\lambda_1 - 65;30'} & \approx \frac{65;5'}{66;50'} \approx 0;58,26^\circ/day \\
\frac{\lambda_2 - 65;30'}{\lambda_2 + 65;30'} & \approx \frac{27;45'}{28;24'} \approx 0;58,38^\circ/day \\
\frac{\lambda_3 - 65;30'}{\lambda_3 + 65;30'} & \approx \frac{35;24'}{38;38'} \approx 0;55,54^\circ/day
\end{align*}
\]

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

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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 (§K6) 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.

106 Same slip perhaps occurs in date given (−199/3/19) for eclipse B2. (Actual eclipse start 3/19; mideclipse, 3/20. I say “slip” since neither error affects the noncelestial, 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 1906, almanacs & most scholars (exception: T.v.Oppolzer) used noon-epoch, not midnight-epoch. (The slips’ source was not Manitius 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 Muffia’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 (fn24 [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 Jonestown nor the Babylonian-arithmetical solutions (of Hipparchos’ solar data) above mere “conjecture”.

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

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 prove [§G7] that such a conception cannot be derived from any kind of tables or rules for solar longitude plausible in this period except the kind represented by the Babylonian System A.”


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 (fn170) of Britton 1967 contains 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: DIO 1.11 §H2.) This sounds at 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.]

111 Goldstein 1978.
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 Hipparchus’s solar longitudes was observed rather than predicted.114

**G8** However, the “abrupt” speed drop between 27°45’ and 35°24’ is simply another addition to the Mufa’s ever-waxing fantasy-catalog. It is based entirely upon several Jones 1991H miscomputations in simple arithmetic — not to mention turning a blind eye to obvious possibility (explicitly suggested115; by Britton 1990 p.5) that the computer’s apogee 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 Hipparchan 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). Didn’t this striking oddity alert anyone at 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 4.1 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* ($126/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 tangled 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°2/3 for the (correct) interval, 67°2/3 — which corresponds to 68°2/3. [A Gongggggggggggggggggggg formerly at this place has been moved to DIO 11.2 in 21, in honor of A. Jones’ correctness (vs DR’s error) on two *Almajest* planet mean motions.] The correct equations are:

\[
\begin{align*}
\lambda_1 & = 65; 30' \\
(1/4 t_2 - 68; 40' & ) \approx 63; 0' \\
65; 50' & \approx 0; 57, 30' /day
\end{align*}
\]

114 Jones 1991H n.37 adds that Toomer 1970H p.219 “has already suggested that at least the longitude for — 126 July 7 was observed.” Use of the astrolabe as an analog computer for placing the Sun (Pappos’ method, R.Newton’s mistaken preference) collapses 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’ — 126/5/7 solar longitude (closest of the three Almajest 5.3 & 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.9; & see Wlodarczyk 1987 pp.177), all 3 of these Hipparchal 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 (to 1'). Rawlins 1991H with computations from Hipparchos’ independently-reconstructed UH orbit (§G10). Toomer 1970H p.219 does not even bother justifying his speculation that the — 126/7/7 solar longitude was observed, but plainly Toomer was simply bothered by the datum’s large (14') disagreement with the Hipparchos’ solar tables (PH orbit) — precisely the supposed-discrepancy now eliminated by the UH orbit (Rawlins 1991H D5; D10; C6; C7), which effects gorgeous triple-1° dovetailing with the Jones-discarded hypothesis that Hipparchos’ solar positions were determined in precisely the (Greek-trig-based) fashion described at Almajest 3.8 (see Rawlins 1991H C9). Jones 1991H does not cite this DR achievement.


116 [Both crude & apocryphal (Britton 1990 p.5), & by jumping 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 (3D & JF) looking vainly for the solution to the numbers so swiftly solved here in eqs. 23 & 24. The professionally-convenient Hisst.cci 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 “idiots-savants”. (Lick Gauss?)] Such misperceptions (which have an obvious resemblance to homeopathy, & are about as true) thrive for a common reason: limited talents crave solace.

Further, since \( \lambda_1 = 128;7/12 \) (Almajest 5.3, Rawlins 1991H eq.29), the first numerator should be 63°05', not Jones’ (§G7) 65°05'. Gongggggggggggggggggggg . . . (NCSwormlow, the Mufa’s Capt.Captious, snidely attacks politically-disliked E.Rosen117 thusly in Isis 72/73, p.79: “Even addition and subtraction pose problems.”) One sees (as noted at §G8) that the Mufa’s fantasized drastic speed-drop (§G7 melts,118 once correct computations have deflated these entertaining JHA proceedings. Not to worry. The JHA attempted to — even boastfully (DIO 1.1 §8 §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 foupuls displayed at DIO 1.1 §8 §G5. So it’ll presumably likewise refuse to correct the 3rd-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.)119 Evidently, an observed-obscess-oldth of editorial integrity has its compensations. See DIO 2 11 5M.

**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') Jones’ allegedly unsatisfiable Hipparchos solar data. Using the ancients’ standard 1° = 1/60, and taking ε for epoch — 1279/24 Alexandria or Rhodos local apparent noon:

\[
\text{mean-longitude-at-epoch } e_1 = 180°1/12 \\
\text{mean motion } F_1 = 360°/365.25/4 - 1°30'0 \\
\text{apogee } A_1 = 67° \\
\text{eccentricity } e_1 = 2°1/3
\]

The 3 Hipparchal solar positions, which this orbit fits (and which Jones 1991H called unfruitful), are (Almajest 5.3 &): 128°7/12 — 1278/5 1/4, 37°3/4 — 126/5 2/4, 100°9/10 — 127/7/2 3/3. The UH orbit calculations are given at Rawlins 1991H §D9. The match is within c.1' in all 3 cases — though, before DR published the UH orbit, the discrepancies were mostly about 1°4/.

117 Whatever Rosen’s academic & temperamental shortcomings, he cited Swerdlow frequently & acknowledged that he owed several enlightenments to him: *JHA* 21/206; 1990. (This despite Swerdlow’s repeated jocular assaults upon Rosen.) DR’s response to Swerdlow’s slanderous attacks on RRN & 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 sometime arrogance. It is curious that Swerdlow fails to discern certain subsequent analogies.) Capt.Captious’ Mufa has yet to acknowledge that DR has ever contributed anything to the ancient astronomy field. The Mufa is proud of that pristine record. And the Hist.sci community’s top journal *Isis* has prominently taken part (§H14 in the effectively censorial and explicitly vindictive (DIO 2.1 §2 §H16-§H17) strategy it’s part of.

118 However, under the underlying ε & A are corrected to equal those of the UH orbit, the speeds will still appear not to gel fully with this model.

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 ε = 2°15’ (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 contretemps over Hipparcos' - 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 (if it never mentioned the correct publication). They know that the prime test of archonhood lies in always setting matters in proper careerist perspective and thus admitting that the Mufa's datasets are statistically hilarious, e.g., consistently confusing single-datum-error with error-of-mean. Luckily, well before the above-noted whole-degree Pliny connections were discovered by DR 1985/5/30-31; but, unlike the above, the Mufa's solutions were published before DR. As long as the Mufa continues to be honored by archons regardless of their careerist-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?

H2 Indeed, the topic could materialize quickly, due to the Mufa's incurable insistence upon its own genius & DR's anathematization. After the present unambiguous exposures, Muffisios must choose one of several typically slippery options. (Muffisios & DR are as one in our confidence that: Hist.sci. archons' policing of gangup-behavior will have even the camcordered L.A. cops begging for lessons.) These options cannot include frank admission of the above. "Rawlins is-right." (Despite vaunted Mufa linguistic facility, rumored-but-still-classified testing is said to have found that Muffisios lips, attempting to masquerade as a mathematician, are nowhere roundly interpreted.) DR cannot be right on any fact. In any sphere. After all, such admission might confer a hint of Reputability upon a heretofore already discoverd elsewhere (DIO 1.1 §7, §8) by Infallible

121 In a 1983/1/28 letter to DR, Isis reacted, to the dreary news that 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. "...what are the odds that Jones was treated likewise?"

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

123 See, e.g., ¶5, Toomer 1980 p.108, & DIO 1.1 ¶6 in 6. The last item exhibits Swerdlov's sarcastic attack on van der Waerden for allegedly slaty citation-practice. Curious. The Swerdlov 1989 discussion — e.g., p.30 on planet mean motions (also p.32 on Ptolemy's inferior planet eccentric, on whole-degree greatest elongations from Pliny 2.38-39) — of the extent to which Ptolemy's parameters pre-existed his observations, was positively obliged to cite R.Newton and Rawlins 1987 p.236 (especially item [5]). Naturally, it didn't. (See also fn 166. Incidentally, the above-noted whole-degree Pliny connections were discovered by DR 1985/5/30-31; but, unlike the equations of Swerdlov 1989 p.43, I do not believe that these were mailed to Isis. As long as Swerdlov continues to be honored by archons regardless of his careerist-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 1991/7 p.35 blurb on DIO concludes by noting (at my telephone request) DIO's "DIO's need to provoke a debate as a face-to-face debate at the level of the JHA's next establishment of a means whereby powerful Wise figures may be allowed (as DR predicted to OG, 1982/11/5) to appropriate credit for others' allegedly Uncomprehending prior work: the JHA Editors explain their 1980 principles (¶see fn 36). JHA 11.2.145 (p.146 item #5), "the first speccific occurrence of an idea is generally far less significant than its later emergence, possibly in other hands, supported by persuasive arguments." (Translation in practice: new discoveries had best be presented in a context supportive of Old Boy broker views.) E.g., ¶5 item [ii]. Academic's vultures know their business.

125 [DIO 1.1 ¶6: "Watch Neugebauer's clones handle the lovely UH discovery by [a] ignoring it, [b] attacking it, or [c] trying to grab prime credit for it." Explicitly, it's been method [a] so far, though implicitly Jones 1991M represents option [g] here, which DR did not anticipate in DIO 1.1.]
129 OG has a deep unstated stake in continuing a coverage 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 deed then compounded by [b] attempting to justify his false 1983/7/23 suppression of correctly-computed DR planet mean motion solutions. (See full math details at DIO 2.1 §3 [C3].) Almajest 9.3-4 Mars motion (degrees/day) = 0.2741,40,19,20,58 degrees/day. DR’s solution (see Rawlins 1987 p.237 for simple ancestor period-relation): 152145°/329621 days = 0.27,41,40,19,20,58 degrees/day. (For all 5 planets’ ancestor period-relations [whose validity is unaffected by the 2003 discoveries], see DIO 2.1 §3 [B]). Gingerich’s preferred solution (Moesgaard 1987 pp.46-47) is: 394920000°/8088987 days = 0.27,41,40,19,51,55. (NB: After common-factor cancellation, this solution requires 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.)

130 Numerator and denominator given explicitly at Almajest 9.3.


132 DIO 2.1 §3 fn 16.


134 [Note added 1993: consult Dave Barry’s equally-explicit (& equally-ignored) rats at DIO 2.2 §8 [F25].]

135 Numerous graphs in [Mufa 1990] bear two curious errors: [a] inversion of axes, [b] scale-error by a factor of two. (We herefore conclude that [i] Mufa’s ‘factual conclusions’ are false, and [ii] Mufa’s ‘factual conclusions’ are false.)

136 The pages fall out of [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 [Ed.].) The sole evidence Jones adduces to support [the] star-placing hypothesis is Hipparcos’ reference (for 1 of the 3 sights) to the “course” of the Moon in a 248 day table of 9 anomalistic returns (see Jones 1983), as if that established Hipparcos’ prime interest 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 (nearly 4°) longitudinal 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: \( t^2/2 \). (See fn 139.) [d] R.Newton 1982 (pp.64) appears to be the first to note the significance ($\S$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) is 264°. And [Mufia 1990] (p.153, in his section 5.4) 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 ([Mufia 1990]) was turned out under the impressively “Editorship” of Mufia capo G.Toomer, BrownU’s History of Mathematics Dep’t. Despite that exalted advantage, [Mufia 1990]’s 250° result is wildly false, being based upon 3 serious snafus: [a] The curve ([Mufia 1990] fits to Catalog star longitude errors (\( \Delta L \)) is so misplaced (to the right) that the bungle is obvious from the merest glance at the figure. ([Mufia 1990] p.152-153 Fig.5.25). Note: the abscissa scale on this figure is linear in factor 2. Same for other figures hereabouts.) If the curve to be fitted is taken to be of the form (Rawlins 1982C p.366, equivalently to [Mufia 1990] p.152)

\[
\Delta L = z - \sin(\Delta - \Theta)
\]

(1)

(where \( \lambda \) = stellar longitude, then: the curve drawn ([Mufia 1990] p.153 Fig.5.20) has Catalog-stars’ longitude-error curve phase \( \Theta_L = 120° \); however, my own least squares fit of the sinusoid [of the sinusoid ([Mufia 1990]’s desire: eq. 1), to [Mufia 1990]’s data points, indicates instead: \( \Theta_L = 112° \) (weighted) or 104° (unweighted). I will use the latter figure since it is close to a DR (weighted) fit for the Catalog zodiacal stars (Rawlins 1991H §F2).

[b] The most astounding & impressive achievement of [Mufia 1990] is: reversing the sign of the Ptolemy solar theory’s error curve. [Mufia 1990] neglected to notice that the correctly rendered curve of the Mufia’s own Britton 1967 pp.51f, 65f (cited by [Mufia 1990] p.150 n.18 as his source)\(^{122}\), is calculated-minus-Ptolemy, not the (now standard) reverse sign-convention. [During this research, [Mufia 1990]’s author was a regular visitor at the Princetitute.] For Hipparchos’ time, the PH solar theory error-curve phase was about \( \Theta_P = 62° \) (Rawlins 1982C pp.370-373,\(^{143}\) but [Mufia 1990] makes it c.242°. Thus, we know that he temporarily concluded for something very close to this figure, since he speaks ([Mufia 1990] p.153) of fundamental stars rising 4° after sunset (i.e., about 240°

139 Even Ptolemy, for his alleged Sun-Moon-star astrolabe observation-pair (fn 146, Almajest 7.2), knew enough to have a Moon observation near the (sunrise/sunset) horizon: §II. For this purpose, the seemingly advantageous feature 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 (not minimized) by this situation.

142 As clever as Hipparchos’ choosing null parallax: Ptolemy’s lunisolar report (also zero parallax) at Almajest 5.3 is adeptly identified when he eqz etr for Sun&Moon are at their maximum by his simple models.

143 This very section of [Mufia 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.

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

145 Notes added 1992&1993: Just when you finally think Mufiosi math can’t get any funnier, those lovable imps pop right up and restore condence 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 [Mufia 1990] by James Evans (Evans&[Mufia 1990] are the Mufia’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 fake, while [Mufia 1990] attacks RN-DR by saying it was fake — which creates a bit of a problem for reviewer Evans. Which is why Evans says it by: [a] half-saying (p.67) he doesn’t agree with [Mufia 1990]’s charge, while [b] half-implying (p.66) he’s always somewhat agreed with 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 JHA’s lively imagination has already seen existing ex-vivs have it botched killed off by new mistakes and vintage parables of his own creation.) (Evans can hardly claim ignorance of Rawlins 1982C since, in the old-reliable Isis 5.3&5 data: Jones 1991M p.336, citing Evans 1987, an article which is immediately followed by Wodarczyk 1987 (on the same subject). If [Mufia 1990] read Rawlins 1992, they would miss these formidable flaws.\(^{146}\)

146 The astrolabe is described at Almajest 5.1; its use for placing stars is explained at Almajest 7.2 for a 139/23 sunset lunisolar observation of Regulus, though (as noted at Rawlins 1982C p.373 & Rawlins 1991H §F5) the large angular distances permitted (between Sun, Moon, and star) are not wise. (See also fn 139.)

147 See fn 138. The sole important exception may be Regulus, whose unexpectedly large error can be explained by supposing it to have been located by a dawn crescent Moon observation when the Sun was near its error-curve minimum, −23° off the mean, at longitude 152°, about 30° ahead of Regulus: that would cause a phase-shift of −2hrs, the reverse of the usual, which would carry the full negative solar error right onto Regulus. (However, there may well be another, unrecognizable explanation for Regulus’ anomalous error — perhaps connected to Regulus’ ancient use as a sidereal marker. Regulus was δ is the nearest 1st magnitude star to the ecliptic.)
armillary astrolobe. Final comparison: whereas [Mufa 1990] (& Rawlins 1982C) star & PH solar error-wave amplitudes disagree by about 0.2°. DR discovered a 1 match of stellar & UH solar error-wave amplitudes (Rawlins 1991H §F3).

15 So, the question suggests itself: did Toomer “Edited” [Mufa 1990] actually read Rawlins 1982C? — and, if not, how did the paper Rawlins 1982C end up: [a] having its conclusion rejected outright of hand, and, [b] being cited & listed in the bibliography of [Mufa 1990] (p.340)? Since we have [§14] just been wondering how [Mufa 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 ([Mufa 1990] p.167 n.42, which instead mis-spells DR’s name) where [Mufa 1990] aschans the central new experiment of Rawlins 1982C, it is curious that the only papers cited are secondary (publication) (1979-1981) discussions of the paper’s argument, not Rawlins 1982C itself.156 But: are we to believe that a $70 Springer-Verlag 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 Mufa’s very own bibliography ethics-monitor, Noel C. Swerdlow, who (falseely)151 believed he had apprehended a far less serious bibliographic slip by R.Newton. Swerdlow 1979 (p.528): “it is best to be clear about one’s conduct, especially” when discussing matters of fraud.

16 The test (of the Catalog’s authorship) invented by Rawlins 1982C is simple: a gross 1.1 mean longitudinal mis-set (which all parties agree infects Ptolemy’s star catalog), of the Catalog-observer’s armillary astrolobe, would produce error-waves (amplitude 1/2) throughout the Catalog’s latitudes152 & northern longitudes. But least-squares investigations do not find these waves. A concurrent ali-binpiration by O.Gingerich (to evade a different Newton argument) caused OG to suggest that perhaps Ptolemy (insanely) observed the Catalog using153 Hipparchos’ obsolete longitudes (mean 137 AD error: 3°34′ instead of 1°1) for his principal stars — and then later Ptolemy computationally added 22°2′ of precession onto all the thousand-plus stars! Not only inexplicably circuitous & blooper- risking 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′ 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” (p.167). The well-known (see DIO 1.1) Catalog rms errors σr = [Mufa 1990] (p.80) are about 1°3; it’s in this context that [Mufa 1990] claims waves of amplitude 1°2′ (five times larger than σr) are too “small” to detect!

[Note added 1993: similar if less egregious NCS [slip] at JHA 2.3 [f in §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) junk’s a least squares demonstration, without coming to the point: DR may not recover from the disappointment, the authority-approval deprivation, the involuntaryショッピング. Little Muffa “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-J.HA 2.3 §8 &C10-C16.] The Muffa seems unaware that continued resistance, when proof is certain, leads only to ethical self-evisceration, with long-term costs for facade-lose artifact.

17 Disoriented readers, perhaps unfamiliar with Muffia fairground, are urged to recall our earlier account (§11) of astonishingly elastic Muffiosi flips & springsuals over the Star Catalog issue. It may all down to this: Muffia 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 ([Mufa 1990]), [b] publishing this, 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 gall155 to try that one.) [Mufa 1990] can’t attack R.Newton often enough. In just
a few paragraphs (pp.88-91), he manages to call him “superficial” (twice), suppressive, blinded by prejudice, adding “Newton’s arguments against Vogt’s article show his small understanding of it.” [Muffia 1990] concludes this salvo by quoting the statement of his book’s “Editor”, Boss Toomer, that Newton’s scholarship is uninformed, uncritical, & disreputable (Toomer 1984 p.viii). Note: This is “Editor” Toomer in action in 1990, promi-


18 There is much of the comic in the Toomer-Muffia [1990] ploy’s clumsy transparency, its politically correct intolerances, its amazing mismath. 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, Tolemay’s Catalog plagiarism) — while suddenly, belatedly, & oinkly bestowing upon themselves ALL of the credit159 for proving that very proposition? (See §H2 option [f].) And [Muffia 1990] is additionally saying ([Muffia 1990] p.215), with the evident approval of Springer’s “Editor” Toomer, that Tolemay’s plagiarizing one of our oldest astronomical heritages (Hipparchos’ 1025-star catalog), shows Tolemay’s “methodological progress”.

19 Evolution in action (fn 72): on the Star Catalog, the original Muffia position #1 was denial (fn 66, §§1) that plagiarism had occurred. Muffia-consensus-alibi #2 was that Tolemay’s forgery was normal ancient science: see Sci Amer 1979/3. When this was disproved at Rawlins 1982 n.12 (& see DIO 1.1 §1 fn 24), the new tack was amusing: forget

156 In the Small-Understanding dept: [Muffia 1990] repeatedly speaks (pp.85, 88, & 162) of R.Newton’s critical fractional-degree-arguments as presuming that the Catalog observer’s astrolabe ring was graduated in half-degrees — and bases one key counterargument ([Muffia 1990] p.163 item ii) on this understanding of RN. But the truth is that R.Newton 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 & (cited at [Muffia 1990] p.167 n.42) R.Newton 1979F. After completing his attacks on his own half-degree straw man, [Muffia 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 discrepancy Newton’s “internal inconsistency” caused by a “slip”! (No other scholar, e.g., K.Moesgaard, 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 RN is a repressively unethical Scholar). Springer-Verlag, these are your experts.

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

158 And Macbeth tired of killing. Once it had placed him upon his (uneasy) throne. See §§3.

159 E.g., Toomer 1975 (quoted at fn 289). He has retracted none of a decade of such slander — instead publicly circulating similar attacks in [Muffia 1990]. See like sincerity (& use of others as attack animals) by Editor O.Gingerich, noted at DIO 1.1 §13 in 3.

150 Upon hearing 2nd hand that Graßhoff was persuading Toomer of a Hipparchan origin of the Catalog, DR wrote K.Hertzog (1986/8/26): “Rather typical that [Toomer] won’t admit that the Disreputable (1984 Almajest, p.viii) Newton-Rawlins work has influenced his possible upcoming conversion re the Star Catalog. Said he’s a scholar of truly admirable talents and accomplishments, yet infected with that familiar (Neugebauerian) arrogant cliquishness.”

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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 Rawlins 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 (Rawlins 1982G) were received by Isis, these were immediately approved for firsthand publication before DR was informed of anything — much less offered a chance to retract, had that been appropriate. (And the original DR paper had been secretly sent to the Mufa by Isis without DR’s permission.) These comments (spun off of 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 no 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 Ptolemy’s 2 clumsily contradictory false observations of Venus’ 136 AD max elongation: see Rawlins 1987 p.236 item [4]) approved by eminent ref.[s] (C.Wilson, K.Moegaard) 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 the “specialized” subject of ancient astronomy (a ban that no one knows fails to be privately loath; see Goldstein & Bowen 1983 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 attacked (I don’t know whether I’m just super-subtle or what; but, somehow, I got a glimpse of a hint of a vague impression that Isis wasn’t exactly breathless-anxious 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 two of the central equations164 discovered by the 1983 DR

164 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 between 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 annoyance (since c.1984) of involvement with Mufa 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 §1 §11, C7, C12, fn 37, 76 in 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 motto: A man who can’t be bribed, can’t be trusted.) One must sympathize with R.Newton’s frustration at fn 9.

165 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-loser 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 in OG1989 (sent 1989 but when we alter the author’s name to that of someone nondeliberate, it is never cited. The sole reference to it — in R.Newton 1989 in OG1989 is referenced. But even though the very same scribal error, which Swerdlow suggests R.N got from Taliaferro, was (as Swerdlow states) also in the Halma translation. Simple consultation of R.Newton

166 E.g., nowhere does Swerdlow 1989 p.54 acknowledge that Ptolemy’s fugacity (fn 166) confirms the long-held (Mufa-loathed) position of RN & DR — whose very names are deliberately (fn 90) never cited. The sole Swerdlow 1989 mention of the issue of Ptolemy’s integrity is p.30 n.1 citing Swerdlow 1979 (an abusive attack) upon R.Newton 1977. Thus, none of the wealth of prominent skeptical papers published since 1977 (e.g., JHA) is referenced. (R-Newton is a very choosy individual; 4 items [a-d] are referenced.) But, even though the very same scribal error, which Swerdlow suggests R.N got from Taliaferro, was (as Swerdlow states) also in the Halma translation. Simple consultation of R.Newton

167 But I cannot complain, since two of the central equations164 discovered by the 1983 DR

[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 Polymath Controversy? The Muffia’s usual tactic for 20 years has been: mention of the Polymath's publications (Toomer told DSB this proves nothing meaningful). The Intention: he might actually have inside-back-cover publication’s statement, which declares that the blunders made here to automatic submissions to, e.g., JHA & Isis. Neither took up the similar suggestion at DIO 1.6 fn 4.) The most meretricious description of these archons is that they are, regrettably, glacially slow learners.

J The Last Shall Be First: Muffia Publication-Scene

The same must certainly be said of the History of Science Society. Its little 1991/7 Newsletter’s p.35 (near the last page) quoted from DIO’s publication statement (editing it to hide the National Geographic Society’s 1973 election of DR’s cat to full NGS Membership); but neither the Society nor its main origin Isis showed (despite our explicit, not-holding-our-breath offer at DIO 1.6 fn 4) the slightest interest in telling readers of the competent & serious Scholar.-

Newton, Gingrich, Neugebauer [DIO 1.2 §§76&77]. I began seeing that Polymath was not the only fakery in this affair.171 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 but is also, well, Gillispie. 

Consider the Saga of Muffia’s publication policies here. The Muffia’s bylaws are clearly written in order to prevent publication of research by persons who are not members of the Muffia. (Some are.)

170 1970 pp.181 & 299 would have dissolved Swerdlov’s hypersuspicious hate-fantasy, since, as there noted, Newton’s Almajest chapter-citations used Halma’s unorthodox chapter-numbering, not Taliaferro’s normal numbering. (NCS Muffia Scholars [at conference 1983] insists that the standard translation is by K.ManiMOTE.) E.g., both the 1969 paper and R.Newton 1979E, 17 & 18 refer to Almajest 3.2 for the equations-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 Scholars.) Comments [a] On the basis of this publication’s claim that this work does not diminish unless the author knows he will be protected from exposure. (No need to speculate on this point: see fn 13 item [a]) [b] Expect no retraction. [c] The triviality of the original charge must have escaped the purview of the Muffia’s reviewing processes; its awareness of the alleged error is quaintly explicit. (DN: you need not bother trying to understand this, but you should be aware that even Muffia’s most casual reading of the issue has given rise to a reconsideration of the comments’ validity.)

171 How’s Britton 1967? Scheduled eventually to be republished in revised form, according to Swerdlov 1989 p.59 & Jones 1991H p.123-124. See fn 110. [Note added 1993: Republication of Britton 1967 indeed finally came to pass 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 R.Goldstein, N.C.Swerdlow, & 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 republication; thus, obviously curious errors survive intact from the original edition. E.g., see the missing reference at p.48 n.1 of the well-known Almajest 5.13 date of C.Polymath’s notoriously fake lunil parallactic “observation.” Also, the self-evident sign blunder in the last angular argument on p.142— a high-level math mathematician might have spotted this error anytime during the last quarter-century by a moment’s comparison to other next-last argument (whose coefficient is, incidentally, mistaken by 10⁻⁴). Britton’s new version is to be cited in every R.Newton work he can think of in 1993. In Britton 1992, however, is now that R.N is safely dead, it’s nice to find Muffia’s citation-practices finally rising to: honest. But, considering that Boardperson Moesgaard has since 1991 (see DIO 2.1 §2D [fn 35] have been well aware that DIO 1.1 & 16 (Rawlins 1991H) has solved the very orbit problem (fn 110) posed at Britton 1992 p.38 (1967 p.47), it is rather strange that Moesgaard co-authored the publication of Britton 1992 without urging a citation of DR’s success & its confirmation of Britton’s intelligent speculation.]
accurate orbital analysis (namely, Rawlins 1991H) underlining 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 journals' pseudo-refereeing, NONCITATION, and ancient astronomy goofiness (see DIO 1.1 ¶1 [C]). Reaction? Having not broadcast much Mufa output for the last few years, Isis (as soon as DIO appeared) evidently hired out a jetpowered taxicab and rrrooooomed right to the printer, to rush into publication — starting on the 1991/9 issue's first page — a hitherto Mufa study (Jones 1991M); [i] praising Ptolemy to 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 ([J14]), and [iii] basing its conclusions heavily on the unprecedented math of the Jostonan analyses of HIPPARCOS' SOLAR ORBIT WORK. (Yes, that's a LOUD echo of [J1].) And, understand, Jones 1991H is not just about orbit-fitting; rather, the paper is attempted orbit-fitting (to ancient positional data) — by a Mufa-promoted Hist.sci scholar who (though admirably well read in ancient literature) [is not experienced in] orbit-fitting, as the slightest expert refereeing [every young scholar's right, for his own protection] would instantly have discerned. In the days of Eratosthenes, suicide was advised as "the philosopher's death". But the Au—PPBalloon said-M-alchemy of those who turn Greeks into Babylonians has managed to make a comedy — a mass self-kill spectacular — even of the once-noble art of self-destruction. A clique who'd trust [the Mufa] for orbit-fitting, would cast Lily Pons in Brünnhilde's Immolation. (The more fastidious JHA would presumably insist upon Susan Alexander.)

Consider in review the impressive range & enormity of Hist.sci's perversity in its Jostonan extravaganza: [1] The author, whom Hist.sci archons were so frantically determined to place in the 1991/5 JHA and 199/19 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 miscomputed data (§G7 vs. §G9) 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 acceptance of the wildest yearlength (§G3) ever to adorn modern academic journals.

When first encountering DR's policy of evenhanded citation, Mufossi 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—nothing if not loyal to their favorite theories, e.g., [§D3 item [B1]]). As happen to know from direct testimony, some very prominent Hist.sci comers operate by a conscious policy of brainkissing archons. [Note added 1993: But none's technique is quite up to the earlier British prototypes quoted at DIO 1.1 ¶3 fn 3 & [§D3].]

Neugebauer 1975 pp.948-949 charges incompetence against the Mufa's ancient company, interpret others' compliments by what psychologists call: projection. (See fn 3 & fn 169.) I.e., Mufosi have never understood — and are incapable of believing — that DR praises their occasional valid work largely as just an occasion — it deserves some fuss, encouragement, & commemoration here.)

The emperor Augustus, born c.20/21 AD, ruled from 270 AD until his 275 AD assassination.

176 Since I criticize archons for slowlearnerhood, 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 Society Newsletter, was "creditable" 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 & conscientious AAS/HAD bibliography.)

177 [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 83:11, 1992/3, emph added): "our publication schedule [has] picked up speed . . . . The [UCHicago] Press has launched a major campaign designed to increase the circulation of Isis . . . . the Society and the Press are committed to maintaining the high editorial standards we have come to expect of our publications. . . . submissions have increased . . . intensifying the already keen competition for space in Isis. We continue to solicit high-quality articles . . . . Typically, we have each submitted manuscript evaluated by two or three experts in the field. The average time between submission and rejection is approximately 3.3 months . . . . We are currently able to publish articles within about nine months of acceptance."]

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

179 When first encountering DR's policy of evenhanded citation, Mufossi 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—nothing if not loyal to their favorite theories, e.g., [§D3 item [B1]]). As happen to know from direct testimony, some very prominent Hist.sci comers operate by a conscious policy of brainkissing archons. [Note added 1993: But none's technique is quite up to the earlier British prototypes quoted at DIO 2.1 §3 fn 18.]

180 The ancient text's failure to cite Pappos (320) or Theon (360) is perhaps indicative. But, a distant-future historian, with access only to Mufa capo literature, might similarly conclude that DR did not exist until the 3rd Millennium.

181 The ancient text's failure to cite Pappos (320) or Theon (360) is perhaps indicative. But, a distant-future historian, with access only to Mufa capo literature, might similarly conclude that DR did not exist until the 3rd Millennium.

182 Neugebauer 1975 p.949 n.6 forced "calculation" for "211/4/25: 30° 49'", text at Jones 1990 p.3 l.18: 30° 48'. Jones 1990 p.51 n.13 covers for ON here by stating that a result 30° — a half-degree — and thereby forces his solar mean longitude to equal the scibal error within one arcmin! (See, under fudge-Muffed calculations, at DIO 2.1 §3 fn 38.)

J3 As if the foregoing weren't grand enough: Hist.sci's Isis is as proud as punchy to be now published by the prestigious University of Chicago (N.C.Swerdlow's hangout), which is said to be set for heavy promotion of its fine new acquisition. (DIO & J.HA are assisting in this worthy publicity campaign. Gratis.) Any university which attaches the genie of Hist.sci to itself knows it is adding immeasurably to its reputation for competent scholarship; and, to celebrate the Univ Chicago's good fortune in this connection, what was 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.

4 In fairness to Jones, we should, however, note that his recent work, "Ptolemy's First Commentator" (Jones 1990), establishes a valuable 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 Rawlins 1984A p.983). Jones 1990 has value regardless of the precise correctness of its title. And Jones may 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 late as c.300 AD. Two other matters are worth note.

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

[b] Neugebauer 1975 pp.948-949 charges incompetence against the Mufa's ancient company, interpret others' compliments by what psychologists call: projection. (See fn 3 & fn 169.)
Carrying the foregoing NCS garbage-test corpus-rejection criterion (§J5) 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 (§J13) 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 J.HA 1.2-DIO 1.3 analysis is hereby submitted to Isis, with no editorial constraints whatever.) Somehow, I doubt Isis will so conclude.183 For the archon-angels above: double norms are the single norm.

183 See fn 104 & §113.