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• Publisher & journal cited (1996 May 9) in *New York Times* p.1 analysis of his discovery of data exploding Richard Byrd's 1926 North Pole fraud. [*DIO* vol.4.] Full report co-published by University of Cambridge (2000) and *DIO* [vol.10], triggering *History Channel* 2000&2001 recognition of Amundsen's double pole-priority. New photographic proof ending Mt.McKinley fake [*DIO* vol.7]: cited basis of 1998/11/26 *New York Times* p.1 announcement. *Nature* 2000/11/16 cover article pyramid-orientation theory: *DIO*-corrected-recomputed, *Nature* 2001/8/16. Vindicating DR longtime Neptune-affair charges of planet-theft and file-theft: *Scientific American* 2004 December credits *DIO* [vols.2-9]. *DIO*-opposites mentality explored: *NYTimes* Science 2009/9/8 [nytimes.com/tierneylab].

• Journal is published primarily for universities' and scientific institutions' collections; among subscribers by request are libraries at: US Naval Observatory, Cal Tech, Cornell, Johns Hopkins, Oxford & Cambridge, Royal Astronomical Society, British Museum, Royal Observatory (Scotland), the Russian State Library, the International Centre for Theoretical Physics (Trieste), and the universities of Chicago, Toronto, London, Munich, Göttingen, Copenhagen, Stockholm, Tartu, Amsterdam, Liège, Ljubljana, Bologna, Canterbury (NZ).

• New findings on ancient heliocentrists, pre-Hipparchos precession, Mayan eclipse math, Columbus' landfall, Comet Halley apparitions, Peary's fictional Crocker Land.

• Entire *DIO* vol.3 devoted to 1st critical edition of Tycho's legendary 1004-star catalog.

• Investigations of science hoaxes of the -1^{st}, $+2^{nd}$, 16^{th}, 19^{th}, and 20^{th} centuries.

Paul Forman (History of Physics, Smithsonian Institution): "*DIO* is delightful!"

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

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

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

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

British Society for the History of Mathematics (*Newsletter* 1993 Spring): "fearless . . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth."

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DIO

&

The Journal for Hysterical Astronomy

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NOTE Added 2013:

The neat discoveries contained here at *DIO 1.3* are expanded in *DIO 20* [2012] ‡3 §§F&G, with novel, surprising results.

DoublyGlacial Ethical Evolution in History of astronomy Ice Age

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

And DR Doesn't Even Believe in Miracles

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

* See *DIO 4.1* ‡4 ("Casting Pearls Before Pyglets"); *DIO 4.3* ‡15 fn 41 (& sources there cited — including here [*DIO 1.2*] at fn 129) and §I2; *DIO 6* ‡1 fn 1, §J4; ‡3 §§B1, D9, H; *DIO 7.1* ‡5 §B4.

DIO

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DIO is primarily a journal of scientific history & principle. At present, most *DIO* copy is written by Dennis Rawlins (DR) and friends (see *DIO 1.1* ‡1 fn 12). Each author has final editorial say in his own article. If refereeing occurs, the usual anonymity will not — except (if the author wishes) in reverse.

The *J.HA* is rumored to be edited by the intrepid feline explorer Admiral Purry, longtime member of the National Geographic Society (election through NGS Board of Trustees: certificate 1973/1/1) and of the American Federation of Astrologers.

Both journals' writings are to be considered as automatic submissions to the appropriate handsome (centrist) academic journals. I.e., permission is hereby granted to these journals' article-space, correspondence columns, and-or approved authors, to print matter from any issue of *DIO* (or *J.HA*), edited to these journals' alleged standards. Indeed, *DIO* encourages handsome journals' *open refereeing* & publication, in whole (except *DIO* vols.3&5) or in part, of *DIO* articles which clarify problems these journals (e.g., *Journal for the History of Astronomy*) purportedly exist to elucidate. No condition is set except this single one (which will presumably serve as a fully sufficient impediment to said hypothetical publication): *DIO*'s name, address, & phone number are to be printed adjacent to the published material & all comments thereon (then *or later*), along with the additional information that said commentary will be replied to (if at all) in *DIO*'s pages, not the quoting journal's. (Copies of the quoted material & attendant comments are to be sent to *DIO* when published & not before.)

DIO invites communication of its readers' incredulity, appreciation, nausea, empathy, scorn, support, and-or advice. (Those who wish to be sure of continuing — or not continuing — on the mailing list should say so. It is hoped that professorial readers will encourage their university libraries to request receipt of *DIO*.) Written contributions are encouraged for the columns: Unpublished Letters, Referees Refereed, and regular Correspondence. (Note: all letters received are accounted public domain. Comments should refer to *DIO* section-numbers instead of page-numbers.) Deftly or daftly crafted reports, on appropriate candidates for recognition in *J.HA*'s pages, will of course also be considered for publication. (A subject's eminence may enhance *J.HA* publication-chances. The writer's won't.)

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

Potential contributors: send to the above address a *spare* photocopy of material (not to be returned) and phone *DIO* about 3 weeks later.

Each issue of *DIO* will be printed on paper which is certified acid-free. The ink isn't.

This printing: 2018\7\29.

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[Note added to 2002/10/10 printing. Rev. 2012/2/26.]

Given the strong nature of some comments in the foregoing material, regarding classicist Alexander Jones, the publisher (DR) wishes to note that though Jones (like DR: www.dioi.org/err.htm) makes occasional reconstruction-misjudgements, he is an excellent & productive scholar in key areas of the field. Indeed, only a few years after *DIO 1.2*, he went on to make a shocking and uniquely valuable Hist.sci discovery (the sole surviving ancient ms record [103-104 AD] of an astronomer's observational verification of a long period-relation of a planet [Jupiter).

See our too-brief comments on this 1999 triumph in *DIO 9.1* News Notes.

Unfortunately, our gratitude to the Muffia's positives remains unilateral, and Jones has in the new millennium (no longer under the late Curtis Wilson's watch) increasingly reverted to cultist stolidity even in the face of the most unambiguous counter-Muffia evidence, degenerating into tactics of non-citation & shameless data-alteration, putting one in mind of the Jonestown 1991 crackpottery detailed in the preceding pages. (See *DIO 16* p.2 and ‡3 §§E, F, esp. incredible G4; and do not miss Jones&D.Duke's unprecedently persistent 6-fold destructive crusade, cataloged at *DIO 20* ‡2 fn 10, http://www.dioi.org/vols/wk0.pdf.) For this reason, our longago pacifist gesture (of suppressing Jones' name at 99% of the critical references to him in later editions of this issue) has been abandoned in 2015, with all original 1991 citations restored.

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

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

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

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

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

[1] The foregoing article analysing Hipparchos' lunar researches has been expanded in *DIO* Vol.20 [www.dioi.org/vols/wk0.pdf], finding that the Hipparchan 1° discrepancy in Trio A (above §M3) was due to a deliberately forged datum. Details at *DIO 20* §§F3&G1.

[2] D.Duke (*Centaurus 47*:163-177; 2005) tries displacing the foregoing spot-on hits by trying to force Toomer's method (above §§D1&P1) to work, by fiddling the data (*DIO 20* §K). A.Jones (*JHA 33*:15-19; 2002) plays a like displacement-attempt rôle vis-à-vis our Diller-DR discovery (*DIO 5* & *DIO 16* ‡3) that all 14 Strabo Hipparchan klimata fit sph trig calculations using accurate obliquity 23°2/3. Both authors assume ancient computational unreliability as conveniently needed (since they get no hits at all without such desperate speculation), while implying nothing is proved by *DIO*'s straightforward multiple perfect fits for ALL data: 4-for-4 (above eqs.19-20&23-24); 14-for-14 for Diller-DR (*DIO 16* ‡3 Tables 1&2.) Neither author even cites the *DIO* analysis he is attempting thus to devalue! — despite (because of?) knowing both were set for highly prominent re-publication at *Isis 93.1*:58-69 (2002 March) pp.60&67.

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

‡9 Muffia Orbituary

Hipparchos' Early & Frankenstein Solar Orbits Discovered
Thanks to: Topbilled JHA-Isis Proof They Couldn't Exist!
The JHA's Nonreffing, 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 heliocentrists' 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 upchuckling, can plunge directly (next page) into an extended Tragicomedy-of-Errors, starring the usual cast of eminent Muffiosi. 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 Muffiosi are not *always* right."

A3 Given their records to date, it seems likely that *JHA*, *Isis*, & Muffiosi 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-or 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 Disrefereeing

The editorship of the *Journal for the History of Astronomy*, raviewing[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 Refereed", *DIO 2.1* ‡3 fn 38.]

[2] Certain exposed Hist.sci archons have tried to portray DR as pure *praeceps horribilis*. A key consideration in evaluating DR's pungent recent evaluations of archonal misbehavior: despite occasional sharp private reactive criticisms (generally correct, though subject to change wherever evidence warranted) of Muffia pre-DR hide&slander tactics, DR's original submissions for publication were pretty mild. E.g., to *Science* 1976/11/20: "Those few U.S. scientists who had previously been aware of the [Ptolemy Controversy] must thank Owen Gingerich for his Aug.6 review [Gingerich 1976] of . . . [Neugebauer 1975] . . . . a commendable departure from the years of silence and systematic non-citation of [Robert] Newton's findings . . . ." To *Science* 1977/1/10: "U.S. professional historians of astronomy, long deeply committed to the position that Ptolemy was the ultimate personification of ancient astronomical wisdom, have affected a Beneath Reply freeze toward [R.Newton's mounting new] evidence . . . . I hope *Science* readers will consult the knowledgeable defenses of Ptolemy in: [Neugebauer 1975] (pp.2, 101-118, 283, 836, 894); [Pedersen 1974] (pp.131, 204-206, 248-258); [Gingerich 1976]; [Toomer 1975] (pp.189, 201: Ptolemy's "method was to [improve] existing theory . . . to get good agreement with observed facts.") and will compare their force with that of Newton's papers ([R.Newton 1973-4]; *Observatory 96*:166) & new book ([R.Newton 1976] p.411 & Chaps.5 & 11)." But OG&co disapproved. Zero space was granted R.Newton's side. Fifteen years of similarly admirable manipulations have earned Hist.sci archons the degree of respect *DIO* is showing them. I realize that: telling the truth in a corrupt milieu will gain one little but enemies, who will of course blame the truthteller for all friction. Another truth, which Hist.sci prefers to suppress: though at first frankly & pointedly critical of Muffia treatment of R.Newton, DR (recognizing some merit in Muffia output) long hoped for an amicable outcome of this controversy — and a pooling of all parties' respective talents, to assist a better understanding of ancient science. (See, e.g., *DIO 1.1* ‡3 fn 7.) The former aim has been killed by irrevocable mistrust. (*Spectator* 1711/3/27: "There is nothing that more betrays a base ungenerous Spirit, than the giving of secret Stabs to a Man's Reputation." See O.Gingerich's schiz private libels of DR at *DIO 1.1* ‡1 fn 20 — circulated behind-the-back even while, in his direct dealings with DR, OG was pretending to genial neutrality. See also *DIO 2.1* ‡3 fn 33.) But that has not prevented DR's unilateral pursuit of the 2nd ideal. DR's inept critics' determination to eliminate such a bad-for-business whistleblower (fn 266) is palpably stronger than their scholarly ability to accomplish that aim. (Which is precisely why Muffiosi braves flee face-to-face dealings, leaning instead on the crutch of whispered libels.) They want me bad? Well, they've got me bad.

[3] Quoting the wisdom of *JHA* Adv.Ed. AND *Isis* Adv.Ed. A.Van Helden, the neutral scholar *Isis* deputed to review the *JHA* (& its wise & quality-insistent Editor-for-Life) in the "Review of Journals & Serials" at *Isis 81.2*:280 (1990) p.298. No incest here. (For a summary of the outstanding qualities of the *JHA* & *Isis* papers under review in the present *J. Hyster Astron* paper, see below at §C6 & fn 92.) [Note added 1993: See *DIO 2.3* ‡6 fn 18.]

[4] See fn 9.

[5] The *JHA*'s #2 Editor, O.Gingerich, is ever at the watch to ensure high quality journal-refereeing. E.g., when a *QJRAS* paper of R.Newton's failed to cite certain material, O.Gingerich wrote *QJRAS* (1982/4/5): "I am somewhat scandalized by the refereeing standards for the *QJRAS* that let Robert Newton's recent article appear before serious flaws in its referencing to the previous literature were corrected." (Note classic irony at fn 167.) Without OG at the ramparts, British journal refereeing quality could get flabby and, well, it might even: Sag (*DIO 2.1* ‡4 §G1).

E.g., eminent BrownU scholars have discovered the previously unsuspected existence of the Winter Equinox (*Journal for the History of Astronomy 22.2*:101 [1991] p.119) and Autumn Solstice (*Dictionary of Scientific Biography 13*:321 [1976]). Both triumphs are praised at *J.Hysterical Astron 1.2* §B4. (Who can fail to repose perfect trust in said discriminating genii's judgement, as they condemn nonmembers of their clique as "disreputable", "incompetent", and "crank"?)[7] These Ivy League firsts will forever rank with W.T.Pooh's equally epochal 1926 discovery of the East Pole of the Earth. An understandably puzzled C.Robin has lamented such envy-submerged scientific breakthroughs: "people don't like talking about them" (*Winnie-the-Pooh* Chap.9). But *DIO* & *J.HA* are consecrated to dispelling this heartwrenchingly unjust neglect — concurrently revealing the proportional shares of competence vs. brainkissing requisite for Ivy League professorship, at least in certain disciplines. (So that there will be no charge of suppression against *DIO*: physicist DR must confess to being a Harvard graduate.) Granted, critiquing these frantic magnates' attempts at math and apologia is ofttimes about as challenging as shooting fishstories in a barrel of monkeys. But, the targets insist: the showbiz must go on.

Above all, this remarkably long-running Hist.sci farce has the social utility of illustrating (with the sort of preciously stark nonambiguity which laymen and even children can follow) just how seriously we should take top academe's aggressively-advertised Deep-Concern for maintaining open discourse and for ensuring reliable, non-crank expertise and refereeing. E.g., in the ongoing ancient astronomy controversy, the Princeton Institute for Advanced Study has profoundly invested[8] its reputation into the hyperglorification of the uniquely clumsy faker-astrologer C.Ptolemy, boldly and devotedly promoting this notorious occultist as "The Greatest Astronomer of Antiquity"[9] — a mission so special that the Institute has itself faked several calculations in support of it.[10] (These math pretenses have, with exquisite irony, simply backfire-enhanced the Institute-resented notion that Ptolemy would behave likewise.) Those eminent journals and scholars (e.g., the extremely handsome *Journal for the History of Astronomy* [*JHA*] and its esteamed Editor-for-Life) whose prestige has been hurled against skepticism of Ptolemy have also emulated the scholarly National Geographic Society's longstanding example of: [1] courageously hiding from open debate or mutual cross-exam situations, during 20^y of behind-the-back slander of dissenters, and [2] graduating from mere judge of an issue to championing-advocate of one side of that issue — ultimately becoming about as open to conversion-by-evidence as astrologers or parapsychologists.

Finally: both as justice and as demonstration of what certain handsome journals are incapable of doing, it will be the consistent policy of *DIO* to praise and utilize the occasional genuine accomplishments of our self-appointed Enemies — including even those of the brave Neugebauer klan, ever honored in these reverent pages as "the Muffia". To quote[11] from our premier issue: "The Muffia's essential attitude is that [hate-objects Robert Newton] and DR are not *ever*[12] right. . . . By contrast, the *J.HA* will merely show that Muffiosi are not *always* right. I recommend careful attention to this distinction. (Though, admittedly, [*DIO* is] not denying the tenuous possibility that the inverse of these propositions is nearer the truth.)"

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

The search continues.

[7] *DIO 1.1* ‡1 §C7, fn 20, & ‡3 §D3.

[8] *J.HA 1.2* fn 172. [Note added 1993-7: See also fn 63, §I3, & *DIO 1.1* ‡7 fn 13.]

[9] See, e.g., *Science 193*:476 (1976/8/6) or *DIO 2.1* ‡3 fn 28.

[10] See Rawlins *Amer J Physics* 1987 *loc cit*, & *DIO 2.1* ‡3 fn 38.

[11] *DIO 1.1* ‡1 §C12).

[12] This lockstep pretense's origins are examined at *DIO 1* ‡9 §D4 & §H2, *DIO 2.1* ‡6 §F4.

> In the Old West, when privately-financed pseudo-lawmen kicked Undesirables out of town, they followed Sulla's 1st century BC example by publicly proscribing them as fair game for the killing. The document posted for this purpose was known as a "White Affidavit". Up to now, there is no record that a Wrongo ever posted a response.

‡10 Black Affidavit

DIO & *J.HA* deem it a duty to keep readers ever-abreast of the latest exciting chapters in the serial Keystone-Kops saga of a certain almost-endearingly netherbrained academic cult of oldboypersons, which has ensconced itself at several high-prestige institutions, including even a few Ivy League universities. This generously-budgeted, extravagantly produced perpetual-opéra-bouffe stars a gaggle of outwardly arrogant, privately insecure businessmen-professors. The costuming is lavish: bumblers, *idée-fixe* zanies, and intuitional mystics are painstakingly dressed up as centrist, technically-expert authorities. Admittedly, the story line is ofttimes improbable; but, it never fails to entertain, with its ever-cornucopic menu of inspirational behavior: unscrupulous promotion of ludicrous cult theories over competitors' patently better-fitting[1] solutions — garnished with [a] cohesive slander and-or noncitation-freezeouts of Unapproved scholars, [b] convenient switching (or conjuring-up) criteria in midstream,[2] [c] ritual apologia or coverup for data fabrication, fudging, and-or plagiarism (including the cult's own),[3] [d] gang-up bullying and blanket denial of all discovery-credit to those who publicly dissent from orthodoxy's sacred-unfalsifiable prescribed historical vision.

It should be clearly understood that none of the above-cited behavior is wrong or deserving of the slightest censure. Indeed, leading History-of-science (Hist.sci) specialists stand ever unsleepingly at-the-ready to reprove reproof — alleging that ethical disapproval of academic hoaxery reveals nought but the commentator's own amateurishly "moralising" perspective.[4] In these gentlemen's learned opinion: faking, fudging, and plagiarism can be excused as trifling[5] or even defended as reflecting a "progressive" or Nobelist intellect! [6] Condemning a Hall-of-Fame scientist for fraudulent scholarship is an inexcusable, even mortal sin in the Hist.sci profession.

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

[1] See, e.g., Dennis Rawlins (DR) *Amer J Physics* 1987 n.30, *DIO 2.1* ‡3 §C3, *DIO 1* ‡9 fn 60, fn 73, fn 209, & §P2.

[2] E.g., *J.Hysterical Astron 1.2* ‡9 fn 99, §H3, §I1, §I7, & §I9. [Note added 1993: & *J.HA 2.3* ‡8 §C17, §C21, & §C31.]

[3] *J.HA 1.2* §J5, *DIO 2.1* ‡3 fn 26 & fn 38. [Note added 1993: & *J.HA 2.3* ‡8 §C8-§C13.]

[4] *Nature 276*:152, *DIO 1* ‡9 fn 96.

[5] E.g., *J.HA 1.2* §H2.

[6] See, e.g., v.14 of Springer-Verlag's series "Studies in the History of Mathematics and Physical Sciences", G.Graßhoff, *History of Ptolemy's Star Catalogue* 1990 p.215. See also Harvard's O.Gingerich, *Quarterly Journal of the Royal Astronomical Society 21*:253 (1980) p.264; *22*:40 (1981) p.43.

Such was hardly the 1st alert issued. DR regards such warnings merely as well-intended advice.[6] Hist.sci archons see them as: Backtalk-Sass — a far more serious offense than mere plagiarism, fraud, & suppression. Lord Hoskin, Editor-for-Life (EfL) of the *Journal for the History of Astronomy*, has (with mysterious imprecision about the details) told inquirers for years that DR is "impossible to deal with". The following excerpts from our correspondence will indicate how typically honest Lord H's slander is.

B2 DR to Lord H, 1983/3/14, assertively responding to the EfL's 1983/3/3 threats of legal action & future *JHA* nonpublication of DR work (EfL's chummy reaction to DR's merely pointing out that the central results of a recent pseudo-refereed *JHA* paper[7] 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[8] for years to improve your process of evaluating and filtering incoming mss (specifically, to replace an obsession with superficialities by attention to substance). In vain.
>
> . . . (You don't even realize that my opposition to [your editorial] policies was intended to help you . . . . I'm not expecting gratitude, but anger seems malapropos in the extreme.) . . . Are you seriously trying to . . . [call your 1982 referees'] cursory comments "effective" refereeing?!!

[6] On 1980/3/22, *JHA*-loathéd DR simultaneously sent 3 papers to the *JHA* for publication; however, these were of course submitted *for prompt refereeing, not for immediate printing*. (All 3 have since been published by far superior professional journals.) But the Editor-for-Life rightly feared that he couldn't positively count on the papers (especially all 3) being rejected by referees; thus, the EfL got panicked, while thrashing about for style-nitpick excuses (to explain why referees would not even be asked to look at any of the 3 papers), and so carelessly blurted out whatever alibi-concoction came swiftly to mind. The result of these combined elements was an invaluable miracle-of-chemistry creation: a Lord H statement that is almost honest. His Lordship's 1980/4/11 letter to DR claimed that, for the *JHA*, "Anything considered for publication must, prima facie, be in a state ready for the typesetter". 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]! I should add that editors who ask for ready-for-the-printer copy upon first 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 far] I will see referee reports; and you in both your replies [1980/4/11 & 4/28] did not answer this question." (DR was naturally curious about this, following R.Newton's 1979-1980 experience with the *JHA*: see *DIO 2.1* ‡3 §B2 & fn 8.) The inquiry was repeated 1980/7/30, "My [letters of 1980/3/22, 4/18, & 5/4] have asked the simple question: if we ever get to the referee stage, will I see the referee reports? Why do [your letters] persist in not replying [to this]? Is the question unreasonable?" Pushed into this corner, Lord H simply did not reply at all! (And nonreply may have had an extra impetus. From a 1980/9/2 letter to RN: the Editor-for-Life "has not replied to my latest [letter, 1980/7/30] — how can [EfL do so] when it contains repetition of [EfL's] remark [DR: see below at fn 8] about why [*JHA*] was publishing ———'s paper? [This admission] would NOT look good if he even *implied* assent by not contradicting it in his letter [of reply], but would lead to my calling him a liar if he denied it. So, silence — at least until he can be plausibly excused for forgetting it.")

[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 refereeing.

[8] See, e.g., fn 6. Also DR to EfL 1980/7/30: "During our mid-June phone chat [1980/6/13] . . . you mentioned an upcoming *JHA* paper by ——— . . . , saying that neither you nor your referees understood it, but that you couldn't refuse publishing it because of ———'s established reputation. I like ———, but that's not a good reason to publish him or anyone else. . . . [DR: see above at fn 6.] Michael, I much enjoy your personal company. (And I feel the same about Owen [Gingerich], despite our differences.) But I cannot refrain from telling you as bluntly as I can that this is no way to run a journal. . . . I suspect that the root problem is just the pressure of time . . . . Regardless, the upshot is — in effect — a fixation on the superficial & the swiftly-gauged, as against the substantial. For obvious reasons, not very many scholars are going to say these things straight at you. But they need to be said. And I think it does you more good to say them than to keep silent. . . . Best wishes to you and David [Dewhirst] & Simon [Mitton]". Perhaps it will save a wiser person than DR the trouble of repeating my experiences if I say that, with Muffians & other ruffians atop academe, private pleas (ethical or logical) are an utter waste of time. Power-operators are not bothered or impressed by anything but the prospect of public trouble.

> . . . 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 severed communication, thereby killing the already refereed, accepted, & advertised[9] paper Rawlins 1999,[10] then in the editing process. A statement in Rawlins 1999 was regarded as intolerable,[11] since it did not meet with the approval of the omniscient "Muffia", Ptolemy's modern protectors & showbiz[12] agents (introduced to our readers in *DIO 1.1* ‡1 §C5-§C13 etc.). Anyone with a sense of humor will enjoy comparing the banned DR sentence's temperate treatment

[9] See *Isis 73*:158 (1982/3). (The original title of the paper was: "Aristarchos' Tropical & Sidereal Years & His pre-Hipparchos Knowledge of Precession". But the *JHA* pushed DR toward its own preferred title, "The Babylonian Ancestry of Ptolemy's Year". Obsessive. Like fn 15.) And see *DIO 1.1* ‡1 fn 25. The main reason the *JHA* initially wanted to publish the paper was: it contained what was then my sole anti-R.Newton finding (which vindicated a criticism of RN made by Moesgaard, Swerdlow, & van der Waerden). The privilege of being published in the *JHA* was seen as an effective attraction, to start prying DR (R.Newton's most forceful supporter) away from RN's heretical view of Ptolemy and thus to isolate RN (a neat plan, lately applied to DR by the same volk). (This technique usually achieves its object in no time; its failure in this instance must have been an awful shock — so great, indeed, that failure was ascribed to insanity: *DIO 2.1* ‡3 §C9.) But *JHA* then still further refined its censorial filtration by cutely waiting until the very last pre-publication minute to suddenly insist upon deleting even *the sole small portion of the paper that backed Newton's thesis*. DR would not assent to this ploy; thus, Rawlins 1999 remains in acceptance-limbo: publicly accepted, but not withdrawn, and not published. (The paper is crucial to the tracing of eq. 6 here to Aristarchos: fn 81, §O8.) 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* ‡1 fn 11), he was still unsubtly hoping to woo DR away from sympathy for R.Newton's troublesome heresy. (Of course, if the *JHA* continues in its habits, it will have little prestige left, to bestow upon anyone.) On 1984/6/28, in the unexpected presence of myself & my wife, 0 tried the same publication&conference-offer ploy with van der Waerden (additionally tossing in travel expenses). No wonder the Muffia doesn't want 2 sided public discourse. Why risk a clash of ideas, so long as there's hope that the Wrong Side can be subdued by more traditional & reliable means?

[10] Rawlins 1999 showed (*DIO 1.1* ‡6 fn 1) that Hipparchos' lunar period ($M_{\rm A}$: eq. 6 here) was from predecessors. This paper now finds that 2 other lunar elements (ϵ & $g_\odot$: eq. 8 & 9) are also from predecessors. I.e., of Hipparchos' 4 lunar elements, only e (or r) is original: eq. 19 (or eq. 20).

[11] Describing it as "forbidden fruit", I read it aloud anyway, at a small Hist.sci symposium, 1983/6/4 at Univ Aarhus. As a result, Lord H's admirer O.Pedersen (U.Aarhus, Editor *Centaurus*) was so furious, that one observer told me he'd never seen him that angry. The response tactic was standard: all audience members that mattered were herded into a nearby room, believed to be just out of DR's earshot, and then told by OG&co 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 archonal outrage at *DIO* so far) must be held under behind-the-back circumstances where 2-sided crossexamination is not possible, fully deserve the degree of enlightenment they will uncritically absorb.

[12] Ptolemy's superhype-billing (in, e.g., AAAS' *Science*, *QJ Royal Astr Soc*, & Springer-Verlag: Gingerich 1976 & §N16) as "The Greatest Astronomer of Antiquity" is worthy of Greatest-Show-on-Earth P.T.Barnum — as is the fraudulent subject of such puffery. But the kilobucks&kilobooks-gross may seem worth it: even aside from some ordmag $100,000 Ivy League salaries, Ptolemy's promoters are raking in handsome royalties on ordmag $100 books which sell ordmag 1000 copies worldwide, since they are dutifully & lovingly raview-advertised (even *pre*viewed similarly: fn 239) by loyal members of the same incestuous fraternity, in such seemingly reliable forums as *Nature* — who evidently lack the initiative to occasionally go outside the Muffia p.r. team when choosing reviewers of ancient astronomy material.

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was a geocentrist (& astrologer). It is DR's contention (a point so self-evident that it would hardly even be controversial, except in a Hist.sci community that has become obsessed with "whiggism" & "paradigms") that: there is a correlation between [i] the sort of intelligence required to discern that heliocentrism is preferable to geocentrism, and [ii] that which is required for contributing new insights to a science. (Likewise, I suggest that, in modern academe, one might detect a correlation between [a] competent, talented scholarship and [b] insistence on honesty, incorruptibility, & rejection of unethical compromise with a gang — or ring[291] — of Nibelungs.)

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

S3 We are by now well beyond the firm inductions that culminated in §N-§O, and are swimming in fun but obviously speculative areas. So, rather regretfully, I leave off analysis at this point. But, it's been grand. And my thanks[292] again to the Muffia for handing me the *Almajest* 4.11 problem, which I'd foolishly skimmed past for so long.

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

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

of the Muffia, vs. the extensive published abuse heaped upon the Johns Hopkins Univ Applied Physics Lab's late Space Sciences Division Supervisor, R.Newton (Ptolemy skeptic) by *JHA* & its various Editors such as O.Gingerich (Muffia satellite) & N.C.Swerdlow (Muffioso): see sampling at *DIO 1.1* ‡§C7. E.g., the editorial gang at Lord H's usually 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[15] to the *JHA* (& its referee K.Moesgaard):

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

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

[13] Sources: HamSwerdlow 1981 p.62 (*JHA*, published almost simultaneously with suppression of DR's intolerable-statement!) & Swerdlow 1979 p.530 (*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 *JHA* referee (Moesgaard 1983 p.57).

[15] One notes that, a few years prior to the incident noted here, Muffiosi 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 "Asger" 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 *Isis* published 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 handsome journals' actual quality (see *DIO 2* ‡4 fn 65), and [b] render them hostage to censors' tantrums. (What priorities could lead a publisher to invite that sort of tradeoff?) The *JHA* learned its lesson and so went to serpentine logical extremes to avoid repeating the mistake of publishing RN: see *DIO 2.1* ‡3 §B. Happy ending: the *JHA* Board now includes enough Muffiosi to ensure that the *JHA* will never do anything stupid again. . . .

[16] Virtually every scholar on both sides of the Ptolemy Controversy now agrees that Ptolemy deceived — even the 2 leading Neugebauer-Muffia capos (Toomer 1984 p.672, Swerdlow 1989 p.54). But the Muffia denies this is lying. Which is a semantic ploy that merits frank translation as: we lost this Controversy to the hated R.Newton & DR (who said right along that Ptolemy deceived) — but haven't the integrity to admit it. See §H2 (options [e]&[f]). Indeed, the sliminess of a certain volk partially accounts for the detail required in this paper when pointing out these bad-losers' follies. Incidentally, let no one wonder at an article which takes space to appreciate others' gyrations: [a] Muffiosi are notorious for the rabidity of their attempts to squish, humiliate, hurt, starve, & destroy perceived competitors — and to salt down the accursed spot they formerly occupied. See: §C11 & fn 41 and *DIO 1.1* ‡1 §C6-§C7 & fn 16. Since *DIO* is bound to be falsely painted as purely negative-critical, I urge a comparison, of these unrelievedly vicious Muffia comments (upon RN & DR), to DR's attitude (fn 174) at: *DIO 1.1* §C12 ‡6 fn 27, *DIO 2.1* ‡3 fn 3, ‡4 fn 18, & here at, e.g., fn 9, §C1, fn 71, fn 73, fn 104, fn 105, fn 211, §N1, fn 223, §P1, fn 275, fn 277, & fn 280. [b] The prime ancient subject of this paper is an astronomer, Hipparchos, whose sole surviving work (his *Commentary*) is a bloated nitpick at the work of previous authors (Eudoxos & Aratos). (Hipparchos protests in his preface that he's not seeking glory through carping at others' work; and Sarton 1959 p.65 accepts the plea. [Vs. Strabo 2.1.36.])

As regards benefits, His Lordship was half right.[17] 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: 0 *positively* belongs at the Princeton Institute. *DIO* hereby nominates OG for permanent Fellowship there.) All to the good. If one is going to butcher math, science, logic,[18] & free speech: let these deeds be staged where we can enjoy some basso echoes. And let those echoes ring down the history of Hist.sci: enshrining the Ptolemy Controversy as a classic case study of a community gone wrong, as convincingly demonstrated by Hist.sci's persistent 22^{y}-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 §I13 and *DIO 2.1* ‡3 §B. The price for DR's offered publication in the *JHA* was: going along with this fix. This he refused to do: fn 9.) If the Hist.sci community can't perform a function so basic to its reason for existence, then: *why do universities have Hist.sci departments*? (DR's increasingly asking this question for the last 15^{y} has not exactly endeared[19] 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[20] to arrange a fair encounter between advocates & doubters, in the disputes over quantum mechanics and relativity. A community so disabled has made itself the farce *DIO* honors it as.

B4 An especially cute feature of the most pompous Hist.sci journals is their elaborate pretense that they have "Editors". (Those familiar with the reality are all too aware that Editors' prominence depends more on socializing than editing.) It is easy to spot Hist.sci neophytes by their amusing innocence on this point. As an example of the sort of slip that gives away so many 1st year grad students' youth: many — even those with incipient doubts about the Easter Bunny — actually suppose that being an influential "Editor" requires that one *read* the material one publishes. This curiously widespread myth already came up (relative to *JHA*) in *DIO 1.1*'s *Journal for Hysterical Astronomy* (‡8 §G7). Yet another example, at Hist.sci journaldom's Reputability-pinnacle: shouting Hi-There! from the extremely handsome 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 calendaric creativity since the Muffia'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 masochistic. . . . You're not a bad person. Why act like one? Best wishes, in spite of all —".

[18] For *JHA*'s putative brains at work, see *DIO 2.1* ‡3 §B8.

[19] The same question is implicitly re-emphasized every time DR achieves a major historical result, since he's a living proof of a ghastly truth, namely, that one doesn't need Hist.sci training to contribute to scientific history. DR is a self-described amateur (see his self-composed bio in the 1982/6 *Isis* p.329), who consciously renounced going the standard Hist.sci grad-school route, and who breaks virtually all the Hist.sci rules (e.g., §C2, §O1), especially the ones requiring: [1] soporific writing, [2] innocence of the mathematical sciences, [3] encrusting papers with layers of superfluous archon-kissing citations (fn 179), & [4] careerist-lawyering for old-guard power-operators' pet views instead of seeking new truths in unapproved directions.

[20] For a similar situation, see D.Rawlins *Peary . . . Fiction* 1973 p.291 item #2. And see, at *DIO 2.1* ‡3 fn 8, Lord Hoskin's magnificently inventive scheme for killing off the airing of ongoing controversies. By comparison, *Isis*' approach (exhibited here at fn 121) is childishly clumsy. The blue ribbon for this category unquestionably goes to Lord H.

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

a measurement.) [b] I am glad to say that, by Muffia criteria (§I1), Hipparchos the scientist was such a poor specimen that he did not force records to support his viewpoint, as did his later superiors Ptolemy and (§I1) Toomer.

S Hipparchos in Scientific History

S1 This seems the best place to offer my own (still-evolving) view of Hipparchos, for what it may be worth. As an astrologer in youth, he made a bundle off the gullible "fatheads" of Bithynia — to quote an ancient writer's delicate appraisal of the rich Bithynian victims of an infamous 2nd century AD occultist (other than Ptolemy). (See Lucian's delightful & instructive "Alexander the Oracle-Monger", which tells us that "fathead" was a professional term, used by magicians to denote easy marks.) On the illgotten proceeds, he then transplanted (or retired) to Rhodos, perhaps during or just before 147 BC (§K3) — not only for the good life there but also in order to carry out (in Rhodos' famous climate) his long-nurtured dream of improving man's astronomical knowledge. Toward this goal, he bought instruments (including a transit-circle & an armillary astrolabe) and hired skilled mathematicians, computers, and observers. (No wonder Tycho hung Hipparchos' portrait on his wall: see *DIO 2* ‡4 §A2.) Possibly he found royal sponsorship at Rhodos.[286] As we saw in eqs. 23 and 24, some of his computational work shows debts to the dominant competent astronomers of his time, who were heliocentrists, of course. These debts (probably to Aristarchos, ultimately) include: synodic & anomalistic monthlengths (eqs. 6 & 7), epoch-values for lunar synodic longitude (eq. 8) & anomaly (eq. 9). As van der Waerden has already pointed out,[287] Hipparchos was himself not as able a mathematician[288] as Ptolemy (or the latter's sources). (Lacking van der Waerden's sense of math, the Muffia has attempted to cast Hipparchos as the inventor of not only the eclipse triad method but the method of spherical projection: §N8 & Neugebauer 1975 p.869.) Indeed, Hipparchos' math betrays deficits in several important departments: [1] talent, [2] ethics, [3] simple coherence. (See fn 235 & fn 253.) Nor was Hipparchos the most skilled of observers: each of his 3 solar orbits' (EH, PH, & UH) equation-of-center was inferior[289] to that of Kallippos, 4th century BC (*DIO 1.1* ‡5 fn 13) — and the cause was empirical, not theoretical. (Hipparchos initially adopted Kallippos' yearlength, as discovered here at §K8 & §K9: the EH orbit. Later, Hipparchos shifted to his own famous PH yearlength, virtually equal to Aristarchos': see §N12, §G10, and Rawlins 1991H fn 1.) And this move may have been directly[290] due to a factor which suggests in itself that Hipparchos was not among the best of the ancient astronomers: he

[286] [Note added 1993: *DIO 4.1* will locate Hipparchos' main observatory at Lindos and will show that his star catalog's southern portion was observed on a transit instrument at Cape Prassonesi.]

[287] E.g., letter to R.Newton, 1986/9/16 pp.3-4 (copy to DR): "His whole style . . . not the style of mathematicians . . . . In the works of Pappos and Proklos many otherwise unknown mathematicians are mentioned, but Hipparchos is not among them." See fn 235.

[288] Hipparchos' most notorious error was the huge discrepancy (over 1°!) — cited at *Almajest* 3.1 — in his 2 lunar eclipse-based determinations of the Autumn Equinox's angular distance from Spica. DR has discovered that this mistake was simply caused by Hipparchos' use of the wrong sign for his 2 lunar parallax corrections of his 2 (accurate) observations. And, more recently, the very same parallax-sign blunder was made by a prominently-published *JHA* investigator, J.Evans, when reducing his own 1981/7/16 evening Seattle cross-staff lunar eclipse observation. The end-result was off by 2°/3, almost triple the lunar semidiameter! The 1981 achievement's ineptitude was so gloriously Ptolemaic (see R.Newton 1977 pp.182-191 on *Almajest* 5.12-13: error also 2°/3) that Evans believed his error to be observational rather than calculational — displaying just the sort of overconfidence which Ptolemy (*Almajest* 3.1) & Graßhoff 1990 (p.206) correctly criticize Hipparchos for. Thus, the result, when published by Hist.sci archons, was adorned with classic *JHA*-style moralizing about certain none-too-able scholars (read: RN&DR) who naïvely overestimate ancient astronomers' ocular & instrumental accuracy: p.275 n.50 of Evans 1987. This article's brilliance & incessant (if frustratingly insubstantial) cavilling at DR were so attractive to Univ Cambridge's Lord Hoskin & Harvard-Smithsonian's O.Gingerich that it was accorded the unprecedented honor of running, in 2 huge pieces, as the Pb paper of 2 successive issues of the extremely handsome *Journal for the History of Astronomy* (1987/8 & 1987/11).

[289] For eclipse-time prediction, Hipparchos' UH orbit was (probably by accident) superior to Kallippos' more accurate solar orbit. See Rawlins 1991H §E1.

[290] See §R14 & fn 280.

The sine of a slim angle is virtually equal to that angle in radians, so we use eq. 49 to rewrite eq. 48. Now, 207·29/60 = 6003/60 = 100.05 ≈ 100; so we have:

$$\theta_{\rm H} = 180^\circ/(207\pi) = (360^\circ/13)\cdot 60/(207\cdot 29) \doteq 360^\circ/1300 \qquad (50)$$

which accounts perfectly for Hipparchos' otherwise inexplicable (& relatively inaccurate: §R7) value, $\theta = 360^\circ/1300$ (eq. 37). (Note that the odd factor 13 in eq. 37 is now explained as merely a byproduct[283] of the proper use of a sexagesimal expression for π: eq. 49.)

R13 Assuming that Hipparchos did indeed adopt Swerdlow's proposed $r_{\rm S} = 490^{\rm e}$, we are left with the discordant Hipparchan value, $R_{\rm S} = 10^{\rm e}1/6$ (Kleomedes: our eq. 42). Toomer 1974D has shown (though see fn 277) that if we also use $R_{\rm M} = 1^{\rm e}/3$, and thus (from eq. 26) get $\sin\gamma = 2/61$, and then use these data along with eqs. 36 & eq. 37 in eq. 27, we have $r_{\rm M} = 61^{\rm e}$, which is close to the truth ($60^{\rm e}.27$). (For the Theon-based $R_{\rm S} = 12^{\rm e}1/3$ of fn 277, Toomer similarly finds about the same $r_{\rm M}$.) It would be nice to suppose that this was Hipparchos' final value, but [a] we don't know so, and [b] it seems unlikely that he would jump back and forth from low solar parallax to high and then back to low.

R14 A conjecture: Hipparchos finally settled on his $7'$ solar parallax — *because* it made the Sun about as small as scholars could possibly accept. Obvious ocular evidence was against it (as noted at *DIO 1.1* ‡5 fn 7 item [a]), since it entailed (from eqs. 26, 35, & 38) half-Moons at elongation c.82° ($\gamma = 8^\circ$ Sunward of quadrature) — while in fact (as I know from repeated outdoor experiment) the Moon is definitely crescent-looking at this elongation (or even for $\gamma = 5^\circ$). However, perhaps desire[284] (assisted by aging eyesight) pushed ever-upward Hipparchos' estimate of γ: as a geocentrist astrologer (even if he perhaps hired heliocentrist computers: §O5), Hipparchos would prefer a smaller Sun because Aristarchos' estimate made it over 100 times the Earth's volume. But, at a distance of $490^{\rm e}$, it's an ordmag less. That made it alot easier to believe it went around the Earth instead of vice-versa. See "Figleaf Salad" (*DIO 1.1* ‡7 §C3). (Give Ptolemy credit for going with a far better figure.) Still, two important positives ameliorate one's disappointment at such (hypothetical) prejudice: [a] The evident preference (§I2) by Hipparchos (at his career's end) for lunar longitude observations made when (as stated by Ptolemy) the longitudinal parallax was null (2 out of the 3 Hipparchos observations at *Almajest* 5.3&5) suggests he died still honestly unsure[285] of what the Moon's distance was. (This entails far greater uncertainty of the solar distance, unless he thought of his $7'$ parallax figure as not a limit but

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

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

[285] If Hipparchos' only extant quadrature observation (*Almajest* 5.3) was for checking Aristarchos' Experiment, he got $\gamma = 3^\circ 3/4$ (not 8°). Against this suggestion is Hipparchos' arrangement for null longitudinal parallax: that is irrelevant for measuring γ, while it is useful for checking the lunar longitudinal position, which is what *Almajest* 5.3 suggests was its purpose.

Scientific Biography (Hist.sci's chief reference-work achievement[22] of this century, overseen by Hist.sci's most exalted archons: fn 172), Toomer's "particularly choice"[23] discovery (Toomer 1976 p.321) that a September 19th event[24] had occurred virtually at the Solstice! It was right while he was working up his Autumn Solstice gem that Toomer was affecting such lordly airs about weighty sins, like R.Newton's grammar (fn 264). So as not to miss the full richness of the larger picture here: keep in mind that Toomer is the Muffia-circle's idea of **the** top ancient astronomy authority (see fn 240).

C Somersaults & Winter Equinoxes

C1 A note in passing. I doubt our shellshocked Muffiosi friends can possibly believe this, but I will here tell them the truth anyway: I do not make special searches for Muffia bumblings. I am simply working in the same areas. (In which point resides an implicit compliment: I wouldn't be consulting Muffia papers if I didn't expect to learn something of worth from them. I occasionally do.) When reading their papers:

[a] The patently false statements require no special talent to notice (presuming one has even modest facility in astronomy) since they're about as subtle as bananapeel somersaults. Or Winter Equinoxes.

[b] The less immediately obvious treasures usually turn up when I am doing parallel work and notice a conflict with results Muffiosi have been trumpetting (as expert and definitive) throughout the Hist.sci community. (An analogy: explorers in other bad US neighborhoods don't have to go out of their way to notice boomboxes.)

C2 I must segue into my larger review here by regretfully announcing that *DIO* has failed (at least with respect to its exemplary-intent). In this respect, it is in awesome company, since all surviving churches are similar failures. (This is the key to their & *J.HA*'s mutual durability. I.e., churches ostensibly aim to eliminate sin; but, if they succeed, they go out of business.) An early hope[25] of *DIO* was to improve Hist.sci: conversion-by-example to the goals of combining [a] genuine, fertile (not make-work)[26] creativity, [b] solid (as against off-top-of-head) originality, [c] technical competence, [d] induction at a scientific level,[27] & [e] two-sided citation-policy. The 1991 buffoonery described below suggests not merely that these are not Hist.sci *priorities* — they are not even considerations. And, under current Hist.sci institutional management, there is so little likelihood of improvement that: attempting to effect such will no longer be a consideration on my side. DR's inconvenient

[22] The *DSB*'s high quality may be gauged from its apparent nonbesmirchment 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 Malignant 1: see Toomer 1974D 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 −96 day error) due to his confusion of Alexandrian Thoth 22 with Egyptian Thoth 22. (The two calendars had been diverging by 1 day/4 yrs for nearly 4 centuries — thanks to Little Augie Caesar's 30 BC "modernization" of the simple old Egyptian 365d calendar, incorporating the 365d1/4 yearlength of Big Julie's now-famous calendar.) The *DSB* was informed of this error 1978/5/18. I do not believe correction has ever been made in the more than 13 years that have passed since. (For DR's helpful-hint on how to spare other unmathematical minds similar embarrassment & strain, see *DIO 2.1* ‡4 fn 5.) From *DSB 16*:504, 508-510, we learn that Toomer's articles on Ptolemy, Theon of Alexandria, & Hipparchos required no correction at all.

[25] Admittedly, my optimism wasn't very warm. Rehab is largely a chimera — as Hist.sci's Jonestown folly has proved all too clearly. My initial attitude toward Muffiosi was, though critical, more optimistic and volunteering; but years of Muffia arrogance have effected a complete cure from such unreality. As recounted in Rawlins 1991H (fn 35), DR in 1986 wrote Muffia satellite N.Hamilton, a voluntary acknowledgement that a Muffia 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* 1980/10/16 item #6. And see inside front cover of each *Archive Hist Exact Sci* issue: *AHES* "nourishes historical research meeting the standards of the mathematical sciences."

fertility and §B1 "impossible" behavior (i.e., nonincorporability into Hist.sci's burnout-mill) will simply 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 jokers?[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. Stifling giggles 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 fn 12) spoke jovially of "numerous Hist.sci professionals' doubtless unbiassed 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 §C5 & ‡6 fn 4) examples of what to look out for: "For samplings of truly epic [Neugebauer-]Muffia 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 reflects all-too-faithfully his rigidity of viewpoint.)

C6 So, as soon 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 miscomputed and uncomprehending astronomical math 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-disaster[30] composed by a scholar from the very Neugebauer-Muffia klan which *DIO 1.1* had explicitly warned of: BrownU-product Alexander Jones. Pronounced as in Jonestown. *Enlighten* Hist.sci Editors? I'd prefer less formidable educational challenges.

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

[29] See *DIO 2.1* ‡2 §H16-§H17. The wellfed Muffia is akin to a pristine-clean Ollie Hardy, who — though having virtually submerged Stan Laurel in still-dripping meringue pie (fn 31, fn 269, etc.) — exudes lordly confidence that Laurel can't or won't fire back. The strutting smugness itself (preferably in a brandnew tuxedo) is what elevates such cinema episodes to the pinnacle of good slapstick. But not even Laurel & Hardy were able to refine comedy to the point where Hardy could: [a] wipe the pastry & meringue off his Macbethian hands (fn 90), [b] turn to watch piefaced Laurel go into his windup, and then [c] prissily admonish Laurel that piethrowing is (§I7) "disreputable".

[30] This is not mere ineptitude or incompetence. After all, clumsiness & ignorance produce a random, aleatory issue. But the sort of Hist.sci behavior *J.HA* is observing looks more akin to what the parapsychological kooks refer to as "psi-missing" — or to a mass version of what their Freudian spiritual brethren call the suicidal "death wish". (And I don't even believe in those fads!) It's systematic. And has been so for decades. In reaction to Hist.sci's current Jonestown incident, no Hist.sci institution will effect any changes beyond the cosmetic. If that. (Numerous academic societies are not in the least upset by trivia such as unethical behavior. They reserve disapproval for the truly grave offense of: *public exposure* of unethical behavior. The reporter, not the perpetrator, is correctly identified as the culprit. Similarly, the much-lamented "unhistoricity" of R.Newton's & DR's reports is what really enrages Muffiosi, while Ptolemy's fakes and thefts don't cause them an eyebat [fn 96, §I1] — as they wish skeptical ogre DR would just emulate their own unexceptionable sense of priorities.) Thus, the only possible educational benefit of this paper's critiques will be: further encouragement for the wider scholarly community to give appropriate credence to the effusions of an academic cult whose institutions have, through decades of effort, refined & isolated such perfect rectatude that praise of it never ceases in these pages (e.g., *DIO 1.1* ‡7 §G4).

R10 Non-disconfirmatory (fn 254) in this connection: there is a lethal internal contradiction within the pseudo-Aristarchos ms — a contradiction which has eluded at least 1 2/3 millennia of commentators, from Pappos to the Muffia. The Proposition 4 discussion conjures up 1/3960 of a right angle (Heath 1913 pp.369-370); this amount, which equals $1'.4$, is then called "imperceptible" to the human eye (Heath 1913 pp.370-371, Neugebauer 1975 p.640). However, comparison to eq. 45 shows that this "Aristarchos" judgement implicitly destroys Aristarchos' own immortal 3° result for γ. The Aristarchos Experiment depends critically upon detecting whether a half-Moon occurs near [a] null γ, which corresponds to $r_S = \infty$, or [b] his famous $\gamma = 3^\circ$, which corresponds via eq. 26 to $r_S/r_M \doteq 19$. Such fine discrimination requires vision good to $0'.4$ (§R9) — i.e., much smaller than the $1'.4$ angle which pseudo-Aristarchos claims is visually imperceptible!

R11 We now speculate that, at a later date, an Aristarchos follower revised eq. 45 by deciding that the angular length of the tiny midperpendicular of §R9 would be the amount to be visually detected, using the original $0'.4$ visual-limit criterion. Eq. 45 thus becomes:

$$\sin \gamma_T = \frac{0'.4}{\theta_A} = 2/75 \tag{46}$$

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

$$r_M = r_S \cdot \sin \gamma_T = 1000^a \cdot 2/75 \doteq 27^a \doteq r_S/37 \tag{47}$$

The Moon's distance r_M is thus deduced as 27^a — or 1/37 of r_S. That completes the tentative tracing of the ratio adopted previously at eq. 39, on our way to finding the attested value $r_M = 67^e 1/3$ (eq. 41) — which we now return to, for the final member of our (speculative) reconstructions here.

R12 We have already seen (e.g., §N7) that Hipparchos was capable of attempting (mathematically unjustifiable) piecemeal improvement of his orbital elements. This suggests the next step in the evolution of his lunisolar theory: adopting a solar parallax of $7'$ (Swerdlow's discovery),[280] he changed his r_S to the value of eq. 38, but did not wish (perhaps because[281] his tables had been reared upon them) to change either his r_M (eq. 41 or eq. 44) or υ (eq. 36). Thus, the only means of assuring a fit to eq. 25 would be to: alter[282] semidiameter θ. I.e., the unknown being sought in the basic equation (Swerdlow 1969 p.298 bottom, or eq. 25 here) may have been not r_M (already known from eq. 41 or eq. 44) but θ_H. This is the (hypothetical) sequence that has not been previously suspected. Merging eqs. 25, 36, & 38:

$$\sin \theta_H = (1/67^e 20' + 1/490^e)/(1 + 2\,1/2) \doteq 1/207^e \tag{48}$$

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

$$2\pi = 377^p \qquad \text{or} \qquad \pi = 13 \cdot 29/120 \tag{49}$$

[280] Despite my dismay (*DIO 1.1* ‡5 fn 7) at some of Swerdlow's procedures (when he tried to overforce desired agreement), I have long been sympathetic to Swerdlow 1969 (even though its implicit γ is remarkably outsized — a problem I attempt to overcome here at §R14). In a phone chat of 1981/4/5 (*DIO 1.1* ‡3 fn 7), I told Swerdlow that both the sign & size of the mean errors in Hipparchos' equinoxes (errors which degraded his PH solar theory's equation of center, making it worse than Kallippos') were pretty consistent with Hipparchos' having reduced his observations using a $7'$ solar parallax correction. Swerdlow said he already knew about that. [See DR to *Centaurus* 1977/3/9 p.B11.]

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

[282] I see that Hartner accused Ptolemy of very similar inverse procedure at *Almajest* 5.14: Van Helden 1985 p.19.

by Swerdlow 1969. From eqs. 40 & 26, one finds:

$$r_{\rm M} = r_{\rm S} \cdot \sin\gamma = 2490^{\rm e} \sin 1^\circ 33' = 67^{\rm e}21' \doteq 67^{\rm e}1/3 \tag{41}$$

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

R8 An alternate route to $67^{\rm e}1/3$: starting with the attested Hipparchos solar volume 1050 Earths (Kleomedes 83), we take the cube root and have

$$R_{\rm S} = 10^{\rm e}1/6 \tag{42}$$

Thus, applying eq. 30,

$$r_{\rm S} = R_{\rm S}/\sin\theta_{\rm A} = 2330^{\rm e} \tag{43}$$

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

$$r_{\rm M} = 67^{\rm e}22' \tag{44}$$

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

R9 We now pause to ask: whence came the crucial (eq. 39) factor, 37? (It presumably appeared before the development set forth here in §R7. Keep in mind that we really don't have to justify the finding that $\csc\gamma = 37$, since it is effectively attested by Theon.) A guess at its origin: Aristarchos' Experiment depends critically upon the observer (of the half-Moon's occurrence) detecting lunar terminator-curvature.[278] Now, the mean distance between the human eye's foveal cones is (Rawlins 1982G p.263) no less than $0'.4$, and this is very near the actual limit of naked-eye discriminatory ability, which is about $1'/3$ or 1/10,000 rad (§R5). If the middle of the terminator deviates by twice as much (that is, $0'.8$) from the straight line (really: great circle) connecting the Moon's horns, then no part of the terminator will be more than $0'.4$ from a straight line parallel to the hornline & bisecting the tiny midperpendicular connecting the hornline & the terminator's middle. Understand: this minuscule angular distance is the crucial basis of Aristarchos' Experiment; i.e., the $0'.4$ is all that visually distinguishes Aristarchos' $r_{\rm S} \doteq 19 r_{\rm M}$ from $r_{\rm S} = \infty$: §R10. For, if $r_{\rm S} = \infty$, the lunar elongation complement γ corresponding[279] to the slight terminator-curvature just described is (for Aristarchos' value of the lunar semidiameter, eq. 30):

$$\gamma_{\rm A} = \arcsin\frac{0'.8}{\theta_{\rm A}} \doteq \arcsin\frac{1}{19} \doteq 3^\circ \tag{45}$$

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

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

[279] Likewise, if $r_{\rm S}/r_{\rm M} \doteq 19$ (the traditional Aristarchos-Ptolemy ratio), then the quadrature Moon will be slightly gibbous, with a terminator-curvature bulge of $0'.8$ (beyond the straight line connecting the lunar horns).

Like teaching sponges to sing & dance. (Deepest apologies for that odious comparison, which I am swift to retract: sponges are scrupulous at filtering worthwhile nutritious matter from the mix submitted to them.) As will be demonstrated below, the *JHA* article is (even aside from its magnificent $366^{\rm d}$ 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-consuming conviction that indoor Babylonian astrologers secretly influenced high empirical Greek astronomy (§E4) — until the ultimo genius & "radical reformer" C.Ptolemy "ruthlessly expunged" (Jones 1991H p.122) all traces of this dependence (an obliteration as conveniently thorough as Collective Amnesia: §F1).

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

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

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

And our genteel Muffia calls dissenters from such inimitable wisdom: "crank", "disreputable", "paranoid", "incompetent", & criminal.[31]

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 deef. 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,[32] 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 archondum'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, §I7, & fn 158). Not that one need be a heretic to get trashed by the Muffia. See, e.g., fn 211. Similarly: is it really necessary to refer to N.Halma's uneven 1822-1825 edition of Ptolemy's *Handy Tables* as "execrable" (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 4.3* ‡15 §G8], but so mercilessly prone to criticize other, usually well-intentioned and honest scholars, who were also handicapped by crude means? The answer is: Muffiosi have made a living selling Ptolemy and simultaneously selling their alleged ability to interpret him better than other scholars. (Some Muffiosi are now gradually switching franchises, from *Almajest* to BabCycles: §F2 & fn 266.) So, the answer to the foregoing pseudo-paradox is simple: demeaning other commentators' work helps generate Muffia income.

[32] My 1973 book, *Peary at the North Pole: Fact or Fiction?*, noted (p.62) the remarkable case of the world-renowned Scott Polar Research Institute (Cambridge Univ) reviewing, as a serious article, Guy Potter's (very) thinly veiled 1970 satire on Peary's N.Pole hoax.

As above (§C7), I remind the reader that: the Muffia calls *other* scholars "crank" (fn 31) and "Velikovskian". (See also §F1, §G3, fn 191, fn 192, §M7.)

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

[c] By an irony which we might have supposed *was* impossible, one of these 3 orbit-fits — which the 1991/9 *Isis* prominently classified as non-existent — HAD ALREADY BEEN ACHIEVED & PUBLISHED, right in the very *DIO* issue[34] 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, shucks, 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 §I4.)

[d] As usual, when skepticism on Ptolemy is mentioned, the accursed works of skeptics (especially DR) remain uncited. (Rawlins 1991H fn 6, describing 15^{y} of the Ptolemy Controversy: "not a single inner member of [the Muffia] has ever[35] cited any work by DR.") Even lower-level citations invariably acknowledge no contribution (e.g., §I5, fn 288). Hist.sci'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 (§I14). (Contrast R.Newton's citation-integrity with Muffiosi's: §E1. Examples of DR's citation-policy are provided at fn 16 & fn 174.) What an inspiring model[36] 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.

[34] Rawlins 1991H §C7, §D7, & §D9.

[35] [Note added 1992: Muffia capo N.C.Swerdlow's 1992/10 *JHA* paper at last cites a DR work. However, the essential Muffia tradition continues, as NCS of course concludes that the paper has contributed nothing whatever to the field. The competency & integrity of NCS' criticisms are displayed at *DIO 2.3* ‡8 §C.]

[36] Analogously: the clique who was proved wrong throughout the Ptolemy Controversy has emerged politically dominant, while those whose charges have been repeatedly vindicated are banished from the scene. (See Rommel's reflections on WW1 at §D2, and his naïve implicit conviction that initial WW2 good fortune augurs a different conclusion.) Well, why not? This situation seems quite consistent with our Hist.sci archons' attitude toward history. From the official guidelines to contributors (see also fn 127), "commonplace among professional historians of science", composed by the *JHA*'s Editors, Lord Hoskin & O.Gingerich (& sent to the printer precisely at the time *JHA* was refusing publication to "a-historical" RN, 1980/3/6: *DIO 2.1* ‡3 §B2-§B3), appearing at *JHA 11.2*:145 (1980/6), p.146 (emph added): "It is, needless to say, a *mortal sin* to judge the past solely in the light of the present and to hand out medals to those who 'got it right'." (Comments: [a] Not all of us have the gift of making truisms simultaneously pretentious & misleading. [b] Nowhere do our *JHA* mentors say that faking data is a Mortal Sin.) After decades of observing the Hist.sci field, I can readily understand its archons' compulsive downgrading (fn 154) of such embarrassing criteria as mere correctness, predictive intelligence, empirical vindication, and ethical rectitude. The only getting-it-right that counts is: adherence to such currently-fashionable, archonally-decreed political-correctness.

[37] Another example: §I14 item [a]. (On the personality-type that behaves so, see independent appraisal quoted at *DIO 2.1* ‡3 §A.) In case it is objected that the "tone" of the present paper makes it uncitable, keep in mind that Muffiosi 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.)

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

$$\theta_{\rm H} = 360^\circ/1300 = 18^\circ/65 \tag{37}$$

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

$$r_{\rm S} = 3438'/7' \doteq 491^{\rm e} \approx 490^{\rm e} \tag{38}$$

But no prior scholar has yet been able to: [a] explain the origin of the weird expression in eq. 37 or [b] recover exactly $67^{\rm e}1/3$ from normal[274] computation. I will next hypothesize an explicitly speculative route which can explain how these parameters came into being.

R7 Whereas Aristarchos' value for the lunar semidiameter θ (eq. 30) is about two times more accurate than eq. 37 (Hipparchos), his alleged shadow-ratio estimate ($\upsilon_{\rm A}$: eq. 29) is crude — far less accurate than Hipparchos' $\upsilon_{\rm H}$ (eq. 36). So, though *Almajest* 4.9 mentions eqs. 36 & 37 together,[275] the empirical[276] impetus to revise υ would be much greater than to mess with θ. Suppose, then, that Hipparchos at first adopted just eq. 36 (his best move, accuracy-wise, as just noted). A Hipparchan choice of γ which was more accurate than that of either Aristarchos or Archimedes ("Sandreckoner" p.223: $\gamma = {\rm c}.2^\circ$) was first highlighted by F.Hultsch (Toomer 1974D p.140): Theon of Smyrna said Hipparchos had the Sun's volume = 1880 Earths, while the Moon's volume = 1/27 Earths. Refining the argument a bit, I will first compute (from eq. 26):

$$\csc\gamma = r_{\rm S}/r_{\rm M} = \sqrt[3]{1880\cdot 27} \doteq 37 \doteq \csc 1^\circ 33' \tag{39}$$

Next, combining eqs. 26, 27, 30, 36, 39, we have:

$$r_{\rm S} = \frac{\csc\gamma + 1}{(1+\upsilon)\sin\theta} = \frac{\csc 1^\circ 33' + 1}{(1+5/2)\sin 0^\circ 15'} \doteq 2486^{\rm e} \approx 2490^{\rm e} \tag{40}$$

(Note: without eq. 39's rounding of γ to whole arcmin, eq. 40 yields $r_{\rm S} = 2489^{\rm e}59'$.) This is the very $r_{\rm S}$ Hultsch proposed was originally stated by Pappos, a value later rejected[277]

[274] The ploy of Swerdlow 1969 p.298 bottom (use of $490^{\rm e}$ in numerator & denominator instead of cancelling it: §R7) is artful but somewhat abnormal. However, it is not at all impossible, since the steps displayed could have occurred in several successive stages of math development. And see fn 277 (which Swerdlow *loc cit* anticipates in a general way).

[275] This important connection is the strongest point in favor of Swerdlow 1969 — overcoming the negative items noted at *DIO 1.1* ‡5 fn 7.

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

[277] Swerdlow 1969 n.4 is commendably clear that there is a discrepancy in mss between "490" and "[space] 90"), adding: "This disagreement doubtless strengthened the suspicion of textual corruption." (Unfortunately, this Swerdlow note consistently uses the Greek symbol for 6 where he intends that for 90.) I must emphasize that there is no direct attestation of $2490^{\rm e}$. That is just Hultsch's proposed emendation (long-accepted, before Swerdlow 1969), simply on the basis of multiplying eq. 39 times eq. 35, which yields $2490^{\rm e}$. (Note: the resurrection of the same figure in eq. 40 is from a different direction.) It should be added that there is here an internal contradiction (to which Swerdlow 1969 is exceptionally immune), namely: one may easily compute the angular size of the Moon (similarly for Sun) from its size ($1^{\rm e}/3$, §R7) and distance (eq. 35), yielding $\arcsin(1/202) = 17'01'' \approx 17'$. This is a neat rounded number, but it is not the value used in eq. 40, by which $2490^{\rm e}$ was recovered here. (However, it does suggest an alternate path to $r_{\rm M} = 67^{\rm e}1/3$, namely: round the Moon's radius $R_{\rm M}$ to simply $1^{\rm e}/3$, and round its semidiameter in eq. 37 to the nearest arcmin so that $\theta_{\rm H} = 17'$, which is arccsc 202; thus, $r_{\rm M} = R_{\rm M}/\sin\theta = 202^{\rm e}/3 = 67^{\rm e}1/3$ precisely. The common factor $1^{\rm e}/3$ in both $R_{\rm M}$ & $r_{\rm M}$ makes this an attractive theory. It accords nicely with — and I hope boosts the credibility of — Swerdlow 1969.) Thus, the hypothetical development here (§R7, leading to eq. 40) implicitly presumes that Theon of Smyrna's material indicated only approximately $r_{\rm M} = 1^{\rm e}/3$, and that, after he multiplied this rough figure by 37 (eq. 39), yielding about $12^{\rm e}1/3$, he cubed it and got c.1880 Earth-volumes, the figure Theon cites (§R7). Thus, to sidestep contradiction, all I am drawing from Theon's data is the ratio 37 (eq. 39). It should be noted that the same small internal contradiction (discussed in this fn) also resides in the Theon & Kleomedes calculations of Toomer 1974D p.140, but Toomer does not notice this.

(characterized by a restraint well known to MadAve devotees) of faker C.Ptolemy's "brilliance . . . In his mastery of the choice and analysis of observations in conjunction with theory he has no peer until Kepler." Shall we now amend to: "until Toomer"?

R5 Given our previous findings here (§N), we are not bound to accept that the basis for Hipparchos' figures are those stated.[270] (Though, I have not the courage or "brilliance"[271] to speculate so imaginatively on Hipparchos' unstated methods as Jones 1991H has done.) We have already noted (§R1) the proximity of the 77^e value to our newly reconstructed Aristarchan value (eq. 31). This suggests that a calculation similar to that of §Q (leading to eq. 31) could be the true source of the 77^e value (of Hipparchos' "Sizes" part 1) — especially given the prominence of Aristarchos' work (§Q1). Now, the slightest testing will show that the later (Hipparchos or Ptolemy) values for shadow-ratio υ will (if used in eq. 25 with any anciently-attested θ) require ludicrous values for γ (well over 10°!) to produce $r_M = 80^e$ — thus, we must resort to the Aristarchan value (eq. 29): $\upsilon = 2$. This suggests we also try the Aristarchan $\theta = 1^\circ/4$ (eq. 30), and presume that only the solar parallax was altered in the Aristarchos scheme. (Variation of parallax is all that is spoken of at *Almajest* 5.11.) Pappos & Ptolemy (sources: §R2) give some hints as to what new parallax was used. My sense of their descriptions is that it was imperceptible but not null. (*Almajest* 5.11: "no perceptible parallax".) Now, we recall that Poseidonios proposed a solar distance (eq. 21) which entails a solar parallax of between $0'.3$ and $0'.4$, which is indeed the limit of naked eye vision, as I know from personal testing. Setting negligible diurnal parallax = 10^{-4} radians makes the Sun's lower-limit distance = $10{,}000^e$ (eq. 21);[272] and thus (from eq. 30) the Sun's radius is:

$$R_S = 10,000^e \sin(1^\circ/4) = 44^e \tag{33}$$

(This is not a bad crude estimate, since the correct R_S is 109^e. See fn 248.) Since Poseidonios wrote in Rhodos (not long after Hipparchos' work on Rhodos), his solar distance, $10{,}000^e$, is as likely as any to have been used. Substituting it and eqs. 29 & 30 into eq. 25, we have (using 100%-attested ingredients):

$$r_M = 1/[(1+\upsilon)\sin\theta - 1/r_S] = 1/[3\sin(1^\circ/4) - 1/10,000] = 76^e59' \doteq 77^e \tag{34}$$

— which is just the value Pappos cited as the mean[273] lunar distance in Hipparchos' preliminary work (§R1).

R6 We now turn to the final value (§R1) which Hipparchos deduces for the lunar distance (part 2 of "Sizes"):

$$r_M = 67^e 1/3 \tag{35}$$

— which is (unlike 77^e) pretty consistent with the Hipparchos shadow-ratio for mean distance (*idem* & fn 273) attested in *Almajest* 4.9:

$$\upsilon_H = 2\ 1/2 = 5/2 \tag{36}$$

[270] Pappos indicates that Hipparchos' lunar distance estimates were based on the Hellespont-Alexandria solar eclipse discussed here. Unlike Pappos' account, that of *Almajest* 5.11 does not definitely say that this calculation was based upon an eclipse. Incidentally, if the eclipse referred to by Pappos was that of −309, then it may be that Hipparchos was using an analysis of it by another astronomer, such as Timocharis or Aristarchos.

[271] Toomer 1988 n.25. And see fn 61 here.

[272] [Note added 1992: Aristarchos evidently chose his lower-limit 10,000 AU distance to the sphere of the fixed stars by just the same uniform 1/10000 rad vision-limit underlying all ancient heliocentrists' cosmic-scale estimates: [a] the foregoing (10,000 AU), [b] the half-Moon experiment (eq. 45 & perhaps eq. 46), and [c] eq. 21. (See "Ancient Vision", in [*DIO 14* ‡2 (2008)].) It is a matter of record that Aristarchos wrote on vision & light: Heath 1913 p.300.) If so, then Poseidonios presumably agreed. The 10,000 AU estimate is far better than the geocentrists', but it is still much too small: the distance to the nearest star outside the Solar System is over a quarter million AU.]

[273] *Almajest* 5.14 says that Ptolemy's "predecessors" used standard eq. 25 (i.e., set lunar & solar semidiameters equal) for mean distance, while Ptolemy (implicitly declaring annular eclipses impossible) uses it for greatest distance. See fn 259. Heath 1913 p.383 n.2 might suggest that Aristarchos could have agreed with Ptolemy, whose γ is so nearly Aristarchos'. If so, then a speculative route to explain the 1st Hipparchos greatest distance of §R1 (83^e) is: use $\gamma = 5^\circ$ in eq. 26 with eqs. 29 & 30, which yields $r_M = 83^e.053$. I mention this possibility largely just to cover all bases — to somewhat discourage Muffia reflex-options [f]&[g] at §H2.

D Even a Hun Can Have Fun: Blitzkrieg in the 'Jest

D1 Aside from DR's customary cleanups after the Muffia's customary messes, the following paper 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" orbit: §K9), which he adopted (for no more than $c.10^y$) 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); it & 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 confirmation, no fruitfulness (vs. fn 85 & §O3). So, right after completing the other paper, I started dabbling (1991/10/27) with the problem of fitting orbits to the *Almajest* 4.11 data: 2 Hipparchos eclipse-trios. (Below, we will follow the chronological convention of Jones 1991H by distinguishing these as "trio A" & "trio B".) Within 2 days, I had broken through on this front and was rolling 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 defied explanation for 2 millenia, at least since Ptolemy (c.150 AD) criticized & recomputed this Hipparchos material; the orbital-element numbers in question are: 3144 & 327 2/3 (trio A) and 3122 1/2 & 247 1/2 (trio B) — pairs of lunar mean distances & eccentric-motion amplitudes, respectively (all in unspecified units). For the last 24 years, these numbers' origin has been researched by Muffia don & eminent Springer-Verlag Hist.sci "Editor" G.Toomer (BrownU, formerly Oxford Univ), to the extent of dozens of admirably erudite published Hist.sci journal pages. It seems to have been the dominant, pet math-astronomy research-puzzle project of his academic life. (See, e.g., Toomer 1967 & Toomer 1973; the hypothetical chord table underlying Toomer's thesis is altered[38] at Neugebauer 1975 p.1132, without explanation — though stated to be identical at *ibid* p.1129 n.1. And Toomer 1984 p.215 n.75 speaks of more to come. See also Toomer 1988 n.44 and here at §D3 & §O1.) He has doubtless expended scores of pages of tedious handwritten analysis on this problem, naïvely attempting to fit it to the claimed methods of the geocentrist astrologers Hipparchos & Ptolemy. The exact solution of both the larger numbers (the first, eq. 23 below, discovered by DR in ordmag an hour,[39] once Toomer's approach had been cast aside) turns out to be expressible in 2 lines of highschool math (below, eq. 23 & eq. 24) — based on the hypothesis (verboten to all obedient little Muffiosi) that competent heliocentric astronomers' work underlay that of the geocentrist astrologers (just as today).[40]

D2 This entire paper was essentially accomplished in a brief but memorable period of just a few weeks, the first breakout-success being that of 1991/10/27-29 (§K9). Recalling my own self-described "molassian slowness of wit" (Rawlins 1991H fn 34) in arriving at the Hipparchos UH orbit, I feel entitled to indulge a bit in the contrast in this instance. (My long-bogged-down periods in the former case help me understand Toomer's quarter-century of frustration at the apparent intractibility of the *Almajest* 4.11 maze.) Since I'm about as popular in Muffiadum as a Nazi in Paris, it will do no additional harm to my status with these

[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 Rawlins 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 n.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.

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 acknowleged that Muffiosi are far more adept[41] than DR, at massing troops to crush Enemies.) And, to refine one's sense of fairness, it helps to try discerning what can be admired and what can be sympathized-with, even in atrocity-perpetrators (whether brownshirt panzers or BrownU pansies). In the entry below, Rommel is reveling in the amazing, seemingly-miraculous moment when the "impenetrable" Maginot line was pierced — and he found himself speeding across France toward the Atlantic and victory.[42] Toomer will see that he is not alone in quarter-century-frustration (in Rommel's case: 1914-1940). From *The Rommel Papers* (ed. B.Hart 1953 pp.18-20), 1940/5/16-17 entry, with the blitzkrieg in the West less than a week old (launched 1940/5/10): after plunging through fierce fire (& taking a face wound), and stifling nearby Maginot forts, Rommel floored it & knifed dozens of km behind enemy lines, in one unprecedented 24 hr tear, much of it nocturnal.

> Slowly the sky darkened and it became night. . . . The way to the west was now open. The moon was up and for the time being we could expect no darkness. . . .
>
> Gradually the speed increased. Before long we were 500 — 1,000 — 2,000 — 3,000 [meters] into the fortified zone [Maginot Line west extension]. . . . still no resistance . . . . The flat countryside lay spread out around us under the cold light of the moon. We were through the Maginot Line! It was 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[44] into enemy territory. It was not just a beautiful dream. It was reality.

[41] Fn 46 & §P1. Muffiosi have that killer instinct for elimination of The Enemy and (Rawlins 1984A p.972) all traces that he ever existed. (See fn 16 item [a], §C11 item [d], §H2 options [b]-[e], *DIO 2.1* ‡3 §A; & note contrasts at *DIO 1.1* ‡1 §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 Muffia's puppy-loyal OG (1983/8/31), warning Il Poochie not to hoop-jump too hastily onto what might appear at the moment to be a (politically) winning bandwagon in the Ptolemy Controversy: "Don't you know that there will *be* no winning side?" To quote a figure slightly more popular (than DR) among Muffiosi: even Hitler of all people publicly said the same thing (1939/10/6), while planning this very offensive (W.Shirer *Rise* . . . ppbk ed p.849). On the morning of the Rommel diary entry here quoted, Rommel got his orders from his superior, 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 & v.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.Mitscher's legendary — i.e., equally successful, 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 $50^\circ 06'$N, $3^\circ 34'$E. The waxing gibbous Moon had set at 3:45, after feeble dawn twilight's onset. (Sun's 3:45 altitude: $-15^\circ 1/2$.) (Two minor astronomical oddities: [a] Though 60° short of full, the Moon set barely 2h before sunrise. [b] For the hour following 4:06, the Sun, Moon, and all 8 planets were below the horizon.)

than to the meridian. For the most popular contenders rejected in this fashion by Toomer: the −309 mideclipse was virtually due east (azimuth $100^\circ \pm 4^\circ$), and the −128 mideclipse was only about $8^\circ \pm 4^\circ$ above the horizon. (The −216 Alexandria mideclipse was probably below the horizon.) Only the Muffia could resort to meridian math under these conditions[266] — all the while quarreling with astronomers' linguistic usage . . . .

R4 According to my calculations,[267] the 3 eclipses that merit examination (i.e., those that were virtually total near the Hellespont) had linear magnitudes[268] at Alexandria of: 76% ± 1% (−309/8/15), 90% ± 4% (−189/3/14), 77% ± 3% (−128/11/20). Thus, for his purposes, Toomer's selection, −189, is probably the worst of the 3 candidates. (His choice is still routinely promoted by Muffiosi; see, e.g., Neugebauer 1975 p.316 n.9, Toomer 1984 p.244 n.38, & Van Helden 1985 p.11.) Facts:

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

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

My favorite Toomer pronouncement[269] has always been the Toomer 1975 p.196 appraisal

[266] It's delightful to see Springer's current series of amazingly expensive books, "Studies in the History of Mathematics and Physical Sciences", in such discerning hands as those of "Editor" Toomer — who is in truth editing this series with as much care as Editor-for-Life Lord Hoskin applies to the *JHA*. And what a lift to realize that, unless one assents to such genii's tenaciously-held creeds, one will be shut out of ancient astronomy conferences, grantfunds, publication, etc. A recent assessment (in another area) may throw light on the actual political & managerial logic behind this situation (*Isis 82.3*:601; 1991/9): the creation of the Space Telescope "demonstrates the requirement for near unity in the relevant scientific community for such great projects to succeed". (Comment upon the implications for dissenters would be tautological.) Extrapolating to the current quest for funds to support the grand Muffia project of recovering (Seleukid-era) Babylonian tinkertoy astronomy: Muffiosi hotly resent having an external scholar exposing late Babylonian astrology's crudity, degeneracy (devolving from skilled Greek astronomy), non-empiricism, & exclusively indoor-horoscope-application (§E3). (Question in passing: why do Muffiosi find it so tolerable, as in Jones 1983, to trace Greek influence in Indian astrology — but not in Babylonian?) I.e., open controversy is seen as simply: bad-for-business. (Ironically, *DIO* supports gov't funding for the Muffia needy. After all, a sacred mission of the *J.HA* is to show just how desperate this cult's plight is.) The underlying reality of the Muffia's strange fixation on hyperpuffing the rôle of Babylonian mathematical astronomy is simply this: while very few Greek astronomical papyri survive, there are lots of extant Babylonian astronomical cuneiform texts which ought to be joined & translated. (Isn't it a provocative coincidence that the alleged ancient Babylonian progenitors of Greek math astronomy happened to write on a material more durable than papyri? Had Greek astronomical texts survived in numbers comparable to Babylonian, would modern scholars be accepting so readily that Babylon was father to great Greek astronomy?) Thus, it is wonderful that Muffiosi wish so to spend their time. And, one understands that grantsmanship must justify this task. (Muffiosi abhor fraud charges against Ptolemy for a related reason: it's tough raising grants for research in a field whose central document acquires notoriety as a fake.) But let's cool the exaggerations (e.g., fn 242) and commence exhibiting some deference towards truth-in-advertising (*DIO 2* ‡4 fn 14).

[267] For adopted Earth-spin acceleration, see *DIO 1.1* ‡5 fn 11.

[268] The uncertainties cited correspond to allowing 1/4 hour ($3^\circ 3/4$) of uncertainty in the Earth's rotational position. The Alexandria magnitudes of the other 3 eclipses: 55%±2% (−281), 90%±3% (−216), 72%±8% (−173). Their Hellespont magnitudes: 83%±1% (−281), 83%±3% (−216), 86%±7% (−173).

[269] Still undead at fn 166. My 2nd-favorite Toomerism is his blanket-libellous characterization *in a standard Hist.sci reference work* (the *DSB*: Toomer 1975 p.201) of Ptolemy-skeptics as "incompetents". (For a wider spectrum of Muffia compliments, see fn 31.) No Hist.sci leader has publicly recorded the slightest disapproval. (Indeed, as we saw at fn 172: when objections were laid before Hist.sci superarchon H.Woolf, Toomer got carte blanche to continue behaving as high-handedly as he pleased.) When pointing out such behavior to the *DSB* in 1978, I proposed a private get-together, which some archons may eventually (if not already) wish had come to pass. DR to Princeton's C.Gillispie (Editor *DSB*) 1978/5/18: "I could be in Princeton at a pre-specified time in early June to meet GJT [Toomer] and co. before a *DSB* panel if you want to find out which side is making sense in this controversy. [Robert] Newton has recently informed me by letter that he would also be willing to meet such a body." Gillispie (former Pres. History of Science Society) 5/24: "it would not seem to me appropriate to ask Professor Toomer to come to Princeton." Understandable. The offenses were merely censorship & slander; so what's the concern? And when R.Kargon (sometime *Isis* personage), and privately no more a Muffia-admirer than Gillispie, was shown N.C.Swerdlow's sweet 1983/6/2 letter (*DIO 1.1* ‡3 §D2-D3) to physicist R.Newton, calling him a "con man", Kargon just shrugged: "well, maybe he is." (The letter was also sent 1983/8/12 to *Isis*, whose reaction was to ban DR's criticism of Swerdlow's solstice folly: §I13 item [a].) When it comes to pristine housecleaning, Hist.sci could instruct Congress.

R2 All these data (§R1) are given by Pappos. (See Rome 1931-43 p.68, Swerdlow 1969 p.289, & Toomer 1974D p.127.) The same material is discussed (without data) at *Almajest* 5.11. Hipparchos evidently indicated that he derived the lunar parameters for his part 1 by analysing an undated eclipse that was total in the Hellespont but only 4/5 total (Kleomedes 95) in Alexandria. Toomer 1974D attempts to show that various able scientists' previous identifications of the eclipse are just the usual "worthless" effusions of inferior nonMuffia figures: G.Celoria & P.Neugebauer (no relation to ON) opted for the "Agathocles" eclipse of −309/8/15, while J.Fotheringham, S.Newcomb, & C.Schoch went for −128/11/20. Most of us have thought (even though none was a Hist.sci professional) that these men were among the finest astronomers & other experts to examine the question; but Toomer 1974D shows that they were morons compared to himself. (His suspicions of this went back at least to Toomer 1967.) Toomer gets even bitchier toward an appraisal by Robert Newton, who had just recently outraged the Muffia by showing that Ptolemy's data were largely faked. Thus, Toomer 1974D sneers at Newton's grammar[264] — in a swipe whose triviality self-nominates it for recognition as Prissiest-Criticism-of-the-Century. (We can almost[265] match it. See p.iv of [Muffia 1990], "Edited" by Toomer & published by Springer-Verlag, where the TEX-based Leslie Lamport system, which both [Muffia 1990] & DR use, has its trademark

LATEX

rendered by [Muffia 1990] as

"LaTeX"

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

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

[264] Evidently following the lead of (uncited) Gingerich 1972, Toomer 1974D n.13 treats an intelligent suggestion by R.Newton 1970 (pp.106-107) as idiotic. (It's revealing that both reviewers quote the same solitary passage from R.Newton 1970, when looking for a means to denigrate the book. Neither reviewer even explains his objection. Perhaps Toomer figured that OG must have had some reason for scoffing at the passage — and so just signed on, to share the credit for abusing The Hated One.) But Toomer improves on OG by attacking even the RN statement's English usage: "Hipparchus stopped work [sic] a year and a half after the eclipse of −128." Now, now, our little fangs are showing. If Toomer is out to sterilize alleged typos, let's *sic* His Immaculacy onto the Augean task of cleaning out the misprints all over [Muffia 1990], for which he was himself "Editor": see fn 149 & §I1. If one did not know better, one would assume that Toomer didn't read the [Muffia 1990] final text before it went to press. However: [a] the book's camera-ready copy was a TEX file; [b] the TEX Users Group is Toomer's Providence, RI, neighbor; [c] TEX is out of the Amer Math Soc; and [d] Toomer is an eminent Ivy League mathematician. He must therefore be completely at ease with the TEX system. (And, from Toomer 1984 p.viii & App.C, one is deeply — nay profoundly — impressed with Toomer's computer facility: why, of the 20 computer-generated calculations in Appendix C of Toomer's world-famous edition of the *Almajest*, fully 55% of the results are absolutely correct! See *DIO 2.1* ‡3 fn 18.) Thus, we know he had [Muffia 1990] right up there on his monitor. To borrow a fn 63 word from Toomer: "inexplicable".

[265] See also *DIO 2* ‡1 §A7.

Granted all the more-than-obvious differences[45] 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 humility[49] 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 homes[51] 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^{y} — is a privilege beyond comparison.

D3 For *Almajest* 4.11, Toomer's rickety trio A&B solutions — which he intermittently (§O1) imagines to be a precious window into the history of the inception of trig![53] — have

[45] As a near-pacifist & anti-nationalist (whose father died in WW2), DR is an odd admirer of anything at all about the obsequiously Nazified Rommel (Hart p.501). But, even aside from his wellknown military intellect — and courage (he was lucky even to *survive* his 24hr spurt) — Rommel was a genuinely gifted writer. (As was Grant. Or his ghost.) His accounts are not only intelligently composed but astonishingly full — especially for contemporary writings, set down during his years of occasional other responsibilities & diversions, such as commanding rapid-armor warfare, often under fire.

[46] See gutsy footsoldier O.Gingerich's idea of combat at *DIO 1.1* ‡1 fn 20.

[47] Romanticizing intellectual exploration appeals to DR, who holds that concentrating upon the pure & unpragmatic quest, after the grail of truth-for-its-own-sake (while consciously, systematically rejecting corrupting influences), strengthens not only one's ethics but (perforce) one's skills. Moreover: careerism is boring, while discovery is adventure. I acquired this strange attitude (which the greater wisdom of archons rightly views as mere immaturity) at, of all places, Harvard. See also E.Schrödinger *What is Life?* . . . 1956 pp.110f. In an earlier-written paper (scheduled to appear in an upcoming *DIO*), DR compares such successes (as those here described) to sarcophagal invasion instead of military. If creative readers have their own favored analogies in this regard, *DIO* invites their transmission.

[48] For instances of the last four threads: see, similarly, the admirably unrestrained joy of the greatest US Arctic explorer, R.Peary (*Nearest 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, Peary was an unusually able writer and thinker. (Writing specialist & skeptic H.Ward, perceptive on so much else about Peary, wrongly supposed the 1907 book to be mostly ghosted; but, in fact, large & wonderfully 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 Peary.)

[49] Stravinsky on *Le Sacre du Printemps* (which he wrote by ear, not by system): "I was the vessel through which *Le Sacre* passed."

[50] Which in this case means mainly: riding the right hypothesis. I.e., heliocentrism's central rôle in great ancient astronomy.

[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 bloodletting to stooges (e.g., *DIO 1.1* ‡3 fn 3), Rommel could also kill face-to-face: in the latter part of the rapid 1940/5/16-17 thrust, when a French officer refused collaboration, Rommel personally murdered him on the spot (Hart *op cit* p.22). But most commanders are, like Muffia capos, unwilling so to dirty their hands — and thus depute live hatchetry to underlings. (My fellow-semipacifist Redd Foxx' reaction to frontline warfare: "I backed up so far, I bumped into a general.")

[53] A delusion encouraged by Jones 1991M n.5. This note also uncritically pushes the persistent misimpression that arc-degrees did not exist in 3rd century BC Greek astronomy, contra the (differently rounded) degree-format star declinations (*Almajest* 7.3) of Timocharis (c.300 BC) vs. Aristyllos (c.260 BC) — a point noted (obviously vainly) by DR in the 1983/12 *Isis*. (See item [c] below.) The misimpression arises from math-historians' familiarity with the geometrically-written production of pseudo-Aristarchos & Archimedes — oblivious to the simple point noted at fn 262. Hardcase types pass off the *Almajest* 7.3 declinations by conveniently speculating that Hipparchos or Ptolemy *must* later have transformed hypothetical pre-degree data into degrees. Comments: [a] A scholar ought to be able to sense when his prejudice is forcing disconfirmational data to fit a cherished theory. [b] In numerous cases, Ptolemy

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 Muffia, who will simply continue lockstep-pretending that Toomer's development is the only valid one.

D4 Alternate possibility: act as if the Muffia thought of DR's permissible[54] solutions first. (See options [b] & [c] at §H2.) E.g., publish a Muffia paper, containing these DR finds, a few months hence — with a preface signed 1989 or 1990. Too cloddish to consider? Hardly. Indeed, something remarkably similar has already happened. The Preface of Toomer 1984 is dated 2 years earlier (1982). Its special App.C (tacked onto very end of book) contains, without the slightest citation, DR's (entirely original) 1980 solutions for the mean motions of Mercury, Venus, & Saturn (published for DR by R.Newton 1982 pp.103-109). All 3 solutions are based on attested numbers drawn right from *Almajest* 9.3 (sample data: below at §H3). Each of the 3 solutions fits precisely, down to the last sexagesimal place: that is, to a 50 billionth of a degree/day. Numbers provided at *DIO 2.1* ‡3 §C3. All three are so obviously correct that their Untouchable origins have established a thrombus or logjam (§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.)[56] Despite my pointed *American Journal of Physics* remark (Rawlins 1987 n.30) on Toomer's noncitation, Toomer remains silent (as does OG) — and so appears prepared to semi-pretend indefinitely that these discoveries are his own. Hist.sci archondum also remains silent in the face of such behavior. What kind of purported *historians* cannot show an interest in honest attribution

(the Muffia'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 Timocharis & Aristyllos. [c] Unlike Timocharis' data, Aristyllos' six star declinations are all rounded to $1^\circ/4$, 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 least as good as that of the ancient declination observers who indisputably used degrees, e.g., Hipparchos. (How could such accuracy occur by likely pre-transit-circle methods of recording altitudes? — and additionally survive an hypothesized subsequent transformation? How else but in degrees were early transit-circles graduated, yielding such high precision?) [d] Third century BC non-meridian planet-star observations are probably not expressed in degrees merely due to lack of armillary astrolabe (which suggests that perhaps this instrument debuted a little later).

[54] Eqs. 23 & 24 are quite safe, being much too heliocentrist-heretical 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 Muffia *Index Cogitationum Prohibitorum*. (It may well transpire that Muffia response to and-or evasion of this paper's solutions will become the subject of yet another *J.HA* paper.)

[55] The Toomer 1984 App.C's patent loathing of these solutions makes the accursedness 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 56) on this issue, the Muffia had never actually carried out the simple divisions of the period relation numbers provided in *Almajest* 9.3 ! — until DR did so and showed thereby that (contrary to the Muffia's longtime repeated Ptolemy-trusting insistences: partial list at Rawlins 1987 n.30 & *DIO 2.1* ‡3 fn 38) the period relation quotients yielded the precise tabular mean motions for Mercury, Venus, & Saturn.

[56] Solutions also sent R.Newton, e.g., 1980/9/2-4. Copies sent K.Moesgaard 1980/11/15. (Inexplicably uncited at Moesgaard 1987 p.45 — though, in a letter of 1983/3/2, he had offered his co-authorship to assist publication-chances of DR's 1983/1/10 ms, which had by then traced these solutions to a further stage in their ancient evolution.) All *Almajest* planet mean motion equations sent *Isis* 1983/3/3 & 1983/8/12. (First submission 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 fn 15: 1991/8/23) Muffia-assistant P.Huber 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 Muffiosi been hoping these transmissions are unprovable? In fact, my files contain detailed replies, from R.Newton (e.g., 1980/9/14 & 11/7), and O.Gingerich (e.g., 1980/11/3), all showing that the solutions were new to them. The solutions were also unknown to Muffiosi, whose longtime persistent upside-down misconstruing of the same data was exposed in n.30 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.

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

$$r_{\rm M} = \frac{1+\sin\gamma}{(1+\upsilon)\sin\theta} \tag{27}$$

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

$$\gamma_{\rm A} = 3^\circ. \tag{28}$$

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

$$\upsilon_{\rm A} = 2 \tag{29}$$

Finally, the corrected[261] Aristarchos value of the lunar semidiameter:

$$\theta_{\rm A} = 1^\circ/4 \tag{30}$$

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

$$r_{\rm M} = \frac{1+\sin\gamma_{\rm A}}{(1+\upsilon_{\rm A})\sin\theta_{\rm A}} \doteq 80^{\rm e} \tag{31}$$

where, again, $1^{\rm e}$ = 1 Earth-radius. This entails (via eqs. 26 & 28)

$$r_{\rm S} = 1536^{\rm e} \tag{32}$$

Since $r_{\rm M}$ is actually $60^{\rm e}.27$, eq. 31's result is about 1/3 high.[262] But it approximates[263] the earliest reconstructable empirically-based estimate of a celestial distance. (Ptolemy's later $59^{\rm e}$ value is far, far better: *Almajest* 5.15.) Note: all of the data producing eqs. 31 & 32 are attested.

R Impure Speculations, Pseudo-Aristarchos' Fatal Contradiction, & The Muffia's Haute Cowture

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

[261] See *DIO 1.1* ‡7 fn 6.

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

[263] [Note added 1992: An incompletely attested but more reasonable case can be made that Aristarchos' $r_{\rm M} = 60^{\rm e}$. See "Ancient Vision", to appear in a future *DIO*.]
[Note added 2008: "Ancient Vision" eventually appeared at *DIO 14* ‡2 (2008 March). See its eq.11.]

to insist on the validity of its own patently inferior solution to the same mystery. Another way of putting it: I am, in effect, effortlessly compelling Muffiosi to insist upon going in the opposite direction from the truth — i.e., betraying their very profession. Not every scholar's detractors are so obligingly cooperative in thus destroying their own intrinsic credibility.) The same sort of thrombus has occurred with respect to, e.g., the *Almajest* 5.3&5 trio of Hipparchos lunar positions (§G7 & §I2), the *Almajest* mean motions (§D4 & fn 129), and Eratosthenes' precision (Rawlins 1982G). When correct solutions are blocked by political cholesterol-clog, then, inevitably, the flow of progress halts in the stricken areas.

P4 During the foregoing, I've tried to appreciate Toomer's erudition. And I'm tempted to sympathize with him, even though he's about as merciful[257] as his Funny-Forum mentor. But sympathy for Malignant T. Gloriosus would be wasted here, for an obvious reason: he will never know disappointment in this matter — because [a] his colleagues & hangers-on will emit no (audible) snickers, & [b] he will himself never accept the obvious preferability of the DR quadruple-success perfect-fit solution. Even if *direct ancient attestation* of the truth surfaced, he would spurn enlightenment. (*DIO* is not speculating. Indeed, two instances of Toomer's stubborn rejection of the plainest possible ancient testimony, have already been presented above: §I1 & §M7.)[258] You see, there *is* a positive side to possessing a robust hallucinatory capacity. *Forum*'s Miles Gloriosus: "*I* am my ideal."

Q Improved Estimates of Aristarchos' Distances to Sun & Moon

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

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

Q3 DR's emendation (fn 261) of pseudo-Aristarchos' infamous 6^{th} hypothesis, in accord with Archimedes' explicit testimony, permits a vast improvement in our estimate of Aristarchos' actual $r_{\rm M}$. The key equation (based on the standard ancient eclipse-diagram, with the traditional assumption that Sun & Moon have the same[259] angular semidiameter) is well known.[260] For lunar distance $r_{\rm M}$, solar distance $r_{\rm S}$ (both in Earth-radii), solar and lunar semidiameter θ, Earth-shadow semidiameter υ times larger than θ:

$$1/r_{\rm M} + 1/r_{\rm S} = (1+\upsilon)\sin\theta \tag{25}$$

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

$$r_{\rm M}/r_{\rm S} = \sin\gamma \tag{26}$$

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

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

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

[260] E.g., *Almajest* 5.15, Swerdlow 1969 p.294, R.Newton 1973-4 p.382, Toomer 1974D p.130, Neugebauer 1975 pp.636-637, Van Helden 1985 pp.7&18.

of major scholarly discoveries? *Is this subject not, after all: history?* Is there no Hist.sci concern for accuracy *or* ethics? To put it yet more plainly: is effectively grabbing credit (for major discoveries) of no account? The Muffia & Lord Hoskin determined years ago to exile uppity DR from ancient astronomy: *DIO 1.1* ‡1 A8. (Even Lord H's *JHA* now admits that fellow-rebel R.Newton was for years similarly treated as a "pariah", by the very same people: fn 90.) But, with customary wisdom, this clique did not anticipate or assess an implicit risk: what if DR continued a series of original & compelling solutions to important ancient astronomical mysteries? How, then, could leaders maintain pride & power by continuing the blackballing[57] under such ghastly unforeseen circumstances? Simple. Having gotten this deeply into slime, the responsible archons' only possible recourse: deny the originator credit. (As was done to R.Newton while he lived.) Again & again & again . . . . As many times as prove necessary in order to maintain the proper pecking order. (Examples & methods partially cataloged at §H2. For details of the open&shut planet-mean-motions case, see *DIO 2.1* ‡3 §C. The predictability of the credit-denial pattern accounts for this paper's heavy annotation, as I feebly attempt to anticipate, aloud, its findings' probable *Old-Man-&-the-Sea* fate, even while I acknowledge my relative limitations at imagining new ways to cheat scholars outside one's cult.) I repeat: no one anticipated this cycle. But, once a clique locks itself into the pattern, there's no way out. Except honest admission of massive error and decades of false defamation of worthwhile scholarship. (But too many other scholars have heard Muffia slanders of it, so such retraction — or indeed *any* perceived success by a Muffia-damned party — would be ruinous to Muffiosi'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, the implicit logic is that one or two such sleights will suffice — neglecting the hideous possibility that the scholar will keep right on making other discoveries, so that a policy of repeatedly denying credit is going to get progressively smellier.[58]

D5 Hist.sci's highest archon-angels may perhaps be tempted (privately) to blame their Jonestown spectacular upon the Muffia. Perhaps also upon servile Muffia 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 naïvely 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-versed, so I withdraw the accusation.) E.g., OG's *JHA* pre-publication promotion (§E2) of Jones' Babylonian fantasy (§E1) shows that 0 was the immediate cause of its special *JHA* exaltation — which no doubt played a part in *Isis*' taking it seriously. Well, far be it from DR to defend Muffia&co. but: crediting them for Jonestown would be unjust. No, the responsibility belongs to Hist.sci's own archons, who have no excuse whatever for being taken in by Muffia&0 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

[57] 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 ejectee's papers, in any field, on any subject. So there's nothing unique about such behavior. What's special here is the naïvete of those who trust these zoos' effusions.

[58] Statistics-wise, this reminds one of Rose Bird, the environmentally-sensitive (thus business-enraging) judge whom anti-death-penalty Gov. Jerry Brown appointed to head 'Fornia'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.

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 archons' backbones & smarts to jellyfishes'.)

E DeToga Party: Lead Paper, Lead Balloon

E1 The primary purpose of the extremely handsome *Journal for the History of Astronomy* is too simple to be written on its inside cover. The *JHA* consciously aims at being the most prestigious journal in the astronomy-history field. It is purportedly edited by U Cambridge's Lord Hoskin (Cambridge Univ, Churchill College) and O.Gingerich (Harvard), with hawkeye expert overseeing[59] by "Advisory Editors" such as Muffioso Noel C. Swerdlow (Univ Chicago's Dep't of Astronomy & Astrophysics) and Albert Van Helden (Rice Univ). (Swerdlow's friend and promoter, Van Helden is also an "Advisory Editor" of *Isis*.) In this magnificent magazine's 1991/5 issue, the long-anticipated[60] *LEAD* paper, "Hipparchus's Computations of Solar Longitudes" (Jones 1991H), announces an astonishing discovery by [a] Neugebauer-Muffia product[61] & Toomer protégé, Alexander Jones (Institute for the History & Philosophy of Science & Technology, Univ Toronto), masterfully proving that Hipparchos was virtually a closet Babylonian[62] in Greek drag. Jones deftly de-togas Hipparchos by fitting a Babylonian-style solar scheme to the "inexplicably"[63] discrepant Hipparchan longitudes of the Sun cited at *Almajest* 4.11 — and thus Jones 1991H actualizes the persistent dream of Muffia capo & *Isis* darling Bernard Goldstein (U Pitts) and of *JHA* co-Editor O.Gingerich (§F1) by establishing at last, through mathematical "proof" (fn 107, Jones 1991H pp.104, 110), the long-sought Babylonian influence lurking invisibly behind Greek astronomer Hipparchos' solar orbit. This grand discovery was swiftly re-trumpetted in *Isis*' 1991/9 *LEAD* paper, "The Adaptation of Babylonian Methods in Greek Numerical Astronomy" (Jones 1991M). And the Jones Pb-papers' revelation is joyous news for Muffiosi, since it initially appears to weaken the simplest argument against Ptolemy's integrity. After all, it is by now generally acknowledged that Ptolemy just took his Greek-trig orbit of the Sun from Hipparchos and faked allegedly outdoor solar "observations", in almost

[59] 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 §A7!) One of the (not very) implicit messages of *DIO*'s *J.HA*: what is "prestige" or "reputable" publication worth? — if evidence for scrupulous editing is undetectable.

[60] Advertised as forthcoming for months, on the inside covers of *JHA* issues. As noted here (fn 114, §K, & §L), the actual method of solution to the 3 solar longitude problems addressed by Jones 1991H is that of Rawlins 1991H. But Jones 1991H p.117 claims (partly due to a critical miscomputation on the same page: §G7) that Rawlins 1991H's solution could not happen — even though Rawlins 1991H was published 4 months before Jones 1991H (1991/5) appeared! [When *DIO 1.1* appeared in 1991, Jones said he wouldn't look at it. But later he agreed with it.] The Muffimmobility is as poriferan as Hegel's 1801/8/31 Univ Jena denial of the existence of Ceres, which had already been discovered on 1801/1/1 and publicly announced in Jena on 1801/5/6. [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 Hoskin simply OMITS the Hegel 1801 analysis' essential final math paragraph on the planets, where he states the very distance formula which is the subject of Hoskin's paper! Our most sincere thanks to His Lordship for so promptly & convincingly exemplifying the earlier disaster's prime lesson, which follows immediately here.] As unperturbed by mere facts as the Muffia, Hegel went on to the heights of professorial power, teaching nothing else so clearly as the lesson: you don't have to be correct, able, or sane to be an exceedingly influential academic.

[61] See, e.g., proud patronization & strong praise at Toomer 1988 n.25 (fn 271 below) & n.43. Jones is, of course, from the same Brownie troop as Muffia capo G.Toomer, the vaunted History of Math Dep't at BrownU. (See p.36 of Jones 1983; paper recommended by Asger Aaboe, also Muffia.)

[62] See fn 15; also Toomer 1988, e.g., pp.360&361.

[63] 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 Jonestown 1991 gradeschool misarithmetic which Britton vetted (§G4) and which is the subject of our present *Journal for Hysterical Astronomy* romp. . . .]

self-satisfaction (see *DIO 2.1* ‡3 §A) is complete, and he — Muffiosi's **the**-expert on Ptolemy (fn 240) — basks in the glow of his genius (Toomer 1973 p.15): "our calculations have proven" his theses. (See also §O1.) From *Funny Thing Happened on the Way to the Forum* (1966 Plautus-based cinema musical),[256] one recalls the equally-modest Miles Gloriosus' appraisal, upon observing himself: "Even *I* am impressed." But Toomer's intricate & learned geometrical development, despite various arbitrary steps & nudgings, has never quite recovered any of Hipparchos' four numbers. His trio A investigation wasn't very convincing: $e = 338'$, & $r_M = 3134'$, vs. the attested (*Almajest* 4.11) numbers, $327'2/3$ & $3144'$. But the trio B search seemed to get within an ace of the *Almajest* 4.11 value $r = 247'1/2$, when Toomer 1973 pp.10-11 came up with $r = 246'1/3$. (His associated value for r_M was not so lucky: $3082'2/3$ vs. 3122 1/2 attested.) But then, the entire trio B analysis of Toomer 1973 turned out to be founded upon a scribal error for the 2nd time-interval. (See Toomer 1984 p.215 n.75. This correction is one of numerous useful contributions Toomer 1984 has made to our knowledge of the *Almajest*. It is highly probable that without this fruit of the massive labors that went into Toomer 1984, I would never have been pushed away from the Toomer 1973 theory and fallen into eqs. 23 & 24.) I have recomputed Toomer's values, based upon the correction. The results: $r = 231'$ & $r_M = 3021'$. These are a long way from the numbers in *Almajest* 4.11: $247'1/2$ & $3122'1/2$. (For trio B, Toomer arbitrarily assumes an unlikely Hipparchos blunder, in order to get even that close; without this convenient step, his trio B's corrected value would be $r_M = 2916'$.) As noted above (§G2, §O1, fn 252): despite these flabbinesses & crumblings, Muffia scholars — including Jones & Toomer himself — continue to act as if this analysis has proved Hipparchos' use of the chord trig table that Toomer hypothesizes for him throughout the attempted reconstruction. Yet it is now all too plain that Toomer's entire intricate structure has been a castle in the air: a lovely, well-crafted, admirably imaginative fiction. But fiction, nonetheless.

P2 Let us tabulate the results of DR vs. those of Toomer for the four numbers of *Almajest* 4.11:

Parameter	*Almajest* 4.11	Toomer	DR
Trio A's e	$327'2/3$	$338'$	$327'39''$
Trio B's r	$247'1/2$	$231'$	$247'30''$
Trio A's r_M	$3144'$	$3134'$	$3144'$
Trio B's r_M	$3122'1/2$	$3021'$	$3122'1/2$

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

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

[256] Lyrics excerpted here are by S.Sondheim. The "ancient" 1966 screenplay contains an unintentional anachronism, when a character worries about rubbing elbows with someone allegedly infected with a supposed Cretan plague. He asks: Is it contagious? Reply: Ever see a plague that wasn't? This line was a bad joke in 1966. Now, it's AIDS-lobby-approved gov't policy. In other words: it's still a bad joke.

for geocentrists to express the Moon's distance in Earth-radii, it was just as natural for heliocentrists to express the Moon's distance in AUs. A final point: since [a] Babylonians preferred sexagesimal expression for numbers over 59 (van der Waerden 1963 p.38, van der Waerden 1978 p.667), and [b] the base distance used for Hipparchos' lunar work was 1000^{a} (a choice of round number which denotes a decimal system for large values), we know that the work behind the Hipparchan *Almajest* 4.11 material was not Babylonian (as suggested by Jonestown) but Greek.

O6 An overview reflection: there is a striking analogy between [a] the foregoing lunar findings and [b] the matter of the *Almajest* planet mean motions. For both problems, DR has found *perfect* fits to Greek data via Greek methods, as against Muffia and Moesgaard Babylonian solutions that *don't* fit the same Greek data. See tabular comparisons at §P2 for the lunar numbers, and *DIO 2.1* ‡3 §C3 (also §H3 here) for the planet mean motions. Notice also that Greek heliocentrism provides part of the solutions for *both* problems: eqs. 23 & 24 (distance of the Moon), and fn 129 (Mars mean motion; see also *DIO 2.1* ‡3 §C3).
[Note added 2003. DR had thought the Mars mean motion tables were based on an integral number of longitudinal (heliocentric) revs. The basis has indeed turned out to be longitudinal (contra Ptolemy), but not that way. See just irony at *DIO 11.2* ‡4 fn 21.]

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

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

P Basking Case

P1 From §N14 & eqs. 23-24, we have found the long-sought solutions to the four mysterious parameters set forth at the outset of our explorations here (§D1 item [b]): $3144'$ & $327'2/3$, $3122'1/2$ & $247'1/2$. The above solutions agree with the attested values, to the precision displayed, in all four cases. (Note that, even if a Muffia anti-heliocentrist-influence fanatic rejects the solutions for $3144'$ & $3122'1/2$, he must face the fact that the other 2 solutions stand on their own merits, being based merely upon entirely attested Greek astronomical math: §N12-§N14. Indeed, of the current paper's discoveries, this solution-pair and §N10's matching integral hits will present Muffia evasiveness its sternest challenges.)[255] Before proceeding further here, let us examine, for contrast, how closely Muffia capo Toomer's quarter-century of labors (§D1 & fn 116) has brought him to the same four numbers. His quasi-agreements are so compellingly semi-good that Toomer's typical

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

exact agreement with Hipparchos' solar theory — and then brought these fakes forth at *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, ghastly truth (explaining all 4 of Ptolemy's solar "observations", on the nose) was first revealed in J.Delambre *Histoire de l'Astronomie du Moyen Age* 1819 (pp.lxvii-lxix, a source never cited by the Muffia): 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 J.Britton of the Muffia, he cited Britton 1967 (R.Newton 1970 p.24 n). RN later found that 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 Muffia itself has, despite almost countless opportunities, not yet cited the same Delambre passage. (§I14. Muffiosi prefer that all citations, of Ptolemy's manifold embarrassments, be to Muffia-orthodox-and-thus-forgiving discussions: §I14 & *DIO 1.1* ‡1 fn 5. Classic insular cultist thought-control.) Despite this unsubtle comparative record, the Muffia's most abusive mout'piece has repeatedly tried (fn 123, fn 169, & fn 252, N.C.Swerdlow 1979 p.528) to portray RN (also van der Waerden), not Britton, as a dishonest citer (a piece of Muffia logic & gentility remarked in passing at Rawlins 1991H fn 6). [Note added 1993: Britton 1992 p.xvi agrees. And doesn't cite NCS' priority. . . .]

E2 Returning to Jones 1991H's hope-opera: if Hipparchos' solar math was Babylonian-arithmetical, not Greek-trig, then perhaps[65] there *never was* (fn 110) a Hipparchan Greek-trig solar orbit for Ptolemy to steal. (The Muffia for many years tried a like libretto when denying that Ptolemy stole Hipparchos' ecliptical 1000 star catalog.)[66] Hosannah! Thus, Jones 1991H "proves" (and Jones 1991M prominently promotes the conclusion . . .) that the solar positions for the two Hipparchan eclipse-trios (A&B) preserved at *Almajest* 4.11 must be based upon the Muffia's belovéd kindergarten fast-arc-slow-arc scheme (Babylonian System A step-function velocity, as against the superior trig-based continuous velocity function 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

[64] See fn 166 & fn 168. Over a decade ago, DR added to this argument the ironic oddity that these arithmetical fakes show that Ptolemy consistently built upon 6h-precision Hipparchan data — to create solar "observations" rounded to 1h precision! Like RN's argument at §F3.

[65] Jones 1991M (especially given the information in its n.28) appears to reject less dogmatically (than Jones 1991H p.122) the general preMuffia perception that Hipparchos constructed a solar theory (the PH orbit: §K10) like that of the *Almajest*; but Jones 1991M p.446 believes that this could only have been at the very end of Hipparchos' career. (Actually, it seems obvious that the full PH solar orbit existed from about 146 BC, the [year] of the [Vernal] Equinox that fits perfectly with the PH theory. See Rawlins 1991H §E5.) And, even so, Jones 1991M believes (p.449; & see n.28) it could have been of Babylonian zone format.

[66] Muffia: "all we know" (Neugebauer 1975 p.280) and "All the evidence" (Toomer 1984 p.330 n.56, comments plainly dependent upon uncited Neugebauer 1975 p.280 for more than this familiar wording — echoes also noted at fn 100 here) tell us that Hipparchos, the reputed discoverer of ecliptical precession (and thus the messy inconstancy of equatorial coordinates), whose extant solar & lunar coordinates were entirely ecliptical, would not record his stars ecliptically! Classic Muffia logic. (The remarkable stolidity of this reasoning was pointed out in a DR 1978/3/18 document sent to Toomer by the *DSB* Editor C.Gillispie 1978/6/16: p.H4, commenting on Toomer 1978H p.217.) That such elementary considerations just *might* argue for Hipparchos having rendered his star catalog ecliptically is finally recognized at [Muffia 1990] p.216; but he & his Muffia patrons will still not frankly admit the obvious. (When the options are face vs. truth, the Muffia always chooses wisely.) On the basis of Toomer's forged *Almajest* 7.3 translation (§I1), accepted and quoted verbatim at [Muffia 1990] p.215, the work concludes . . . by saying (p.216) that we cannot "on the basis of presently available data" know whether Hipparchos compiled an ecliptical star catalog. Had Toomer translated *Almajest* 7.3 without Muffian bias, [Muffia 1990] could not say that. (For one likely cost of this amazing exercise in Muffia tenacity, see *DIO 2.1* ‡4 fn 29.)

who preferred a step-function arithmetical approximation to a continuous trig function: fn 87. But no hype[67] is beyond Muffiosi 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 insight[68] 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 Hipparchus's 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 Jones 1991H disaster. That reality: not a single empirical solstice or equinox from Babylon is known to us. Neugebauer 1975 (p.366): "The insight that the solstice-equinox-Sirius dates were based exclusively on the cycle [$19^{y} = 235^{mo}$] 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* ‡2 §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 scientists) anywhere in the Babylonian record. It is remarkable to find the description "impressively accurate" (Jones 1991H p.118) applied 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 alibiing 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 Oblivious to the plain implications of the foregoing, the refined Muffia nose smells a Babylonian lurking beneath every incompletely understood Greek achievement. (It's the same familiar, preternaturally penetrating brilliance[71] by which other fundamentalists[72] 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-conscious comments on grant-hustling exaggerations. He dislikes 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 funding and their race for grants."]

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

[69] Rawlins 1982G n.17, Rawlins 1985G p.257, Rawlins 1987 p.236 item [2].

[70] Ptolemy's latitude for Babylon: 35°N (e.g., *Geogr Dir* 5:20:6). See also *Geogr Dir* 8:20:27 and Rawlins 1985G pp.260f & fn 13. [And see confusion at Strabo 2.1.23-30.]

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

[72] Anyone who doesn't believe in evolution should try tracing Muffia Ptolemy-apologia. Especially §I9. See also, e.g., §I1 & §I7 item [c], *DIO 1.1* ‡1 fn 9, & DR to OG 1983/11/25. [Note added 1993: see Pedersen at fn 99 !]

degree-fraction; e.g., Swerdlow 1979 (pp.527-528) savages[252] R.Newton 1973-4 (pp.112-113) for being misled by just such an error (a longago misreading of 344°1/12 as 344°12′). [Neugebauer 1975 p.166 n.3 requires such an ancient scribal error. Same error: *ibid* p.729 n.15.] Now suppose a member of Hipparchos' school, deputed[253] to calculate r from eclipse trio B (for which he would need $r_{\rm M}$ for use in eq. 18), started by misreading the eq. 23 distance $r_{\rm M} = 52^{a}24'$ as: $52^{a}1/24$. This would transform distance $r_{\rm M}$ into:

$$r_{\rm M}(\text{trio B}) = 52^{a}1/24 = 52^{a}02'1/2 = 3122'1/2 \qquad (24)$$

Which recovers the *Almajest* 4.11 trio B value (§D1, §N10) for $r_{\rm M}$, on the nose.

O4 Note that, without the good luck (for us, anyway) of Hipparchos' computer having made his fateful scribal slip, the above heliocentrist explanation (§O2) of the origin of 3144′ would not be utterly unevadable. Even though true. Fellow explorers will better empathize with my pleasure at discovering (1991/11/28) the confirmatory scribal miscue, if they realize that I had already (since 1991/11/9) come upon eq. 23 by theorizing that Hipparchos was (at this point in his career) drawing his astronomical scale (eq. 22) from real contemporary astronomers, who were, naturally, heliocentrists. Though I was confident[254] of heliocentrist eq. 23 anyway, the moment of confirmation — the finding of eq. 24 — was exquisite. I then knew positively that the hypothesis behind eq. 23, namely that heliocentrist work underlay all high ancient astronomy, was (§D2): "not just a beautiful dream. It was reality."

O5 Since scaling the Moon's distance by the Earth-Sun distance implies realization that the Sun was the central body for astronomers, the foregoing two astonishing matches (eqs. 23 & 24 vs. §P2) leave no doubt that genuine ancient astronomers: [a] used the Astronomical Unit, and [b] were heliocentrists. (To scholars of scientific history, it should be self-evident that geocentrism was as nutty to competent ancient scientists as to moderns — a point demonstrated in detail by "Figleaf Salad": *DIO 1.1* ‡7.) While it was perfectly natural

[252] Fn 169. To the open & detailed retraction at R.Newton 1977 p.130, Capt.Captious NCSwerdlow responds, by attempting to portray such refreshing behavior as dishonest! [Note added 1993: Britton 1992 p.xvi repeats this hilariously inverted slander. As a historical & probably verifiable fact, I can report that (contra Britton's unsubstantiated claim of "errors scattered throughout Newton's work" allegedly caused by his use of the Halma & Taliaferro editions of the *Almajest*): R.Newton had both the Manitius & Heiberg editions of the *Almajest* out of the JHU library for years on end. (At the start of his researches, RN indeed openly used primarily the Halma edition; but he henceforth consulted all available versions of the text.) Britton *loc cit* offers the usual Muffia capo gotta-go-now excuses for not providing "extensive references" to RN's allegedly copious resultant errors. The same Muffia pretense to apprehension of scores of serious RN mistakes has been going on for over 20^{y}: Swerdlow 1979 p.530 says the very few RN errors he alleges "could be multiplied twentyfold". DR's 1979/10/26 letter forcefully challenged NCS to prove it. NCS instead just repeated himself unquantitatively at HamSwerdlow 1981 p.61, still refusing to substantiate the big lie. See *DIO 1.1* ‡5 fn 6; also *DIO 2.3* ‡8 fn 61, which directly charges that "the Muffia klan has simply been bluffing in this regard", especially with respect to its capos' (unwritten) slanders against DR's accuracy. But Hist.sci's brave & just leadership still hasn't required that *2 decades* of Muffia libels be made good.] (Compare Newton's frankness to Toomer's §G2 behavior when upset by the very same problem: altered number-basis.) Thank heavens for NCS' alertness. Without the Muffia on guard, why, academe might become corrupt.

[253] Given the contrast between eq. 23 & eq. 24 (even accounting for the likelihood that they were produced at different times for different works: §N3): it is unlikely that the same person performed the lunar calculations leading to eq. 19 as well as eq. 20. (The Muffia also regards the trio A & trio B calculations as well separated events, e.g., fn 209.) More revealing: If Hipparchos understood the identity of the ultimate basis for both choices of $r_{\rm M}$ (eq. 23 & eq. 24), then he could not have innocently published two different versions of the exact same amount! (Had the bungled eq. 24 been published first, we could suppose that eq. 23 was set forth later to quietly correct the $r_{\rm M}$ given. But it is generally agreed that the reverse was the case: §N3. And trio B not trio A became the basis of Hipparchos' later work: §R1.) So the blunderful disparity, between eqs. 23 & 24, provides unexpectedly strong evidence for some historically intriguing realizations: [a] Hipparchos headed a stable of talent. (None of whose names are known to us. Did a now-anonymous Hipparchos-circle Kepler go on to produce work now glimmering in the fragments we have of Poseidonios' corpus? Was Poseidonios himself a Hipparchos protégé?) [b] The alertness & comprehension of Hipparchos' editing of his computers' work were about on the order of "Editor" Toomer's when he vetted [Muffia 1990].

[254] Nonetheless, in the 1991/11/14 first draft of this paper (laserprinted 11/25), DR felt obliged (before finding eq. 24) conservatively to call eq. 23 speculative. (DR's attitude in such matters is best gauged from his remarks at Rawlins 1985G p.255 last sentence, & here at §R10.)

mistake for the real scientists of antiquity. I should add that the import of heliocentrism in serious ancient astronomy has, from the outset (1976), been a theme[246] of DR's studies in this area. The idea of heliocentrists' rôle in 3rd century BC astronomy (establishing a tradition that carried down through Seleukos the Chaldaean[247] and into eastern astronomy) was earlier broached by van der Waerden 1970 (whose proposals were attacked in *Isis*, with classic Muffia surety & haughtiness, by Swerdlow 1973: see Rawlins 1991H fns 6 & 36). So the following surprise developments (eq. 23 & eq. 24) represent a remarkable — and wholly novel — double-success for this Muffia-resented, flagrantly whiggist viewpoint.

O2 We know that the Poseidonios school's distance[248] to the Sun was 10,000 Earth-radii, or:

$$r_{\rm S} = 10,000^{\rm e} \tag{21}$$

So, suppose heliocentrists such as Aristarchos scaled the universe (perhaps very much the way moderns use the Astronomical Unit) similarly, using 1000 units (not necessarily Earth-radii) = $1000^{\rm a}$ for the solar distance:

$$\text{Astronomical Unit (Sun to Earth distance)} = r_{\rm S} = 1000^{\rm a} \tag{22}$$

We will call $1^{\rm a}$ a milli-AU. Now, the traditional Aristarchan ratio of the Sun/Moon distance (also adopted by the geocentrist tradition: Delambre *Histoire de l'Astronomie Ancienne* 1817 vol.2 p.207, R.Newton 1977 p.199) is based on the half-Moon being 3° from quadrature — i.e., the transverse Earth-Moon line subtends[249] 3° as seen from the Sun. So, if the Solar System was based on this scale (eq. 22), then in the same milli-AU units, the Moon's distance $r_{\rm M}$ will be

$$r_{\rm M}(\text{trio A}) = 1000^{\rm a} \tan 3^\circ = 52^{\rm a}24' = 3144' \tag{23}$$

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

O3 A hypothetical Muffioso, secretly reading *DIO*, is now about to (indeed is *required* to) protest that the eq. 23 shocker just unveiled: [a] is mere-coincidence (albeit a *very* long longshot) and [b] anyway-doesn't-fit-trio B. So, let's next unleash the remarkable clincher, as we now use *the same theory* to solve also trio B's superficially discordant Hipparchan $r_{\rm M}$. (Compare this classic success, by the fruitfulness criterion of fn 85, to the earlier failures of DR, Toomer, & Jones: §D1, §D3, §F4.) One of the best known & peskiest types of scribal errors in Greek astronomy & geography is the confusion[251] of arcmins with

[246] See, e.g., *DIO 1.1* ‡7 §F3 (from 1976), DR to *DSB* 1978/5/18, Rawlins 1984A.

[247] Neugebauer 1975 pp.610-611, 697-698. In a 1985/4/13 letter to DR, van der Waerden stresses the often-overlooked point that the name "Seleukos" is Greek, not Babylonian.

[248] Kleomedes 79 (Neugebauer 1975 p.656 eq.16; R.Newton 1977 pp.179&181). [Heath 1913 p.348 suggests an Archimedean origin.] This figure entails (eq. 33) a solar radius of about $R_{\rm S} = 44^{\rm e}$, which has the right ordmag. Another (tenuous) suggestion, of high accuracy astronomy here, is the hint of ancient use of 60 stades/1°: Neugebauer 1975 p.656.

[249] The actual Aristarchos Experiment (*DIO 1.1* ‡7 fnn 5-6) would be most accurately described using sin 3°, not tan 3°. (See, e.g., Van Helden 1985 p.6 Fig.2.) But the standard ancient lunisolar diagram (*ibid* p.7 Fig.3 or *Almajest* 5.15) draws & computes similar situations using transverse lines shown falsely perpendicular to the Sun-Earth axis (instead of the correct way, e.g., Heath 1913 p.404, Neugebauer 1975 pp.641, 1354 Fig.13), thus effectively replacing sine with tangent. Swerdlow 1968 p.60 n.30 & Swerdlow 1969 p.293 nn.10-11 both regard as erroneous (rightly remarking that the difference is trivial anyway) the replacement of tangent with sine for the lunar angular semidiameter in such diagrams (see, e.g., S.Newcomb *Compendium of Spherical Astronomy* 1906 p.158 eq.20), even though sine is actually the correct choice. Which suggests that eq. 23 is a follower's version of Aristarchos' scheme, not the original. (This can be taken as evidence against Aristarchos' use of trig.)

[250] The mixture here of decimal & sexagesimal systems creates no difficulty, since such a hybrid system is used, e.g., throughout the *Almajest*. For 3rd century BC Greek celestial distances expressed in simple powers of 10, see Archimedes ("Sandreckoner" pp.226f).

[251] The least damaging example was the ubiquitous confusion of 1°/8 with 8′. This was virtually an ancient identity: two examples at §M10 here.

1991H is just the latest chapter in a lengthy[73] history of religious pursuit of that particular Muffia will-'o-the-wisp. (To hear Toomer 1988 & Jones 1991H tell it, Hipparchos was virtually a Babylonian, merely posing as a Greek. Well, if Lord Hoskin & G.Toomer can pose as Editors, any disguise is credible.) To this recipe — already guaranteed to set Muffiosi salivating — Jones 1991H adds, as [the] crowning touch, the ecstatic proposal that his reduction of Hipparchos to arithmetical-Babyling shows that Muffia ultimo-hero C.Ptolemy (not Hipparchos) was the true developer of the *Almajest* solar tables. [Note added 2015. Check this against *DIO 20* ‡2!] (To make this idea more credible, Jones 1991M even juggles ancient testimony to put over the delicious proposal that Hipparchos & earlier Greek astronomers didn't compute or use astronomical tables: §M7. [Note added 2015. Check this unique perception against Tihon's papyrus findings: *DIO 20* ‡2!] Didn't use TABLES?[74] Hmmm. The Muffia simultaneously claims[75] that Hipparchos was a major figure in the history of trig. Gee, what was the prime tool for trig computations in those days? Electronic pocket calculators? Pyramid power? . . . . How *does* the Muffia keep mining these pyrites?) This shiny new demonstration allegedly[76] shows that Ptolemy was not a plagiarist but rather (Jones 1991H p.122, emph added): a "*radical reformer*" — bringing (Jones 1991M conclusion, p.453) "consistent[77] methodology"[78] and a hitherto-little-appreciated "originality" into Greek astronomy! (Yes, it sells. But, please, sympathize with the quandary[79] of *DIO*'s *J.HA*: how does one satirize material that already reads like satire?)

[73] The *Isis 82.1*:87 (1991) obit for Muffia godhead O.Neugebauer sums up his lifetime "fundamental conclusion . . . that . . . the various [occidental] civilizations of the world have all depended on the Babylonians for their basic understanding of mathematical astronomy". We now have 3 alleged crucial Muffia proofs of Hipparchos' use of Babylonian arithmetical methods: [a] Hipparchos refers (quoted *Almajest* 5.3) to a 248 day cycle (Toomer's excellent discovery, which everybody else had missed for centuries). But Toomer 1988 p.357 creditably admits the catch with connecting it to Babylon: Hipparchos' table is not known to have been arithmetical, since periodicity in itself proves nothing of the periodic curve's shape. (Using a 248 day table would only cause an error of c.$1^{\rm m}$/month, and so he presumably used such for a quick check or reference. But the current paper shows that when it came to serious math work, Hipparchos used a more refined scheme for anomaly: eq. 7. Indeed, that fact has long been obvious from *Almajest* 4.2. Finally: we do not know that use of the 248 day cycle was peculiar to Babylon or even originated there.) [b] Muffiosi claim (Neugebauer 1975 p.305 & Toomer 1988 p.356) that Hipparchos' klimata (Strabo 2.5.34f) were arithmetical-Babylonian. Actually, they were computed by a sph trig formula (A.Diller *Klio 27*:258, 1934; Rawlins 1982C), which fits the Strabo data for 11 of the 12 Hipparchan klimata — while *half* the same dozen data don't fit the Muffia explanation. (See *DIO 2.1* ‡3 fn 3. [Also *DIO 4.2 Competence Held Hostage #2* pp.55-57.]) [c] And now we have the gorgeous Jones 1991H proof (§G4) that Hipparchos' solar theory could not be Greek trig and so had to be based upon a Babylonian(-style) arithmetical step-function. This declaration is demonstrably false, since the Greek trig orbit solutions deemed "impossible" by the Muffia are provided right in this paper: §G10, §K9, §M4.

[74] Jones 1991H p.120 (emph added): "Hipparchus did not, *of course*, work out a set of solar tables from his solar theory"!

[75] Toomer 1973, Jones 1991M n.5, §D3.

[76] What such airbrained ingenuity actually shows: if a cult isolates itself from dissent, it begins to imagine it's as smart and as sane as its propaganda keeps insisting.

[77] Yet Gingerich 1981 p.44 (while defending Ptolemy's essential honesty) acknowledges that R.Newton "deserves credit for bringing so forcefully to our attention the inconsistencies and anomalies in Ptolemy's work." Comparing this to the Jones 1991M quote here annotated, DR must responsively acknowledge that the Muffia "deserves credit for bringing so forcefully to our attention the inconsistencies and anomalies [§F1] in [Muffia perception of consistency in] Ptolemy's work." Is this [a] accidental irony, [b] customary incoherence (§F4), or [c] a new brand of suicidally-vicious circle?

[78] Recent Muffia writings (see also [Muffia 1990] *passim* & *DIO 2* ‡2 §H14) bristle with its latest buzzword, "methodology" — almost as much as the *Almajest* bristles with inconsistencies: e.g., *DIO 1.1* ‡8 §C1; Rawlins 1987 n.43. And see *DIO 2.1* ‡3 fn 16. Indeed, Rawlins 1987 p.237 and nn.27, 28, & 30 show that the very type of stationary-point data & math (Neugebauer 1975 p.390), which Ptolemy uncomprehendingly spurns at *Almajest* 9.2, are in truth the basis of all five planet mean motion values & tables of *Almajest* 9.3-4 (§H3). If the "consistency" hoax continues to be promoted by the Muffia, I guess *DIO* will eventually have to take the space to catalog the *Almajest*'s inconsistencies. But, hey: why not just cut it out and save us both the hassle? (The most striking consistencies about the *Almajest* are: [a] the amazing agreement of "observations" with theory, as well as [b] the author's dependence upon plagiarized materials.)

[79] Rather like the continuing challenge faced by professional standup-comics: how to make Nixon's verbal delivery funnier than it already is.

E5 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.[80] E.g., the *Almajest* cites numerous Babylonian eclipse observations. And see Rawlins 1987 n.28 and Rawlins 1991H §A,[81] §D10, & §G5. The main issue is rather: 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[82] that might have led him to gloss over embarrassing inconsistencies in Hipparchus's opinions." Pure fantasy. In truth, Ptolemy notes Hipparchos' every slip in detail and points out his own allegedly superior results. (As Jones 1991H p.105 is well aware.) Indeed, another Muffia work ([Muffia 1990] p.207) comments on *Almajest* 3.1 (the very book of the *Almajest* in which Jones 1991H p.103 suggests Ptolemy avoids exposing Hipparchos' inconsistencies): "Here Ptolemy criticizes Hipparchos as inconsistent." (See fn 78 here on Ptolemy's alleged consistency-fetish. And see *Almajest* 4.11 & *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",[83] that doesn't prove he didn't act Babylonian in some way or at some time. The original (& 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)[84] 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 $94^{d}11^{h}57^{m}$, only a trifle over 3^{m} short of Hipparchos' Spring, $94^{d}1/2$.) Question: what sort of "Editors" would buy this used kiddiecar, without ever reading the fine print? I.e., if the computed Babylonian *Springlength* agrees with Hipparchos' value, then the natural question one would expect a multicelled animal to ask is: what about the *other three*[85] season lengths we may compute from the same scheme? Wellllllllll, Muffiosi

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

[81] A point never faced by the Muffia: if the "Babylonian" month $M_{\rm A}$ (eq. 6) was not taken from Greek astronomy (as DR claims), then how was it determined? (It is accurate to a fraction of a timesec, yet nothing in our records of Babylonian work indicates an ability to perform the sort of precise math that might be needed to make such an accurate determination of the month's length as $M_{\rm A}$.) 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 1^{m} (though getting better), the Aristarchos tropical yearlength is consistent with a monthlength which is within 1^{s} of $M_{\rm A}$ and of reality. (So, whatever month he actually used *had* to be near or — as I believe — equal to $M_{\rm A}$. See fn 9.) 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?

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

[83] Which also suggests to Jones 1991H that even if Hipparchos was using his famous e & A, he might have used "instead of . . . [trig] a simple schematic [Babylonian] function" (Jones 1991H p.103).

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

[85] 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

I never expected to know this datum, which I have long wondered about.[243] It is not a bad value, but, as noted above (§N5), it is not remarkably near the truth, either. The pinnacle of accurate ancient lunar theory came later in antiquity. (See Rawlins 1985G §5 ¶3; also Rawlins 1985K: Venus & Mars.) Note that the errors in true longitude, caused by the ancient apogee-at-epoch A being c.5° in error, will only affect a monthlength estimate (based on eclipses 345 yrs apart: *Almajest* 4.2, Rawlins 1985S) by a fraction of a timesec. Aristarchos' $M_{\rm A}$ (eq. 6) is about this accurate, so the error in his A (recovered here) would not have noticeably affected his admirably correct deduction of $M_{\rm A}$ (Rawlins 1991H fn 1). A final reflection: had Hipparchos not shortcut-faked his eclipse math by adopting (and thus inadvertently preserving for us) Aristarchos' lunar apogee-at-epoch ($A = 96°$), that datum would have disappeared forever. So, since Aristarchos was a far more critical figure (than Hipparchos) in scientific history, Hipparchos' pretense here has the fortunate accidental rôle of being the cobweb-thin thread that now connects us to one of the most important of the hard-earned astronomical parameters of a great & genuine intellectual pioneer. This is among the strangest ironies of a history overflowing with them.

O Ancient Heliocentrists' Adoption of the Astronomical Unit

O1 It now remains only to explain the peculiar numbers which Hipparchos gave for the distance to the Moon, $r_{\rm M}$ (*Almajest* 4.11 & §N10): 3144′ (trio A) and 3122′1/2 (trio B). These values we took as already existing for Hipparchos when he computed eqs. 19 & 20. On the other hand, Toomer 1973 (p.16 & p.27 n.14) claimed that he had "conclusively" & "inevitably" established a very different explanation, from which (for each trio) these $r_{\rm M}$ values emerge during the calculation of e (or r). Toomer 1973 also proposes that, since his theory involves assumption of a relatively crude chord table (suggesting nascent trig), he has cast light into the murky origins of this wonderful mathematical device. His proposal is intelligent and intriguing — and sufficiently attractive that it took me a few days of stubborn testing before I abandoned it. (The chord table theory has a solid parallel, as convincingly shown by Toomer 1973 n.4 & p.24.) But one of this paper's prime bases has utterly collapsed (§G2 & §P1). (That inconvenient trifle does not, however, prevent the paper's unqualified citation — by Toomer 1988 n.44 & Jones 1991M p.443 n.5 — as evidence of Hipparchos' alleged pioneering of primitive early trig tables! We will return to examine this shambles at §P1. Meantime, we simply note that P.Tannery & B.van der Waerden have already persuasively argued against Hipparchos' invention of trig: fn 287.) After naïvely hopeful effort to salvage[244] the ingenious theory of Toomer 1973 (thinking such an outcome would give both of us a pleasant shock), I finally turned to search in other directions and almost instantly (same day: 1991/11/9) hit upon the solution, a crucial and amazingly fortunate relic of ancient heliocentrism's vitality — outside[245] the Hipparchos-Ptolemy succession of geocentrist-astrologers, those useful conduits (note innocent irony at Neugebauer 1975 p.943), whom the kneejerk anti-whiggists of modern Hist.sci consistently

[243] See Rawlins 1991H fn 1, which (implicitly) somewhat overpresumes the need for accurate A when determining accurate M. Whether $A = 96°$ is due to Aristarchos or Apollonios (or both or neither) is not now provable. However, we know that Hipparchos used Aristarchan material, and trio B is from Apollonios' era. Both Aristarchos & Apollonios worked on lunar theory, and the existence of the latter's lunar tables is directly attested: fn 242.

[244] Only by stepping back from the math details can one see an inherent weakness of the Toomer 1973 theory (as against DR's §O3 proposal of a small scribal error) for explaining *Almajest* 4.11's large $r_{\rm M}$ numbers, 3144 & 3122 1/2: these numbers differ by less than 1%, which Toomer 1973 *implicitly regards as just an amazing coincidence*. After abandoning the theory of Toomer 1973, my first realization was this oddity, which led right on to the watershed question: did these numbers perhaps *precede* Hipparchos' math, as against being (as Toomer has supposed) a product thereof. Now that the answer is found, I expect we'll all eventually realize the *a priori* improbability of the idea that *both* e (or r) and $r_{\rm M}$ were products of Hipparchos' math. Obviously, normal procedure would be to set some value for $r_{\rm M}$ at the outset (as Ptolemy himself does at *Almajest* 4.6, where $r_{\rm M} \equiv 60^{p}$), and then express e or r in the units thus established.

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

Thales? Schmales.[238] (Toomer 1988 n.48 says he is preparing a whole book[239] on Hipparchos, which will doubtless be founded upon similarly accurate speculations, posing as the intuitions of "**the** expert"[240] in this area.) Toomer's astounding misperception (perhaps original with the Muffia) of the state of pre-Hipparchan astronomy cannot be blamed on a recent stroke. He's been this smart all along. Toomer 1974D p.137: "I do not believe in the myth of a 'school of astronomy' at Alexandria in the Ptolemaic period [3rd century BC]. We can name some (very few) people who were at Alexandria at various times during that period and who made astronomical observations or calculations. But the notion that this activity was in any sense continuous is without supporting evidence." (Viewpoint loyally echoed at [Muffia 1990] p.6, with credit to Toomer 1978H.) A "few" observing astronomers in the 3rd century BC? If we wish to explore the lower limits of the word "few", let's ask: how many observers can we name from Ptolemy's time? (And: why must Ptolemy's Mercury theory depend so heavily upon observations of the 3rd century BC? — made by a "mythical" group of astronomers that was so organized that it even had its own "Dionysios" calendar: van der Waerden 1984-5 pp.125f.) Muffia disbelief-rejection of high astronomy (in the nonBabylonian world, at any rate) in the 3rd century BC — a fantasy also promoted by O.Gingerich — is one of the more amusing byproducts of worship of hyperhero C.Ptolemy, the Muffia's & OG's "Greatest Astronomer of Antiquity" (*DIO 2.1* ‡3 fn 28, *DIO 1.1* ‡6 §H7 & ‡7 §B2) and is typical of the warp & damage which that reverence has visited upon modern Hist.sci specialists' perspective on the state and evolution (and devolution) of ancient astronomy. (Students of the Darwinist debate will find Toomer's conveniently-selective "missing links" faucet-agnosticism all-too-familiar: similar reasoning infects most Muffia writing on ancient astronomy.) Such comedy is yet another benefit of Muffia-watching: the spectacle of a leading power in contemporary Hist.sci seriously proposing that astronomical predictivity was a novelty for 2nd century BC Greek astronomers! — as if the idea of ephemerides had never occurred to Kallippos,[241] Timocharis, Aristarchos, Aristyllos, Archimedes, Eratosthenes, Apollonios.[242] The complaints (of planet motion untamability) in Plato's *Republic* (7.4:528-530) suggest that Greek attempts at planetary tables went back at least to his time (4th century BC).

N17 The upshot of the foregoing math: we have at last, by the remarkable good fortune of the Hipparchan lunar data's survival (embedded in Ptolemy's criticisms at *Almajest* 4.11), discovered the Aristarchos lunisolar theory's lunar apogee-at-epoch: $A = 96^\circ$.

[238] I believe this faithfully reflects the attitude of Neugebauer 1975 p.604. (However, see A.Aaboe *JHA 3.2*:105, 1972, & the sources cited at his n.3.) There is no question that the ability, to predict the detailed circumstances of a solar eclipse, did not yet exist in the 6th century BC. Our main information from Herodotos (1.74) is implicit: astronomical prediction was valued in Greece as early as his day (5th century BC).

[239] See Muffia-salesman O.Gingerich's pre-publication raview, which perfectly exemplifies the hypercritical attitude which seizes OG whenever he is publicly appraising the academic output of the powerful (*JHA 22.2*:187; 1991/5, same issue as Jones 1991H): Toomer 1988 is "brilliantly concise . . . . Toomer leaves us in a state of enthusiastic expectation, waiting for his promised book on Hipparchus." (See also *DIO 2.1* ‡6 §F1 & fn 18.)

[240] P.Huber to *DIO* 1991/10/1 (*DIO 2.1* ‡2 §H21). Boldface in original. (Huber's work appears regularly in Muffia collections, e.g., Berggren & Goldstein 1987 & Leichty, Ellis, Gerardi 1988.) But Huber omits to comment on Toomer's inventiveness. I am proud to point out that *DIO* cannot be accused of similar neglect: see, e.g., fn 24.

[241] B.Goldstein (Muffioso) even attacks the idea that Kallippos authored a calendar, asserting that his discovery was merely a cycle: B.Goldstein & Bowen 1989 p.285 & fn 190. Compare to Neugebauer 1975 p.627 & *DIO 1.1* ‡5 fn 13.

[242] Lunar tables due to Apollonios (c.200 BC) are cited by Vettius Valens (Neugebauer 1975 p.306 n.1). Toomer 1988 n.43 says that this reference "has been shown by [Jones 1983 p.31] to be due to a manuscript corruption of the name of the much later astronomer Apollinarius." (See §M7.) Consultation of the source (Jones 1983 pp.30-33) reveals not demonstration but speculation "with trepidation" — i.e., a once-explicitly-shaky speculation which has now been transformed (into Toomer's confident assertion), as the Muffia has gotten into aggressive public salesmanship of the grant-generating (fn 266) if magnificently improbable theory that the practice of making predictive astronomical tables was a late Babylonian legacy to the dreamy, previously untabulating Hipparchos-era Greeks! — a legacy suddenly absorbed 2 centuries *after* the Greek conquest of Babylon. . . .

don't exactly volunteer to talk publicly about that part of the deal.[86] [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 1975 pp.627-628 for various Greeks' seasonlengths.) The computed[87] Babylonian seasonlengths are easy to compare to Hipparchos' values (*Almajest* 3.4, R.Newton 1977 p.76):

Season	Babyl (0° VE)	Babyl (10° VE)	Hipparchos
Spring	94^d12^h	94^d12^h	$94^d1/2$
Summer	93^d09^h	92^d17^h	$92^d1/2$
Autumn	88^d14^h	88^d14^h	$88^d1/8$
Winter	88^d19^h	89^d11^h	$90^d1/8$

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 [Muffia 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 smartbuyer like yew seen the latest thing in hypothetical automobiles? — why, this roomy 4-door beauty rightcheer's the "Boobyloonian Unicycle": the world's first ONE tire sedan. Engineern' geenyus! Just imagine the savins in rubber alone. And, ah tellya, that little tire's the best fit since Hoskin saw *DIO*. Dealer & reepair outlets allover: BrownU, Harvard, Yale, Wisconsin, the Pitts, London, Cambridge, Aarhus . . . And if (Godf'bid) deefex popup, them guys come together like in heat. Why, it's better'n a lifetime garntee. Trustme. And all she costs is a few measly grants from here to eternity. Low installments. Eeeeeeasy credit[88] . . . .

confirmation in a 2nd, independent arena. (Even for speculation, e.g., Rawlins 1985G §1: the Pyramids & Karnak.) Some examples here that mark this paper's solutions (of the *Almajest* 4.11 data) as plainly superior to the Muffia's: eq. 23 produces a theory that neatly explains eq. 24 as well; our method (§N11-§N14) of solving for trio A's e (eq. 19) also solves trio B's r (eq. 20). See fn 209.

[86] O.Gingerich (1980/4/22 to DR pp.2-3) tries privately: "Hipparchos would have had difficulty calculating other cardinal phenomena from the Babylonian system because of the discontinuities, a problem not present in the interval from vernal equinox to summer solstice." Both false (for Autumn, which is not interrupted by discontinuity) and absurd: Hipparchos could compute Spring = $90^\circ M_A/(28^\circ 1/8)$ = 94d11h57m, but was defeated by Autumn = $90^\circ M_A/30^\circ$ = 88d14h12m? I.e., the alleged introducer of trig into Greek astronomy, who founded his solar orbit on Babylonian arithmetic (Jones 1991H, prominently published by OG's *JHA*), couldn't do first grade arithmetic?! — which is all that's behind the seasonlengths computed in this section. (More projection?) Couldn't even multiply his adopted month M_A by three to find Autumn?! No comment could possibly do justice to OG's logic. Which is probably why he didn't try putting it over at Gingerich 1980 p.255.

[87] The System A solar motion scheme: Sun moves constantly at 30° per month between longitudes 163° & 357°, and constantly at 28° 1/8 per month for the rest of the year. "Sophisticated" stuff, remember: §E2. (Note: Babylonian Syst A "apogee" = 80°, grossly false — and nowhere near Hipparchos' rather accurate value $A = 65^\circ$. Actual A = 66° 1/2 for epoch −130: *DIO 1.1* ‡6 §C8.) The Muffia computes here with the System B month (eq. 6). In the table at §F1, I give the seasonlengths for both of 2 choices of the VEqx: 0°Ari (Greek VE) & 10°Ari (Bab Syst A's VE). See Neugebauer 1975 pp.371-372, 1317 Fig.8. The Bab-minus-Hipparchos disagreements, for all 4 seasons (starting with accordant Spring): 0h, 21h, 11h, −32h (0° VE); 0h, 5h, 11h, −16h (10° VE).

[88] Fn 144.

F3 By their cohesive harrassment of R.Newton's creative contributions, Neugebauer's clonies hounded Newton to his grave. (The Muffia now has the equally genial Macbeth family's traditional residue[89] problem on its sanguinary hands.)[90] The Muffia has also attempted to kill off DR in the same fashion, by the usual banishment & slander (fn 2, & *DIO 1.1* ‡1 §A8, §C7), permitting no right of reply (e.g., §I13 & *DIO 1.1* ‡1 §A9 & C10). But it has instead merely scotched a Scot.[91] So, there's [Marlovian] justice in R.Newton's intelligence now coming back from the dead to haunt the Muffia, 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 Muffia's amusing superiority-complex, as Newton has elsewhere[92] pointed out) adopts Kugler's speculation (fn 68) as fact: "the summer solstice date given by Hipparchus 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 Hipparchus fabricated his summer solstice on the basis that his [H's] value for the length of spring [94 1/2 days] agrees with the value calculated from Babylonian numerical astronomy? [DR: Precisely this claim is explicitly lodged by Jones 1991H p.118, from Bowen & Goldstein 1988 pp.68-69.] Has he tried calculating the statistical significance of this agreement? When we remember that Hipparchus's solar data [twenty extant in *Almajest* 3.1] **are all rounded to the quarter day**, . . . there is no statistical significance to the agreement.

[89] *Macbeth* 1.7, 2.2, 3.2. See fn 157. (And fn 29. . . .)

[90] *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. Jos. McCarthy writing similarly of another brilliant Johns Hopkins scholar, Owen Lattimore — as if McCarthy himself were just a blameless neutral onlooker-historian. . . .) This confession does RN no good now [note added 1993: *DIO 2.3* ‡6 §E1], nor will equally-sincere OG late-regrets regarding DR (years after he has left this field) cancel decades of Muffia careerist filth. If OG&co are implicitly imagining otherwise, they are as smart and as decent as ever.

[91] *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".)

[92] R.Newton (*DIO 1.1* ‡5 §D14) notes that HamSwerdlow 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 Muffia 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 motions 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 for academic superiority-airs is an enemy's mere ignorance or unfashionability [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*:112, a book reviewer sniffs: "If the generalizations were accurate and based on current scholarship, the book would be useful as a survey . . . . Unfortunately, the generalizations are too often based on outdated prejudices". (Instead of current prejudices?) Likewise at *Isis 83.1*:116-117 (1992). Similarly, Gingerich 1972 rejected the scholarship of R.Newton 1970 by saying that it naïvely accepted "the controversial and now rather outmoded view that Ptolemy adapted Hipparchus' star catalogue by adding $2^\circ 2/3$ to the earlier longitudes." (As one may see from §I1, OG & his fellow Muffies have since expended years of bizarre rationalizations & hundreds of their captive publications' pages, trying to justify the now-permanently-discredited former-orthodoxy behind this hilariously maltimed putdown.) For more on how OG implies nonexpertise, see *DIO 2.1* ‡3 fn 6. As noted, the Muffia has habitually portrayed as incompetent virtually everyone else in the field (see *DIO 1.1* ‡1 §C5); well, since it has itself raised this point so ritualistically, I ask the reader to judge the Muffia's own competence (from the material surveyed in the present *J.HA* paper) in *all* the relevant fields: astronomy, scientific judgement, math, even (curiously) history (fn 116) & translation (§I1) — and philosophy as well (§F4, fn 85). Is it any wonder that kindhearted gov'ts assist the Muffia? — as they would any other deserving charity.

[a] The basis for our induction is a secondary account. [b] The incidents are twofold but otherwise isolated (as far as we know). (Compare items [a] &[b] to Ptolemy's massive frauds: *DIO 1.1* ‡5 fn 18.) [c] Hipparchos' fakes are mathematical, not empirical.
Expanding on the last point: [i] Hipparchos did not (unlike a certain later devotee) fabricate the A1 ϕ to make it fit his A2&A3-based orbit. (This suggests that the main pretense on Hipparchos' part may only have been that he put his name on math work actually carried out by others. This is not the same as faking math, but: it is not a trifle. And his failure later to expose the 1° error, even after jettisoning trio A's e, suggests Hipparchos at least knew of the error but said nothing of it publicly; so he cannot be held unaccountable.)[235] [ii] His *Almajest* 4.11 theories (whatever their faults) *were based upon real observations*. And [iii] he kept trying to improve upon them — which virtue is just what led to the temporary confusion-inconvenience of his having 2 disparate lunar theories. The Muffia fails to understand an obvious related item: if we speak (as, e.g., at [Muffia 1990] p.216) of superior-intellect Ptolemy's preference for Hipparchos' star data over his own, we must ask what the basis for this preference was. Ptolemy's observations? *What* observations? (Same for non-existent Babylonian transit-circle observations for seasonlengths. See §E3.) The rockbottom fact is: *some* parties had to make founding observations in order to have any sort of empirical basis for ancient astronomy. But Ptolemy didn't make outdoor observations. (See, e.g., Rawlins 1985G §10.) That's the Muffia's "Greatest Astronomer of Antiquity" for you (§N16).

N16 And Hipparchos' own self-evident heavy dependence upon previous astronomers' work (revealed by his adoption of their lunar epoch-position values, as just demonstrated: §N11f) is consistent with his being an energetic but far from primary figure in the development of Greek astronomy. This is an apt moment to sample a typically unsupported recent effusion of one of the most academically powerful of the genii[236] that now bestride (politically) the field of ancient astronomy history (Toomer 1988 p.361, implying that Hipparchos' major debts were to Babylonian not Greek predecessors): "I have no doubt whatever that it was Hipparchos who changed Greek astronomy from a largely theoretical to a predictive[237] science. In this his originality and inventiveness are beyond question."

[235] We thus have perhaps 5 cases of questionable ethics here: [1] faked math for trio A, [2] same for trio B, [3] pretended (double) confirmation of previous astronomers' integral ϵ & $g_\circ$ (this sham is depressingly redolent of Ptolemy's M.O.), [4] probable intra-school semi-plagiarism (fn 253), & [5] failure to acknowledge 1° error in trio A after its later (probable) apprehension. These sins are inferior in rank to Ptolemy's; but they are not to be admired, either. The picture emerging here of Hipparchos' school is of a sloppier and more poorly supervised group of astronomers than was my previous impression. (Though, van der Waerden & DR have long held that Hipparchos was not himself a remarkably able mathematician: fn 287 — contra, e.g., Sarton 1959 p.65.)

[236] *DIO* jesteth not. As a MacArthur Fellow, Swerdlow is even certified. One recalls the touchingly hopeful 1979 popular coverage of the inception of the MacArthur Foundation, e.g., *Newsweek* 1979/8/13 p.50: "A Fund for Geniuses." The original MacArthur aim was to help bright & productive unfunded scholars outside the establishment. (E.g., Ted Heckathorn & Keith Pickering would be apt current choices.) Thus, a MacArthur grant to an already-generously-salaried centrist (a prime Muffia mout'piece for the [2nd] Greatest Faker of Antiquity) represents nearly as far & as ironic a fall as possible, from the original MacArthur ideal. The only thing more perverse would be to grant a MacArthur to an establishment hatchetman, who has cemented his exalted social position by defaming the very sorts of persons (e.g., R.Newton) whom the MacArthur grants were initially intended for. Fortunately, Swerdlow has never done anything like that.

[237] Since the very next page of Toomer 1988 agrees with Rawlins 1984A (see §G2 here) that the predictive hokum of astrology was not much of a factor with Greek astronomers before Hipparchos, Toomer's typically insightful conception of ancient astronomy presents a chronology that must inevitably encourage the too-popular notion that astrology's fascination with predictivity helped launch astronomy's. Rawlins 1984A tentatively argued the reverse, mainly on the ground that 3rd century BC Greek astronomers (with no known connection to astrology) did fine orbital work. This is obvious from, e.g., Rawlins 1985S, *DIO 1.1* ‡6 fn 1 & §B11, ‡7 fn 8 — as well as the current paper's finding that Hipparchos' adopted epoch values for both synodic & anomalistic lunar mean motions predate him. Indeed, one of them, $\Delta\epsilon = 310^\circ 1/3$, introduced here at eq. 8, was carried unaltered, 3 centuries later, into Ptolemy (*Almajest* 4.2-4), pseudo-empirically "proved" by him from the usual suitably-fudged observations at *Almajest* 4.6-7. (Note R.Newton 1977 pp.122-123, and here at fn 218 & fn 223.) Ptolemy gets perfect fits to accurate pre-known elements, even while using Hipparchos' PH solar theory, which throws most of the eclipse intervals (for trios A&B) off by ordmag 1 hr! [Note added 1993: See, e.g., Britton 1992 pp.61-67.]

N14 For trio A, we have α & β from eqs. 10 and δ from eq. 13, so these are substituted into eqs. 16 & 17 to obtain U = 5.4253 & V = 12.043. Substituting $r_{\rm M}$ = 3144′ (*Almajest* 4.11, §D1, eq. 23, §N10) into eq. 18, along with the U & V just found, yields:

$$e = 3144'/9.59554 = 327'39'' \doteq 327'2/3 \qquad (19)$$

in accord with the attested e of *Almajest* 4.11 (§D1, §N10). Relative to the needlessly-contended question (e.g., §M7, §N11) as to whether Hipparchos possessed the *Almajest* tables for solar mean longitude & lunar mean elongation (& thus those for lunar mean longitude: §N11), it should be pointed out here that exact — not tabular — calculation in eq. 12 would have produced 173°08′05″, which would have seriously degraded the agreement of the above-deduced e with the attested trio A value 327′2/3, since the outcome of eq. 19 would have been 327′36″ = 327′3/5. For trio B, substituting $r_{\rm M}$ = 3122′1/2 (*Almajest* 4.11, eq. 24) into eq. 18, along with U = 6.2169 & V = −2.30 (computed by substituting eqs. 11 & 15 into eqs. 16 & 17), we have:

$$r = 3122'30''/12.6161 = 247'30'' = 247'1/2 \qquad (20)$$

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

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

A1: $\phi = 084°14' + 4°30' = 088°44' = 088°3/4$
A2: $\phi = 263°59' - 2°15' = 261°44' = 261°3/4$
A3: $\phi = 077°07' - 0°16' = 076°51' = 076°7/8$

(rounding 51′.5− to 7°/8). Computing trio B likewise, using eq. 20 (& §L2's d values):

B1: $\phi = 352°00' + 3°54' = 355°54' = 355°11/12$
B2: $\phi = 180°41' - 4°27' = 176°14' = 176°1/4$
B3: $\phi = 340°28' + 4°17' = 344°45' = 344°3/4$

Most of these six opposition-matches to the corresponding solar positions (trio A §M10, trio B §L3) are as good (i.e., within a few arcmin of exact 180° elongation) as are comparable Sun-Moon longitude matches for the eclipses calculated in the *Almajest*. Except for the A1 lunar longitude (above vs. §M10). (And, of course, the 1° discrepancy in A3: §M3.) The A1 lunar longitude exhibits a serious disagreement with both the stated & computed A1 solar longitude — i.e., with one of the very longitudes which Hipparchos' eclipse calculation was supposed to match (but didn't, because A1 was not used in the calculation: eqs. 12 & 13). The original 1° slip for trio A (§M3, fn 162) probably occurred (from a needless borrowing) during the simple longitudinal subtraction, A3−A2. Note: had Hipparchos genuinely tried to extract (from trio A) all 3 parameters (ϵ, $g_\circ$, e), which Ptolemy & Toomer have previously understood was the case, Hipparchos would probably have noticed the 1° error in this work, since some longitude-comparison is necessary to find ϵ. (*Almajest* 4.6 finds the middle eclipse's mean longitude for 2 other trios, of Ptolemy's choice.) One presumes Hipparchos later noticed A3's 1° discrepancy and A1's 1°/8 mismatch. These problems (and the accidentally not-bad match of unused B3's computed solar & lunar longitudes) may help explain why Hipparchos' later work neglected the trio A-deduced e and instead used (Toomer 1967) the r gleaned from trio B. However, it appears (since Ptolemy mentions no explicit retraction by Hipparchos) that the trio A error was not publicly admitted, and both pretenses (to mathematical exploitation of *all* data of eclipse-trios A & B) were allowed to stand. This constitutes mathematical fraud. Ameliorating factors:

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 Hipparchos solar arguments.[93] (By contrast, this criticism does not at all affect DR's EH orbit proposal, below at §K9 — since the EH orbit is based upon Hipparchos' data, not the reverse.)[94] In brief: the entire argument for a Babylonian base to Hipparchos' 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.

F4 One other point in passing: what sort of philosophy-of-science criteria can have led *JHA* and *Isis* into promoting their Jonestown papers? — papers whose mathematical deductions never come close to gelling into a convincingly coherent picture. (The unsupported Jones 1991H Babylonian-scheme solution 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[95] (p.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.[96] Wasted advice in that direction.

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[93] It is possible that, even after several readings of Newton's point, Muffiosi still won't get it. If so, I shall not here attempt again to disturb their invincible innocence. Of course, one might (ignoring unicyclicity's inherent implausibility) salvage 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 Muffia theory that Hipparchos' Springlength had a Babylonian origin.

[94] I emphasize this because, after the Muffia realizes — to its unspeakable joy — that its least favorite scholar has found the "impossible" Greek solutions to all nine of Hipparchos' *Almajest* 4.11 and 5.3&5 solar positions, this sinuous 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 Muffia's *long*-belovéd causal order Babylon-to-Hipparchos is not merely false for the Sun but reflects hilariously revealing nonrealization of the plain implication of data-precision here. It also shows what the Muffia's vaunted overview-wisdom is actually worth — i.e., how genuinely perceptive is the "expert" judgement, that underlies the entire Muffia 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 Muffia's own chief hero, whatever his downsides, was far superior to no selectivity: the random rubble of Babylonian cuneiform astrology-text caches.)

[95] Jones 1991H p.112 (emph in orig): "The foregoing analysis is, of course, guaranteed to yield *some* division of the ecliptic . . . ."

[96] DR to *Isis* 1980/10/16 p.t2. In the context of inductive-strength criteria, I note the relative prominence *Isis* has given to Rawlins 1982G & Jones 1991M. The former, solving some famous ancient problems with a perfectly coherent fit to attested material, was the last article published, in the last section ("Notes") of its *Isis* issue. By contrast, the incoherent speculations of Jones 1991M were published as the first article in the first section of its *Isis* issue; i.e., it was given all the attention *Isis* could muster for it. But: no complaint here. The astounding implications of Jones 1991M certainly do merit a wider audience than Rawlins 1982G. So, by this special issue of *J.HA*, I'm doing my bit to help out *Isis*' aim in that respect. Incidentally, Rawlins 1982G (*Isis*) remains uncited by Muffiosi, except for the numerically-misaimed (though useful) criticism of it which *Isis* itself published: §I13. This was written by a (delightfully independent) protégé of the *JHA* Editor-for-Life's old friend D.Whiteside (see *AHES 1.3*:179); Whiteside was the 1970 author of article #1 in issue #1 of the *JHA*. (His 1961 *AHES 1.3*:179 paper is actually dedicated to Lord H.) Reviewing R.Newton 1977 in the 1978/11/9 *Nature*, Whiteside's embarrassingly error-studded & amoral reaction to RN's over-"modern" disapproval of crooked science — such trifles as "mere manufacture of evidence" — was to call RN: a "ranting pulpit-thumper". Data-fixing is OK, but "moralising" is an *unforgivable* Hist.sci crime. Yes, this review appeared in *Nature*. And, yes, the word actually *is*: "mere". The uninitiated reader may not realize: such attitudes are pure Hist.sci orthodoxy.

G TrigOut Orgy

G1 Now, it is essential also to realize that our Muffia-*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 astronomy[97] 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 Hipparchan math, which are consistent with the availability to Hipparchos of extremely accurate trig tables: §N14. This lowers the likelihood[99] 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 scorning those benighted nonMuffiosi 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 rôle ([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 Muffia 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 Babyling-Hipparchos scheme's apogee 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 c.300 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 inferiors to copy. E.g., today's astrologers use the orbits published by the US Naval Observatory, not those

[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 Muffiosi will react to DR's discovery: the Muffia 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., Muffia 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 Ptolemist rightly called such behavior "swindling with the very method of science": O.Pedersen 1974 p.23. But up-to-date orthodox Muffiosi now praise it as intelligent, admirable, or even progressive: e.g., Swerdlow 1989 pp.43, [Muffia 1990] p.215. [Note added 1993: Pedersen himself, ever politically-correct, now swears he believes this! — read it for yourself at *Isis 84.3*:558 (1993) p.559. Compare to above & Pedersen 1974 p.258.]

[100] Toomer 1988 p.360: "everything that we know" says so; n.42 scoffs at even *attempting* to show otherwise. (Whenever, as at fn 66 & Neugebauer 1975 p.868, Muffiosi chant the "everything-we-know" mantra, one may translate: we can't prove our assertions, so Muffia superintuition will be invoked instead.) At least I agree with Toomer 1988 p.362 that Greek astronomy was relatively untainted by astrology until Hipparchos' century. See same appraisal at Rawlins 1984A p.979, which also comments that Ptolemy's Intro to his *Tetrabiblos* betrays nonuniversal acceptance of astrology even 3 centuries later. (P.Huber reasons similarly, from the *Almajest* preface: 1991/10/1 to *DIO*.) See fn 237.

N13 Trio A's attested time-interval is (*Almajest* 4.11, §M2): $177^{d}01^{h}2/3$. For entering the *Almajest* 4.4 lunar mean longitude table, this must be broken into 5 months (150^{d}), 27^{d}, 1^{h}, plus 2/3 of 1^{h}; adding the corresponding tabular entries, we have[232] the computed mean longitude interval:

$$\Delta f_{\rm A} = 176^\circ 27' 26'' + 355^\circ 45' 44'' + 0^\circ 32' 56'' + 0^\circ 21' 58'' = 173^\circ 08' 04'' \qquad (12)$$

Thus, since the corresponding attested true longitude interval (*Almajest* 4.11, §M2) is $\Delta\phi_{\rm A}$ = $175^\circ 1/8$ or (as above)[233] $\Delta\phi_{\rm A} = 175^\circ 07'$, we have:

$$\delta_{\rm A} = \Delta\phi_{\rm A} - \Delta f_{\rm A} = 175^\circ 07' - 173^\circ 08' 04'' = 1^\circ 58' 56'' \qquad (13)$$

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

$$\Delta f_{\rm B} = 176^\circ 27' 26'' + 8^\circ 56' 19'' + 3^\circ 17' 39'' = 188^\circ 41' 24'' \qquad (14)$$

The corresponding attested true longitude interval (*Almajest* 4.11, §L1, §N10) is $\Delta\phi_{\rm B}$ = $180^\circ 20'$; thus:

$$\delta_{\rm B} = \Delta\phi_{\rm B} - \Delta f_{\rm B} = 180^\circ 20' - 188^\circ 41' 24'' = -8^\circ 21' 24'' \qquad (15)$$

(Toomer 1973 p.9 has $-8^\circ 22'$.) At this point, instead of finding 2 more δ values for each trio (as in *Almajest* 4.6 & Toomer 1973), we veer off into the Hipparchos shortcut[234] proposed at §N7. Taking

$$U = -[(\cos\alpha + \cos\beta) + \cot\delta(\sin\alpha - \sin\beta)]/2 \qquad (16)$$

and

$$V = \cos(\alpha - \beta) + \cot\delta\sin(\alpha - \beta) \qquad (17)$$

we can find e (or r) immediately from:

$$e = r_{\rm M}/(U + \sqrt{U^2 - V}) \qquad (18)$$

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

[233] The $07'$ ending here is, of course, precisely that of §M10 (restored) or §N10 for the A3−A2 interval.

[234] Mathematicians will quickly see that eqs. 16-18 are based upon [a] the standard angle-sum trig equation of "Ptolemy's Theorem" (*Almajest* 1.10), [b] our eq. 3, and [c] the ability to solve quadratic equations. (Though never sophisticated in math astronomy, even the early Babylonians could handle quadratic — and cubic! — equations: van der Waerden 1963 pp.69-71 or van der Waerden 1978 pp.668-670; van der Waerden 1963 p.71 also shows incidentally that these old Babylonian mathematicians were more adept at such math than O.Neugebauer.) Toomer's lifelong promotion of a wobbly fantasized Hipparchan trig table (§D1), built up merely from the half-angle theorem (thus using a $7^\circ 1/2$ interval), has been based upon Toomer's disbelief that Ptolemy's Theorem existed as early as Hipparchos. (See Toomer 1973 pp.6, 8, 18, Toomer 1984 p.50 n.59.) The high-precision success of the present reconstruction suggests (as does fn 283) that both the theorem and resultant highly accurate trig tables predated Hipparchos. Of course, one may safely predict that an enraged Toomer will attempt to discredit this DR reconstruction's support for the theorem's existence in Hipparchos' day by pretending that everything-we-know (which may be translated as: nothing; fn 100) tells us the theorem didn't exist then. It's hard to draw a circle that's rounder than Muffialogic. (See fn 99.)

N12 For trio A, we use the 3 eqs. just cited, following the tabular method of ancient calculation. (The tabular format was obviously that of *Almajest* 4.4, where the anomalistic tables are almost exactly equal those of Hipparchos, which were based upon eqs. 6 & 7. We again adopt the notation $1^{E} = 365^{d} = 1$ Eg.yr.) For finding g_1 (using the absolute time[227] t_1 of §M9), the tabular addition is as follows: $g_1 = 82^\circ\ (g_\circ) + 92^\circ 14'\ (-72^{E}) + 73^\circ 21'\ (13^{E}) + 326^\circ 37'\ (25^{d}) + 9^\circ 48'\ (18^{h}) + 0^\circ 20'\ (3^{h}/5) = 224^\circ 20'$. Next, from the same tabular method, the anomalistic motion is found for the interval $t_2 - t_1$ ($177^{d}13^{h}3/4$, §M2): $g_2 - g_1 = 159^\circ 45'\ (150^{d}) + 352^\circ 45'\ (27^{d}) + 7^\circ 05'\ (13^{h}) + 0^\circ 25'\ (3^{h}/4)$[228] $= 160^\circ 00'$. Likewise, for the interval $t_3 - t_2$ ($177^{d}01^{h}2/3$, §M2): $g_3 - g_2 = 152^\circ 30'\ (177^{d}) + 1^\circ 05'\ (2^{h}) - 0^\circ 11'\ (-1^{h}/3) = 153^\circ 24'$. Toting up these results, we have: $g_2 = 224^\circ 20' + 160^\circ 00' = 24^\circ 20'$, and $g_3 = 24^\circ 20' + 153^\circ 24' = 177^\circ 44'$.

For trio B, we compute very similarly but not quite identically. (This calculation is generally regarded as having been computed separately from that of trio A: §N3.) In this case, [i] we won't need g_3, and [ii] we express our sums (given the hint at §O3, regarding the trio B computer's predilections) in standard-ancient-rounded fractions of degrees instead of arcmin. Thus (using the §L2-§L3 absolute time[229] $t_1 = 122^{E}\ 345^{d}\ 07^{h}$), we have $g_1 = 82^\circ\ (g_\circ) + 221^\circ 40'\ (108^{E}) + 162^\circ 04'\ (14^{E}) + 351^\circ 27'\ (330^{d}) + 195^\circ 58'\ (15^{d}) + 3^\circ 49'\ (7^{h}) = 296^\circ 58'$ which nearly[230] equals 297°. For the interval $t_2 - t_1$ ($178^{d}1/4$, §L1), the anomalistic motion $g_2 - g_1 = 159^\circ 45'\ (150^{d}) + 5^\circ 49'\ (28^{d}) + 3^\circ 16'\ (6^{h}) = 168^\circ 50'$ or $168^\circ 5/6$. So $g_2 = 297^\circ + 168^\circ 5/6 = 105^\circ 5/6$.

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

$$\beta_{A} = g_2 = 024^\circ 20' \qquad \alpha_{A} = g_3 = 177^\circ 44' \tag{10}$$

For trio B:

$$\beta_{B} = g_1 = 297^\circ 00' \qquad \alpha_{B} = g_2 = 105^\circ 50' \tag{11}$$

The anomalistic differences, $\alpha - \beta$, are: $153^\circ 24'$ (trio A) & $168^\circ 50'$ (trio B). (Toomer 1973 pp.9 & 13 has: $153^\circ 25'$ & $168^\circ 50'$.) We then compute difference δ between the true & mean longitudinal motion over the stated time-interval, for trio A (§M2) & trio B (§L1). Now, each subsequent calculation (of e & r) will be exceedingly sensitive to tiny errors in δ. (This is especially so for trio A, where even a lapse of $1''$ is critical: §N14.) Thus, we will compute the mean longitudinal motion precisely — to the arcsec — from the lunar longitude tables of *Almajest* 4.4. (These tables are merely the precise sum of: the solar tables of *Almajest* 3.1 added to the much older lunar elongation tables of *Almajest* 4.4. The former are precisely based upon Hipparchos' mean solar motion F_{J}: §G10. The latter tables are precisely based upon 360° divided by the standard eq. 6 ancient monthlength used by Hipparchos, M_{A} — which indisputably goes way back (to Aristarchos, DR asserts: §N2). It would thus seem reasonable — to anyone outside the Muffia — to suppose that both tables[231] were available to Hipparchos. A byproduct of the development below is: new confirmatory evidence at §N14 that this idea is not only unshocking but, better yet, true. See contrary arguments at Jones 1991H pp.103, 113: here at §F1 & §G2.)

[227] Phil 1 is Nab 425, thus Nab 366 (§M9) is -59^{E} after epoch Phil 1. (Ancient lunar tables used 18^{E} macro-intervals; see *Almajest* 4.4; thus the split of -59 yrs into intervals of -72 yrs & $+13$ yrs.)

[228] The motion for 3h/4 is almost exactly $24'30''$, very slightly ($0''.2$) under. But my guess (naturally influenced by the resultant perfect match!) is that the ancient computer just quickly reasoned mentally: $33'$ for an hour, so $8'$ for a quarter hour; thus, $25'$ (the difference) for three quarters of an hour. (Had the computer used 14h minus 1h/4, the result would've been $159^\circ 59'$, which would probably have just been rounded anyway to 160°.)

[229] Phil 1 is Nab 425, so Nab 547 (§L2) is 122^{E} after the Phil 1 epoch. (Similarly at fn 227.)

[230] If this (rather conventional ancient) rounding is discarded, then eq. 20 (below) would produce $r = 247'27''$, which would of course be rounded to $247'1/2$; thus, our deduced r will agree with Hipparchos' value (*Almajest* 4.11), regardless.

[231] The realization (1991/11/30) that arcsec-precision tabular calculation produced precisely the e & r of trios A&B provided welcome confirmation of this independently obvious point.

of indoor occultists.) So, why would sophisticated scientists have a Muffiose[101] 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 calendaric-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 366^{d} beaut explicit in his *Isis* paper (Jones 1991M), though in fact Jones 1991M (pp.447, 449, & n.28) unqualifiedly continues his insistence on precisely the Babylonian scheme that contains this precious 366^{d} yearlength discovery. (Muffiosi slander nemesis R.Newton as "Velikovskian": §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 360^{d} 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' 366^{d} solar theory, leaves my dull & meagre findings here quite in the shade. The Jones 1991H theory's net error (Sun slower than reality by $22^{h}1/2$ 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' and Meton's famous S.Solstices! And, from the time of Hipparchos back to trio A is about 240^{y}, 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[102] author just blithely suggests that Hipparchos mightn't have noticed or cared: perhaps (Jones 1991H p.110) "the year length[103] was not a conspicuous element in Hipparchus's 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^{d}. However, even it has an error (Sun fast by 86^{m}/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 Muffia fundamentalists to explain why Hipparchos would adopt such grossly erroneous solar speeds as Jones 1991H theorizes. Note also: the arc-speeds he proposed for trio A (Jones 1991H p.112) and trio B (*op cit* p.114) have no relation[104] to each other. (See fn 209.) Of course, from Neugebauer 1975 (p.601 & Rawlins 1999), we know that all

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

[102] E.g., fn 84.

[103] Of course, the trio A 366 day-year solution is conveniently abandoned (for the year of §K9 here) by MacOccam Jones when trio A's first longitude is reconstructed over the long interval between Hipparchos & trio A: Jones 1991H p.119.

[104] A pre-example for our upcoming review of Hist.sci's Bureau of Double Standards (§I13 & fn 183, and see *DIO 2.1* ‡3 fn 8): when DR found evidence (Rawlins 1982C) for Hipparchan adoption of 2 successive different obliquities, Evans 1987 pp.276-277 n.66 scoffed (in the *JHA*, which now gives Jones 1991H to the world) that such disparate solutions merely indicated unreliability of solution. No matter that DR's deduced 1st Hipparchos obliquity ($23^\circ 55'$) is close to that Ptolemy attests for him (& see fn 195), while the unattested 2nd value ($23^\circ 40'$) has been independently elicited by 4 different scholars from 4 separate ancient data sources (Rawlins 1991H fn 21): Hipparchos' *Comm*, Strabo, Pliny, & the *Almajest* Ancient Star Catalog's north portion. It may be that (in *DIO 1.1* ‡5 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 even 1/10th as gross, to try killing a Muffia-proscribed party: e.g., *DIO 1.1* ‡3 §D3, ‡5 §D13 & fn 12.) Hipparchan 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.[105] 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.

G4 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[106] occurred early on −199/9/12). Finding that calculations from Ptolemy's trig-based *Almajest* 3.2&6 tables (Hipparchos' prime solar orbit = "PH" orbit) produce highly discrepant results for these 2 eclipses, Jones 1991H fatefully concluded that Greek trig-founded solar theory is hopelessly irreconcilable with these two Hipparchos eclipse reports. The underlying "proof"[107] of this alleged irreconcilability:[108] "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 Muffia's customary well-deserved gov't funding,[109] additionally benefitting from the wisdom of review & commentary by various learned Muffiosi & co: A.Bowen, J.Britton,[110] C.Haines, & B.Goldstein. (BG is the perfect-choice scholar who hatcheted R.Newton 1977 in the AAAS's *Science*[111] — and whom *Isis* regards as highly expert in ancient astronomical matters.) When the Muffia & *JHA* Pb-paper buried the idea of a Greek

[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 pp.214-215, who himself evidently copied them from a pre-1925 study (though not uncritically: Toomer 1984 p.215 n.74). Before 1925, almanacs & most scholars (exception: T.v.Oppolzer) used noon-epoch, 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 (fn 24 [note added 1993: & see fn 170]) — should damn (as cranks) scholars who do. (The date-confusion difficulty here reveals an obvious & slightly relevant fact: neither Jonestown nor the Malignant 1 have ever computed real eclipses. Trios A&B or any others. The limit of their experience with eclipses is computation by Ptolemy's highschool-level-math methods.)

[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 Hipparchus's figures. The first part of my argument will therefore be to prove [§G7 here] that the figures 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."

[109] Jones 1991H p.122. Likewise for Jones 1991M (p.441 fn).

[110] In support of the claim (Jones 1991H p.106) that "modern historians [have not] put forward a satisfactory explanation" of the *Almajest* 4.11 solar positions, Jones says the upcoming republication (fn 170) of Britton 1967 contends 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 1.1* ‡6 §H2.) This sounds alot 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] B.Goldstein 1978.

N9 Another note on ancient procedure: if Ptolemy's approach (*Almajest* 4.2 f) is any guide, Hipparchos' math formally used anomalistic motion (eq. 7) rather than apogee motion (fn 217). Thus, recalling that adoption of $\epsilon = 178^\circ$ (eq. 8) produced A nearly equal to 96° (§N5), we see that the corresponding Phil 1 epoch value for mean anomaly $g_\circ$ would have been:

$$g_\circ = \epsilon - A = 178^\circ - 96^\circ = 82^\circ \tag{9}$$

— just as integral a parameter as input ϵ & A, of course. We will now find out if this was indeed Hipparchos' adopted value.

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

Reported Interval	Assumed	Deduced
A3−A2 = 175°07′	e = (327′2/3)/(3144′)	$g_\circ = 81^\circ 59'$
B2−B1 = 180°20′	r = (247′1/2)/(3122′1/2)	$g_\circ = 81^\circ 58'$

N11 The $g_\circ$ found here are strikingly close to each other and to the already-suspected (eq. 9) integral value, $g_\circ = 82^\circ$. (The odds are ordmag 1000-to-1 that both $g_\circ$ would hit by chance so close to the same integral value.) As just noted, the associated Aristarchos-EH $\epsilon = 178^\circ$ (eq. 8) is also integral. So I am proposing (§N7) that Hipparchos adopted these two integral values for ϵ & $g_\circ$ at the outset — and thus he solved only for a single unknown (e or r) from a single interval, for each eclipse trio. Since it is clear[225] that he pretended otherwise, this is a double mathematical hoax.[226] (Comment: Though I have sometimes been critical of Hipparchos — whose scientific acumen was rather overrated by later ancients — neither I nor any other scholar ever previously suspected him of any sort of dishonesty.) As noted, the single-interval math would be relatively easy. Two true longitudes form the given interval. And we could find the corresponding two mean anomalies by computing from eqs. 6, 7, & 9; however, the first step (§N12) of my reconstruction is simpler than this. That reconstruction's ultimate success (§N14) therefore evidences, on the ancient computer's part, a subtle feel for the problem. He understood 2 related points: [a] the anomaly-differences needed to be more accurate than the absolute anomalies; [b] after computing the 1st eclipse's anomaly from the epoch value ($g_\circ$, eq. 9) and his anomaly tables (founded upon eqs. 6 & 7), the remaining required eclipses' anomalies are most easily found differentially (much as at §L3).

modern ones: through decades of incremental adjustment, in response to scrupulously observed & analyzed failures of theory. Thus, the very format of the [*Almajest*], which modern Ptolemists . . . worship above all, is ironically one of its most egregiously phony aspects." (Neugebauer 1957 p.191 rates the *Almajest* "one of the greatest masterpieces of scientific analysis ever written".) Jones 1991M p.445 (emph added) innocently lauds "Hipparchus's use of *small* numbers of carefully selected observations to determine the parameters of his models". Of course, no one knew just *how* small was the actual number of data Hipparchos used — until the present discoveries, starting at §N7. These findings thus dramatically demonstrate (since no real astronomer would compute r from 2 data!) that Hipparchos was not at all a serious contributor to ancient astronomy's development of the lunar theory, e.g., the excellent value of r ($5^p1/4$) preserved in the *Almajest* — which I suspect was that of Aristarchos and-or Apollonios. (Trio B occurred at the very time Apollonios flourished. But we do not possess evidence that Hipparchos used his work, while the connection of Hipparchos to Aristarchos is undeniable. See *Almajest* 3.1; also above at §N2.) Further: given the number of small but nontrivial then-unknown perturbations in the lunar motion, it was not possible in antiquity to mathematically elicit a highly reliable estimate of r from an eclipse triad (no matter if the 3 observations were perfectly accurate), nor could extremely consistent values be obtained even from a string of perfectly observed eclipse triads. (In *Almajest* 4.6 & 4.11, Ptolemy outrageously pretends that all 4 of his adduced triads are consistent, to ordmag *one arcmin*, with the same eq.ctr & thus r: see R.Newton 1977 pp.122-123.) An astronomer with genuine experience in such matters would have known that.

[225] See *Almajest* 4.5 & 11 and Toomer 1984 p.181 n.24.

[226] Some may aver that this charge evidences an overcritical nature in DR. Comment: this is a serious mathematical pretense, which misled numerous scholars (in various ways) for over 2000^y; indeed, the most tragic victim of the imposition is a living scholar, Hipparchos' modern biographer, G.Toomer. Thus, one is justified in plainly calling it fakery (on the part of someone in Hipparchos' school).

N7 Suppose the 2 peculiarities noted at §N5 are related. Realization of the implications of overdetermination here suggests a simple hypothesis which solves both oddities simultaneously: Hipparchos did not use the full trio to solve for 3 elements; instead, he merely, in each case (trio A & trio B), used one interval (between one pair of eclipses) to solve for *one* element: e for trio A and r for trio B. The full 3-unknown calculation for a trio is extremely laborious (see *Almajest* 4.6-8 or, e.g., Toomer 1973 or Pedersen 1974 pp.172f), so Hipparchos took a shortcut, which we will now examine. (If this ploy was that of hirelings, did Hipparchos even know of it? See fn 253.) For reconstructing his shortcut, we assume that he used the intervals A3-A2 and B2-B1. The four retained eclipses[223] — A2, A3, B1, B2 —- will (§N10 & eq. 9) prove to be consistent with the same value of A (or $g_\circ$). Moreover, this value is integral; such rounded parameters are typical of antiquity. (See Rawlins 1985K, which long predated the current findings, and whose speculations are now being confirmed here by repeated neat success: §K10, fn 196, §N4, §N7.) Our reconstruction will culminate swiftly & beautifully below, at §N14.

N8 Pedersen 1974 says (p.174 n.5) that O.Neugebauer suggested (as does Toomer 1973 p.8) Hipparchos as inventor of the eclipse trio method of *Almajest* 4.6. The results of the current paper show otherwise — and leave us with no direct evidence of the method's use before Ptolemy. (Same for equation of time: Jones 1991H n.25. Ptolemy's math at *Almajest* 4.6 is far superior to what we are here revealing to be behind the Hipparchan work preserved at *Almajest* 4.11.) Comments: [a] There is no evidence that Ptolemy originated it (though he seems admirably adept at it). In fact, Hipparchos' mention of *trios* in both cases A&B suggests that the triad technique (*Almajest* 4.6) predates him (thus his pretense to being up to the mathematician-standards of his day). [b] The triad method involves lovely math but is impractical because highly sensitive to small observational errors (fn 218 & §P1). [c] The actual ancient determiner of the *Almajest* lunar elements was much more likely[224] to have accomplished his purpose by examining fits of dozens of eclipses.

[223] [Revised 1997.] The unreduced reports (seasonal hours, apparent time) of the trio B eclipses (Alexandria) are creditably accurate; trio A (observed at Babylon, but dated by Athenian calendar), curiously poor. (The eye can discern a total eclipse's mid-time to ordmag 1 timemin; so the main sources of ancient eclipse times' errors may be: [a] lunar hr-angle via sundial for time, & [b] reporters' roundings.) Comparing to modern calculations, we find equinoctial-time O−C errors for each observed eclipse bound: $-46^{\rm m}$ (A1), $-40^{\rm m}$ (A2), $+10^{\rm m}$ (A3), $-7^{\rm m}$ (B1), $+10^{\rm m}$ (B2), $-10^{\rm m}$ (B3). (Trio A rms error = 0h.6; trio B, merely 0h.2. So, I do not agree with R.Newton 1977 pp.122f & 345 that the trio B raw observational data were fabricated.) Note, at *Almajest* 4.11: most trio A eclipse-times are given in whole hrs; trio B, thirds of hrs. (Halving these figures produces roughly the above-cited rms errors — which should be the case if the data are genuine.) This presumably reflects the (real) greater precision-needs of 200 BC Greek theorists vs. 382 BC Babylonian theorists. And Greek borrowing of the trio A Babylon observations hints that 382 BC Greek astronomical observers were inferior to Babylon's. Ptolemy's fudged reductions (to Alexandria mean time of mid-eclipse) infected the data with serious additional errors: $+11^{\rm m}$ (A1), $+24^{\rm m}$ (A2), $-13^{\rm m}$ (A3), $-6^{\rm m}$ (B1), $+22^{\rm m}$ (B2), $-21^{\rm m}$ (B3). Thus, Ptol−C errors of his *Almajest* 4.11 intervals: $+20^{\rm m}$ (A2−A1), $+12^{\rm m}$ (A3−A2), $+45^{\rm m}$ (B2−B1), $-63^{\rm m}$ (B3−B2). Ptolemy's lunar A and r were well chosen, but his acceptance of the Hipparchos PH lunisolar theory introduced a large annual periodic error (amplitude c.$20^{\rm m}$ after accounting for then-unknown lunar theory annual equation): effect much worse for trio B (Mar-Sep) than for trio A (Jun-Dec). Thus, his intervals' errors (above) **had to be** larger for trio B than for trio A. Such conveniently scaled errors are typical of "The Greatest Astronomer of Antiquity". I believe that the first clear enunciation of this crucial point (in another *Almajest* context) is due to Gingerich 1980 p.262 & Fig.3. (Ptolemy was fudging towards a theory that happened to give nearly correct times for trio A; but for trio B, where his theory happened to be very wrong, the same habit led to disaster. I.e., his improvement of trio A times' accuracy doesn't undo RN's correct conclusion that both trios are fudged.) Bottom lines: [a] Ptolemy pretended that his theories fit outdoor eclipses to ordmag a timemin, though his adopted trio B eclipse time-intervals were off by ordmag an *hour*. [b] Trio B's 3 observed times were (rms) thrice as accurate as trio A's; but, after The-Greatest's massages, trio B's 2 intervals were (rms) thrice as ***in***accurate as trio A's.

[224] This elementary point was never understood by Ptolemy, nor is it grasped by most modern commentators on him — few of whom have experience at fitting orbits to data (a point which needs no further defense after the current paper's revelations). An exception is Muffia-friend P.Huber (1991/10/1 to *DIO*, quoted more fully at *DIO 2.1* ‡2 §H22): "I am pretty sure that the ancient astronomers . . . must have derived their parameters by trial and error from rather inadequate sets of observations." Compare to DR's comments (1986/2/28 to van der Waerden): "A . . . general thought about our remnants of ancient astronomy. Real evolution of empirical astronomical theories has nothing to do with the sort of artificial math we find in the [*Almajest*]. The orbit of a planet is not based on a handful of observations which are then treated by Euclidean postulates in a neat formal manner. No, ancient orbits obviously evolved like

orbit explaining *Almajest* 4.11, it was: a well-attended funeral.

G5 To appreciate the *JHA*'s tough & aggressively insistent (fn 5) refereeing standards, the reader need only consult *DIO 1.1* ‡1 fn 9, fn 11, & fn 25, ‡5 §A, ‡6 fn 15, ‡8 §G. (Though, given the *JHA* Editor-for-Life's personal loathing of parenthetical clutter, the unsightly sentence on Jones 1991H pp.105-106 is inexplicable.) In an unstable world, it's a comfort to watch the stout *JHA* tradition of quality journaldom, carrying on. E.g., had refined Jones 1991H and feral DR's EH orbit analysis fallen simultaneously upon the *JHA* Editor-for-Life's desk, His Lordship's knowing eye would instantly have discerned the former's superiority. After all, throughout, Jones 1991H does not say "trig". The paper always says, very properly: "trigonometry". A journal must have standards.

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

G7 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[112] of 128&127 BC (*Almajest* 5.3&5) — establishing yet another "impossible" feat. This *Journal for 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's numbers. According to Hipparchus's solar model, the Sun reaches its apogee ([$65^\circ 30'$]) 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:
>
> $$\frac{[(]\lambda_1 - 65;30^\circ)}{(t_1 - 67;40\ \text{days})} \approx \frac{65;5^\circ}{66;50\ \text{days}} \approx 0;58,26^\circ/\text{day}$$
>
> $$\frac{(\lambda_2 - 65;30^\circ)}{(t_2 - 67;40\ \text{days})} \approx \frac{27;45^\circ}{28;24\ \text{days}} \approx 0;58,38^\circ/\text{day}$$
>
> $$\frac{(\lambda_3 - 65;30^\circ)}{(t_3 - 67;40\ \text{days})} \approx \frac{35;24^\circ}{38\ \text{days}} \approx 0;55,54^\circ/\text{day}$$
>
> The quotients, which should represent the mean solar daily motion between the apogee and the date of observation, obviously do not behave as they

[112] Jones 1991M p.448 inadvertently cites *Almajest* 4.3&5, when *Almajest* 5.3&5 is meant. An alert referee, familiar with the material, would have known that.

[113] Rawlins 1991H eqs.13, 17-18, 28-31. See also here at fn 60.

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 Muffia'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 the obvious possibility (explicitly suggested[115] by Britton 1967 p.47: our fn 215) 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 complex[116] 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* 4.11 problem.)

G9 To illustrate the reliability-quotient of the work so prominently published by Michael (Univ Cambridge) Hoskin's extremely handsome *Journal for the History of Astronomy* ($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 bungled equations of Jones 1991H p.119 (reproduced above at §G7). An obvious glitch in the data going into these equations was Jones 1991H's reading (§G7) of $67^{d}2/3$ for the (correct) interval, 67°2/3 — which corresponds to $68^{d}2/3$. [A **Gonggggggggggg** formerly at this place has been moved to *DIO 11.2* fn 21, in honor of A.Jones' correctness (vs DR's error) on two *Almajest* planet mean motions.] The correct equations are:

$$\frac{(\lambda_1 - 65;30^\circ)}{(t_1 - 68;40\text{ days})} \approx \frac{63;05^\circ}{65;50\text{ days}} \approx 0;57,30^\circ/\text{day}$$

[114] Jones 1991H n.37 adds that Toomer 1978H 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' −126/7/7 solar longitude (closest of the three *Almajest* 5.3&5 data to a solstice) is obviously the *least* likely (of the 3) to have been observed. In any case, as is self-evident from *Almajest* 5.1 (Toomer 1984 p.219 n.4; & see Włodarczyk 1987 pp.177f), all 3 of these Hipparchos solar longitudes were computed from his solar theory and were then used in setting his armillary astrolabe for the 3 co-reported lunar observations. All 3 data are consistent (to 1′: Rawlins 1991H) with computations from Hipparchos' *independently*-reconstructed UH orbit (§G10). Toomer 1978H 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.

[115] [Note added 1993: The reference here is to pp.38-39 of the 1992 edition of Britton.]

[116] Hist.sci volk traditionally console themselves (when caught at technical foulups) by falling back into a pose of superior Feel (vs. those "unhistorical" scientists) for the broad-historical-picture. Yet, the truth is that Muffiosi's Big-Picture of ancient science is even worse than their computational limitations. (See §C7 & fn 92.) E.g., banning from his mind (and all the journals he can possibly influence) the import of heliocentricity in ancient astronomy is precisely why Toomer wasted a quarter-century (§D1 & §P1) looking vainly for the solution to the numbers so swiftly solved here in eqs. 23 & 24. The professionally-convenient Hist.sci pseudo-surety, that ability to do science is somehow correlated with inability to understand its history, is as durable a myth as the notion that lightning calculators are all "idiot-savants". (Like Gauss?) Such misperceptions (which have an obvious resemblance to homeopathy, & are about as true) thrive for a common reason: limited talents crave solace.

we will use here the intervals of §M2, not §M3.) In order to arrange[219] that lunar & solar positions will be 180° apart at the given mid-eclipse times, I have, using §K10, set (§N10 & fn 237):

$$\epsilon = 178^\circ \qquad \Delta\epsilon \equiv \epsilon - \epsilon_{\text{P}} = 178^\circ - 227^\circ 2/3 = 310^\circ 1/3 \qquad (8)$$

(Note that both 178° and 310°1/3 are merely the *Almajest* 4.2 values transposed to Phil 1.) I believe mean-elongation-at-epoch (or mean-synodic-longitude-at-epoch) $\Delta\epsilon$ = 310°1/3 was Aristarchos'. (See Rawlins 1985K. Since eqs. 23&24 and *Almajest* 3.1 all establish a connection of Hipparchos to Aristarchos' work, I will use Aristarchos' name for the pre-Hipparchos epoch values, from c.300 BC, which we are recovering here; however, one cannot really be sure who was responsible for much of this work: Kallippos 330 BC, Timocharis c.300 BC, Aristarchos 280 BC, Aristyllos c.260 BC — perhaps Apollonios c.200 BC, who is actually credited in antiquity with lunar tables: §M7 & fn 242. Or anonymous.)[220] Any value for ϵ will suffice at this point, though it turns out that, here and below, eq. 8 will provide the ϵ which Hipparchos used. From the intervals (§M2 & §L1), I have solved for the other Hipparchan elements, with the following results:[221]

The trio A solution: A = 96°.136 & e = 6^{p}.403 = (335 1/2)/(3144).
The trio B solution: A = 95°.410 & r = 4^{p}.773 = (248 2/5)/(3122 1/2).

N5 Now, the most striking aspects of these solutions are: [a] The e & r results are not equal to the values cited in *Almajest* 4.11, namely, 6^{p}.253 = (327 2/3)/3144 & 4^{p}.756 = (247 1/2)/(3122 1/2), respectively. [b] Both values for A are around[222] 96°, which is far from the correct value (91°.4) or the *Almajest* value (92°43′) for epoch Phil 1.

N6 In our earlier investigations of Hipparchos' solar work, we were seeking unattested orbital elements that would produce his given intervals within standard ancient rounding precision. But here in the lunar case, we are examining a more precise (and inverse) situation: certain given intervals (§M2 & §L1) are the basis of Hipparchan *Almajest* 4.11 calculations which will produce partly *attested* elements (§D1 item [b]). So our demands on the precision can be higher, which should make our findings more revealing.

[219] This is easy to accomplish, since, when using eclipse intervals (differential data) for searching out Hipparchos' A & e (or r), ϵ cancels out of the problem. (See fn 217.) It may thus be adjusted later, at one's leisure, to ensure 180° elongations at mideclipse times.

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

[221] The lunar anomaly configuration of the eclipses is far more advantageous for solution than the solar anomaly configuration. Thus, the results of analysis are fortunately tight; e.g., one has no long groove (as at fn 205) of possible solutions for the lunar elements.

[222] First noted 1991/11/3.

The times for trio B will be those of §L2, and the times for trio A will be those of §M9. (While computing the foregoing solar places, we ignored fractions of hours. This is not advisable for the Moon, whose mean motion is rapid: 33′/hr sidereal, 30′/hr synodic.)

N2 Each trio establishes 3 equations of condition, from which one may solve for 3 lunar elements: [1] mean-longitude-at-epoch ϵ, [2] apogee-at-epoch A or mean-anomaly-at-epoch $g_\circ$, & [3] the lunar epicycle-radius r (for trio B) or eccentricity e (for trio A). Also required are the mean motions[216] of the Moon (& Sun) and the lunar apogee. The standard ancient length of the synodic month is the admirably accurate "Babylonian" value,

$$M_{\rm A} = 29^{\rm d}31'50''08'''20'''' = 29^{\rm d}.5305941 \qquad (6)$$

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

$$269 \text{ anomalistic months} = 251 \text{ synodic months} \qquad (7)$$

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

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

N4 Since the *Almajest* 4.11 data are actually just (for each eclipse trio) 2 intervals instead of 3 independent data, we may simplify (& make surer) our search by just using these 2 intervals. Moreover, if one uses just the intervals, ϵ cancels[217] out of the equations of condition; thus, we have merely 2 equations for finding 2 unknowns: A and e (or r). (Note: the 1° correction to the solar intervals has no effect here, since the 1° error occurred between[218] the solar and lunar calculations. Thus, to reconstruct Hipparchos' trio A work,

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

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

[218] As a matter of interest: had the 1° slip not occurred in trio A, Hipparchos would have found $e = 5^{\rm p}1/2$ & $A = 89°$, by treating both as unknowns (which he didn't: §N7). Both elements are more accurate than those he actually got (*Almajest* 4.11) for either trio A or trio B. (Averaging these with trio B's results produces elements not far from Ptolemy's.) With some justice, the Muffia has criticized R.Newton (e.g., Swerdlow 1979) for his shaky speculation that the *Almajest* lunar epicycle radius $r = 5^{\rm p}1/4$ came from Hipparchos. Since both Ptolemy's proofs of this value are the usual superneat fabrications, we ought to look elsewhere for its origin. Note: Ptolemy scorns Hipparchos' two discrepant values, but fairminded scholars ought at least to credit Hipparchos with nonfudgery, while instead scorning Ptolemy's own laughably overprecise agreements: $r = 5^{\rm p}13'$ & $5^{\rm p}14'$, both at *Almajest* 4.6. See R.Newton 1977 pp.122-123, which finds that an error of merely $15^{\rm m}$ in 1 eclipse-time would affect deduced r by 9′, and $g_\circ$ by 43′! See fn 237.

$$\frac{(\lambda_2 - 65;30°)}{(t_2 - 68;40 \text{ days})} \approx \frac{27;45°}{29;25 \text{ days}} \approx 0;56,36°/\text{day}$$

$$\frac{(\lambda_3 - 65;30°)}{(t_3 - 68;40 \text{ days})} \approx \frac{35;24°}{37;00 \text{ days}} \approx 0;57,24°/\text{day}$$

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′. **Gonggggggggggg** . . . .
(NCSwerdlow, the Muffia's Capt.Captious, snidely attacks politically-disliked E.Rosen[117] thusly in *Isis 72*:73, p.79: "Even addition and subtraction pose problems.") One sees (as noted at §G8) that the Muffia'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 *boasted* (*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* calendaric foulups 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 image-obsessed dearth of editorial integrity has its compensations. See *DIO 2* ‡1 §M.

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 $1^{\rm p} \equiv 1/60$, and taking ϵ for epoch −127/9/24 Alexandria or Rhodos local apparent noon:

mean-longitude-at-epoch $\epsilon_{\rm U} = 180°1/12$
mean motion $F_{\rm J} = 360°/(365^{\rm d}1/4 - 1^{\rm d}/300)$
apogee $A_{\rm U} = 67°$
eccentricity[120] $e_{\rm U} = 2^{\rm p}1/3$

The 3 Hipparchos solar positions, which this orbit fits (and which Jones 1991H called unfittable), are (*Almajest* 5.3&5): 128°7/12 (−127/8/5 1/4), 37°3/4 (−126/5/2 1/4), 100°9/10 (−126/7/7 2/3). The UH orbit calculations are given at Rawlins 1991H §D9. The match is to 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: see *JHA 21*:206; 1990. (This despite Swerdlow's repeated jugular 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' Muffia has yet to acknowledge that DR has ever contributed anything to the ancient astronomy field. The Muffia is proud of that pristine record. And the Hist.sci community's top journal (*Isis*) has prominently taken part (§I14) in the effectively censorial and *explicitly vindictive* (*DIO 2.1* ‡2 §H16-§H17) strategy it's part of.

[118] However, until the underlying e & A are corrected to equal those of the UH orbit, the speeds will still appear not to gel fully with the data.

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

[120] Indian tables used $e = 2^{\rm p}15'$ (Toomer 1973 p.149, Neugebauer 1975 p.317 n.11), which might be a traditional (fn 197) 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 Hipparchos' −127&−126 data: the History of Science Society put at the back of its small *Newsletter* a brief mention of the publication containing the correct & accurate solution to these *Almajest* 5.3&5 data (& drew no attention to this solution: fn 176) — while almost simultaneously running at the front of its Important Journal (*Isis* — from which DR is effectively banned)[121] an article which [a] denied THE VERY EXISTENCE of these ALREADY-published solutions, and [b] promoted a misconceived *and* mathematically-botched treatment of THE VERY SAME DATA-TRIO — all of this topped off with: [c] the fantasizing author's deliberate-snub[122] noncitation (§I14) of the prominently-published corpus of the correct author (DR). (Once upon a time, the Muffia stood proudly on-guard against citation-failure, implying dishonest scholarship on the part of those hapless scholars who failed to measure up to exacting Muffia standards in this critical[123] department.) The deed is diamond-like in the multiplicity of facets utilized to flash its brilliance to the world. How am I dazzled? In the tradition of a poetperson whose name escapes me, let me count the ways: timing, egregiosity, irony, pretended-expert evaluation, rubberstamp pseudo-refereeing of archon-Browning pseudo-research, political arrogance, technical innocence, ostracism of dissent.[124] One involuntarily marvels — as if at a satanic satire on a religious miracle — and, awestruck, asks: how can Hist.sci archons *ever* top this one? Well, believe me, I've asked that question before — and thus can offer some well-founded voice-of-experience advice: don't bet they can't.

H2 Indeed, the topper could materialize quickly, due to the Muffia's incurable insistence upon its own genius & DR's anathematization. After the present unambiguous exposures, Muffiosi must choose one of several typically slippery options. (Muffies & DR are as one in our confidence that: Hist.sci archons' policing of gangup-misbehavior will have even the camcordered L.A. cops begging for lessons.) These options *cannot* include frank admission along the lines of "Rawlins-is-right". (Despite vaunted Muffia linguistic facility, rumored-but-still-classified testing is said to have found that Muffia lips, attempting to master this excruciatingly painful 14-letter tongue-twister, automatically lock in a mysterious involuntary eternal-stammer paralytic-freezeframe — leaving a facial expression reportedly resembling that of one whose fingernails & heart are being ripped out simultaneously.) DR cannot be right on any fact. In any sphere. After all, such admission might confer a hint of Reputability upon a heretic already decreed otherwise (*DIO 1.1* ‡1 §C7, ‡8) by Infallible

[121] In a 1983/1/28 letter to DR, *Isis* reacted, to the dreary news that both its own referees had recommended publication of a DR submission, by stating that *Isis* wished indefinitely to receive no further DR ancient astronomy contributions *and* that, if the current one were (published & then) attacked, DR would get no reply space: §I13. Question: what are the odds that Jones was treated likewise?

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

[123] See, e.g., §I5, Toomer 1980 p.108, & *DIO 1.1* ‡6 fn 6. The last item exhibits Swerdlow's sarcastic attack on van der Waerden for allegedly shady citation-practice. Curious. The Swerdlow 1989 discussion — e.g., p.30 on planet mean motions (also p.32 on Ptolemy's inferior planet epicycle radii being based 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 1982 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 Swerdlow 1989 p.43, I do not believe that these were mailed to *Isis*.) As long as Swerdlow continues to be honored by archons regardless of his citation-practices, then: why indeed should he bother to start behaving in a way that may be distinguished from what he himself has publicly ridiculed as sleazy scholarship?

[124] The *History of Science Newsletter*'s 1991/7 p.35 blurb on *DIO* concludes by noting (at my telephone request) *DIO*'s dismay at the lack of face-to-eggface debate of a dispute as central as the Ptolemy controversy. Period. (No mention of setting up such a debate.) Who are these Hist.sci people, Martians? Have they no terrestrial potency? Guys, you don't let a remark like that just sit there. *You* have the capacity to arrange such a debate, don't you? So stop talking and start scheduling it. While not inclined to setting rigid conditions, I do now propose (given my long experience with Hist.sci archons' capacity for welshing) that prominent, contiguous publication of position papers by both sides should precede the debate by several months (see *DIO 2.2* §L1-§L4, §L8). A few years ago, *Isis-JHA*'s A.Van Helden was talking of a possible debate, to astronomer Sam Goldstein. But when I then phoned Van Helden about it, he backed off and said that maybe there could be a debate 15^{y} hence! Van Helden's Swerdlow-promoting book (*DIO 1.1* ‡5 fn 7) was published at this time by Swerdlow's Univ Chicago.

compute the trio A solar positions were not randomly distributed. We should re-emphasize that the split is correct, timewise — i.e., the laboriously-computed tables used are for the older solar orbit, EH, while the simple constant epoch-values are from the new solar orbit, PH. (Recognition of such a reasonable chronological pattern lends confirmatory support to the conclusions of this section.) By-product of these researches: we now know that solar trio B was computed before[214] solar trio A.

M9 To check these computations, it helps to start by reconstructing[215] (from the intervals listed at §M3 and from the details given at *Almajest* 4.11) the most likely estimates of Hipparchos' absolute times for the 3 eclipses of trio A.

A1: Nab 366 Thoth [01] 26 $18^{h}3/5$ = −382/12/22-23 $06^{h}3/5$.
A2: Nab 366 Pham [07] 24 $08^{h}1/3$ = −381/06/18-19 $20^{h}1/3$.
A3: Nab 367 Thoth [01] 16 10^{h} = −381/12/12-13 22^{h}.

M10 We next compute solar longitudes, much as previously for trio B (§L3) — but now using the §M9 times (again rounded to the nearest hour), and not with pure EH elements, but instead from the hybrid Frankenstein elements just discussed (found at §M4). From eq. 5, mean longitude (for time since Phil 1 epoch, $-59^{E} + 25^{d}19^{h}$) is found: $f_1 = 17°44'$ $(-72^{E}) + 356°48'$ $(13^{E}) + 24°38'$ $(25^{d}) + 0°47'$ $(19^{h}) + 227°40'$ $(\epsilon) = 267°37'$. The interval between this eclipse (A1) and the next eclipse (A2) is $177^{d}14^{h}$ (§M2). Computing the corresponding mean-motion arc, from F_K-based tabular entries: $f_2 - f_1 = 147°51'$ $(150^{d}) + 26°37'$ $(27^{d}) + 0°34'$ $(14^{h}) = 175°02'$. And, working double-differentially in this simple case: since $t_3 - t_2$ is 12^{h} less than $t_2 - t_1$, we have $f_3 - f_2 = f_2 - f_1 - 0°30'$ $(-12^{h}) = 175°02' - 0°30' = 174°32'$. Thus, summing up: $f_2 = f_1 + 175^{d}02' = 82°39'$, and $f_3 = f_2 + 174°32' = 257°11'$. The true longitudes ϕ_i now follow (as in §L3) from eq. 4:

A1: $\phi_1 = 267°37' + 1°15' = 268°52' = 268°7/8$
A2: $\phi_2 = 082°39' - 0°54' = 081°45' = 081°3/4$
A3: $\phi_3 = 257°11' + 0°41' = 257°52' = 257°7/8$

The longitude intervals A2−A1 and A3−A2 are then 172°53′ & 176°07′, or: 173° − 1°/8 and 176° + 1°/8. These are just the trio A solar longitude intervals we set out (§M3) to track down. Restoring the 1° scribal error (*idem*) for our 2nd interval, we have recovered the attested intervals of *Almajest* 4.11 (§M2), and our precise reconstructed A3−A2 interval will be: 175°07′.

N From Hipparchos' Sham Emerges: Aristarchos' Lunar Apogee

N1 Thus far, we have dealt almost entirely with Hipparchos' solar model. We now turn to his syzygial lunar model. As Toomer 1973 (n.10) & Jones 1991H (nn.20&25) have helpfully & correctly pointed out: the *Almajest* 4.11 times, cited as those used by Hipparchos (in his lunar deductions), lack correction for equation-of-time. (By contrast, Ptolemy rightly uses eq.time for the Moon, though not for other bodies, where the effect is less critical.)

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

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

(and the precise confirmatory implication of §N14), it is delightful to encounter, in a Pb paper of the History of Science Society's *Isis*, the straightfaced, mindblowing Muffia fantasy that Hipparchos & prior Greek astronomers never computed predictive astronomical tables. (Jones 1991M p.446; see here at §E4. So Hipparchos' 600-year eclipse table was *entirely* backwards? He wanted to predict only *past* eclipses? Well, most of the eclipses in the canons of moderns T.v.Oppolzer and J.Meeus are past ones, but neither stops at his present. Have Muffiosi forgotten that astronomy's historical pre-eminence has been built upon its predictive power?! See Schmales at §N16.) Jonestown's (anti-tabular) conception of ancient astronomy is presumably the sort of special Hist.sci insight which uncomprehending modern scientists are forever denied. Do not lose sight of what is certain: if a frontpage paper in *Isis* proposes this idea, it's got to be sober scholarship. Now, a naïve scholar, unfamiliar with Muffia wisdom & determination, might imagine in his innocence that Jones' revolutionary notion founders upon ancient astrologer Vettius Valens' explicit attestation to [predictive] Hipparchan solar tables & lunar tables by Hipparchos' near-contemporary, Apollonios. But these 2 impediments are brushed aside with ease: Jones just implies that Valens' testimonies may be based on a fake *and* an identity-confusion. This speculation is assisted by the fact that only one of a vast catalog of Hipparchos works is extant. (Guess who else takes advantage of paucity of early records — to let fly his psychoanalytic speculations on colliding worlds?) So Jones 1991M (p.446, emph added) points to: "the apparent absence of predictive tables in Hipparchus's [surviving] work. Only one author, the astrologer Vettius Valens (late second century [AD]), speaks of Hipparchian tables for the sun . . . . [possibly] *a later fabrication* based perhaps on Hipparchus's solar theory . . . ." Same suggestion at Jones 1991H (p.102). Note, too, the related & equally-forced (contra §N16) Jones-Muffia assertion (fn 242) that the also-attested lunar tables of Apollonios (c.200 BC) never existed and that the ancient (Valens) reference must be to Apollinarios (an Apollinarios who lived long after Hipparchos), since *the name is similar to Apollonios*. Just drop the "onios" and add "inarios". Well, the Muffia may be right. I can't prove otherwise.[212] But, since Muffiosi routinely slander dissenters from Muffia wisdom (like §G3) as Velikovsky's mental kin (see *idem* and *DIO 1.1* ‡1 §C7, ‡3 §D2), I cannot help recalling Johns Hopkins religion prof W.Albright's deft comment, cited by M.Gardner, who expresses it thusly:[213] "Velikovsky's historical method . . . is on a level with that of the professor who identified 'Moses' with 'Middlebury' by dropping the '-oses' and adding '-iddlebury'." Is it not inspirational to see similarly creative anagramology given top billing as bigleague scholarship, in the *JHA* and in the History of Science Society's *Isis*?

M8 Presumably, Hipparchos used hired calculators. (Which for the lunar analyses — performed long after the solar calculations — suggests consultation of his own school's §M7-cited 600^{y} catalog of mideclipse solar longitudes. This, without availability of — or concern to doublecheck — the data earlier used for the original computations, over several years of work on this catalog. See fn 214.) When these men were originally churning out the trio A solar data, they used the most up-to-date constants (ϵ_{P} & A_{P}) but found it most convenient to use (§M6) the older EH tables to compute mean longitude motion (since epoch) and equation of center. It was noted above at §M6 that the orbital elements used to

not to 600^{y} into the future but to the interval from Nabonassar (747 BC) to about the time of Hipparchos. However, the reader is not reminded that gentle Toomer 1967 p.146 called "absurd" P.Tannery's acceptance that Hipparchos had calculated 600^{y} of eclipses (in either direction). Perhaps we may salvage Toomer's judgement a bit here by supposing (as DR has done, throughout this paper) that Hipparchos' past catalog involved, in many or all cases, merely computing for each eclipse the solar position at an empirically reported time. (Calculating future eclipses required at least a syzygial lunar theory and involved much more work.)

[212] However, a common-sense point here: Valens refers to the Apollonios lunar tables along with those of Sudines & Kidines (both Babylonian), who were much nearer chronologically to Apollonios & Hipparchos than to the particular late Apollinarios whom Jones&Toomer refer to: Neugebauer 1975 pp.262-263, 306, 574, 601 & n.2, 610-612, 666. [Note added 2018. Doubt that Hipparchos' authored celestial tables vanished by 2005: Rawlins 2018C fn 86 item [i].]

[213] Original from *NY Herald Tribune Book Review* 1952/4/20, cited & relayed in M.Gardner *Fads & Fallacies* NYC 1957 ed, p.327.

sources such as N.C.Swerdlow & D.Hughes. (Archons are frequently torn thusly between considerations of: [i] mere honesty, vs. [ii] political correctness. They know that the prime test of archonhood lies in *always* setting matters in proper careerist perspective and thus opting intelligently.) Admitting Muffia error is possible in theory — but gets very sticky in practice when wrongness has been first pointed out by DR. So, let's review *allowable* Muffia options for handling the Jonestown quackmire.

[a] Bluff it out. Insist on the superiority of Jones' mismath over DR's correct math. Same for Toomer's concurrent catastrophe.[125]

[b] Half-pretend that the Muffia thought of DR's math first. (This option has already been tested in the field: see §D4. Sanity-wise, contrary options [a]&[b] remind one of anti-Jewish fanatics' amusingly incoherent treatment of relativity & Einstein; one faction says Einstein's relativity is degenerate Jewish-pseudophysics, while the other admits it's true physics but insists it must have been discovered first by a non-Jew.)

[c] Just proceed as if[126] the Muffia thought of DR's math independently and don't cite DR's publication. (Thoren 1990 p.293 explains a Tycho nonattribution: "class ethics had undoubtedly conditioned Tycho to expect as his due the right to harvest the fruits of his social inferiors." See fn 122 & *DIO 1.1* ‡1 §B2.) The Muffia's appropriation (§I14) of the great Delambre's critical 1819 argument against Ptolemy has also taken route [c] (noncitation). (It should be remembered that Muffiosi excuse plagiarism[127] in their hero: §I1 & fn 154. Thus, it is difficult to see how they could consistently condemn it in historians.)

[d] Say nothing[128] at all, and just keep submitting the same high-quality Muffia work to the same discriminating Hist.sci journals.

[e] Adopt DR's position and pretend otherwise. (This approach is actualized at fn 16.)

[f] Adopt DR's position by an alternate route and then treat his evidence as immaterial or idiotic. Sound like fantasy? Actually, Muffiosi have already justified (fn 127) and even attempted such a resourceful ploy. (Details below: §I1 & §I8.)

[g] Publish a wild speculation (unattested method and-or inferior fit) which the *JHA* can then pretend is a viable alternate explanation of whatever DR has solved. (Jones 1991H vs. Rawlins 1991H provides an example of this.) The details of an earlier test-trial of this ploy are provided here (§H3) since, as a display of 0falsifiability, the incident exceeds anything known this side of parapsychology labs.

H3 Gingerich 1988 extensively puffs K.Moesgaard's delightfully clever (well-written & flawlessly calculated) but unfortunately baseless explanation (Moesgaard 1987 p.43)

[125] See §D1, §D3, fn 116, §O1, fn 252, §P1.

[126] Why worry about chronology, if the prior author is uncitable? E.g., on 1982/6/14, DR gave *Centaurus* ancient astronomy referee K.Moesgaard a detailed least-squares study of ancient star declinations, the main novel result of which was that Aristyllos — who had until then usually been misdated to c.300 BC, making his declination data look like the ancients' worst — actually observed c.260 BC, and thus Aristyllos' declinations are instead probably the ancients' best. In a letter of 1982/7/14, DR asked Moesgaard if *Centaurus* could publish the paper, which was also seen at this time by O.Gingerich, P.Huber, & Lord Hoskin. Instead, without *Centaurus* notifying DR, its Editor, O.Pedersen, who is best-friends with His Lordship, published the same result (in Maeyama 1984): Aristyllos' date = c.260 BC. This even though: [i] the latter's paper was received at *Centaurus* a year later than DR's, & [ii] it is statistically hilarious, e.g., consistently confusing single-datum-error with error-of-mean. Luckily, well before Maeyama 1984 was even submitted, Rawlins 1982G p.263 published the correct new date for Aristyllos: c.260 BC. However, even this (extremely prominent) prior publication has provided no protection from Hist.sci types — they uniformly refuse (see, e.g., [Muffia 1990] p.120) to cite the earlier publication! (Though, non-Hist.sci mathematician van der Waerden has cited DR's 1982 unpublished study.)

[127] See also the *JHA*'s neat establishment of a means whereby powerful Wise figures may be allowed (as DR predicted to OG, 1982/1/15) 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 #3), "the first speculative 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 Boyperson views.) E.g., §H2 item [ii]. Academe's vultures know their business.

[128] *DIO 1.1* ‡1 fn 6: "Watch Neugebauer's clonies 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*.

for the *Almajest* Mars synodic mean motion. (This solution depends upon an unheard-of monthlength & uses an equally unattested & lunar-parallax-degraded method for finding planet-motions.) By contrast, Gingerich 1988 passes off as merely "idiosyncratic" DR's solution for the very same Mars mean motion, but does not make clear to the reader that, while the solution OG prefers is a nonfit, DR's fits[129] precisely [though false: fn 129], and is based on a simple period relation similar (except heliocentrist in format) to that underlying the other *Almajest* planet mean motions. DR's period-relation solutions (using mostly *Almajest*-attested numbers) fit all planets' synodic mean motions (degrees/day) on the nose (§D4 & fn 78). For 3 of the 5 planets, we find in each case that the degrees (numerator) & days (denominator) whose ratio yields DR's perfect fit are attested (right in *Almajest* 9.3) by Ptolemy himself! (Saturn, Venus, & Mercury.) E.g., for Venus, the *Almajest* 9.3 mean motion = 0;36,59,25,53,11,28 degrees/day. DR's solution (§D4), previously specifically *denied* by the math-befuddled Muffia (e.g., Neugebauer 1975 p.157 vs. n.6) is:[130] $1800^\circ/(2919^d 2/3) = 5400^\circ/8759^d$ = 0;36,59,25,53,11,28 degrees/day. Moesgaard's solution: $326592000^\circ/529744391^d$ = 0;36,59,25,52,07,12 degrees/day. (Similarly for all other planets: *DIO 2.1* ‡3 §C3. I urge readers to investigate these matters in detail. You will learn much about Ptolemy's integrity & judgement — and the Muffia's. I must add that it is a credit to Moesgaard's ingenuity that his fits are as remarkably good as they are.) Yet Toomer 1984 pp.671-672 actually proposes that not only this perfect match but the same 50billionth-of-a-degree/day precision for *all three* of these planets (Saturn, Venus, Mercury) could be mere coincidences!! Such an instinct for statistics. This from a Springer-Verlag "Editor", atop BrownU's Hist.of Math Dep't. And Harvard's expert OG backs him: fn 129. (New-Frontiers-in-Plasticity Dep't. The Muffia used to argue[131] that perfect 6-place fits showed Ptolemy got his planet mean motions from observed data, as he consistently stated.[132] But then RN showed that all the Muffia-alleged fits were false. This finding, and DR's flock of perfect 6-place fits, showed that Ptolemy had lied about all the planet mean motions' origins: *DIO 2.1* ‡3 §C3. So the new Muffia position of Gingerich 1988 and NSF-funded Swerdlow 1989 p.30 is: perfect 6-place fits are meaninglessly overexact!) Which explains why environmentalist DR urges gov't support for Muffiosi. Sympathy-wise, it's no different from preserving any other pathetic species of clumsy-but-rare&precious wildlife. Why, if longago gov'ts had looked after the care and feeding of the dodo, it might still be with us.

[129] OG has a deep unstated stake in continuing a coverup here (*DIO 2.1* ‡3 §C), [a] to hide his own prominently published, mathematically-misbegotten solution (Gingerich 1981) of the same material [i.e., his difficulty with simple arithmetic], a deed then compounded by [b] attempting to justify his fateful 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;27,41,40,19,20,58. DR's solution (see Rawlins 1987 p.237 for simple ancestor period-relation): $152145^\circ/329621$ 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 fn 17.) Gingerich's preferred solution (Moesgaard 1987 pp.46-47) is: $349920000^\circ/758089897$ 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]) While tabulating no less than four (6-sexagesimal-place) versions of the Mars mean motion, Gingerich 1988 nowhere provides either DR's or Moesgaard's solution, to permit readers to make the foregoing comparison.
[Note added 2003. On Mars (& Jupiter), Gingerich's caution was ultimately redeemed. (If only by chance: see *DIO 1.1* ‡1 fn 9 & *DIO 11.2* p.30.) Though DR's Mars solution indeed fit perfectly (while those of OG&KM didn't), it was not historically true. In 2003, A.Jones found the valid solution: see below at p.178. However, there is no justification for OG's 1983 suppression of DR's three completely original, then-heretical, and now unquestionably valid solutions (Mercury, Venus, & Saturn), especially since OG's Harvard colleague Toomer was at this very time preparing to publish them without credit (§D4).]

[130] Numerator & denominator given explicitly at *Almajest* 9.3.

[131] Neugebauer 1975 pp.152 & 157, Pedersen 1974 pp.270, 296-297, 308. (Rawlins 1987 n.30.)

[132] *DIO 2.1* ‡3 fn 16.

M4 Solving analytically[207] finds that, for the traditional (§K10) H-P value, $\epsilon_P = 227^\circ 2/3$ (choice justified by matching lunar positions later on here: §N15), and $Y_K = 365^d 1/4$ (§K9), the elements of the precise-fit orbit solution are (fn 205): $e = 3^p 09'$, $A = 64^\circ 1/3$. Thus, taking into account [i] the trio B solution, [ii] Hipparchos' known PH orbit elements, [iii] ancient rounding practice, & [iv] the lunar solution to come (eq. 8, also epoch Phil 1), we induce (after some testing & checking) the solar orbit Hipparchos used for trio A:

$\epsilon_P = 227^\circ 2/3$
mean motion $F_K = 360^\circ/(365^d 1/4)$, adopted from Kallippos
apogee $A_P = 65^\circ$
$e_E = 3^p 1/4$

M5 This presents us with an odd but (as we shall see presently, in §M6) surprisingly informative Frankensteinesque[208] orbit: half[209] of the elements are from EH (§K9); the other half, from PH (§K10)!
M6 But the split is not random. We note a revealing correlation: those elements that are associated with tables (namely, F and e) are EH. Those which are just epoch-constants (ϵ & A) are PH. Simple explanation: the Hipparchan school's calculation of the solar positions of eclipse trio A was done during the transition period when Hipparchos was in the process of adopting his PH orbit.[210] (Contemporary PH epoch −145/9/29 noon = Nab 603 or Pot 1 Thoth 1 noon; Rawlins 1985K.) At this point, he had determined the PH elements (§K10), but had not yet compiled the necessary associated tables. This has to have been around the PH epoch: 146 BC. Thus, by the luck of happening upon transition-period work, we've learned the date of a famous H eclipse calculation. Later (§N3), we'll find that, by the time he performed the associated *Almajest* 4.11 lunar calculations, he'd fully adopted (including contingent, prepared lunisolar tables) the PH solar orbit: §N2. This gives us the essential chronological order of the Hipparchos work described at *Almajest* 4.11.
M7 OK, comic-relief-time again. Thanks to Pliny,[211] it is wellknown that Hipparchos computed 600^y of eclipses. In the context of considering this famous *tabular* monument

[207] Some details are provided at fn 205. The math of orbit-fitting (see, e.g., Rawlins & Hammerton 1973A or Rawlins 1991H) reminds me of a common alibi for Ptolemy, which is as belovéd of the Muffia as it is irrelevant to Ptolemy's pretensions. Muffia loyalist Peter Huber to *DIO*: Ptolemy never heard of Gauss. (See *DIO 2* ‡2 §H6 & §H14.) Well, has the Muffia? It was not DR talent but Muffia innocence — as to how even to *compute* the EH (or UH) problem — that left DR alone at the portals of the ancient intellectual temples we are now discovering & exploring here: the EH & Frankenstein orbits, Aristarchos' lunisolar elements, and ancient use of the 1000^a Astronomical Unit. (My oft-expressed gratitude to the Muffia has never been entirely facetious.)

[208] The pieced-together Frankenstein orbit & A's integrality were discovered 1991/10/31-11/4. Realization that Hipparchos had (for both trios) taken all but 1 of his lunar elements from previous work occurred 1991/11/3-6.

[209] This may at 1st look *ad hoc*, but I recommend 3 counter-considerations: [a] See the chronological-correlation discussions at §M6 & §M8. [b] The trio B monthly speeds of Jones 1991H (pp.114-115, 121) are unattested *and* ludicrously over-precise — down to the *arcsec*: $30^\circ 21' 05''$ & $27^\circ 58' 08''$. By contrast, his trio A speeds are the traditional values, rounded to *degree*/eighths. And elsewhere in the discussion (Jones 1999A pp.113, 120), roundings of $1^\circ/2$ are tossed in at the author's (apparent) whim! (These high-precision trio B speeds have no other basis than Jones' rigging them, in order to fit trio B exactly to his also-unattested equal-arc version of System A.) By contrast, DR's rounding procedures are standard-ancient for all 3 trios discussed (see fn 203), and the EH orbit elements are based on foundation-data which may be easily deduced, independently, from Hipparchos' attested output: §K4. [c] Jones' hypothetical Babylonian schemes both require "modified" parameters (p.104; §F4): two unrelated sets of speeds. (See fn 104. The common factors are merely apogee A & the model: half the circle at one constant speed, another constant speed for the other half.) I.e., the trio A & trio B solutions of Jones 1991H not only lack Babylonian attestation (other than trio A's speeds, which lead to $12.4 \cdot M_A = 366$ day hilarity): they have only one common parameter. (Jones 1991H p.113: "There are reasons for doubting whether Hipparchos's [determinations of trio A e & trio B r] were carried out in a single work.") For contrast: half the 4 elements of DR's trio A solution are shared with DR's trio B solution (§M6), and the other half match the attested PH orbit.

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

[211] Pliny 2.53 called these calculations "predictions". There is a reasonable interpretation by Neugebauer 1975 pp.319-321 & Toomer 1988 p.355, namely, that the Pliny report of Hipparchos' 600^y of calculated eclipses referred

data which are the most reasonable choices we could induce for the young Hipparchos (§K).

L4 But: hold on there! The Editor-for-Life's extremely infallible *JHA*, on the advice of a gaggle of Muffia-circle archons, has decreed (Jones 1991H) that no such trig-based Greek-orbit solution is even *possible* for eclipse trio B: see the irrefutable Muffia analysis quoted above at §G4. (And note: the *JHA* co-Editor most directly responsible for publishing Jones 1991H is one whose name is printed as senior author of the extensive orbit computations published in 1983 as vol.59S of the *Memoirs* of the American Philosophical Society.) So, we must be imagining the foregoing. If Muffiosi, the greatest ancient astronomy experts in the history of the universe (just ask them), say it ain't so, believe them: it ain't so.

M Frankensteinorbit Meets Trio A

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

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

A2−A1: $173^\circ - 1^\circ/8, 177^d 13^h 3/4$
A3−A2: $175^\circ + 1^\circ/8, 177^d 01^h 2/3$

M3 Now, whether we're going Greek or Babylonian here, there is a 1° or 1^d discrepancy in the foregoing (vis à vis a $365^d\,1/4$ calendar), as the slightest testing will show. (See fn 162!) It is precisely by refusing to face this (thus taking the intervals literally) that Jones 1991H (p.112) ends up boxed into the risible conclusion that Hipparchos used a 366^d calendar (§G3). Understand, Jones 1991H is a gov't-funded paper, by a protégé of gov't-funded Muffiosi, experts whom we trust to discriminate between their Received orthodoxy vs. "garbage" (fn 13) produced by "cranks" who extract "money from the government on false pretenses" (*DIO 1.1* ‡3 §D3). To a low thing like DR, who has not yet attained to the lofty plane of Muffia enlightenment, it would instead seem that there is just a 1° error here in the Hipparchos report of one of the intervals, and thus with either eclipse A1 or A3. After some testing, I have opted for the assumption that the problem is with eclipse A3, thus the A3−A2 interval (§M2) must be (as R.Newton already discovered)[206] enhanced by 1°. Adopting this correction, our revised solar interval-data are:

A2−A1: $173^\circ - 1^\circ/8, 177^d 13^h 3/4$
A3−A2: $176^\circ + 1^\circ/8, 177^d 01^h 2/3$

The locus-groove's slope in the x-y plane is: $dy/dx = -3/4 = \tan 143^\circ$. The EH solution is $8'2/3$ distant from the minimum-pt, in the direction 146° — i.e., nearly in-the-groove. A similar situation holds for trio A, where the intervals are $173^\circ - 1^\circ/8$, $176^\circ + 1^\circ/8$, & their sum -11°. The trio A eclipses' radius-vector interrelations are similar to those of trio B; so, in the trio A case, the elliptical groove of valid solutions has about same narrowness (23/3). But the slope $dy/dx = +3 = \tan 72^\circ$. The point on the x-y plane where trio A's S is null is where $e = 3^p09'$ & $A = 64^\circ 1/3$. Our trio A solution (§M4) is $6'1/7$ distant from this point, in direction 71°: i.e., almost precisely in the locus-groove of solutions that satisfy the intervals of §M3 within ordmag $1'$. For trio B, the EH orbit (§K9) corresponds to residual-sum (for all 3 intervals) $S = 8.8$ sq-arcmin; for trio A, the solution of §M4 corresponds to $S = 2.7$ sq-arcmin. This shows that, even without the roundings of §L3 & §M10, the rms of both solutions' non-fits is merely 1-2 arcmin (& even this largely disappears during rounding) — which, given the interpolation-uncertainty noted in fn 204, is fully adequate precision for solving the mystery of the *Almajest* 4.11 intervals.

[206] After completing this entire analysis of trio A, DR noted that R.Newton 1977 p.119 (not cited by Jones 1991H) already — 14^y ago — independently reached exactly the same conclusion [triggered by realization of the insane fn 162 solar speed required by the uncorrected places]: the A3−A2 longitude interval ($175^\circ 1/8$) must be restored to $176^\circ 1/8$. (Except for this groundwork point, my analysis here is not much like RN's.) So Hist.sci's Jonestown disaster could have been averted, had the Muffia not anathematized R.Newton 1977.

I It Is Best To Be Clear About One's Conduct

I1 Now, for an example of evasion-technique [f] of §H2. For decades, Muffiosi's godhead decreed[133] the falsity of charges that Ptolemy plagiarized the Ancient Star Catalog (*Almajest* 7.5-8.1) from Hipparchos. In *Science*, Gingerich 1976 gave wide publicity to the Muffia position (& no reply was permitted). In late 1976, DR sent to *JHA* a very simple, end-of-argument proof (later published in Rawlins 1982C: see §I6 below) that Ptolemy had indeed plagiarized the Catalog from Hipparchos. (Earlier sent to *Science* 1976/11/1, through O.Gingerich's friend, Book Review Editor Kathy Livingston. The only reaction was a unique anonymous 1976/11/12 phonecall from Cambridge, MA, inquiring of my wife — in my absence — regarding my academic background, researches, & projected publications. Harvard's O.Gingerich claims to know absolutely nothing of the incident.) An independent & equally certain proof soon appeared in R.Newton 1977. (See repeatedly failed *JHA*-Muffia attempts to vandalize these proofs: Evans 1987, [Muffia 1990], as well as M.Shevchenko JHA *21*:187, 1990, & J.Włodarczyk JHA *21*:283, 1990. Evans 1987 did not even convince Thoren 1990: see pp.155, 172-174 n.52, 299.) For over a decade, Muffia capo Toomer denied Ptolemy's plagiarism anyway — actually (at Toomer 1984 p.330 n.56) going so far as to forge his *Almajest* 7.3 translation of the key word συναγομεναις ("compiled") to mean "computed", so that his translation agreed with the long-established Muffia position (above, fn 66). (This false translation is still accepted by every Muffie — e.g., [Muffia 1990] — in order to evade the clear *Almajest* 7.3 statement, *by Ptolemy himself*,[134] that Hipparchos compiled an ecliptic-frame star catalog. Toomer's capacity for rejecting even attested truth is also examined here at §P4.) Yet now a whole book ([Muffia 1990]) is published under Toomer's Springer-Verlag "Editorship" (which somehow did not notice the amazing infestation of typos and worse[135] throughout this flimsily-bound[136] *seventy dollar* volume), a book which now contends that, after all, Ptolemy did indeed base much of the Catalog upon Hipparchan observations — despite Ptolemy's contrary *Almajest* 7.4 claims (admitted at [Muffia 1990] p.215). The book's conclusions: [i] Ptolemy is not dishonest. He is brilliant, and his faking & stealing observations shows how "progressive" he was! ([Muffia 1990] pp.4-5, 215-216.) [ii] Newton & DR (who had correctly asserted plagiarism all along) are portrayed as incompetents (*passim*) while the Muffia (who incorrectly denied plagiarism) are regarded as the truly knowledgeable (if not utterly infallible) experts on the matter. Read the book and marvel. (See §I7 & §I8 below.)

I2 Granted that a comic interlude to comedy is unconventional theatre — but, when turning from one Muffia klansman's work to another's, what's to be done? There is a connection between Jones 1991M & [Muffia 1990] which merits attention. At Jones 1991M p.448, it is stated that the 3 Hipparchos lunar observations of *Almajest* 5.3&5 were not (as previously believed) taken for improving his lunar theory but "were associated with *stellar* observations". Not only untrue[137] but obviously untrue. [a] In each case, the elongation is too large (Rawlins 1982C p.373). [b] The first two observations were made in the morning, which was not the best time[138] or Hipparchos' usual time for star-Moon work. [c] For the

[133] See Neugebauer 1957 p.69, Neugebauer 1975 pp.280f; also Pedersen 1974 p.258, quoted at Rawlins 1982C p.362, Rawlins 1987 n.18. Comments at Rawlins 1982C n.3.

[134] [Note added 1993: consult Dave Barry's equally-explicit (& equally-ignored) rats at *DIO 2.3* ‡8 §C25.]

[135] Numerous graphs in [Muffia 1990] bear two curious errors: [a] inversion of axes, [b] scale-error by a factor of two. (We recall Toomer's fussiness about others' editorial failings: fn 264.)

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

[137] The observations' purpose has always been obvious: even to Gingerich 1980 p.256. (And see Rawlins 1991H §E6.) The sole evidence Jones adduces to support [the] star-placing hypothesis is Hipparchos' reference (for 1 of the 3 sights) to the "course" of the Moon in a 248 day table of 9 anomalistic returns (see Jones 1983), as if that established that Hipparchos' prime interest is in lunar speed (as against absolute position). But there is no confirmation of this Muffia speculation.

[138] Shevchenko *JHA 21*:187 (1990) n.11; his judgement redeemed at Rawlins 1991H §G1.

sole evening observation, the Sun (c.37° high) was not near[139] the horizon and would not set for c.$3^h1/4$ — a delay that would cause needlessly inflated uncertainty in estimating the (nearly 4^h) 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: $1^h/2$. (See fn 139.) [d] R.Newton 1982 (pp.64f) appears to be the first to note the significance (§R14) of the fact that 2 of these 3 observations (*Almajest* 5.3&5) were performed when the Moon had virtually null longitudinal parallax — a feature which would have (less than) no value for stellar work (where the Moon is used at 2 different positions: fn 139) but would be ideal for correcting the lunar theory — the very purpose jettisoned by Jones 1991M (p.448)!

I3 From wherever did Jones get the refreshingly original but distinctly bizarre idea that Hipparchos would use the half-Moon (*Almajest* 5.3) for stellar observations? (There are several instantly-obvious objections to such procedure.) The lunar mean elongation for the −127 observation (*Almajest* 5.3)[140] is 264°. And [Muffia 1990] (p.153, in his section 5.4)[141] claims that the average lunar elongation for the observations underlying the Ancient Star Catalog is 250° — promoting (also p.152) the weird idea that Hipparchos cataloged his stars (using the Moon) when they were far from the Sun. This Springer-Verlag book ([Muffia 1990]) was turned out under the impressive "Editorship" of Muffia capo G.Toomer, BrownU's History of Mathematics Dep't. Despite that exalted advantage, [Muffia 1990]'s 250° result is wildly false, being based upon 3 serious snafus:

[a] The curve [Muffia 1990] fits to Catalog star longitude errors ($\Delta\lambda$) is so misplaced (to the right) that the bungle is obvious from the merest glance at the figure. ([Muffia 1990] pp.152-153 Fig.5.20. Note: the abscissa scale on this figure is misprinted by a factor of 2. Same for other figures hereabouts.) If the curve to be fitted is taken to be of the form (Rawlins 1982C p.366, effectively equivalent to [Muffia 1990] p.152)

$$\Delta\lambda = z - G\sin(\lambda - \Theta) \tag{1}$$

(where λ = stellar longitude), then: the curve drawn ([Muffia 1990] p.153 Fig.5.20) has Catalog-stars' longitude-error-curve phase $\Theta_C = 120°$; however, my own least squares fit (of the sinusoid [Muffia 1990] desires: eq. 1), to [Muffia 1990]'s data points, indicates instead: $\Theta_C = 112°$ (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 [Muffia 1990] is: *reversing the sign* of the Ptolemy solar theory's error-curve. [Muffia 1990] neglected to notice that the correctly rendered curve of the Muffia's own Britton 1967 pp.51f, 65f (cited by [Muffia 1990] p.150 n.18 as his source)[142] is calculated-minus-Ptolemy, not the (now standard) reverse sign-convention. [During this research, [Muffia 1990]'s author was a regular visitor at the Princetitute.] For Hipparchos' time, the PH solar theory error-curve phase was about $\Theta_S = 62°$ (Rawlins 1982C pp.370-373),[143] but [Muffia 1990] makes it c.242°. Thus, building on errors [a]&[b], he should arrive at a phase difference of $120° - 242° = 238°$. We know that he temporarily concluded for something very close to this figure, since he speaks ([Muffia 1990] p.153) of fundamental stars rising 4^h 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 the daytime observation near the (sunset) horizon: §I2. For this purpose, the seeming advantage of having low lunar parallax (Moon near meridian & zenith) is illusory: all that matters is the magnitude of parallax-shift between the 2 observations — which is generally maximized (not minimized) by this situation.

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

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

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

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

L3 Next, we compute the solar longitude for each of these times. (Note: all times for computing the Sun's place may be rounded to the nearest hour — though that's not necessary for d_1 here — because the worst possible error caused thereby will be barely 1′.) The process is, effectively: substitute the time-data just given, along with the elements of §K9, into eqs. 2-5. We will start by finding trio B's f_1 from figuring the time d_1 elapsed since epoch Phil 1 = −323/11/12 Alex noon (Toomer 1984 p.11, using $1^E = 365^d$ = 1 Egyptian year): $d_1 = 44875^d7/24 = 122^E\ 345^d\ 07^h$. However, when using eq. 5 to find mean longitude, we may easily forget that ancient computation was tabular, not electronic. And ancient tables (e.g., *Almajest* 3.2) were formed in units of 18^E, 1^E, 30^d, 1^d, & 1^h. Thus, d_1 must be broken into: 108^E, 14^E, 330^d, 15^d, 7^h. Tables founded upon mean motion F_K (§K9) will provide the corresponding arcs, which we add to ϵ (§K9) in order to find trio B's f_1. & ϵ_E of §K9.) The sum: $f_1 = 333°23' (108^E) + 356°33' (14^E) + 325°15' (330^d) + 14°47' (15^d) + 0°17' (7^h) + 228° (\epsilon) = 178°15'$. An experienced ancient mathematician would then save labor (see also §N12) by proceeding henceforth merely differentially: just find the tabular mean-motion arc for the time-interval elapsed from eclipse B1 to eclipse B2, stated (§L1, *Almajest* 4.11) to be 178^d06^h or $150^d + 28^d + 6^h$. Similarly for the interval between eclipses B2&B3. Thus, summing F_K-based tabular entries, we have: $f_2 - f_1 = 147°51' (150^d) + 27°36' (28^d) + 0°15' (6^h) = 175°42'$; and $f_3 - f_2 = 147°51' (150^d) + 25°38' (26^d) + 0°02' (1^h) = 173°31'$. Collecting & summing our results,[202] we have: $f_1 = 178°15'$; $f_2 = f_1 + 175°42' = 353°57'$; $f_3 = f_2 + 173°31' = 167°28'$. From this point, the computation is standard (eq. 4): mean longitude f plus eq-ctr E_E (eq. 3) = true longitude ϕ; we round to the arcmin as we go, and then express the sum rounded according to normal ancient custom:[203]

B1: $\phi_1 = 178°15' - 2°19' = 175°56' = 175°11/12$
B2: $\phi_2 = 353°57' + 2°18' = 356°15' = 356°1/4$
B3: $\phi_3 = 167°28' - 2°40' = 164°48' = 164°4/5$

The longitude intervals B2−B1 and B3−B2 are, then: 180°20′ and 168°33′ — which precisely[204] accord with the stated intervals (*Almajest* 4.11, Jones 1991H p.106 Table 1 last column, or above at §L1). A gratifying outcome, since the orbit used has been founded — independently of the *Almajest* 4.11 Hipparchan intervals[205] — from equinox-solstice solar

[202] Had we proceeded double-differentially here, as at §M10, f_3 would have come out = 167°27′; but this would not have affected the final result at §L3, since $\phi_3 = 164°47'$ anciently-rounds to 164°4/5.

[203] For consistency, same rounding procedure is adopted for these calculations as at Rawlins 1991H §D9 — & later here for trio A (§M10).

[204] The 3 perfect matches are based on the tiny ancient-convention roundings adopted. (Compare to both of Jones' extremes: fn 209 item [b]!) But, even forgetting the last column, the greatest discrepancy is 1′, which is about the uncertainty attendant to interpolation from an ancient table for the equation-of-center (e.g., that of *Almajest* 3.6). I.e., all considered, the agreements found in this paper are a good deal closer than are really necessary to establish the propositions broached. See also fn 205. And I would point out the long odds against the neat (ordmag 1′) explanation of trio B's data by an orbit (EH) founded upon 6h-rounded data! The same remarkable fit occurred (Rawlins 1991H) for the trio of *Almajest* 5.3&5. (The *a priori* odds against these fits remind one of the peculiarity that Ptolemy's four 1h-precision solar fakes are founded upon 6h-precision Hipparchos & Meton data: fn 64.) If one alters merely by an eighth of a day (3h) any one of the EH foundation-times (−157 VE, SS, AE), then the orbit deduced from these will fail to satisfy the trio B intervals by ordmag 0°.1, which would utterly destroy the match.

[205] If one adopts the times of §L2 and the 1st two elements of §K9 ($\epsilon = 228°$ & F_K), and solves analytically for the eccentricity e & apogee A, then: the exact-fit result for the *Almajest* 4.11 trio B intervals (180°20′ & 168°33′) is: $e = 3^p10'$ & $A = 46°1/5$. I.e., using these elements in Greek trig-based orbit calculations will accomplish just what JHA-Muffia wisdom has called The Impossible: reproducing, on the nose, the *Almajest* 4.11 intervals: 180°20′, 168°33′, & their sum −11°07′. The square-residual sum S for all 3 intervals is obviously null for this case. (Of course, only 2 of these 3 intervals are independent data.) Defining (with consistent units) $x = e\cdot\Delta A = e\cdot(A - A_\circ)$ and $y = \Delta e = e - e_\circ$ and mapping S on the x-y plane, we find that the foregoing ideal solution is only trivially better than the EH solution (§K9), contrary to what one might at first suppose. For, the near-parallelness of the 3 eclipses' radius vectors produces (in the x-y plane) a long, gradual-curvature paraboloidal minimum (in S), whose constant-S cross-sections are nearly elliptical, with narrowness (ratio of semi-major-axis a to semi-minor-axis b) ≈ 23/3 or 7 2/3. The major-axis is virtually a locus or long groove of solutions that adequately satisfy the trio B intervals.

K11 The ancients took mean solar anomaly g with respect to the apogee A:

$$g = f - A \quad \text{so} \quad g_{\rm E} = f_{\rm E} - 44^\circ \tag{2}$$

where f = mean longitude. (Throughout the remainder of this paper, A will refer to apogee-at-epoch.)[200] The EH orbit's equation-of-center $E_{\rm E}$ is now determined (see Rawlins 1991H eq.22):

$$E = -\arctan\frac{e\cdot\sin g}{e\cdot\cos g + 1} \quad \text{so} \quad E_{\rm E} = -\arctan\frac{\sin g_{\rm E}}{\cos g_{\rm E} + 240/13} \tag{3}$$

and the true longitude ϕ is:

$$\phi = f + E \quad \text{so} \quad \phi_{\rm E} = f_{\rm E} - \arctan\frac{\sin g_{\rm E}}{\cos g_{\rm E} + 240/13} \tag{4}$$

where

$$f = \epsilon + F\cdot d \quad \text{so} \quad f_{\rm E} = 228^\circ + 360^\circ\cdot(d/365^{\rm d}1/4) \tag{5}$$

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

L Hipparchos' Eclipse Trio B Reveals His Early Solar Orbit

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

B2−B1: $180^\circ 20'$, $178^{\rm d}06^{\rm h}$
B3−B2: $168^\circ 33'$, $176^{\rm d}01^{\rm h}1/3$

L2 First, we find from these two time-intervals (§L1) and from the *Almajest* 4.11 discussion, the absolute times Hipparchos probably used for the 3 eclipses: B1, B2, B3. These times are:

B1: Nab 547 Mesore [12] 16 $07^{\rm h}$ = $-200/09/22$-23 $19^{\rm h}$.
B2: Nab 548 Mechir [06] 09 $13^{\rm h}$ = $-199/03/19$-20 $01^{\rm h}$.
B3: Nab 548 Mesore [12] 05 $14^{\rm h}1/3$ = $-199/09/11$-12 $02^{\rm h}1/3$.

party who altered the original $A = 65^\circ$ to $A = 65^\circ 1/2$. Note: both here-reconstructed A values of Hipparchos (EH & UH) are in whole degrees, which is reasonable, given the looseness of A's empirical determination. (Ptolemy's sometimes amusing overprecision is also evident at *Almajest* 3.1 & 4.7.) I note that the math of Neugebauer 1975 p.58, $\arcsin(2^{\rm p}16'/2^{\rm p}30')$, yields 65°, not $65^\circ 1/2$ as Ptolemy gets from $\arccos(1^{\rm p}02'/2^{\rm p}30')$. Without computationally checking a thing, Neugebauer 1975 p.58 simply forced his result to be the "right" answer, anyway — i.e., Ptolemy's $65^\circ 1/2$. For similar Muffia inventiveness, see fn 38. Muffiosi are unarguably the perfect cult to defend astronomy's most notorious faker.

[200] Hipparchos-Ptolemy's solar apogee is constant, while the lunar apogee moves at a constant speed.

[201] Taken from epoch Phil 1 = $-323/11/12$ noon, the d are (for the -157 orbit-foundation dates): d_1 = 60398d (VE), d_2 = 60493d3/4 (SS), d_3 = 60585d (AE).

east of the Sun in longitude).
[c] But [Muffia 1990] then makes his final blunder and inexplicably says that this difference is not 238° but 250° (*idem*). (Actual stars−PH phase difference is between 30°&40°.)

I4 Now, let's total up the score (so far) for this grand §I3 Muffadventure. Error [a] is about 16°. Error [b] is exactly 180°. Error [c] is about 12°. Net error: about 208°. Impressive. But it is not merely the math (published under the "Editorship" of the star of BrownU's History of Math Dep't) that is wondrous — no, the delicious-irony-pinnacle of the incident is our realization that most of this very math exercise had already been done correctly in a paper (Rawlins 1982C) which is cited at [Muffia 1990] p.167 n.42 and is listed (p.340) in the book's extremely impressive-looking bibliography! (As we saw at §C11, Jones 1991H accomplished a remarkably equivalent feat. How *do* they do it?)[144] In Rawlins 1982C (pp.367, 370), a valid least-squares fit of a sinusoidal function (eq. 1) to the Catalog's zodiacal star longitude errors found error-curve phase $\Theta_{\rm C} = 92^\circ\pm3^\circ$. (A later, differently-weighted solution found $\Theta_{\rm C} = 101^\circ\pm6^\circ$.) And the phase of the solar longitude error-curve for Hipparchos' time was correctly[145] provided as $\Theta_{\rm S} = 62^\circ$ (PH: §I3). For the UH orbit (the actual basis of the Catalog's zodiacal stars), Rawlins 1991H (§F3) found $\Theta_{\rm S} = 71^\circ$. Thus (as noted in Rawlins 1991H §G2), the Catalog-minus-Sun phase difference ($\Theta_{\rm C} - \Theta_{\rm S}$) is roughly 25°: about $1^{\rm h}1/2$ or $2^{\rm h}$, indicating that Hipparchos used[146] the young crescent Moon just after sunset[147] to place his principal stars with his

[144] [Notes added 1992&1993: Just when you finally think Muffiosi math can't get any funnier, those lovable imps pop right up and restore confidence in their irrepressible creativity. In the 1992/2 number of the extremely handsome *Journal for the History of Astronomy*, there is a memorable review of [Muffia 1990] by James Evans (*JHA 23.1*:64). Evans&[Muffia 1990] are the Muffia's top experts on the Ancient Star Catalog, which RN-DR have shown (1976-1991) was faked by Ptolemy. Evans 1987 attacks RN-DR by saying it wasn't faked, while [Muffia 1990] attacks RN-DR by saying it was faked. Which creates a bit of a problem for reviewer Evans — but he manfully overcomes it by: [a] half-saying (p.67) he doesn't agree with [Muffia 1990]'s charge, while [b] half-implying (p.66) he's always somewhat agreed with it. Regarding the Catalog controversy, Evans manages to cite Peters, Knobel, Vogt, Nadal, & Brunet (who did not prove Hipparchos' authorship) — but not RN or DR (who did). (The *JHA*'s lively imagination has evidently convinced it that its botched mass assaults have permanently killed off the RN-DR findings. Right as always, Guv.) After attempting to weaken the central finding of [Muffia 1990], diplomat Evans then looks for some peripheral merit in [Muffia 1990] — and his prime candidate for this honor is a selection which I could not possibly improve upon (Evans 1992 p.67, emph added): "Grasshoff argues, rather plausibly, that the periodic error in longitude in Ptolemy's catalogue derives from the periodic part of the error in the solar theory. This is, I think, a *significant contribution* to our understanding of the star catalogue." Evans then tells us that he thinks [Muffia 1990] hasn't gone far enough — and so Evans offers us his own "further research" into this matter, which has been so significantly "opened" by the [Muffia 1990] analysis. Well, I can only offer the prediction that, for decades to come, sane scholars will be scratching their heads trying to figure out how Evans could have "further-investigated" this *triply*-bungled & upside-down (§I3-§I4) [Muffia 1990] solar-stellar-link research — while never noticing anything amiss with it! (Nor does O.Pederson's *Isis* review; see fn 99.) Moreover, when Evans credits [Muffia 1990] with the "significant contribution" of linking the periodic errors of Hipparchos' Sun and the Catalog's stars, he ignores 2 prior, competently-rendered DR proofs of this very link: Rawlins 1982C p.370 and Rawlins 1991H §F-§G. (Sent to prominent scholars in 1977 and 1986, respectively — well before [Muffia 1990]. Each of the DR papers presents a reasonable and accurately-computed Sun & star error-curve phase-relation, while [Muffia 1990] and thus Evans 1992 both have this relation fantastically wrong. The discovery of this relation is completed by Rawlins 1991H, and the fit is a glovelike success: §I4.) Evans can hardly claim ignorance of Rawlins 1982C since, in the old-reliable *JHA*, Evans 1987 spent 64 pp in a wandering wan-attempt to denigrate other parts of Rawlins 1982C. And so we have yet another (weirdly ironic) case where Hist.sci has given some lucky chap credit for a DR discovery. Shame on *DIO* (Rawlins 1991H fn 4) for calling the Muffia ungenerous. . . .]

[145] Włodarczyk 1987 also provides (his eqs.5-6) correctly-signed formulas for the solar error-curve. [Muffia 1990] (p.336) cites Evans 1987, an article which is immediately followed by Włodarczyk 1987 (on the same subject). If [Muffia 1990] read Evans 1987, how did he miss these formulas?

[146] The astrolabe is described at *Almajest* 5.1; its use for placing stars is explained at *Almajest* 7.2 for a 139/2/23 sunset lunisolar observation of Regulus, though (as noted at Rawlins 1982C p.373 & Rawlins 1991H §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, unrecoverable explanation for Regulus' anomalous error — perhaps connected to Regulus' ancient use as a sidereal marker. Regulus was & is the nearest 1st magnitude star to the ecliptic.)

armillary astrolabe. Final comparison: whereas [Muffia 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).

I5 So, the question suggests itself: did Toomer-"Edited" [Muffia 1990] actually read Rawlins 1982C? — and, if not, how did the paper Rawlins 1982C end up: [a] having its conclusion rejected out of hand, and [b] being cited & listed in the bibliography of [Muffia 1990] (p.340)? Since we have (§I4) just been wondering how [Muffia 1990] read Rawlins 1982C without learning the correct solar-error phase, we may also ask how he managed to mis-spell the name of the journal in which Rawlins 1982C was published. In the footnote ([Muffia 1990] p.167 n.42, which instead mis-spells[148] DR's name)[149] where [Muffia 1990] ashcans the central new crucial experiment of Rawlins 1982C, it is curious that the only papers cited are secondary *prepublication* (1979-1981) discussions of the paper's argument, not Rawlins 1982C itself.[150] But: are we to believe that a $70 Springer-Verlag book, "Edited" by no less than Muffia capo G.Toomer, would produce a partly faked bibliography? Unthinkable. Especially when we recall the immortal words of the Muffia's very own bibliography ethics-monitor, Noel C. Swerdlow, who (falsely)[151] believed he had apprehended a far less serious bibliographical slip by R.Newton. Swerdlow 1979 (p.528): "it is best to be clear about one's conduct, especially" when discussing matters of fraud.

I6 The test (of the Catalog's authorship) invented by Rawlins 1982C is simple: a gross −1°.1 mean longitudinal mis-set (which all parties agree infects Ptolemy's star catalog), of the Catalog-observer's armillary astrolabe, would produce error-waves (amplitude 1°/2) throughout the Catalog's latitudes[152] & northern longitudes. But least-squares investigations

[148] After finding my name spelled "Swerdlow", "Bennett", "Maeyama", "Toomer", or "B.Goldstein" when my results are respectfully cited in Muffia-related professional literature, I assumed that my name would be spelled correctly there only when my work was attacked. But now I can't even count on that correlation. . . .

[149] See §I6. [Muffia 1990]'s German origin might suggest the cause here, except that [Muffia 1990] tends to spell historians' names correctly; but positional astronomers' names buffalo him. This spelling-block places DR in excellent company (not a top Muffia desideratum, I'd previously thought): besides D."Rawlings", there are Simon "Newcombe" (p.270) and E."Wooland" (p.342). (See fn 150.) This is inexplicable, given "Editor" Toomer's intimate familiarity with positional astronomy, as well as his primness about proper usage: fn 264.

[150] It should not have been difficult to obtain Rawlins 1982C. (On 1983/6/4, DR personally presented an offprint to Hist.sci scholar R.Lorch of [Muffia 1990]'s University of Hamburg.) The journal is: *Publications of the Astronomical Society of the Pacific*. But it is often abbreviated *Publ Astr Soc Pacific* or somesuch, which fails to convey the plurality of the 1st word. The [Muffia 1990] bibliography makes that word singular (p.340), an error which he could hardly make had he consulted the article itself. (The journal's title is spelled out in full on the paper's first page.) However, I must admit that this is, in itself, not absolute proof, given the number of other mis-spellings in [Muffia 1990], which somehow eluded "Editor" Toomer's sharp eye: fn 149. (When quoting other scholars, I normally pass *sic*lessly over bad spelling and-or grammar and just silently correct it, since since such complaints are frequently a symptom that a reviewer has been so superficial that he must attack nits. I have started making an exception for Muffiosi&co because: [a] The Muffia has itself repeatedly fussed over such stuff. [b] Certain errors in [Muffia 1990] & Jones 1991H unambiguously reveal the degree of editing-care that was applied to these works.)

[151] See fn 169. [Note added 1992: The cited footnote gives a special edge to OG's ongoing puffery (*JHA 23.2*:149, 1992) of the "meticulous scholarship" of *JHA* Adv.Ed Noel C. Swerdlow. (Question: when did academic reviews begin to Reed like Rex the Wonder Flog, that fay ad-man who's been posing for decades as a cinema critic?) Swerdlow is indeed capable of useful scholarship. See, e.g., fn 280 & *DIO 1.1* ‡6 fn 35. (I have sought his scholarly advice on at least one occasion: *DIO 1.1* ‡3 fn 7.) But OG's syrup reads rather like a suitor's political-favor try at NCS-credibility-restoration, born out of OG's finally-blossoming (long-arrogantly-unrequited) adoration of NCS. (And it's perhaps a much-needed restoration, after the flock of NCS scholarly & slanderly gaffes revealed in our first issue: *DIO 1.1* ‡1 §C7, ‡3 §D3, ‡5 fn 7, fn 20, ‡6 fn 6, fn 35, fn 36. See also here at fn 169 & *DIO 2.1* ‡3 fn 38.) In future, how about some critical balance in reviewing these archons' work? — instead of the boringly predictable & almost insultingly transparent correlation of [a] deepness of kiss bestowed & [b] strength of kissee's political connections (e.g., fn 3). See fn 179.]

[152] For a Ptolemaic −1°.1 longitudinal mis-set, the resultant latitude error wave $\Delta\beta$ will be $29'\cos\lambda$ (Rawlins 1982C Fig.2 & pp.361-362). But the actual cosine wave found in the Catalog's zodiacal latitudes is $(9'\pm 1')\cos\lambda$ (Rawlins 1982C). Evans 1987 p.251 makes it $8'\cos\lambda$, since his latitude error function $\Delta\beta = 0°.31\cos(\lambda + 63°)$ breaks down into $8'\cos\lambda - 17'\sin\lambda$. Nonetheless, Evans 1987 p.252 has the nerve to attempt applying $-17'\sin\lambda$ against $29'\cos\lambda$, blithely noting oh-by-the-way that "the phase is not exactly right". I.e., a phase difference of *sixty-three degrees* equals: "not exactly" in phase! (See the *JHA*'s editorship's near-fainting conniption at another journal's "scandalously" low refereeing standards: fn 5. The *JHA* show is a *bargain* at $126/year.) For Hipparchos'

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

K7 The −145/3/24 Alexandria VE cited at *Almajest* 3.1 is reported to have occurred at 11 AM. The 12^y interval between this date and the −157/3/24 VE is divisible by 4, so (given that the year's length is c.$365^d1/4$) the Alexandria krikos observation of the −157 VE would be made under similar geometric conditions, thus it should have been recorded at roughly the same time (likely c.1 PM). Hipparchos (who always rounded such data calendarically to the nearest $1^d/4$) would express this as noon. Thus, we have reconstructed Hipparchos' other equinox datum (§K4 item [b]): −157/3/24 1/2.

K8 Finally, the interval from −157 back to Kallippos' −329/6/28 1/4 epoch is 172^y, which is also divisible by 4. Thus, it is easy to see that, if Hipparchos used the Kallippic calendaric Summer Solstice (§K4 item [c]), it would have been −157/6/28 1/4, a quite inaccurate time. (See §K4 for Kallippos' calendaric year and epoch.)

K9 From these data, we have: Spring = $95^d3/4$, Summer = $91^d1/4$. It remains now merely to solve for the orbit which satisfies the 3 data of §K6-§K8. The math method is set forth in *Almajest* 3.4. (It is available, in modern dress, at Neugebauer 1975 p.58 or Rawlins 1991H §C6f.) The resulting orbital elements are given below (for Hipparchos' chosen tabular epoch,[196] which was −323/11/12 noon = Nabonassar 425 or Philip 1, Thoth 1 — as is obvious from *Almajest* 3.1). We will henceforth call this orbit the "Early Hipparchos" solar theory or just the EH orbit. (DR started attacking the *Almajest* 4.11 solar orbit problem on 1991/10/27. He is ashamed that it was all the way to 1991/10/29 before he'd zeroed in on the krikos-Kallippos-Hipparchos-based EH orbit.) The EH elements (with $1^p \equiv 1/60$):

mean-longitude-at-epoch $\epsilon_E = 228°$
mean motion $F_K = 360°/(365^d1/4)$, adopted from Kallippos
apogee $A_E = 44°$
eccentricity[197] $e_E = 3^p1/4$

K10 These elements are rounded much as one expects of ancient work (Rawlins 1985K). [Note added 2018. Solst −157 & $365^d1/4$ ***shock-confirmed in 2005***: Rawlins 2018C §F4.] All 4 elements differ from those of the famous Prime Hipparchos orbit[198] (*Almajest* 3.1-7). For the PH orbit:[199]

mean-longitude-at-epoch $\epsilon_P = 227°2/3$
mean motion $F_J = 360°/(365^d1/4 - 1^d/300)$
apogee $A_P = 65°$
eccentricity $e_P = 2^p1/2$

a gnomon for solar altitude, and adoption of a polestars-based geogr latitude L, would explain most of these three AE errors — as well as the obliquity error, if we assume that the gnomon-measured S.Solstice zenith distance was subtracted from this L to find the obliquity. (It appears that adopted L was low by a few arcmin.) Gnomon-use indicates that Hipparchos' early empirical work was subprofessional. The suggestion here is that the first Hipparchan obliquity, 23°55′, was measured by the same party (probably himself) who observed the 162-158 BC equinox data.

[196] DR has long proposed (Rawlins 1985K) Phil 1 as the formal epoch of Hipparchos' solar tables, up until his Ultimate (UH) solar theory (Rawlins 1991H). Phil 1 was also presumably the formal epoch of Aristarchos' ephemerides (eq. 8). Likewise for Kallippos, since his work was published very near Phil 1.

[197] The *Almajest*'s usual rounding of eccentricity is to a quarter of 1^p. (Exceptions: Saturn & the Moon.)

[198] I must point out my own stupid, unexamined temporary-belief, at Rawlins 1991H fn 16, that trios A&B were computed from the PH orbit — a footnote based entirely upon my cursory, overCartonesque glance at a table (R.Newton 1977 p.126) which merely listed Ptolemy's recomputed (not Hipparchos' original) longitudes for these eclipses. (The error is wholly mine, not the late RN's.) Some of the latter part of *DIO 1.1* ‡5 fn 7 isn't worth much, either. (Looking for the positive side: perhaps future historians will appreciate the enormity of the gulf between those superficial notes and the current paper, and how little time elapsed between them.)

[199] Ptolemy computes A to the half-degree from Hipparchos' data and gets 65°1/2. (And *Almajest* 3.4 explicitly states that this was Hipparchos' value.) I don't wish to guess whether: Hipparchos or Ptolemy or either was the

purposes: §K1), during a stable 4^{y} geographical way-station in his career, before he later — presumably sometime not long before the 147 BC Autumn Equinox — settled on Rhodos (for the remaining decades of his life), and started[189] to set up sophisticated instruments there. But, regardless of when it was that Hipparchos commenced making regular outdoor observations, he (a prominent astrologer) would have required possession of a solar theory, in order to compute his extensive eclipse catalog (§M7).

K4 In any case, let us pose a question: if Hipparchos developed a solar orbit early on (before −146), based (in the standard Greek fashion of *Almajest* 3.4) upon VE, SS, & AE observations, then what 3 data would he use? Regardless of our guesses as to his peregrinations, the answers are all easily induced from *Almajest* 3.1, where we learn: [a] Hipparchos' AEs before −146 were explicitly listed by him (*Almajest* 3.1 & §K3). [b] He was a student of the Vernal Equinoxes observed annually on the large bronze krikos in the "Square Stoa" at Alexandria (see Toomer 1984 p.133 n.7). [c] Hipparchos dated all his equinox-solstice observations by the calendar of Kallippos (*Almajest* 3.1) — a calendar in which each year started with a Summer Solstice, an event computed as occurring an integral number of Kallippic years (exactly $365^{d}1/4$ each) after the Kallippic Cycle epoch,[190] Kallippos' accurate (near-syzygial) −329/6/28 Summer Solstice observation,[191] which DR has already earlier independently reconstructed as having been recorded (by Kallippos) for dawn. (See Rawlins 1985H, which notes that this solstice occurred, exceptionally, less than an hour before a New Moon — which made it a particularly ideal[192] choice for the foundation of a luni-solar calendar.) The current paper confirms[193] this Rawlins 1985H reconstruction, on the nose.

K5 Items [a]-[b]-[c] of §K4 are about to supply all 3 of the foundation stones of Hipparchos' longlost earliest solar orbit (§K9).

K6 Before −146, we have (*Almajest* 3.1) only 3 Hipparchos solar observations: the AEs of −161, −158, −157. The −161 datum is discordant, and each of these 3 early data disagrees[194] with each of the later AE set (−146 to −142, *Almajest* 3.1). (The likely cause of the disagreement: the 3 early AEs were probably observed via gnomon.)[195] So, for the

[189] If we assume, from our ignorance of any site for him except Bithynia & Rhodos, that Hipparchos had no other observation-locale, then the 162-158 BC trio were observed in Rhodos. But this leaves unexplained the 11^{y} gap (158-147 BC) between equinox observations.

[190] Muffia capo B.Goldstein, U Pitts Dep't Hist & Philos of Sci, denies (as part of the Muffia's downgrading of pre-Ptolemy Greek astronomy: see fn 241) that Kallippos founded a calendar (a bizarre denial, naturally disconfirmed by this paper's findings), suggesting instead that this 4th century BC Greek's famous 4th-century-epoch calendar (which uses Athenian month-names: *Almajest* 7.3) was perhaps actually invented in *Egypt* in the *3rd* century BC. B.Goldstein & Bowen 1989 (p.279 emph added; see also p.285) delight in the "possibility, *never before considered*, that the dates are really Egyptian *despite the Athenian names*." (In the previous sentence, the superficial notion that the Athenian names are Athenian is called "a move of desperation": see fn 192.) If only Dr.Velikovsky were alive to enjoy & chirp-in on all this.

[191] The date is correctly induced by van der Waerden 1984-5 p.120, acknowledging Fotheringham's priority. Yet B.Goldstein & Bowen 1989 (p.279 & nn.34, 36) scoff at van der Waerden's belief that Kallippos observed the −329 S.Solstice, adducing Neugebauer 1975 pp.617-618 in support — a clear case of these co-authors' Collective Amnesia (see also §F1), since, only 10 pp later, Neugebauer 1975 (p.627) rightly states that Kallippos did indeed observe S.Solstices (see *DIO 1.1* ‡5 fn 13).

[192] Ignoring Rawlins 1985H, Goldstein has no inkling of the specialness of the proximity of the S.Solstice to the New Moon in −329. Though Alexander's regnal year is (since he conquered Persia during Nab 417) universally attested as starting −331/11/14 (Toomer 1984 p.11), B.Goldstein & Bowen 1989 (pp.274, 279, 284) try to convert the nonregnal year −329 into his regnal year anyway! (Velikovsky was also famously gifted in the Revolutionary-Chronology Dep't. See "Worlds in Collusion", in I.Wallach *Hopalong Freud & Other Parodies* Dover ed 1966 p.25.) It was, they plead, "the year in which Alexander assumed the title of Great King." Recall that it is B.Goldstein, of all reputed scholars, who ritualistically resorts to the phrase "move of desperation" (fn 190) whenever he encounters another scholar's inconvenient reality, standing in the way of his desire-fantasies.

[193] §K8, §K11, & §L3.

[194] Discrepancies here refer to comparisons with Kallippic motion F_{K} (§K9), which is an adequate & useful approximation (to the truth) for a few years of data.

[195] The $+12'$ declination mean error ($+12^{h}$ time) of the three 162-158 BC Autumn Equinox data agrees with the $+12'$ error of Hipparchos' early obliquity $23^{\circ}55'$ (Rawlins 1982C & Rawlins 1985G). Note: Hipparchos' use of

do not find these waves. A concurrent alibi-inspiration by O.Gingerich (to evade a different Newton argument) caused OG to suggest that perhaps Ptolemy (insanely) observed the Catalog using[153] Hipparchos' obsolete longitudes (mean 137 AD error: $3^{\circ}3/4$ instead of $1^{\circ}.1$) for his principal stars — and then later Ptolemy computationally added $2^{\circ}2/3$ of precession onto all the thousand-plus stars! Not only inexplicably circuitous & bloop-risking but: the error-waves produced this way would then be over 3 times larger (than for the conventional scenario of Rawlins 1982C). About $1^{\circ}2/3$ 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 [Muffia 1990] p.167 where he states that such an error (which [Muffia 1990] does not even quantify!) is "SO SMALL for both coordinates [longitude λ & latitude β] that it cannot be significantly tested" (caps added). The wellknown (*single*-datum) Catalog rms errors σ_1 ([Muffia 1990] p.80) are about $1^{\circ}/3$; it's in *this* context that [Muffia 1990] claims waves of amplitude $1^{\circ}2/3$ (*five times larger* than σ_1) are too "small" to detect!

[Note added 1993: similar if less egregious NCS [slip] at *J.HA 2.3* ‡8 fn 31.]

[Muffia 1990] adds (n.42) the comment: "We cannot follow contrary claims by Rawlings [*sic*]." Well, when such a master of least squares analysis (§I3) junks a least squares demonstration, without computing a thing: DR may not recover from the disappointment, the authority-approval deprivation, the involuntary chortling. Little wonder Muffia "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 Muffia seems unaware that continued resistance, when proof is certain, leads only to ethical self-evisceration, with long-term costs far more lethal than the short-term face-loss Muffiosi so frantically fear.)

I7 Disoriented readers, perhaps unfamiliar with Muffia fairyland, are urged to recall our earlier account (§I1) of astonishingly elastic Muffiosi flips & springsaults over the Star Catalog issue. It may all come 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 ([Muffia 1990]), [b] publishing this, littered with attacks on R.Newton's & DR's proofs of the *same proposition*, & [c] now suddenly switching criteria and claiming plagiarism isn't wrong! (Not even Ptolemy had the gall[154] to try that one.) [Muffia 1990] can't attack R.Newton often enough. In just

era, Rawlins 1982C p.367 found $\Delta\beta = 9'\cos\lambda - 13'\sin\lambda$. The $9'$ cos-component (explained at Rawlins 1982C fn 16; see also Rawlins 1991H §G4 & Włodarczyk 1987 p.183) is nowhere near the $29'$ needed to exculpate Ptolemy. The sin-component (which, as noted, Evans attempts to mixup with the cos-component!) is obviously due mostly to the young Hipparchos' wellknown obliquity error: *Almajest* 1.12 testifies to his adopted obliquity being near $23^{\circ}51'20''$, which was $9'$ high for that time. Subtracting $9'$ from $13'$ leaves a sin-component-discrepancy of merely $4'$. (DR nulls this by presenting a variety of evidences suggesting that Hipparchos' 1st adopted obliquity was $23^{\circ}55'$: Rawlins 1982C p.368.) While Evans 1987 wanders all over town trying to explain away alleged mysteries of the Catalog, he fails to inform the reader of a simple but lethal truth: adopting the hypothesis that Ptolemy plagiarized the Catalog leaves very little error that even needs explaining away. [Note added 1992/12: The Peters latitude error curve is not a pure sinusoid, as remarked by NCSwerdlow at *JHA 23.3*:176. If one assumes that this is due to the periodic error of the observer's adopted solar theory (with the usual thin waxing crescent stepping-stone Moon: fn 138), then the best-fit periodic error curve would be virtually that for Kallippos' solar orbit. (The effect is small; but, formally at least, it is statistically significant.) (Kallippos' name was transmitted cryptogrammatically — "call us up" — in our naturally-unmet challenge to the Muffia's mathematicians, published at *DIO 2.3* ‡8 §C16. To solve this orbit problem, I was hoping that Muffiosi might at least find brain doubles; but, even selection of these was evidently beyond them.) The original Kallippos solar orbit (from data at Neugebauer 1975 p.627 and Rawlins 1985H), epoch $-329/6/28$ 1/4: $\epsilon = 91^{\circ}$, $Y_{\mathrm{K}} = 365\mathrm{d}1/4$, $e = 2^{\mathrm{p}}$, $A = 59^{\circ}$ or 60°.]

[153] Note the implicit OG admission that Ptolemy's (undeniably ghastly) fundamental astronomy must be strictly derivative — though Ptolemy pretends (*Almajest* 3.1, 7; 7.2-4) to have done firsthand fundamental astronomy. So this defense of Ptolemy against the charge of plagiarism is little more than: lawyer0 pleading his client guilty to a different sort of plagiarism.

[154] Letter to *DIO* from Muffia-associate P.Huber (*DIO 2.1* ‡2 §H13): "Customs can vary widely. Compare for example our current attitudes with regard to copyright and plagiarism to those prevailing among medieval authors and, more close to our days, among Singers of Tales [e.g., A.Lord's book]". DR is repeatedly amused (*DIO 2* ‡2

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[155] 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, prominently disseminating his 1984 attacks on R.Newton. But a 1986/4/4 Toomer letter[156] to Velikovsky-disbeliever L.Ellenberger claims: "I am now[157] utterly disgusted with academic disputes, odium philologicum, and attempts to discredit[158] people with whose views one disagrees." Medical history: 1984, malignant Toomer; 1986, benign Toomer; 1990, malignant Toomer again — in a widely-advertised Springer-Verlag series. (Is schizoid cancer a new frontier in pathology?)

I8 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, Ptolemy's Catalog plagiarism) — while suddenly, belatedly, & oinkily bestowing upon themselves *ALL* of the credit[159] 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 Ptolemy's plagiarizing one of our oldest astronomical heritages (Hipparchos' 1025-star catalog), shows Ptolemy's "methodological progress"!

I9 Evolution in action (fn 72): on the Star Catalog, the original Muffia position #1 was denial (fn 66, §I1) that plagiarism had occurred. Muffia-consensus-alibi #2 was that Ptolemy's fudgery was normal ancient science: see *Sci Amer* 1979/3. When this was disproved at Rawlins 1987 n.12 (& see *DIO 1.1* ‡1 fn 24), the new tack was amnesiac: forget

§H3, §H24) at certain scholars' (neatly selective) receptivity to the notion that condemnation of plagiarism is just a trivial modern affectation. (One doubts these alibi-fonts would yawn-away theft of their own works!) I have called this a temporal brand of (inadvertent) false racism. (E.g., AAS talk 1990/10/22.) For the actual ancient situation, see Pliny's (77 AD) condemnation of plagiarism as "theft" (Rawlins 1982C n.4 or *DIO 1.1* ‡1 §B1); also, Synesios (to Herculian, c.400 AD; A.Fitzgerald 1926 ed, p.238), "it is much greater sacrilege to steal the verses of the dead than to steal their garments, a thing called grave-robbing." (And almost 4 centuries ago, Tycho called Ptolemy's plagiarism "usurpation": *DIO 2.1* ‡4 §C1.) As for alleged higher modern standards (we were discussing *gall*?): [a] The public speeches of every modern US President are read (usually off deliberately-invisible idiot-boards) from prepared written copy of "speechwriters". [b] TV 'snews anchormen operate similarly. (A practice betrayed by their eyes' tiny horizontal oscillations.) [c] The only US person, to whom a US national holiday is dedicated, faked his PhD dissertation by systematic plagiarism. (Detailed textual comparisons: *Wall Str J* 1990/11/9, *NYT* & *Wash Post* 1990/11/10. Memory-Holed since.) The degree & holiday still stand. Thus, I see no basis for ritualistically claiming that condemning Ptolemy's plagiarism betrays an anachronistic ethic (fn 36 & *DIO 1.1* ‡7 §G3). Unless the point is: *moderns* don't care about honesty, which is why they fake such ludicrous excuses for dishonesty.

[155] In the Small-Understanding dep't: [Muffia 1990] repeatedly speaks (pp.85, 88, & 162) of R.Newton's critical fractional-degrees argument as presuming that the Catalog observer's astrolabe ring was graduated in half-degrees — and bases one key counterargument ([Muffia 1990] p.163 item ii) on this understanding of RN. But the truth is that R.Newton 1977 merely considers this option (hypothetically) for a moment (p.246), before presenting (p.247) evidence of whole-degree division, which he then adopts in all his discussions: R.Newton 1977 (pp.247, 252, 255) and (cited at [Muffia 1990] p.167 n.42) R.Newton 1979F. After composing 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 R.Newton is a repulsively unreliable scholar). Springer-Verlag, these are your experts.

[156] Revealing contrast: 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 fn 7.

[157] And Macbeth tired of killing. Once it had placed him upon his (uneasy) throne. See §F3.

[158] E.g., Toomer 1975 (quoted at fn 269). 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* ‡3 fn 3.

[159] 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. Sad. He's a scholar of truly admirable talents and accomplishments, yet infected with that familiar (Neugebauerian) arrogant cliquishness."

K Old Turkey: the Mystery of Hipparchos' Roots

K1 We have no direct biographical note indicating when Hipparchos of Nicaea, Bithynia (now northwest Turkey), moved to Rhodos, which is where his extant outdoor[184] celestial observations were made. But we have several clues regarding this and other aspects of Hipparchos' pre-Rhodos career. Before his serious observing began at Rhodos in -146, the only Hipparchos celestial observations cited by Ptolemy are a much-earlier threesome of Autumn Equinoxes -161, -158, -157 (*Almajest* 3.1). One immediate oddity here: why no matching Vernal Equinoxes? Possible explanation (other than better Autumn weather): throughout this era, the AE happened (by chance) to be so close to Thoth 1 of the Egyptian calendar (Rawlins 1991H fn 7) that the AE was naturally of special calendaric interest. A far more striking peculiarity: since Hipparchos was the prime source of Ptolemy's klimata-based latitudes L (*Geogr Dir* 1.4.2, Rawlins 1985G p.261), it is remarkable that the L values for Hipparchos' native Byzantion-Bithynia region are off by about $+1^\circ 1/2$ to $+2^\circ$. These are huge errors, and they are impossible to make even with the common crude gnomon. (This instrument consistently misleads[185] the observer, but only by about $-1^\circ/4$ in deduced latitude.) The gnomon was most probably (fn 195) the instrument used to record the systematically late 162-158 BC Autumn Equinoxes reported by Hipparchos. (Each of these equinoxes was late by a fraction of a day. But an observer whose latitude was off by 2° would be expected to record an equinox wrong by c.5^d.) Therefore: these 162-158 BC A.Equinoxes were not observed in Bithynia. (Otherwise, Hipparchos would have gotten the L there correct within about $1^\circ/4$, just like the equinoxes.)

K2 Since 1979, DR has proposed that Ptolemy's amazingly large $+2^\circ$ error in the L of Byzantion (modern Istanbul) was initiated by Hipparchos' use of a primitive solstitial ortive amplitude of $\text{atn}(2/3) = 33^\circ 41'$. Exactly this value is anciently attested for Byzantion (Neugebauer 1975 p.983); and, by ancient sph trig formulas,[186] this would lead to longest-day $M = 15^h 1/4$ and $L = \text{c}.43^\circ$ (*Geogr Dir* 8.11.7, 3.11.5 & *Almajest* 2.6, resp). (Actual Byzantion $L = 41^\circ 01'$.) Hipparchos' nearby native city, Nicaea (modern Iznik), is listed with $M = 15^h 1/8$ & $L = 41^\circ 11/12$ (*Geogr Dir* 8.17.7 & 5.1.14, resp; Rawlins 1985G p.262). (Actual Nicaea $L = 40^\circ 26'$; error $+1^\circ 29'$.) These considerations suggest that Hipparchos never advanced beyond horizontal astronomical observations until after leaving Bithynia. And this almost has to be the case, since the slightest vertical-instrument celestial work by Hipparchos would have revealed his 2° error in L.

K3 As for the 162-158 BC equinox trio (so inconsistent with this §K2 error): Muffiosi doubt[187] that this trio was observed by Hipparchos. However,

[a] Hipparchos says (*Almajest* 3.1) that they were observed with great care.[188]

[b] They are uncited to another party.

So, I will suppose (very uncertainly) that they are his own observations, made with a gnomon — before he moved up to a transit circle (used for the equinox observations made from 147 BC to 128 BC: *Almajest* 3.1). If the 162-158 data are his, then (§K1) Hipparchos had left Bithynia by 162 BC. Given the long, data-bare 11^y break between 158 BC & 147 BC, one may speculate that the 162-158 BC trio was taken by Hipparchos (mainly for calendaric

[184] Neugebauer 1975 p.275 speaks of Hipparchos' observations in Bithynia (Heiberg 1907 p.67), but these are mere weather astrology. They are not evidence that Hipparchos made any serious astronomical observations in Bithynia. See Neugebauer 1975 pp.277 & 928.

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

[186] Neugebauer 1975 p.37 eqs. 4a, 5a, and-or 6. For probable Bithynian interest in ortive amplitudes at about Hipparchos' time, see *ibid* p.766.

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

[188] The remarkable classicist-turned-astronomer J.Fotheringham stresses this hint. And Neugebauer 1975 (p.276 n.20) sneers at him for doing so.

J6 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.)

J7 A final comment on the Jonestown affair: *Isis* has published (§I13) 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 & §I13.

position #2 (*what* position #2?), and ungrindingly shift gears to flow right into position #3 (§I8): Ptolemy wasn't typical of ancients — he was better![160] (The old vaudeville-comic's rape-defense: [1] But I don't even know the girl. [2] And I was nowhere near Judy that night. [3] Anyway, she consented.) See [Muffia 1990] *loc cit*; also: concluding sentences of both Jones 1991H & Jones 1991M. We must acknowledge that Springer-Verlag has taken aboard some remarkable pioneers in academic ethics.

I10 Reviewing the various above-mentioned Jones treatments of Hipparchos' solar data, culminating in Jones' new-math at §G7-§G9: we see that a hypothetical *JHA* referee with the slightest relevant expertise, reading the Jones 1991H claims of orbit-fit impossibility (§G4 for eclipses, §G7 for -127 & -126 data), would have realized at once that they *must* all be false. Reason? Each "impossible" problem has 2 equations of condition (longitude intervals)[161] and 2 corresponding unknowns (apogee & eccentricity). A perfect fit is therefore guaranteed, as I realized the moment I first read Jones 1991H's statement to the contrary. The only question was: will the orbits that fit the interval-sets be reasonable[162] or not? That I couldn't know until checking. The results of these checks proved highly gratifying, as we will see, starting at §K.

I11 Prediction: DR's results here (§K-§O), though founded on valid math (replacing the Muffia's own hilarious botches) will be automatically rejected by the Neugebauer-Muffia — simply & entirely because the findings constitute a discovery made by a scholar whom the Muffia loathes. Such behavior I have come to expect, for I know from long experience the cohesiveness & integrity of those who now dominate this Hist.sci area. But, let's not permit an Ivy League buffoon-union's antics to distract us from the part of this problem which *cannot* be answered (§I12 items [1]&[2]) and will thus be most hysterically evaded.

I12 Notice that there are really 4 separate questions here:

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

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

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

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

Now, as with any controversial & novel historical discovery, there will of course be some disagreement (at least for awhile) about item [4]. Though there'll be no dissent whatever in certain quarters: the Muffia's genii will (as just noted: §I11) unquestionably consign item [4] to the stake, sight unseen. But I ask that observers of the Ptolemy Controversy not be distracted (by that unconditionally pre-ordained roast) from keeping careful watch on the Muffia's reaction to items [1]&[2], where DR is demonstrably, unevadably correct — i.e., where the *JHA*-Muffia assertion of impossibility (upon which the *entire JHA* paper, Jones 1991H, is founded) is self-evidently false. In this situation, proper academic procedure

[160] Cults of every stripe get twisted into such logical pretzels for a common reason, cited at fn 169. See also §H2.

[161] The intervals will permit solution for e (or r) and $g_\circ$. Solution for ϵ may be accomplished separately, when absolute positions are recoverable (*Almajest* 4.11) or attested (*Almajest* 5.3&5). See fn 215 or fn 217, resp.

[162] A good example of an unreasonable solution: adopting Kallippos' yearlength (§M4), if one fails to make the needed 1° correction (§M3) to the reported trio A intervals (of *Almajest* 4.11), then the fitting orbit will have $e = 7^\text{p}46'$ (!) and (for $\epsilon = 227^\circ 2/3$) $A = 77^\circ 1/4$. The eccentricity e may be insanely outsized, but [a] the yearlength is standard (unlike the 366 day canard of Jones 1991H), and [b] this solution proves (even for the wildest case possible here) the falsity of the assertion (of Jones 1991H & Jones 1991M) that Greek trig-solutions for the uncorrected *Almajest* 4.11 data are impossible. If one wished to match Jones 1991H by positing a zany yearlength, it would be possible to deflate the huge e of the previous solution. E.g., for Y = 366.25 days, we find $e = 3^\text{p}$, $A = 126^\circ$. (More exactly: $e = 3^\text{p}03'$, $A = 126^\circ 1/4$.) If it is desired to preserve Ptolemy's $A = 65^\circ 1/2$, then using this with Y = 366d 22h1/2 & $e = 0^\text{p}47'$ will satisfy the trio A intervals. In short: a Babylonian-arithmetical solution has no advantage whatever over a Greek-trig solution here.

requires a printed retraction-correction — regarding items [1]&[2], with credit[163] 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.

I13 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 Muffia by *Isis* without DR's permission.) These criticisms (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 §A9) to have a DR paper (on Ptolemy's 2 clumsily contradictory fake observations of Venus' 136 AD max elongation: see Rawlins 1987 p.236 item [4]) approved by eminent refs (C.Wilson, K.Moesgaard) for *Isis* publication, the journal now (*Isis* 1983/12/20 letter):

[a] demanded excision of all substantial criticism of N.C.Swerdlow (note fn 269),

[b] asked that I also not submit to *Isis* "in the immediate future" (whatever that means) any *other* papers on the "specialized" subject of ancient astronomy (a ban that somehow fails to apply to genii B.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![164]

(I don't know whether I'm just super-subtle or what; but, *some*how, I got a glimpse of a hint of a vaaaague impression that *Isis* wasn't exactly breathless-anxious[165] 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 equations[166] discovered by the 1983 DR

[163] One takes it for granted that, if the Muffia ever alters its opinion on this matter, *DIO* will receive neither credit nor even mention. I would point out to the Muffia (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 avoidance (since c.1984) of involvement with Muffiosi is not due to personal animosity or snobbishness (I leave such degrading games entirely to the Muffia) — but, rather, to repeated experiences of the sort detailed in *DIO 1.1* (at, e.g., ‡1 §A11, C7, C12, ‡3 fn 7, ‡6 fn 35). Muffia-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 OG's frustration at fn 9.

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

[165] I had already just been through a Hist.sci journal double-cross (*DIO 1.1* ‡1 fn 25) at *JHA* regarding Rawlins 1999, which Lord H privately was loathe to publish (a point not only self-evident but directly verified by *JHA* co-Editor OG, 1983/6/6), despite *JHA* referee-recommendation and even a published *JHA* statement of acceptance (*Isis 73*:158; 1982/3). When a journal of this stripe does not wish to publish a paper, it will delay, snip, and generate impedimenta at will. The wise author will not go down that road in the first place. (The *DIO* inside-back-cover statement's standing offer to *JHA*, *Isis*, *et ilk*, has been designed to ensure that DR will never again become enmeshed in editorial wrangling with actors.)

[166] Compare Swerdlow 1989 p.43 to p.E7 of the DR ms sent for publication to *Isis* 1983/8/12. These equations show that certain Venus fakes were not even out of the *Almajest* tables but were just from highschool-level trig. (The stellar fakes, like the solar ones of fn 64, were more primitive yet: mere gradeschool-level arithmetic; see R.Newton

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 genii 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.

J4 In fairness to Jones, we should, however, note that his recent work, "Ptolemy's First Commentator" (Jones 1990), establishes a valuable[179] 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.[180] So, the horoscope might originally have been computed as late[181] as c.300 AD. Two other matters are worth note.

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

[b] Neugebauer 1975 pp.948-949 charges incompetence against the horoscope's ancient computer in large part because the solar & lunar dates of computation allegedly disagree by 2 Egyptian years: 211/4/25 vs. 213/4/24. Yet, Jones 1990 p.51 n.13 now very plausibly ascribes the earlier date to a mere scribal slip: see p.30 (note to line 18).

J5 With item [b], we are again reminded of N.C.Swerdlow's attack upon R.Newton for (on a single occasion) reaching a dubious skeptical verdict — which also turned out to be based merely upon a scribal-slip (fn 169). NCS has (in a journal whose Ed.Board included Mr.Nice-Guy, archon-angel O.Gingerich) used such trifling material to suggest that *all* of RN's ancient work is "garbage": fn 13, *DIO 1.1* ‡3 fn 3 (& §D3). So: do we now also turn over Muffia-don Neugebauer's lifetime of ancient analyses to Swerdlow's overworked garbage collector? Perhaps the obvious analogy here explains why, when Jones 1990 p.51 n.13 corrects Neugebauer, no mention is made of ON's bungle-based slander of the ancient computer. Nor does Jones mention that Neugebauer 1975 p.949 n.6 miscomputes the solar mean longitude by $30'$ — a *half-degree* — and thereby forces his solar mean longitude to equal[182] the scribal error within one arcmin! (See, under fudge-Muffed calculations, at *DIO 2.1* ‡3 fn 38.)

[179] When first encountering DR's policy of evenhanded citation, Muffiosi 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 Muffia — but, Muffiosi are nothing if not loyal to their favorite theories. E.g., §D3 item [b].) As I happen to know from direct testimony, some very prominent Hist.sci comers operate by a conscious policy of brainkissing archons. [Note added 1993: But none's technique is quite up to the earlier British prototypes quoted at *DIO 2.3* ‡6 fn 18.] Thus, they interpret others' compliments by what psychologists call: projection. (See fn 3 & fn 169.) I.e., Muffiosi have never understood — and are incapable of believing — that DR praises their occasional valid work largely as just an expression of decency & proper scholarship. (Additionally: when a Muffioso is right about something, the event is, well, an *occasion* — it deserves some fuss, encouragement, & commemoration here.)

[180] The emperor Aurelian, born c.212-214 AD, ruled from 270 AD until his 275 AD assassination.

[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 Muffia 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: $30^\circ 49'$; text at Jones 1990 p.30 lines 18-19: $30^\circ 48'$. Jones 1990 p.51 n.13 covers for ON here by stating that a result $30'$ off — about *twice the solar semidiameter*, mind you — is "nearly" correct.

accurate orbital analysis (namely, Rawlins 1991H) underlying DR's novel key discoveries regarding HIPPARCHOS' SOLAR ORBIT WORK, published in *DIO 1.1*. (And the current exposé is the direct result of that bit of snobbery.)

J2 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[176] in the face of: [a] manifold (& explicitly documented) *DIO 1.1* warnings regarding the Muffia'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 Muffia output for the last few years, *Isis* (as soon as *DIO* appeared) evidently hired out a jetpowered taxicab and vrrroooooomed right to the printer, to rush[177] into publication — starting on the 1991/9 issue's *first* page — a hilarious Muffia 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 (§I14), and [iii] basing its conclusions heavily on the unprecedented math of the Jonestown analyses of HIPPARCHOS' 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 Muffia-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 admired as "the philosopher's death". But the Au→PbBalloon sadiM-alchemy of those who turn Greeks into Babylonians has managed to make a comedy — a mass selfpiekill spectacle — even of the once-noble art of self-destruction. A clique who'd trust [the Muffia] for orbit-fitting, would cast Lily Pons in Brünnhilde's Immolation scene. (The more fastidious *JHA* would presumably insist upon Susan Alexander.)[178] Consider in review the impressive range & enormity of Hist.sci's perversity in its Jonestown extravaganza: [1] The author, whom Hist.sci archons were so frantically determined to place *first* in their 1991/5 *JHA* **and** 1991/9 *Isis* issue's offerings, [was] the *last* scholar one would choose to perform orbit-fit math, on which both these prominent papers are based. His statements of impossibility for trig fit-solutions are invariably false. [2] Jones 1991H has moreover seriously miscomputed data (§G7 vs. §G9) which he alleges justify his central thesis. (Again: no referee-checking, at even a subprofessional math level — gradschool or gradeschool. 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.

[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 bibliographies.)

[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 83.1*:1, 1992/3, emph added): "our publication schedule [has] . . . picked up speed . . . . The [UChicago] Press has launched a massive promotional 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 Herrmann, who [with 1 exception] never won an Academy Award, for the excellent reason that: he was personally disliked.)

paper were eventually published, after all, by elite Hist.sci journaldom, in the extremely handsome *JHA* (1989/2 p.43), on whose board sits N.Swerdlow. And my research was fiscally supported by the National Science Foundation and the MacArthur Foundation! Golly, that just made up for everything. OK, OK, it was a trifle odd that my name was spelled "N.Swerdlow", on both the equations & the grant-cheques. But, then, nothing in life is perfect. Except Hist.sci archons' ethical record.

I14 Aside from refereeing for accuracy & competence, *Isis* might also entertain the shocking notion of requiring honest & up-to-date citations.[167] Case in point: when Jones 1991M p.445 speaks of Ptolemy's "notorious" equinox & solstice observations (see Rawlins 1987 p.236 item [1] and its previous §B3 suppression by the *JHA*, detailed at *DIO 1.1* ‡1 fn 11), his n.18 adds: "For my argument the genuineness of Ptolemy's solstice and equinox observations is irrelevant; this vexed topic is expertly investigated by [Britton 1967]." (I.e., whether Ptolemy was a liar & a plagiarist is irrelevant to whether his *Almajest*'s "originality" has heretofore been insufficiently realized! — as asserted in Jones 1991M's concluding sentence.) Now, the transparency of the Muffia's persistent Britton-citation ploy was already exposed at *DIO 1.1* ‡1 fn 5 — well before publication of Jones 1991M. Additionally, the Britton 1967 discussion is obviously inadequate:

[1] It realizes[168] Delambre's 1819 argument but doesn't credit him, as does R.Newton (§E1) — so Jones' sending the reader to Britton 1967 is just another case (as at §C11 item [d]) of citing the nonciter & nonciting the citer. (Option [c] at §H2.)

[2] The Britton 1967 search, for innocent explanations of Ptolemy's solar errors, investigates (problems with) observing data on an equatorial ring, whereas Ptolemy clearly states (*Almajest* 3.1) that the (faked) solar "observations" were instead made on a transit instrument. Regardless, Jones 1991M ought to have been required (as a condition for *Isis* publication) to cite a sampling of the skeptical analyses that have appeared during the quarter-*century* since Britton 1967. (*JHA*'s citation-integrity-monitor[169] O.Gingerich would doubtless be

1977 pp.245f.) Yet Swerdlow 1979 p.526 had attacked R.Newton 1977 because fudging or faking *Almajest* data was too "intricate"! Somehow, the obligation to retract that former surety got overlooked (see also fn 123) when Swerdlow 1989 p.43 published DR's simple 1983 Venus fabrication-equations. Instead, this NSF-funded paper (p.31) continues imperviously to insist on a blanket claim (echoing §R4) that "The relation between observation and theory in Ptolemy's astronomy is complex." And Swerdlow 1989 continues (e.g., p.30) to admire Ptolemy, even though (p.54) he "could not have observed some of the reported [Mercury] elongations" & so "adjusted" (i.e., fudged) them to accord with faked (indoor-computed) "observations".

[167] E.g., nowhere does Swerdlow 1989 p.54 acknowledge that Ptolemy's fudgery (fn 166) confirms the long-held (Muffia-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's citation of Swerdlow 1979 (an abusive attack upon R.Newton 1977). Thus, none of the wealth of prominent skeptical papers published *since 1977* (e.g., §I14) is referenced. (For *Isis*' even-more-retarded behavior, see §I14 item [i].) Re-read, at fn 5, OG's purported "scandalization" at a minor and obviously *nondeliberate* RN failure at "referencing to the previous literature". (The citation-records of RN-DR vs. the Muffia are clear: massive citation vs. massive noncitation. OG has no comment on this contrast.) Note that the uthor of this sentence is the very editor responsible for publishing nonciting-Swerdlow 1989 in OG's *JHA*! (Likewise for Jones 1991H, which is guilty of similar citation-dereliction.) Is such *JHA*-level scholarship — and citation-integrity — appropriate to NSF & the MacArthur Foundation? (Are these awards given for research, or for: Best Imitation of a Careerist?) I particularly recommend a careful comparative reading of 4 items: [a] *DIO 2.1* ‡3 §C3 (planet mean motions); [b] the startling new Rawlins 1987 discovery regarding Mercury's mean motion (cited at *DIO 2.1* ‡2 §H9); [c] gov't-funded Swerdlow 1989's pathetic p.30 alibi-evasions (e.g., here at §H3) & noncitations of these arguments, findings which unambiguously demolish his cult's most revered tenets; [d] Swerdlow's charge (*DIO 1.1* ‡3 §D3) that RN was "a crank and a con-man, whose principle accomplishment has been extracting money from the government on false pretenses." (It is amusing that such language should emanate from a modern gov't-funded lawyer-evader — representing an ancient gov't-funded establishment-astrologer-hoaxer, who worked 40^{y} for the Alexandrian gov't's official Serapic religion. See *DIO 1.1* ‡7 fn 4 & §G4.)

[168] Britton 1967 pp.29&43-44. But at pp.42&44 Britton speaks of Ptolemy's computing indoor "observations" from Hipparchos' trig-based solar "model", whereas the whole point at p.29 is that Ptolemy's fakes are just from simple arithmetic: fn 64. [Note added 1993: References here are to pp.25&35-36 of the 1992 edition of Britton.]

[169] Noel C. Swerdlow has attempted to paint R.Newton as dishonest (§E1, §I5, fn 252) by the flimsy and questionably-relevant implication that Newton's early research had depended upon R.Taliaferro's *Almajest* translation (which was not cited in, e.g., R.Newton 1970) — even though the very same scribal error, which Swerdlow suggests RN got from Taliaferro, was (as Swerdlow states) also in the Halma *Almajest* translation. Simple consultation of R.Newton

"scandalized" by such omissions: see fn 5.) This literature includes numerous *publications* in eminent forums, e.g., *Science* 1969, Johns Hopkins Univ Press 1970+ (R.Newton 1970, R.Newton 1976, R.Newton 1977, etc.), *QJRoyAstrSoc* 1973+ (R.Newton 1973-4, etc.), & the Greenwich Centenary's Longitude Zero Symposium (Rawlins 1985G). Though useful & generally competent, Britton 1967 is, in any case: [a] by a Muffioso, & [b] *unpublished*.[170] The contrast raises obvious questions:

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

[ii] What sort of scholars fear *Isis*' readers even *seeing* the reasoning of the other side's arguments in the Ptolemy Controversy? The Muffia's usual tactic for 20 years has been: mention the *existence* of the Ptolemy debate (Toomer told *DSB* this proves noncensorial intent: *DSB* to DR 1978/7/6); but, don't tell the reader where he might actually find the publications of the opposition! (Similar to option [c] at §H2 — but rather less liberal than St.Cyril's response to Porphyry.) This systematically suppressive Muffia procedure has been assented to by, e.g., [1] *Sky & Telescope* (J.Henderson 1976/2 review[171] of Pedersen 1974); [2] the *Dictionary of Scientific Biography*'s Editor C.Gillispie (despite extensive pre-publication DR-*DSB* correspondence[172] regarding Toomer 1978H); and now [3] *Isis* Editor Ronald Numbers, of the University of Wisconsin at Madison (fn 177). When institutions repeatedly give prominence to such lordly scholarship, even while simultaneously impeding or killing[173] the works of those who regard it as a matter of principle to cite & credit both sides' output fully,[174] they reveal not only the censorial authors' integrity-level. They expose their own, as well.

I15 We all know how honest scholars & institutions — capable of setting aside personal likes & dislikes — ought to behave in circumstances where error has been found by one's opposites. (DR has attempted to set a proper example even in far worse circumstances: *DIO 1.1* ‡1 §C3 & ‡6 fn 35.) But impersonal evaluation & equity are precisely what neither the Muffia nor the *JHA* are constitutionally capable of engaging in. (See *DIO*'s inside back-cover publisher's statement, which declares that the corrections made here to their bungled math are automatic submissions to, e.g., *JHA* & *Isis*. Neither took up the similar suggestion at *DIO 1.1* ‡6 fn 4.) The most merciful description of these archons is that they are, regrettably, glacially slow learners.[175]

J And The Last Shall Be First: Muffia Immolation-Scene

J1 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 organ *Isis* showed (despite our explicit, not-holding-our-breath offer at *DIO 1.1* ‡6 fn 4) the slightest interest in telling readers of the competent &

1970 pp.181 & 299 would have dissolved Swerdlow'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 fails to mention that RN's 1969 article, *Science 166*:825 n.20, explicitly cites Halma's edition as his primary source, adding that "the standard translation is by K.Manitius".) E.g., both the 1969 paper and R.Newton 1970 pp.17 & 20 refer to *Almajest* 3.2 for the equinox-solstice data, which are at *Almajest* 3.1 in every edition besides Halma's, including Taliaferro's: black&white proof that Newton's original work with the *Almajest* was through Halma, and thus that the malicious remarks of Swerdlow 1979 were unjustified. (These were published in the journal of Phi Beta Kappa!) Comments: [a] Scholarship this low does not flourish unless the author knows he will be protected from exposure. (No need to speculate on this point: see §I13 item [a].) [b] Expect no retraction. [c] The triviality of the original charge shows what thin material the Muffia must reach for, to portray Enemies as dishonest. (RN's use of various translations is openly discussed at R.Newton 1985 p.53.) Fn 252 here notes another perverse Swerdlow attempt to portray Newton as dishonest. One might say that Swerdlow missed his calling as a lawyer, except: he *is* a lawyer, for the tight Muffia cartel of businessmen-scholars. In this respect, he & his are inspired by their ancient hero, who was a lawyer for geocentricity: *DIO 1.1* ‡7. The Muffia & Ptolemy share lawyers' most characteristic genius-prescience: they know their conclusion *before* investigating the facts of a case [fn 160]. Which presumably explains how Swerdlow came to co-author a prominent review (published by O.Gingerich in *JHA*) which did not understand the purpose or even the *title* of the book being reviewed. (See R.Newton's astonished response in *DIO 1.1* ‡5, e.g., §A2.)

[170] However, Britton 1967 is scheduled eventually to be republished in revised form, according to Swerdlow 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 B.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, curiously obvious errors survive from the original edition. E.g., see the misrendering at p.48 n.1 of the wellknown *Almajest* 5.13 date of C.Ptolemy's notoriously faked lunar parallactic "observation". Also, the self-evident sign blunder in the last angular argument on p.142 — a highschool math student could have spotted this error anytime during the last quarter-century by a moment's comparison to the next-last argument (whose coefficient is, incidentally, mistaken by $10''$). Britton's new bibliography makes a point of citing every R.Newton work he can think of — and citing zero works by DR. Well, now that RN is safely dead, it's nice to find Muffia citation-practice finally rising to: half-honest. But, considering that Boardperson Moesgaard has since 1991 (see *DIO 2.1* ‡2 §D) been well aware that *DIO 1.1* ‡6 (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-sponsored the publication of Britton 1992 without urging a citation of DR's success — and its confirmation of Britton's intelligent speculation.]

[171] On 1974/11/15, O.Gingerich (close to *S&T*) informed me (as he told many others, including his adoring grad students, as well as *Scientific American* persons) that the R.Newton 1973-4 articles on Ptolemy were crank and were regarded as scandalous nonsense by all the Ivy League Hist.sci experts (e.g., O.Neugebauer of BrownU, A.Aaboe of Yale). Implicitly making the classic statistical error of presuming data-independence, I stupidly assumed these "experts" couldn't *all* be wrong, and so was put off the Ptolemy scent for over a year — until encountering Henderson's *S&T* piece, which raised my suspicions by its ploy (which I did not yet know was standard for Muffiosi) of mentioning doubts of Ptolemy but not citing R.Newton's prominent papers. After 1976 phone chats with Henderson, Aaboe, Newton, Gingerich, Neugebauer [*DIO 1.2* §§D&G], I began seeing that Ptolemy was not the only faker in this affair.

[172] These exchanges included Gillispie's consultation with *DSB* Associate Editor Harry Woolf. And it was from that moment that Gillispie started leaning away from attempting to get at the truth. Gillispie has high scholarly standards and is commendably critical of the degeneration of technical facility among modern Hist.sci scholars. But his deferring to Woolf at this time betrayed those very goals — besides being inexcusably prejudicial, since Woolf was head of the Princeton Institute, whose star ancient astronomy scholar was O.Neugebauer, the Muffia's don of dons. Woolf is not an uninformed scholar. (He personally first steered me to J.Delambre's great works.) Woolf is also: an academic socialite, Iranian-Shah-family-booster (honorary Johns Hopkins degree), and probably the most successful politician ever in Hist.sci (its closest approach to having an Izzy Bowman in its midst), a veritable archon's-archon, who is around alot of money, and whose career credits include (according to the entry he wrote for *Who's Who*): *Isis* Editor, JHU Provost, several honorary doctorates, Board of Directors Alex Brown [the very stockbroking firm whose breeding is noted at *DIO 2.1* ‡3 fn 28] Mutual Funds (not Hist.sci's only connection to stockbroking), Director of the Princeton Institute for Advanced Study, trustee of a Merrill Lynch fund, chairman of Congress's MX Missile basing advisory panel of Techn Assessment, director-at-large Amer Cancer Soc, trustee Rockefeller Foundation, board of governors of Tel Aviv Univ. (Woolf's high Middle East assocations are redolent of another wealthy Hist.sci archon, Iranian arms-dealer Adnan Khashoggi's goodfriend, recently-resigned Amer Univ Pres Squareza: *DIO 1.1* ‡8 fn 2. Indeed, it happens that one of Woolf's honorary degrees is an Amer Univ 1982 DSc from fellow Hist.sci&Iran-contact biggie, Squareza himself. Whether or not one makes sense out of the bizarre connection between [a] arms-money, [b] rich Hist.sci archons, & [c] rich Iranians, one may wonder about the connection, of this type of connection, to the quality-trend of modern Hist.sci archons, as well as of the high scholarship they promote & protect — scholarship to which our *J.HA* has so frequently been indebted.) Woolf's other credits include abhorrence of the publicity given to an ikkie thing like science fraud. For *DSB*'s 1978 review-look at Toomer's suppression of R.Newton's findings, Gillispie obviously chose Woolf as someone with plenty of time on his hands, to investigate the technical particulars of ancient calendars, Greek sph trig, & the Ptolemy Controversy. (Perhaps the *DSB* should have conferred with Squareza, who, we now know, actually had alot more spare time than anybody'd suspected.) Gillispie also simultaneously consulted with another Hist.sci archon at the Princeton Institute, M.Clagett. For appropriately sober evaluation of ancient astronomy material published by Clagett, see "Figleaf Salad" *Journal for Hysterical Astronomy 1.1* ‡7 fn 13.

[173] See *DIO 1.1* ‡1 §A9 & fn 25, ‡6 fn 4. Let's have no misunderstanding of what systematic noncitation is about: it constitutes attempted murder of a scholar's academic career.

[174] See the sometimes respectful DR (& RN) citations of Muffia work (as at fn 16): above at fn 2, §C11, §E1, fn 73, & fn 110. Or see Rawlins 1982C n.8, Rawlins 1984A p.985, *DIO 1.1* ‡6 §H2, fn 34 & fn 35.

[175] Of course, the Muffia regards DR similarly, since its own repeated attempts to teach him lessons (e.g., that power can suppress truth: fn 129) have been impressive but unsuccessful. These efforts have not only failed to quell rebellion. They have turned it into *DIO*. Intelligent superarchons could have predicted that possibility a long time ago, had they merely discerned (or cared) that certain Editors' heads were getting too big for their britches.