

### ‡3 Referees Refereed

#### A Malignitas Gloriosa

**A1** In 1980, Muffia capo Gerald Toomer (BrownU) attempted in vain to kill an *Isis* paper of mine (*Isis* 1982/6), through an anonymous<sup>1</sup> referee report that was as haughtily snide as it was insubstantial. [Note added 1993: Toomer is now at Harvard's Hist.sci Dep't.]

**A2** DR will return the favor by now quoting a letter to DR (1985/5/25) from an unnamed but internationally respected ancient astronomy specialist (not RN or DR or proponent thereof), whose appraisal here of the Malignant 1 was reflected in *DIO* 1.2 (§P1):

Neugebauer's sycophantically eulogised *H.A.M.A.*<sup>2</sup> . . . is, I fear, going to be the [inspiration for] as many errors in the interpretation of sources as was Sarton's work of some 50 years ago (at which Neugebauer has so often sneered) — cf. . . . [Neugebauer's] perverse refusal to credit Hipparchus with any knowledge of spherical trigonometry.<sup>3</sup> Toomer's work is characterized by a colossal conceit (apparently engendered by his many years as Neugebauer's

<sup>1</sup> It should be pointed out that an exception to the near-ubiquity of starchamber refereeing in modern academe is the policy of C. Truesdell's valuable *Archive for History of Exact Sciences*. This is pointed out by Truesdell during a sharp exchange at *Isis* 82.1: 90 (1990), where CT also claims, incredibly, that "No paper is ever rejected by the *Archive*." Craig Fraser (Univ Toronto) notes (*idem*) that neither this statement nor the nonsecrecy of refereeing appear in the statement-to-authors published in each issue. I can add the information that CT has unofficial ways of discouraging authors, such as carping about style and putting material through uncomprehending (but not prejudiced) editorial rewrite. (E.g., *AHES* will insist that "trig table" be rendered as "table of trigonometric functions". And Truesdell cares that Johns Hopkins University be called The Johns Hopkins University. Does the *AHES*'s extreme concern with style have any relation to the novel contributions to the English language found in the 1991/10/29 number, in the brief Muffia-Yale-communicated article at *AHES* 43.2:133-144? — such creative expressions as "insivable", "detailsmeant", "appears", "equation", & "the many reference to". And, at p.134 n.3, a citation to Neugebauer *HAMA* p.895 has been mysteriously rewritten as p.869.) In addition, CT has been known to (very politely) kick unapproved scholars out of his office. (And his Editorial Board now exhibits the same virtually-exclusive attraction to the institutionally Eminent as, say, the *JHA*. By contrast: at the *AHES*' 1961 inception, half of those able scholars, who joined Truesdell on his then-noncentrist Editorial Board, listed street addresses. [Note added 1993: Of the 24 *AHES* Boardmembers, 23 now list institutional addresses.]) However, for those who can swallow these endearing eccentricities, CT will honor the equitable and intelligent *AHES* rules he has set down — which is more than I can say for other journals in the Hist.sci field. But I must also regretfully add that Truesdell takes the Neugebauer Muffia amusingly overseriously — possibly because both parties share a traditional contempt for *Isis*. [Note added 1993: The *AHES* Editorial Board — once justly proud of its strength in math & science — recently added to its elite number [a then-Muffioso], none other than the Muffia classicist A. Jones, whose prominent *misarithmic* was the spectacular centerpiece of *Journal for Hysterical Astronomy* 1.2. This elevation further confirms the trend of *AHES*-Muffia merging — and further melts my former very high estimation of the *AHES*. (See *DIO* 1.1 ¶1 §A9 & *DIO* 1.2 fn 27.)]

<sup>2</sup> *History of Ancient Mathematical Astronomy* (usually cited by DR as Neugebauer 1975). A highly useful, encyclopedic work, marred here&there by ON's occasionally irrational pet theories. As with the *Almajest*, there remains an unresolved question of whether *HAMA* — and indeed the entire Neugebauer Muffia — has done more good than harm. Curiously, if his Muffia's attempted blackout of heresy is successful, the answer will be: No.

<sup>3</sup> This is a critical misperception (*DIO* 1.2 fn 73), as DR stressed at length to the *DSB* & Toomer 1978/5/18 pp.H1-H2, H5. (Some of DR's 1978 reasoning has now been published at p.140 n.10 of J. Berggren's useful article at *AHES* 43.2:133; 1991.) Muffia stubbornness on the point is still inhibiting proper evaluation of ancient work: *DIO* 1.2 fn 73 item [b]. However, it would be improper & ungrateful for DR not to acknowledge his many debts to Neugebauer & Toomer. DR's discoveries have often been triggered by their wisdom & folly, their stubborn drive & equally stubborn inflexibility. Another Muffia plus: the cross-referencing in Neugebauer 1975 is astonishingly full. I accomplish such tasks (a habit Neugebauer 1975 helped inspire) by computer, but my guess is that for *HAMA*, a lot of the work was achieved by lapdog. While disapproving of his nongenerosity towards dissenters, I must also credit the massive labor & care that went into this feature of *HAMA* & his own 1984 *Almajest* — which reflects the expenditure of much of his own time (arranging, by hand, thousands of crossreferences) in order to save that of his readers.

lap-dog) that enables him to suppress or ignore other scholars' work that does not find favour with him — see especially the introduction to his [1984 Springer-Verlag] translation of the *Almagest* which positively reeks of his unpleasant egotism. With people like these at the head of the history of science establishment in the U.S.A., who in his right mind would want to have anything to do with it?

## B Forked Pen Dep't: the Mind-Behind

**B1** The following letter was received by R.Newton, in belated response to a paper he'd submitted (during the previous decade) to the extremely handsome *Journal for the History of Astronomy*. The letter was written by the *JHA*'s Editor-for-Life, Michael A. Hoskin (Churchill College, Univ of Cambridge).

To: Dr R.R. Newton, Appl Physics Lab, The Johns Hopkins Univ 1980/3/6  
From: M.A. Hoskin

**B2** . . . It has taken far too long to come to a decision about your paper . . . . The [3 alleged referee] reports [DR: *NOT* enclosed] gave me no clear advice. . . . it is of the essence of history of science that one attempts to understand *why* [emph in orig] people acted as they did, and by using terms such as 'crime'<sup>4</sup> and 'fabrication'<sup>5</sup> you put a barrier between yourself and the writing of history. The great question is, what was Ptolemy's *intention*? [Emph in orig.] To act as counsel for the prosecution is not to write history as understood in this journal, even if the facts you submit as part of the case for the prosecution are established by you. . . .

**B3** . . . the problem for me has been whether the ingenuity and penetration more than compensated for the a-historical<sup>6</sup> approach. . . . [your] arguments have been stated in somewhat different form in your book [R.Newton 1977] . . . [the current paper is] a reply to critics, rather than an announcement of newly researched material.

**B4** . . . I feel that I must give priority to other articles under consideration which do present new researches. I apologize once again for the delays, which I would like to think are very atypical<sup>7</sup> of this journal.

**B5** I look forward to publication of your researches. Yours sincerely<sup>8</sup> . . . .

<sup>4</sup> See below at §C10.

<sup>5</sup> Compare to ¶2 §H15.

<sup>6</sup> For a glimpse of what the *JHA*'s Editors regard as worthwhile historical analysis, see *JHA* 1.2 fn 36.

<sup>7</sup> When DR 1st submitted a paper to *JHA*, response took 11 months. (See also *DIO* 1.1 ¶6 fn 15.) What a card.

<sup>8</sup> Placing the word "sincerely", anywhere near Lord H's fervent wish for more RN researches, requires a special kind of nerve, which evolution has reserved especially for journal-editors. But, aside from questions of frankness, there are here a couple of Catch 22s here that are easily missed:

[a] All relevant Hist.sci journals have printed papers that allegedly show Ptolemy was honest. But if skeptical papers say he was dishonest, they are rejected as unhistorical. (More of Hist.sci's kaleidoscopic Bureau of Double Standards: *JHA* 1.2 fn 104.) What a neat way to hold a public debate on Ptolemy's integrity: tape one side's mouth shut.

[b] If the *JHA* refuses (§B4) to print anything except new research, then, in order to publish in the *JHA*, RN would have been obliged originally to send his novel results (not to the relative security of his own university's press, at Johns Hopkins, but rather) to an obviously unfriendly journal, whose editorship contains several of the leading enemies of his viewpoint — this while knowing that his discoveries would be privately circulated, unprotected, for perhaps a year of purported refereeing. (Recall: Lord H did not even send the 3 alleged referee reports on RN's refused paper! Similarly: see *JHA* 1.2 fn 6.) So His Lordship's conveniently-conjured-up criterion represents a neater ploy than immediately meets the eye. It would seem to most of us that journals have another function than publishing new research, which is (*DIO* 1.2 fn 20) to permit (nay, encourage) all sides of a controversy to argue their already-published cases in detail in its pages. But the Editor-for-Life is ruling that out. In *this* case, anyway. (Compare to van der Waerden vs. the *JHA*'s D.Pingree, around the same time: *JHA* 11:50-58; 1980.) Of course, the EFL did, understandably, not wish to squander precious *JHA* pagespace upon the "a-historical" (§B3) nonsense of RN & DR — e.g., the banned paper, Rawlins 1999 (entirely new research, the very commodity EFL claims to

**B6** In brief, the *JHA*'s Editor-for-Life has told RN that the *JHA* cannot publish a paper which does not explain Ptolemy's *intent* (*JHA*'s own emphasis), that being the "essence of history of science". Let us call this argument "BLACK", and next turn to the *JHA*'s promulgation of "WHITE". (See also *DIO* 1.2 fn 15.) Emphases are added:

To: Dr R.R.Newton, Johns Hopkins U, Applied Physics Lab 1983/6/21  
From: O.Gingerich, Harvard College Observatory, Smithsonian Astrophysical Observatory

**B7** . . . I have just returned from a small conference in Aarhus, Denmark. Dennis Rawlins was there, and his [1983/6/4] announcement<sup>9</sup> that Ptolemy had reported [DR: reported *observing*] the same greatest elongation of Venus for two separate dates created quite a debate. [DR: OG's exact 1983/6/6 term was "brawl".]<sup>10</sup> Dennis hoped that this would convince us all finally that Ptolemy was indeed a fraud, whereas he promptly discovered<sup>11</sup> that while everyone thought his discovery to be fascinating, the question of Ptolemy's moral uprightness or turpitude was ruled out of bounds for legitimate history of science, given our inadequate understanding of Ptolemy's *intentions*<sup>12</sup> in writing the [*Almagest*] or the expectations of his age.

**B8** Soooo, let's see. The Univ Cambridge's *JHA* Editor-for-Life says (§B6) that finding Ptolemy's intent represents the "essence" of Hist.sci — while the *JHA*'s now-#2 Editor (Harvard OG) says evaluating Ptolemy's intent is "out of bounds" (§B7). We conclude that the vast editorial-expertise mind-behind the *Journal for the History of Astronomy* has succeeded in kicking the Essence of the *JHA* right out of the *JHA*. Logical as usual, Governor.

see assiduously for the *JHA*), which would have run about 7 *JHA* pages in the originally-accepted version (about 4 pp, in the censored version belatedly sent DR). The Editor-for-Life was justifiably anxious to leave lots of *JHA* pagespace for solid, coherent work — such as J.Evans 1987 (64 pp!) and A.Jones 1991H (25 pp). Well, that explains everything. (In fact, little of Evans 1987 produces new research results: it is almost entirely — though, see ¶4 fn 18 — a fruitless, ineffectual, & not-overly-capable attempt to tear down the original work of RN & DR; i.e., just the sort of controversial material the EFL was banning from *JHA* in the letter quoted above. Some of the lowdiums of Evans 1987 are highlighted here at ¶4 §G1 & fn 65, and at *DIO* 1.1 ¶6 fn 15, *DIO* 1.2 fn 144, fn 152, & fn 288. The even more amusing — but undeniably original! — scholarship of Jones 1991H is also displayed in *DIO* 1.2.)

<sup>9</sup> See *DIO* 1.2 §I13; also *DIO* 1 ¶6 fn 37 on continued Muffia alibis, lately persisting (less adamantly) in Swerdlow 1989. The alleged Venus *observations* (136/12/25 & 11/18) are reported at *Almagest* 10.1&2.

<sup>10</sup> I notice that this letter makes no mention of another important DR discovery: that (between the *CanInscr* & the *Almagest*) Ptolemy altered the daily lunar nodal motion by 1°/311784. I had mentioned to OG (across the Aarhus conference table 1983/6/4) my having made a remarkable find, expressing a concern that, if Toomer had already found it, I did not wish to interfere with his priority. (A letter from OG had stated that Toomer possessed then-unspecified evidence that the *CanInscr* preceded the *Almagest*.) The details of the discovery were specifically imparted to OG on 1983/6/6. He reacted with no recognition of the equation, nor did he ever comment on it. Long after this, he mailed DR a photocopy of Toomer 1984 p.205 (containing this equation). Again: no comment. (Muffia interpretation of the equation was better than DR's: *DIO* 1.1 ¶6 fn 35.)

<sup>11</sup> The proud sense of redemption OG displays here is too precious: imagine *boasting* of being able to depend upon equally brilliant colleagues to join hands in an unfalsifiability-exercise. I possess a record of this "brawl", which will probably end up being printed in a future *DIO*. Question: if OG's side did so well during the impromptu Aarhus fracas (during which DR alone took on a flock of Ptolemy-defenders simultaneously), then: why has OG remained reluctant for so long to accept DR's repeated challenges to arrange a public debate of the Ptolemy Controversy? (See ¶3 fn 24.) Everyone attending the Aarhus exchange knows why. The OG-founded Historical Astronomy Division of the AAS invited N.C.Swerdlow to speak at the 1992/6 HAD meeting, this after a member of the committee handling the meeting specially requested of DR (and received) 3 copies of *DIO* 1.1 which: [a] set forth details of technically amateurish (though professionally priced: fn 28) and ethically repellent NCS behavior (see list at *Journal for Hysterical Astronomy* 1.2 fn 169), and [b] repeated these debate challenges (*DIO* 1.1 ¶1 §A7 & §C10). But, as yet, there is still no visible interest in an HAD debate. (Indeed, an earlier DR suggestion of debate, to HAD chief E.Krupp, was unanimously turned down by the HAD rulership.) The President of the History of Science Society (when *DIO* 1.1 appeared) is a member of the HAD. He hasn't revealed any efforts to alter this situation.

<sup>12</sup> Of course, when it's convenient, schlemieleon 0 takes the very opposite tack! — as in Gingerich 1981 p.44, where OG echoes the Editor-for-Life's §B2 demand for the *why*, emphasis again in original. And *this* in turn disagrees with p.40 of the very same article — which questions whether Ptolemy's motives are knowable. Is this a half-hearted or half-witted or half-somethinged attempt at pioneering a schizoid Skinner-Freud-hybrid brand of Hist.sci? Will we end up debating how many motives can stand on the brain of a Hist.sci editor? . . .

## C The Doctor Is In

**C1** The foregoing may seem bizarre. But it seems almost sane, compared to the history to be described in this next section. And it is best to commence our account with a sober reminder: though this is a personal experience, the ultimate loser from the misbehavior here documented is not DR. The prime losers are: [a] Other<sup>13</sup> modern scholars, whose freedom is being squeezed out. [b] The public, which is being taxed to support scholars who are palming off fake knowledge on that very public. (*DIO* argues the case that Hist.sci grants are justified — but merely asks that our public be permitted to have the appropriate paternal and moral satisfaction of being thanked, for grants which are largely charity, not reward.) [c] The ancient scholars who genuinely observed, computed, and created the intellectual advances that are our scientific heritage — and then had their work stolen by the intellectual forebears of today's archons. (I.e., certain modern scholars are suppressing the very research which exposes ancient suppressions of appropriate credit. It would be hard even to invent a more ironic situation.)

**C2** As described in *DIO 1.1* (§1 fn 9), the *QJRoy Astr Soc* in 1983 appointed Ptolemy-p.r. man O Gingerich (whose decades of assiduous catering to the right archons has definitely rendered him: In) as sole referee of a paper by DR (who is proud to be Out, with the very same archons). This DR paper traced all 5 of Ptolemy's hitherto-unsolved planet synodic daily mean motions (*Almajest* 9.3) back to simple *integral* period relations (all 5 listed here in fn 17). In each of the 5 cases, the DR solution explained the Ptolemy daily mean motion down to the last sexagesimal place displayed. — i.e., about one 50 billionth of a degree precision for 4 planets; down to the 5<sup>th</sup> place for Jupiter's motion (only attested to the 5<sup>th</sup> sexagesimal place: see fn 23), merely a billionth of a degree precision. Most [All! See fnn 23&24] of the degree/day ratios' components turned out to be numbers found right in the *Almajest* 9.3 discussion! (Unknown until DR sent<sup>14</sup> solutions [3 of them historically correct] to the *Journal for the History of Astronomy's* #2 Editor, O Gingerich, on 1980/4/13: see letter's text, below at §C6.) The degrees/days components which produce precisely the long-unsolved *Almajest* 9.3-4 tabular mean motions are given below in §C3, along with (for comparison) the Neugebauer-Muffia's<sup>15</sup> uniformly nonfitting solutions, which are simply those (falsely) stated by Ptolemy<sup>16</sup> to have been the sources of his mean motions.

<sup>13</sup> I well recall my own pre-*DIO* difficulties. But now, serial-disappointment at archonal shortcomings is counterbalanced by: [i] the bliss of publication without censorship, [ii] the pleasure of my continuing good fortune in making serious & contributory discoveries (*DIO 1.3* constitutes a precious cluster), and [iii] the educational playtime spent chronicling the archons whose irrepressible antics make possible our readers' enjoyment of the *J.H.A.*

<sup>14</sup> The longterm grandfather period-relations (found 1982) were then unknown, but the prime factors of all 5 planets degrees/day motions were given in the 1980/4/13 letter, and those which related to the *Almajest* 9.3 attested numbers were (§C6) all identified. The Mars numerator ( $288^{\circ} + 4^{\circ}$ ), later appearing in Rawlins 1987 p.237, was also provided, though I'd not yet noticed (§C6 here) the seeming confirmation (noted in 1982) that the Mars denominator (224630d82/135) also exhibited a  $4^{\circ}$  excess, over 615 ancient tropical (Metonic) years. [This misled DR to suppose Mars' period-relation in the *geocentrist*-bible *Almajest* was integral in *heliocentric* not synodic revs. Oddly, heliocrevs turn out to be the basis after all: see Alex Jones' (correct) inversion of DR's miscue at *DIO 11.2* ‡4 fn 20.]

<sup>15</sup> The Muffia cartel was introduced to *DIO* readers at *DIO 1.1* ‡1 §C5-§C13 etc. It comprises, e.g., G.Toomer (BrownU), A.Aaboe (Yale), N.C.Swerdlow (Chicago), B.Goldstein (Pitts). Its satellites include O Gingerich (Harvard) & G.Graßhoff (Hamburg). See also *J.H.A 1.2*, e.g., fn 13. Muffiosi's attitude of unalloyed contempt (usually entailing just noncitation) towards the ancient astronomy work of R.Newton-DR is largely peculiar to the Muffia itself. RN-DR historical research has long been published in leading academic forums (see *J.H.A 1.2* §I14); it has been respectfully (& far from uncritically) cited by, e.g., W.Hartner (Frankfurt), K.Moesgaard (Aarhus), B.van der Waerden (Zürich), C.Wilson (St.Johns), V.Thoren (Indiana), S.Goldstein (Va), J.Carson (Md), etc. The discreditable process, whereby the Hist.sci community has assented to the elevation of an arrogant & intolerant cult to the status of Dominant Experts — which lends an air of current pseudounanimity to their cultish views — is less logical than sociological. I suppose one could be depressed by the spectacle of academe effectively saying: either it doesn't mind, or it actively approves, of scholars rising politically via cohesive noncitation and slander towards dissenters they hide from meeting face-to-face. But the optimist in DR just gratefully notes that: the higher the number of unprincipled Hist.sci archons who honor the Muffia, the easier it is for *DIO* to illustrate how much respect & trust the Hist.sci political center merits.

<sup>16</sup> *Almajest* 11.7 (Saturn), 11.3 (Jupiter), 10.9 (Mars), 10.4 (Venus), 9.10 (Mercury). Note the curious inconsistency that the order of the planets here is the reverse of that in *Almajest* 9.3-4. This point is minor in comparison to the plain fact that Ptolemy does not know the origins (*J.H.A 1.2* fn 78) of his own tables, a fact now tacitly admitted even

**C3** I provide, below, for each planet: [a] Ptolemy's tabular synodic mean motion; [b] the DR solution;<sup>17</sup> [c] the Ptolemy solutions long promoted by the Neugebauer-Muffia (§C4), helpfully tabulated<sup>18</sup> by Toomer 1984 (now realizing at last that the Ptolemy solutions are false, Toomer 1984 p.672 says the tables' origin is probably unknown); [d] the K.Moesgaard solution (for source & *JHA's* promotion of it, see *DIO 1.2* §H3), based on hypothetical ancient adoption of an unattested month  $M_s$  ( $29^{\text{d}}31^{\text{h}}50^{\text{m}}20^{\text{s}} = 318931^{\text{d}}/10800$ ). I draw particular attention to the Ptolemy-Muffia Mars solution [c], since Gingerich 1981 pp.41-42 falsely insisted it matched the tabular<sup>19</sup> value (Mars motion [a], below) even *after* R.Newton had specifically warned OG it didn't check arithmetically (the late RN was the very first to point this out for the planets), and after DR had twice sent OG [a perfectly-fitting (though not historically true)] solution (§C2 & *DIO 1.2* §D4) to help<sup>20</sup> keep him from blundering in his upcoming paper. (Gingerich 1980. 0 had sent DR a prepublication copy for comment.) OG has (publicly) stuck to this story for years. (Also key to maintaining the pretense, OG & the *QJRAS's* Editor David Hughes later — as noted at §C2 & §C4 — suppressed DR's much better-fitting [but also false!] solution: below, Mars item [b].) The comparisons for all 5 planets follow. (Every attested<sup>21</sup> number is italicized.)

by Toomer 1984 p.672 (though naturally with no credit to DR, who first proved this truth, via the math of §C3 here). But ineducability is ever with us: one smiles at 1991 Muffia insistence on Ptolemy's "consistency" (*idem*).

<sup>17</sup> The grandfatherers of the DR solutions are merely integral sidereal period relations.

Saturn: 11 heliocentric revolutions = 313 synodic revs = 324 sidereal years.

Jupiter: 36 heliocentric revs = 391 synodic revs = 427 sidereal yrs.

Mars: 344 heliocentric revs = 303 synodic revs = 647 sidereal yrs.

Venus: 803 heliocentric revs = 309 synodic revs = 494 sidereal yrs.

Mercury: 901 heliocentric revs = 684 synodic revs = 217 sidereal yrs.

Each but the Mars relation is anciently attested. (See Neugebauer 1975 pp.906, 390, 605, 466.) For samples of the precise development of *Almajest* 9.3-4 planet mean motions from these period relations, see Rawlins 1987 p.237 (Mars), fn 27 (Jupiter). [Though perfectly-fitting mathematically, both solutions are historically false. See brackets at fnn 24&23, resp.] All 5 planets' developments, and their empirical sources, will be thoroughly detailed in an upcoming *DIO* analysis. [See *DIO 11.2* (2003).]

<sup>18</sup> The five values are correctly computed in App.C of Toomer 1984 (pp.669-670). But, of the other fifteen App.C calculations of Toomer 1984, 60% are wrong in the last place displayed (*DIO 1.2* fn 264) — this despite his highly expert use of "computer programs" and "modern mechanical aids" (Toomer 1984 p.viii, no assistant cited). By contrast, the ancients' figures for such divisions, even though hand-computed, are almost always accurate all the way to the last place given. Exceptions: [a] the *Almajest* 9.3-4 annual Venus motion, an error revealed, creditably, by Toomer himself. [b] The sidereal year of Ptolemy's *Planetary Hypotheses*, relayed in Neugebauer 1975 p.902 eq.7, should be 9 not 7 in the last place, since (though Neugebauer does not remark the fact) it is merely 36000/35999 times Ptolemy's tropical year; i.e., it's 13148880d/35999. And the 4th place should be 32 not 22, but that is Neugebauer's error, not Ptolemy's. R.Mercier's denial (*Arch Int Hist Sci* 26:197, 1976; at p.198) of Ptolemy's possession of a figure for the sidereal year was based on innocence of this attestation; and Mercier's value 365d.2568126 is also erroneous in the last decimal place displayed — evidently having been found decimally on a truncating pocket calculator. I.e., the ancient value is, though trivially imperfect, more accurately computed than either of the modern renditions: Neugebauer or Mercier.

<sup>19</sup> Gingerich 1981 pp.41-42: "R.R.Newton states that Ptolemy does not use the observations he quotes [*Almajest* 10.9 & below, Mars solution: item c] to find his adopted mean motion of Mars [the tabular motion: item a] . . . Thus in [*Almajest* 10.9, Ptolemy] finds that the motion in the epicycle . . . is 192 cycles [ $192 \cdot 360^{\circ} = 69120^{\circ}$ ] plus  $61^{\circ}43'$ , leading to the synodic period of 779d.938; given Ptolemy's solar period [365d1/4 — 1/300], the mean tropical period (686d.944) is easily found as I have listed it above [ $1^{\text{y}}.88077$ ]. Hence, I am convinced that Ptolemy has correctly used the early observation [−271] to derive the value of the mean motion he has claimed to find and which he used in the tables. R.R.Newton denies this, saying 'the reader can easily verify the point himself by doing the required arithmetic'. I have done this above with results contrary to his, but . . . I cannot be sure if Newton has confused the units in tropical solar years . . . with the units in Egyptian years . . ." DR's immediate reactions: [a] Bluffing this grossly deserves the extended verbatim attention it is here receiving. (*Readers should check all arithmetic here for themselves, to full sexagesimal precision. Amusement guaranteed.*) [b] The *QJRAS* Editor of this gem was David Hughes (star of *DIO-J.Hyster Astron 1.1* ‡8). It figures.

<sup>20</sup> In return for DR's effort at assistance, OG has been party to over a decade of suppression of crediting DR's originality in these discoveries, as well as suppressing DR's 1983 paper presenting them, while OG's correspondent Toomer proceeded to publish most (3/5) of them first: 1984 *Almajest* App.C. (Luckily, RN had gotten all three of these DR results into print before then.)

<sup>21</sup> The unattested Mars solution's ancestry is traced (back to the same sort of simple period relation which underlies each of the other 4 planets' motions) on p.237 of D.Rawlins *Amer J Physics* 55:235; 1987. The founding relation (303 synodic revs = 647 sid yrs) is the most accurate possible sub-millennial period-relation for Mars.

## Saturn:

Ptolemy mean motion =

$$[a] 0;57,07,43,41,43,40 \text{ degr/day}^{22}$$

$$\text{DR solution: } 20520^\circ/21551^d 18' = 205200^\circ/215513^d =$$

$$[b] 0;57,07,43,41,43,40 \text{ degr/day}$$

$$\text{Ptol-Muff solution: } 126711^\circ 27'/(133079^d 3/4) = 2534229^\circ/2661595^d =$$

$$[c] 0;57,07,43,41,44,18 \text{ degr/day}$$

$$\text{Moesg solution: } 360^\circ \cdot 880/(11267M_s) = 3421440000^\circ/3593395577^d =$$

$$[d] 0;57,07,43,42,10,05 \text{ degr/day}$$

Jupiter:<sup>23</sup>

Ptolemy mean motion =

$$[a] 0;54,09,02,46,26 \text{ degr/day}$$

$$\text{DR solution: } 23400^\circ/(25927^d 137/225) = 658125^\circ/729214^d =$$

$$[b] 0;54,09,02,46,26 \text{ degr/day}$$

$$\text{Ptol-Muff solution: } 124305^\circ 45'/(137732^d 23^h) = 2983338^\circ/3305591^d =$$

$$[c] 0;54,09,02,45,09 \text{ degr/day}$$

$$\text{Moesgaard solution: } 360^\circ \cdot 199/(2688M_s) = 2014875^\circ/2232517^d =$$

$$[d] 0;54,09,02,44,55 \text{ degr/day}$$

Mars:<sup>24</sup>

Ptolemy mean motion =

$$[a] 0;27,41,40,19,20,58 \text{ degr/day}$$

$$\text{DR solution: } 103684^\circ/(224630^d 82/135) = 152145^\circ/329621^d =$$

$$[b] 0;27,41,40,19,20,58 \text{ degr/day}$$

$$\text{Ptol-Muff solution: } 69181^\circ 43'/(149881^d 2/3) = 4150903^\circ/8992900^d =$$

$$[c] 0;27,41,40,19,28,07 \text{ degr/day}$$

$$\text{Moesgaard solution: } 360^\circ \cdot 90/(2377M_s) = 349920000^\circ/758098987^d =$$

$$[d] 0;27,41,40,19,51,55 \text{ degr/day}$$

Venus:<sup>25</sup>

Ptolemy mean motion =

$$[a] 0;36,59,25,53,11,28 \text{ degr/day}$$

$$\text{DR solution: } 1800^\circ/2919^d 40' = 5400^\circ/8759^d =$$

$$[b] 0;36,59,25,53,11,28 \text{ degr/day}$$

$$\text{Ptol-Muff solution: } 92138^\circ 25'/(149452^d) = 1105661^\circ/1793424^d =$$

$$[c] 0;36,59,25,49,08,51 \text{ degr/day}$$

$$\text{Moesgaard solution: } 360^\circ \cdot 84/(1661M_s) = 326592000^\circ/529744391^d =$$

$$[d] 0;36,59,25,52,07,12 \text{ degr/day}$$

## Mercury:

Ptolemy mean motion =

$$[a] 3;06,24,06,59,35,50 \text{ degr/day}$$

$$\text{DR solution: } 52200^\circ/16802^d 24' = 21750^\circ/7001^d =$$

$$[b] 3;06,24,06,59,35,50 \text{ degr/day}$$

$$\text{Ptmff sltn: } 456726^\circ 53'/(147013^d 13^h 1/2) = 109614452^\circ/35283255^d =$$

$$[c] 3;06,24,06,58,39,48 \text{ degr/day}$$

$$\text{Moesg solution: } 360^\circ \cdot 329/(1291M_s) = 1279152000^\circ/411739921^d =$$

$$[d] 3;06,24,06,59,00,50 \text{ degr/day}$$

**C4** OK, so the five DR solutions match the *Almajest* 9.3 mean motions exactly. (And they use *smaller* factors than the *non*-fitting competing theories. See especially fn 25.) *BUT* — as we've also just seen above — lots of top Muffiosi<sup>26</sup> had already (pre-1980) publicly proclaimed solutions (for these same motions) which uniformly failed to fit. (See discussion & full sources at §C15 here and at *J.HA* 1.2 fn 55, fn 56, §H3, & fn 129.) Thus, there was a problem, very simple and purely political: the new solutions were lovely, but the *solver* was of the worst possible social caste (*J.HA* 1.2 §H2) — top supporter of the hated<sup>27</sup> R.Newton. So O.Gingerich, himself publisher of one of his clique's failed solutions (which he agrees privately is false: OG referee report to *QJRAS* 1983/7/23), suppressed publication of the above perfect-fit solutions (same ref report: see *DIO* 1.1 ¶1 fn 9). He & his Muffia friends have managed for *twelve years* to hide from the academic community their own foulups and DR's [partial] success in this regard. One can only stand in awe of the political savvy & persistence such an achievement<sup>28</sup> has required, given the stark simplicity of the numerical

<sup>25</sup> DR's solution for Venus achieves a perfect fit (6 sexagesimal places) from 4 digit components, while the Ptolemy-Muffia & Moesgaard solutions fail (by the 4th place) even with 7 digit & 9 digit components. See *DIO* 1.2 fn 129.

<sup>26</sup> Moesgaard is not blood-Muffia, though the *JHA* & co. have attempted to use his work to obscure DR's. (See also *DIO* 1.2 fn 126.) I have learned from Moesgaard's papers. (Indeed, he has contributed serious improvements to DR's own papers. . . .) However, this able scholar's alternate explanations for DR-solved ancient mysteries are rather cleverer than the ancients: nothing to be ashamed of! (DR also once [twice! — see fnn 23&24] committed the error of imposing more structure on data than actually resided there: cited *DIO* 1.1 ¶1 §C3. And his mistake was worse than anything Moesgaard ever did or will do.) Moesgaard is a protégé of a top committed archon detractor of RN-DR (O.Pedersen: see *J.HA* 1.2 fnn 11 & 126); & KM was among those scholars who circulated (starting 1980) continued-fraction-style solutions of the origin of Eratosthenes' obliquity (11/83 semicircle) — shortly after DR transmitted the first continued-fraction solution (for Eratosthenes' precision) to various archons here & abroad (1979/8/31 & days following). But, while KM had highly original twists to his solution & was innocently open about his paper (even mailing DR a pre-publication copy without being asked to), DR was told by a well-known editor (1985/5/30) that a Muffia capo was "secretive about his continued fractions paper", part of which agreed with DR's 1979 equations, digit for digit. (Irony: it turns out that the continued-fractions explanation for 11/83 was not initially proposed by DR. It was 1st published by O.Neugebauer at p.453 of a long-forgotten 1943 review at *Amer J Philology* 64:452. However, [a] Neugebauer does not at all realize the critical import of this interpretation, namely, that it tells us Eratosthenes' intended ordmag 1' precision; see DR at *Isis* 73:259 (1982) p.262. [b] Neugebauer 1975 p.734 n.15 says that 11/83 has not yet been explained, thereby rejecting his own 1943 continued-fraction explanation. The 1943 review is, exceptionally, not cited anywhere in encyclopedic Neugebauer 1975.) Note: Moesgaard wrote one of the most amiable & witty letters (¶2 §D) received by *DIO* from scholars specializing in this field. Nice to see.

<sup>27</sup> If "hate" seems too strong an expression, consult *DIO* 1.1 ¶1 §C7 & ¶3 §D.

<sup>28</sup> As is clear from *DIO* 1.1 ¶1 §C12, ¶6 §H2, fn 35, & ¶7 fn 3: I do not regard the Muffia as less intelligent than DR. To the contrary, I am humbled as I watch Muffia tactical skills, e.g., scoffing at the eminent physicist R.Newton

<sup>22</sup> This common sexagesimal notation expresses a daily motion (in degrees) of:  $0 + 57/60 + 07/60^2 + 43/60^3 +$  etc. — i.e., each denominator is 60 to a power equal to its position in the sexagesimal display.

<sup>23</sup> [Note added 2003. DR's 1980 Jupiter solution was originally presented here (with inexcusable carelessness: *DIO* 11.2 ¶4 fn 26) as a high-odds fit. In 2003, Alex Jones found the actual solution, accounting even for the final place's zero, which DR had mistakenly attributed to *CanInscr* rounding. (See *ibid* eq.45.)]

<sup>24</sup> DR's Mars numerator & denominator not in extant ancient texts, but circulated by DR in 1980, years before realization [1982] of integral period-relation (fn 14). [Note added 2003. This attractively neat Mars theory turned out to be false, since A.Jones discovered (2003 Sept) another perfectly-fitting solution which (unlike DR's) was largely based upon attested numbers. See *DIO* 11.2 (2003) p.30 & ¶4 fn 21.] (For the simple integral-sidereal grandfather Mars period-relation [still valid], see fn 21.)

contrasts between the DR & Muffia solutions (§C3). An optimist will see the positive side here: cases as egregious as this are useful, since there are neophyte power-operators-to-be who (before matriculation) may doubt the blanket-censorship power of academic archons. E.g., fledgling censors may be downcast by the demise of the premodern legendary models (starchambers, etc.). So, when youngsters need inspiration, it takes incidents such as this to get them out of the doldrums into brighteyed anticipation for what can be accomplished by determined manipulating — especially when a juggling 0 is trying to save several of his faces from the wringers they're caught in.

**C5** Some excerpts of DR-O.Gingerich correspondence follow. We begin with the DR 1980 letter that transmitted the *Almajest* planet mean motion solutions to Harvard's O.Gingerich, second-top editor of the world's most consciously prestigious astronomy-history journal, the *JHA*. (The reader may well wonder just how it came about that most of the DR solutions ended up being published, unattributed, in the 1984 *Almajest* App.C of OG's friend, G.Toomer of BrownU: §C15. [Note added 1993: Toomer has now become officially attached to OG's Harvard Hist.sci dep't. Snug.]

To: O.Gingerich, 100 Avon Hill Str., Cambr., MA 02140 1980/4/13  
From: DR

**C6** . . . Regarding the *Almajest* mean motions: . . . I uncovered the actual distance/time ratios on which all the *Almajest* planetary mean motions (synodic) are based. . . Results [using  $1^E = 1$  Egyptian yr =  $365^d$ ]:  
Mercury,  $725^r / (230^E + 62^d)$ ;  
Venus,  $15^r / (24^E - 1^d)$ ;  
Mars,<sup>29</sup>  $(288^r + 4^\circ) / (615^E 155^d 14^h 34^m 40^s)$ ; [found invalid in 2003: fn 24]  
Jupiter,  $325^r / (355^E 63^d 01^h 04^m)$ ; [found invalid in 2003: fn 23]  
Saturn,  $285^r / (295^E 81^d 12^h)$ .  
[Sidenote in orig: . . . the time-spans for Mars & Jupiter are 224630<sup>d</sup>82/135 & 129638<sup>d</sup>2/45, respectively. . . ] Comparing prime-constituents of numerators proves a connection to the ratios given in Ptolemy's *Preface* (*Alm.*9.3) for all but Mars. A similar check of Ptolemy's distance/time figures he alleges were the *observational* basis of the tables' mean motions: failure for all 5 cases. [This is the late R.Newton's discovery.] So Neugebauer's scenario ([*HAMA*] 1975, p.152f) is plain wrong. And *Crime*'s charge [RN] that the mean motions are pre-assumed is verified.

**C7** As noted above, OG was not about to let mere facts impede his upcoming *QJRAS* 1980 & 1981 defenses of Ptolemy, so he simply ignored these findings in both papers. Later, when I attempted to publish my solutions (more in the form of §C3 here, plus fn 17 ancestor period-relations), OG was appointed *QJRAS* referee. After massive computer attempts failed to find a computational flaw in the DR paper, OG rejected it anyway. . . .

as "disreputable" scum (G.Toomer's 1984 *Almajest* p.viii), even while selling, for considerable coin: [a] superfaker C.Ptolemy as "The Greatest Astronomer of Antiquity" (Princeton Institute's O.Neugebauer & Harvard's O.Gingerich, *DIO* 1.1 ¶5 fn 24, ¶6 §H7, ¶7 §B2), [b] a flock of Babylonian astrologers as the "sophisticated" (*JHA* 1.2 §E2) mathematical astronomers behind high pre-Ptolemy Greek astronomy, and [c] its own Muffios as the sole reputable, trustworthy sources of wisdom on these subjects. (I recall a broker who used to brag that his highly-Reputable firm didn't sell penny stocks. He failed to recall that it had recently underwritten & aggressively promoted a stock that, within a few months of issue at \$20, had plunged to ordmag \$1. So a more accurate boast would have been: the firm indeed peddles penny stocks, but its good breeding forbids selling them for anything less than bluechip prices: fn 11 & *JHA* 1.2 fn 172.) As I was saying, Muffios have exhibited a brilliance which DR wholly lacks.

<sup>29</sup> Note DR's 1980 nonrealization (yet) that the denominator also (like the numerator) has 4° of excess over an integral number of revolutions. See fn 14. Since the number of heliocentric revolutions equals the difference between numerator & denominator, the later (1982) discovery was an apparent confirmation of the validity of the 1980/4/13 letter's ratio for Mars — suggesting that the underlying period-relation was simply (using heliocentric revolutions)  $327^r / (615^E + 4^\circ)$ , a relation of exactly the same format as the other four planets' (§C6), but of heliocentric not geocentric design (i.e., numerator is integral in helioc not geoc revs).

(Some details<sup>30</sup> at *DIO* 1.1 ¶1 fn 9, and below here. The results instead later appeared in DR papers in *Bull Amer Astr Soc* 17:852, 1985, and *Amer J Physics* 55:235, 1987.) The OG 1983/7/23 report's justification for rejection was: alternate period-relations could *easily* be found, which would satisfy the *Almajest* planet mean motions just as well as DR's. I called this "pure bluff" and challenged OG to go right ahead and find these solutions: e.g., DR letters of 1983/9/1, 9/27, 11/25. OG kept puffing *anything* that even faintly resembled an alternate parent solution, no matter how bad the fit: see above at §C3, e.g., the [d] solutions.) [Note added 2003. While neither OG nor DR nor K.Moesgaard (fn 26) ever found valid alternate solutions for Mars&Jupiter (OG&KM's don't even fit!), A.Jones did so (2003 Sept): fnn 24&23. (DR gave here the true solutions for the other 3 planet-motions, plus [fn 17] all 5 ancestor period-relations.)]

**C8** It may come as a pleasant surprise to learn that Dr.O.Gingerich is an expert psychologist. After preventing *QJRAS* publication of the DR solutions (*which headed-off public exposure of his own foulup of the same Mars equation in the same journal*), he wrote:

To: DR 1983/8/26  
From: O.Gingerich, Harvard College Obs, Smithsonian Astrophys Obs

**C9** . . . a [PBS] lecture . . . on Beethoven . . . gave a perceptive analysis of why [he] never managed to marry despite being passionately in love over and over again. Some quirk of his character caused him always to choose women who would be unattainable, and the analyst concluded that this was a deliberate subconscious maneuver by Beethoven rather than the repeated cruelty of fate. I am beginning to suppose that there must be an analogy in your own preference to play the martyr rather than getting published.<sup>31</sup>

**C10** Unlike Dr.O (or DR), my wife has a Harvard degree in social psychology. Her reaction to this Hist.sci archon's kook speculation: it bears a remarkable resemblance to other cults' standard alibis for human misbehavior. Psychoanalysis has made a similar contribution to criminal law: criminals aren't guilty, they're just victims . . . . (Result: our streets are as ethical as our scholars.) Thus the unstated essential Hist.sci objection to R.Newton's book, *The Crime of Claudius Ptolemy* (Johns Hopkins Univ 1977): in Hist.sci, there is no such thing as crime! Except using the word "crime".<sup>32</sup> (See §B2.)

**C11** While viewing a 1976 award ceremony for a man who had suffered years of persecution (for a very decent though unpopular cause), a reporter whispered to me: "people like that thrive on suffering". (And I thought I was cynical.) But, at least this reporter was not himself trying to inflict it.

**C12** Thanks to his superarchon-serving gossip-slanders, his gofer-manipulations, his pseudo-refereeing, and his mental & ethical limitations, O Gingerich has probably caused me more needless bother than any living (2nd-level) archon — and he claims I *seek* such impedimenta? (If he really believes this, then: why are his libels spread behind-the-back? — e.g., the wildly defamatory private OG letter<sup>33</sup> quoted in *DIO* 1.1 ¶fn 20.) Evidently, it

<sup>30</sup> OG's 1983/7/23 referee report concluded (p.4) with a delicious slip of phraseology (emph added): "I have . . . put in an inordinate amount of work *in order to convince myself* that . . . [DR's] conclusions are still [??] largely unfounded." Intentional or unintentional confessions of referee bias (whether conscious or not) are so unusual that this deserves preservation.

<sup>31</sup> In fact, DR was publishing lots at this time, though not in the *JHA* or any other place OG could influence through his private libels against DR & Johns Hopkins physicist R.Newton, — libels which go back to the mid-1970s, when there was certainly no unkind behavior towards him from DR (or RN). For OG, it has always been merely: damning skeptics RN & DR will curry favor with useful archons, so it's worth the gain, no matter how slimy the muck.

<sup>32</sup> This Alice-in-Wonderland inversion reminds one of Tom Lehrer's wry comment that, when he was a boy, there were certain words you couldn't say in front of a girl; but, now, the only word you can't say is: "girl".

<sup>33</sup> See *J. Hysterical Astron* 1.2 fn 2. Gingerich had the brass to treat the party, who gave DR this libellous letter, as if *he* were the offending party in the matter & OG had done nothing amiss! The hyperinsensitivity so perfectly

helps suppressors of his stripe explain their behavior to colleagues, if they give it out that the object of their connivings<sup>34</sup> is not actually engaged in principled resistance to tyrannical censorship, but is a nut who subconsciously *invites* mistreatment. The implicit amorality (internal & projected) is suspiciously typical of Hist.sci archons' general approach (§C10) to all scholarship, past & present.

To: O.Gingerich A-209, SAO, 60 Garden Str., Cambr., MA 02138 1983/8/31  
From: DR

**C13** . . . classic: abuse a dissenting scholar, increase the dosage if he doesn't agree to take it quietly; and then, when he reacts, accuse *him* of abuse — and of wanting abuse when he knowingly chooses the honorable route of not agreeing to go along with this political filth.

**C14** . . . [yours is] the fiercest refereeing assault on a paper I've ever heard of (revealing in itself) . . . . Your 1981 *QJRAS* [paper (Gingerich 1981)] . . . and 1983/7/23 [rejecting referee-]report's explanation ([Ptolemy's] observations)<sup>35</sup> fails to reproduce these motions for all 5 planets . . . . So your explanation ([based upon] false mathematics: simple division[!]) is published in *QJRAS* [Gingerich 1981], and [now] you of all people recommend that my correct mathematics not be published [*in the same journal*]. That's fair.

**C15** My final letter to OG on this matter started from one of funniest pinnacles of the entire improbable history of the Ptolemy Controversy, namely, that a flock of Hist.sci's leading archons had for years been publishing false (non-fit) solutions of the *VERY SAME Almajest* planet mean motions. The institutions & publishers whose pages or leading officers have promoted this extended pretense include: Princeton Institute, BrownU, Harvard, Springer-Verlag, Royal Astronomical Society, *JHA*, *Centaurus*, *Arch Int Hist Sci*. In several prominent cases (fn 38), including OG's own Mars solution (fn 19), the reader was given to believe that careful mathematical check-matchings (mere *arithmetic* I must emphasize) had occurred, when this was not true. The point is crucial to the Ptolemy debate since the Muffia solutions were based on Ptolemy's purported method of computing the mean motions, which were lies<sup>36</sup> in all 5 cases. (Even Toomer 1984 p.672 now agrees that Ptolemy was "not of course justified in concealing [the mean motions' sources] from his readers". Yet Toomer conceals from his own readers that the accursed DR discovered the perfect-fit solutions which Toomer grudgingly reviews on his previous page!)<sup>37</sup> The modern imposition began with pseudo-checked statements<sup>38</sup> that Ptolemy's

exemplifies power-insulated archonhood, that the charge of insanity merely reflects. And: 0 slanders others as "paranoid" (*DIO 1.1* ¶1 fn 20), while believing part of Beethoven conspired against the rest of him?

<sup>34</sup> If this word seems too strong, just keep reading the raw material that continues to appear in *DIO*.

<sup>35</sup> Arc/time ratios, based on alleged observations reported in the *Almajest*: items [c], above in §C3.

<sup>36</sup> This strongly indicated that Ptolemy's observations (allegedly the source of the mean motions) were in fact faked or fudged from those very mean motions. For a new and ironclad proof that Ptolemy possessed the mean motion of Mercury before his alleged 139/5/17 "observation" (which he supposedly founded this motion upon!) see Rawlins *Amer J Physics* 55:236 item [5]. Muffia-circle assent here at ¶2 §H14 (ref to §H9).

<sup>37</sup> Another ancient-modern analogy (on suppression) has been remarked at *DIO 1.1* ¶7 §G4-§G5 & fn 17.

<sup>38</sup> Pedersen 1974 p.308 eq.10.17 (Venus), p.270 eq.9.5 (Saturn), pp.296-297 eqs.10.1a (Venus) & 10.3 (Mercury); Neugebauer 1975 pp.151-152 eq.6 (Saturn) & n.25 (Saturn), p.157 eq.2 (Venus). See DR comments here at §C15, also at *DIO 2.1* §J7 & at *Amer J Physics* 55:235 n.30 (1987). DR has exposed the *leading* Muffiosi at precisely this sort of Ptolemaic fake-math, time after time after time after time . . . . The reader is urged to examine also the similar Muffia math-forcings exposed at *DIO 1.1* ¶5 fn 7 (NCSwerdlow, UChicago), *JHA 1.2* §J4-§J5 (Neugebauer, BrownU & Princeton Institute), *DIO 1.3* fn 199 (ditto), & *Amer J Physics* 55:235 n.35 (Gingerich, Harvard). [Note added 1993: see also *DIO 2.3* ¶8 §C12 (NCSwerdlow, UChicago).] This record presumably explains Muffiosi's peculiar sympathy for the more longterm-successful prankster, Ptolemy. (Both parties' chief thrill: forcing preconception upon uncooperative data.) For still further Muffia struggles with simple arithmetic, see *JHA 1.2* fn 24 & §G9. (Note: back when Muffiosi imagined they had mastered arithmetic, that field was favored as a presumably safe arena for Muffia holy wars. E.g., Swerdlow's assault on E.Rosen, quoted at *JHysterical Astron 1.2* §G9: "Even addition & subtraction pose problems.") Yet, while the prestigious *JHA* (\$134/yr to institutions) *remains silent to all examples*

claimed source (observed arc divided by corresponding observed time interval) was the mean motions' common source, out to explicitly stated full sexagesimal precision. When informed otherwise by RN & DR, Gingerich ran for cover by expressing his solutions only out to half the places required for a full check (fn 19) — and did so decimally,<sup>39</sup> not sexagesimally (Gingerich 1981). Finally, Toomer 1984 (App.C) admits the nonequality (& does cite R.Newton for being "almost correct" here: the nicest thing any Muffia capo ever said about RN) and acknowledges Ptolemy's deception; but simultaneously Toomer memoryholes (to this day): [a] the insufferable source of the solutions that fit perfectly, and [b] the Muffia's own previous persistent mis-statements (fn 38 & Toomer *Arch Int Hist Sci* 27:137; 1977, pp.144-145).

**C16** The longstanding Muffia mean-motions farce raises a question (see also ¶1 §A7) that goes to the heart of why centrist academic publications exist: is anybody actually *reading* the papers published in these handsome, extremely expensive journals? (No wonder Hist.sci publications are not famous for extensive correspondence columns: *DIO 1.1* ¶5 fn 24.) Note ¶4 fn 65 or *JHA 1.2* §J2 (item# 2). [1] Far too often, the authors aren't checking their own work. [2] The journal "editors" aren't reading much of anything. [3] Invisible alleged referees are letting the most obvious blunders go to press.<sup>40</sup> (E.g., the *JHA*'s Winter Equinox: *DIO 1.2* §B4.) And [4] the readership isn't noticing them either.<sup>41</sup>

To: O.Gingerich A-209, SAO, 60 Garden Str., Cambr., MA 02138 1983/11/25  
From: DR

**C17** . . . For years, you and yours have published (1974-1981,<sup>42</sup> including in *QJRAS*) solutions for the *Alm.* planet mean motions which are not only non-unique (your unsupported charge against my paper's solutions), they are false. (Simple division. Awesome experts here. . . .) Yet on a flimsy *suspicion* of non-uniqueness (your [1983/ 7/23 *QJRAS* referee report's crux), you are prepared to deny my right to publish<sup>43</sup> the first admittedly accurate solutions anyone has produced in 1800<sup>y</sup> — & you then have the cake-topping gall to suggest I'm a mental case when I remark that this screaming contrast indicates that there is something inequitable about the refereeing process. (May I ask

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*of Muffia botched math*, it has lavished dozens of its extremely handsome pages attacking the most trivial imagined shortcomings in RN-DR's work — including special entertainment abuse-obligato, sung for by Noel Coward Swerdlow. Contrast: if RN or DR had ever in their lives, even *once*, pulled a fudgesicko stunt of the sort repeatedly exposed here as Muffia currency, the *JHA* would devote an entire issue to applying standard Muffia labels, sampled at *DIO 1.1* ¶1 §C7. Note: the *JHA* Editor-for-Life and #2 Editor OG are both highly regarded at the Royal Astronomical Society, the Historical Astronomy Division of the (otherwise rather levelheaded) AmerAstronSoc, & the International Astronomical Union. Indeed, the IAU gave the *JHA* its written blessing at birth: *JHA 1.1*:3-4, 1970/2. Thus, in the absence of the slightest subsequent public disclaimer, one assumes that *JHA*'s renowned editorial balance meets with the approval of these bodies.

<sup>39</sup> D.Fowler, in *Isis* (1983), has correctly criticized DR for similar anachronism. (DR publicly acknowledged his failing, though it was harmless in that case. However, OG's similar 1981 sloppiness is essential to maintenance of his entire mean-motions pretense; thus, his negligence has never been owned up to, at least publicly.) So, having published such a criticism of DR, will our brave Hist.sci journals now print a criticism of OG for the very same lapse? . . . (Such criticism is hereby submitted to them, as one may see from our *DIO* publisher's statement.)

<sup>40</sup> *DIO-J.Hyster Astron 1.1* ¶8 §E8. As predicted at fn 29 there, the Royal Astronomical Society isn't talking publicly about it, but its childish *QJRAS* Comet Halley disaster (exposed at *op cit* §C1&§E1) seems not to have been picked up during 5 years of purported reading by any of *two thousand* RAS Fellows, all of whom automatically receive the *QJRAS*. The most superficial scanning by an astronomer would immediately have revealed the paper (written by RAS Vice President David Hughes) to be entirely miscomputed. The RAS has solved its embarrassment with Royal schiz-integrity: it refuses to acknowledge *DIO*'s existence. Score this one: childish-squared.

<sup>41</sup> See not only the previous fn, but the mass of Hist.sci prestige-journal howlers revealed throughout *DIO 1.2* All of which suggests questions each Hist.sci scholar might well start asking himself, while brankissing his way up the ladder to "prestige" journal-publication: am I suffocating my decent self in pursuit of an empty goal? I.e., what is worthwhile in being published by a sham journal? Should I switch into a more honest profession? Like wrassling.

<sup>42</sup> Sources: §C15, fn 38; *JHA 1.2* fn 56.

<sup>43</sup> Thanks to OG's barriers, his pal Toomer published the 3 most unevadable of these 5 solutions before DR (though not before RN). Isn't there another Tom Lehrer lesson? — on the secret of success in mathematics . . . .

that you cease dabbling in shrinkoanalysis until you've mastered skills closer to home, such as arithmetic?)

**C18** When challenged to make good on the suspected non-uniqueness allegation (i.e., to *produce* your fantasized alternate solutions), you can't cut it and are finally (11/14 — 10 months and stacks<sup>44</sup> of computer readouts after your receipt of the paper in mid-Jan) reduced to pleading that you have not had enough time (!) and that you need my help. . . .

**C19** A review of your review's shiftiness:

[1] You say (1983/2/18) that . . . [you're checking] the paper's computations and add (3/17) that you're doing so with a "fine-tooth comb" for the *QJRAS* — and I respond with grateful encouragement (3/22).

[2] Months later, this assault has failed to find a single miscomputation, so [without warning or consulting DR, ere sending the ref report to the *QJRAS* you suddenly] recommend the paper's rejection (7/23) because: maybe the solutions aren't unique. Maybe, mind you.

[3] I brand this "pure bluff" and challenge you find *any* of the alternate solutions you've alleged (7/23, 8/26) were the determining factor in your rejection-recommendation.

[4] More months later, you've still discovered not a one, so now you claim (11/14) that you're too busy to search for them — & you supply no estimate of how long you'll stay "busy".

**C20** Are you representing the Roy.astr.Soc. or Franz Kafka? The reason your stories have become phantasmagoric nonsense is elementary: you are [a] trying to censor, while [b] hoping to evade the onus of censorship. . . . I'm not going to sit still while you have it both ways. Choose [a] *or* [b].

**C21** Your grossly incredible gyrations have rendered all-too-believable the unsavory hypothesis that . . . . All action since your assignment last winter as referee has merely been a search for a plausible technical alibi . . . [for rejection], an "intensive" (7/23) search that unexpectedly has become a year-long, frustrating, & sinuous<sup>45</sup> ordeal, since the paper turned out to have none of the errors your clique is so dreadfully accustomed to (and accustomed to publishing in its own sloppy output) — and thus quite naturally took for granted would be easy to find in this paper.

**C22** . . . you have revealed yourself nakedly for what others have reported you are. I yet remain genuinely reluctant to believe this. But, for me, this is a simple crucial experiment.

**C23** OG's suppression of this paper (which ended all OG-DR relations) was simply the dowry for a convenient political marriage, as *QJRAS* "Editor" D.Hughes<sup>46</sup> and the *JHA* joined forces — and shut down any further (skeptical) discussion of Ptolemy's fakery at either journal. The trifling cost: the inception of *DIO*. To this day, OG refuses — along with the entire Muffia — to admit the slightest misbehavior in this matter. And the miscalculations — implicit and-or explicit — of Pedersen 1974, Neugebauer 1975, Toomer 1977, & Gingerich 1981 (fn 38, §C17, or *J.H.A.* 1.2 fn 56) have never been publicly acknowledged.<sup>47</sup> Isn't being a power-type wonderful? Reality is so: Arrangeable. . . .

<sup>44</sup> Shown to DR 1983/6/4: *DIO* 1.1 ¶1 fn 9.

<sup>45</sup> Compact DR footnote in original letter: "No rule of law — as revealed by contrasts: [a] comparison to treatment of other papers, and [b] shifting (make-up-the-alibis-as-needed, as-we-go-along) criteria for [rejecting] this paper."

<sup>46</sup> Featured in *J.H.A.* 1.1 ¶8. (O had previously been a severe critic of Hughes' "editing": *loc cit* §B2.)

<sup>47</sup> Toomer 1984 App.C admits Ptolemy's solutions are wrong & cites RN (never DR); but forgets the long history of Muffia-supported false solutions: §C15. [*DIO* always alerts readers to & publicly corrects every one of its occasional errors. Some of the above turned out to be unhistorically erroneous. In 2003, A.Jones found better solutions for Mars & Jupiter: see *DIO* 11.2. This doesn't excuse suppression of the other 3 solutions (& the rest of the 1983 paper, whose revolutionary cyclic thesis Jones establishes firmer than DR), but it vindicates Gingerich's caution on 2.]