

## †8 Current Developments

### A How Double-Sunsets Triggered the Discovery of America

**A1** G.Corriface, star of the 1992 film *Christopher Columbus, the Discovery*: While making the film, “We were on those long boats for *ten hours*, before we finally set foot on the newfound land. And it was quite a relief, let me tell you, because everyone was getting sea-sick.”<sup>1</sup> *Ten hours*. Mmm-Mmm, old Chris C didn’t know how easy he had it.

**A2** By the time Columbus-hype crested on 1992/10/12, every nonhibernating creature on Earth had learned that Columbus made his 1492 journey because he was confident<sup>2</sup> that: [a] the globe was much smaller than it actually was, and [b] the Eurasian landmass wrapped much more around the globe than is the reality. His Earth-size estimate was low by about 25%. It is well known that he got<sup>3</sup> his fateful overestimate of Eurasia’s longitude-spread ultimately from Marinus of Tyre (c.100 AD), whose geographical data underlay C.Ptolemy’s famous *Geographical Directory (GD)*, c.150 AD. Columbus adopted precisely Marinus’ value<sup>4</sup> of  $225^\circ$  for the longitudinal breadth of Eurasia (adding  $28^\circ$  more for Marco Polo’s extension of the knowledge of China, plus  $30^\circ$  more for Japan).<sup>5</sup>

**A3** The geographies of both Marinus & Ptolemy used the famous Earth-circumference of Poseidonios (c.50 BC),  $C_P = 180,000$  stades or 18,000 nautical miles (nmi); this is  $5/6$  of the correct value, which is 21,600 nmi.<sup>6</sup> But: what was the origin of the huge error in  $C_P$ ? — an error so crucial to Columbus’ decision to sail west in search of the Indies. The simple answer is: the error (factor of  $5/6$ ) occurs quite naturally, during application of the “double-sunset” method of measuring the Earth.

**A4** In 1978, when watching a sunset at La Jolla beach, my wife & I noticed that, even after the Sun set on the beach, one could (for ordmag a minute after) see the Sun’s image reflected off windowpanes of houses on the heights. So we began regularly testing the effect (via stopwatch), computing the Earth’s size from the observer’s height  $h$  and the time-difference  $t$  between sunset at sealevel and sunset at height  $h$ . For the simple case where the observation is made at the Equator and at an equinox, the geometrically deduced Earth-radius in km will be  $r = 378000h/t^2$ , for  $h$  in meters &  $t$  in timeseconds. DR wrote up the method for the *American Journal of Physics* 47:126 (1979/2); and, thanks to Jearl Walker, it then appeared in *Scientific American* (1979/5 p.172).

**A5** But the method has a nontrivial flaw. The mathematical result is infected by atmospheric refraction (which the ancients, who lacked quantitative tables, couldn’t correct for): a horizontal ray of light is bent downward (due to the vertical density-gradient of the Earth’s atmosphere), and the curvature of that bend averages about  $1/6$  of the curvature of the Earth itself.<sup>7</sup> An extreme example will show that this effect will artificially reduce an observer’s double-sunset-based estimate (of  $C_P$ ): for, if the atmosphere’s effect were 6 times stronger

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<sup>1</sup> Cinemax promo interview; aired, e.g., 1992/10/13.

<sup>2</sup> Note that Leverrier’s 1846 success in the discovery of Neptune was also born of overconfidence in his theory’s precision: §9 §110.

<sup>3</sup> See D.Boorstin *Discoverers* NYC 1983 p.230. (Also: J.Thomson *Hist. Ancient Geogr* Cambr Univ 1948 p.3.) For a photograph of Columbus’ handwritten comments on Ptolemy, see p.20 of *Log of Christopher Columbus* transl. R.Fusion, Camden ME 1987.

<sup>4</sup> *GD* 1.11.1 (Marinus  $15^h = 225^\circ$ ) vs. 1.14.10 (Ptolemy  $12^h = 180^\circ$ ).

<sup>5</sup> S.Morison *European Discovery of America: the Southern Voyages* Oxford Univ 1974 p.30.

<sup>6</sup> Now rigidly defined as 1,852 m, the naut mi was originally designed to be  $1'$  (gt-circ) on the Earth’s globe; and, naturally,  $C = 360^\circ \cdot (60'/1') = 21,600'$ . For the stade’s length ( $185\text{ m} \pm 2\%$ ), see, e.g., E.Bunbury *HistAncGeogr* (1883) 1:209f & 624, *EncBrit* 23:488H (1961), *Random House Dict* 1967: “stadium” = c.607 ft (185 m).

<sup>7</sup> See S.Newcomb *Compendium of Spherical Astronomy* 1906 pp.198-203.

(i.e., if a horizontal light-ray's curvature were equal the Earth's), then a horizontal light-ray would eternally skim the Earth, and the Sun would never set.<sup>8</sup> Thus,  $t$  would be infinite, and the Earth-circumference  $C$  deduced (from the above formula) would be zero.

**A6** Instead, for the actual Earth atmosphere, the error is minus 1/6. So the double-sunset method will lead to a result equal to 5/6 of the actual Earth-radius, i.e., 5/6 of 21,600 nmi, or 18,000 nmi. As already noted at §A3, the Poseidonios-Marinos-Ptolemy value  $C_P$  equals just this amount!

**A7** There is another way of measuring the Earth's size, namely, the dip-method. At a large height  $h$ , measure how much the zenith-to-horizon angle exceeds 90°. (This method was modernly resuscitated by J.Gerver. See 1979/5 *Scientific American*.) Navigators call this small angle the "dip". Geometrically, an estimate of Earth-size should be inversely proportional to the square of the observed dip. But this math, too, is affected by refraction. Again (as in §A5): if the atmosphere's density gradient were 6 times larger than it actually is, then horizontal light-rays would travel the Earth forever. So, rays coming to any observer would arrive without apparent dip, and computed  $r$  would involve division by the square of zero, yielding infinity. Thus, the Earth would look flat (sphere of infinite  $r$ ) at any small height  $h$ . In brief, refraction inflates a dip-method result. For the actual Earth atmosphere, the result will be high by the factor 6/5, yielding 25,920 nmi = 259,200 stades, agreeing (to c.3%) with Eratosthenes' famous 200 BC estimate,  $C_E = 252,000$  stades = 25,200 nmi.

**A8**  $C_E = 252,000$  stades and  $C_P = 180,000$  stades were the only values widely adopted in antiquity. Their average is exactly correct: 216,000 stades (= 21,600 nmi); but  $C = 216,000$  stades is not attested in antiquity — despite the fact that the famous & laborious "Eratosthenes method" (supposedly entailing direct traverse of arid desert along the straight line between Alexandria & Aswan-Syene), ascribed to him in every modern astronomy textbook, would give this correct value. Obviously, academic-socialite Eratosthenes simply used or adopted the dip-method result (which could have involved merely ordmag an hour's work atop the famous lighthouse, as against weeks of wearing & dangerous travel from Alexandria to Aswan) — not the method for which he has been unjustly immortalized.<sup>9</sup>

**A9** All of these points (providing a double-confirmation of ancient  $C$ -values' dependence upon stay-at-home methods) are set out in Appendices A&B of DR's paper in the *Archive for History of Exact Sciences* 26:211 (1982), communicated to the AHES by the world-renowned mathematician van der Waerden. See also p.259 of DR's *Vistas in Astronomy* 1985 paper (delivered by invitation at the 1984 Greenwich celebration of the 100<sup>th</sup> anniversary of the prime meridian's establishment). Thus, the problem of what went awry in the famous ancient Earth-size estimate  $C_P$  — from which Columbus drew the confidence to sail — has been solved and placed on the record for over 10 years. Nonetheless, no Columbus celebrity-scholar has yet shown the slightest familiarity with the DR solution, despite its series of eminent publications. Perhaps the current interest in Columbus can help. [See DIO 4.1 p.2 & DIO 6 ‡1 fn 47.]

## B Heckathorn Scores Again

**B1** DIO 2.2 presents Ted Heckathorn's sensational discovery of Roald Amundsen's transverse (nonmeridian) observations of the Sun, sextant double altitudes which were shot for determination of azimuth & longitude, in order to aim his immortal 1911 expedition,

<sup>8</sup> Thus, on the surface of Venus (where the atmospheric pressure is 90 times Earth's), there can be no sunrises or sunsets. (Though, for solar altitude  $h = -90^\circ$ , atmospheric extinction would be stone-total.) This point is so unrecognized that W.Kaufmann's wellknown textbook *Discovering the Universe* (NYC 1989 p.135) innocently speaks of Venus sunrises.

<sup>9</sup> Such persons as E.Lehmann-Haupt & C.Sagan depend upon fiddling with the length of the stade, in order to make 252,000 stades seemingly equal the correct figure. (To their great credit, D.Dicks & O.Neugebauer wisely avoided and explicitly rejected following that evidence-twisting precedent.) However, it now turns out (as shown above) that the answer to the ancient mystery of these disparate  $C$  estimates is not metrological but physical.

which genuinely first reached the South Pole on 1911/12/15,<sup>10</sup> 4 weeks ahead of the martyred loser of the S.Pole race, Britain's R.Scott. (Scott died on the return trip, late the following March — just short of a depot.)

**B2** These Amundsen observations have long been prominently declared nonexistent by Cambridge University (1979), the President of the University of Alaska (1983), and the National Geographic Society's hireling "Navigation Foundation" (NavFou) — in its 1989 Report's<sup>11</sup> whitewash of National Geographic's "North Pole" explorer R.Peary, aggressively promoted in the 1990 January *National Geographic*. The NavFou Report lost one leg when the scientific community creditably failed to accept NGS' photogrammetry (e.g., 1990/3&6 *Scientific American*; also *Nature* 344:902, 1990/4/26). So, when Ted's 1991 finding (of Amundsen's longitude data) destroyed the other leg, NavFou-National Geographic's doubly-costly Peary-apology was instantly reduced to pathetic paraplegia.

**B3** The bizarre idea that Amundsen would attempt to reach the S.Pole without transverse observations became popular among Peary-defenders (starting 1983) because the prime navigational oddity of Peary's incredible 1909 N.Pole fable was its lack of transverse solar sextant shots (for steering toward the Pole) — a point made assertively in DR's 1973 book *Peary at the North Pole: Fact or Fiction?* at, e.g., pp.87-88 & 140-143. Attempting to answer DR, the NavFou (1989 Report<sup>12</sup> pp.61-62 and *National Geographic* 1990 Jan p.47) argued: if Amundsen could reach the S.Pole in 1911 without transverse observations, why couldn't Peary reach the N.Pole in 1909 without them?

**B4** Understand: meridian solar observations (noon or midnight) tell a poleward explorer how far forward he has proceeded. Transverse solar observations (morning or evening, preferably in the rough vicinity of the prime vertical) tell him how far to the left or right he has wandered off his intended path.

**B5** Ted found Amundsen's "nonexistent" transverse data right in the Norwegian edition of Amundsen's widely read 1912 book! (Ted bought the book for \$3. What he discerned in it now scuttles a National Geographic report costing ordmag 100,000 times as much.) Page 127 of the Norwegian observationsbook is reproduced photographically at vol.2 p.115 of Amundsen's *Sydpolen*. In the light of widespread current institutional insistence that Amundsen made no longitude observations, it is interesting to read the caption Amundsen prints below the photoreproduction of these data: "A page of azimuth and longitude observations." Ted transmitted the data to DR, who computed<sup>13</sup> the longitudes  $W$  and compass variations  $V$  from the spherical trig formulas which are standard in such work. The match

<sup>10</sup> By Australian dating, which is used in the diaries and observationsbook.

<sup>11</sup> The Report rightly rejects DR's erroneous initial reading of Mrs.Peary's sealed Betelgeux Document — though the NavFou's own solution was also false. The correct interpretation, published for the first time in DIO 1.1 ‡1 fn 14, was verbally assented to in private by the NavFou's rep at the 1991/4/19 Naval Inst debate on Peary; but no NGS acknowledgement of this has been published, nor will it ever be, since *National Geographic* is constitutionally incapable of owning that its critic DR is ever right about anything. (See §6 §F4.) This statement is itself a criticism; and, given NGS' consistent level of neutrality & integrity, one could confidently engrave it in granite. (NGS' degree of openmindedness is as wellknown among serious scholars as is the personal nature of its positive & negative views on any issue. *Nature* 1990/4/26: "the National Geographic Society . . . will always believe [Peary] reached the Pole.")

<sup>12</sup> The NavFou also adduces Cagni's 1900 alleged Farthest (skeptically analysed at *Fiction* pp.65-68) without noting: [a] Cagni never even claimed to have approached the Pole, where steering for the polar point is critical. [b] Cagni did report a transverse observation anyway at his turnaround-point (A.Luigi Osserv Scientif Milan 1903 pp.50-51 obs.#149; cited at *Fiction* pp.65 & 297). [c] Cagni's reported compass variations (cited by NavFou Report p.62 as confirming Cagni's supposed steering by solar culminations) are mistaken by over 20° (as noted for the first time at *Fiction* p.65).

<sup>13</sup> Each DR-calculated longitude  $W$  (at latitude  $L$ ) is good to ordmag a mile. (The chronometer rating supplied by H.Mohn 1915 is adopted. Mohn also provided all of the Amundsen expedition's extensive compass variation  $V$  results, calculated in the field from its Nonexistent transverse solar data.) For 1911/11/9 ( $L = 83^\circ S$ ):  $V = 132^\circ 37'$ ,  $W = 165^\circ 30' W$  (Hanssen);  $V = 134^\circ 32'$ ,  $W = 166^\circ 07'$  (Amundsen). For 1911/11/13 ( $L = 84^\circ S$ ):  $V = 141^\circ 14'$ ,  $W = 165^\circ 12'$  (Amundsen). (The two seemingly discrepant 11/9 longitudes actually differ by less than 5 mi, so near the Pole;  $V$ 's uncertainty is due to the roughness of solar compass bearing observations expressed largely in quarter-points of the compass.) DIO 2.2 also finds that Amundsen approached the Pole nearly parallel to the 164°W meridian. At the Pole, he found that the compass pointed along about 18°W.

with Amundsen's own field calculations shows that he used sph trig. This should interest the Peary contingent, which has insisted for a decade that Amundsen did not "waste time" with sph trig and that the variously-inept R.Scott was a fool for having done so. (Scott's critics seem to imply that he virtually deserved his ghastly death, for the crime of having used sph trig navigation! However, what killed Scott was not overprecise math but [a] censurable failure to anticipate adverse travel conditions, and [b] creditable determination genuinely to reach a Pole — instead of faking it, like some.) Indeed, Scott's lately-much-lampooned navigation is utterly vindicated by the new findings, since (though both explorers would have been better served by cartesian navigation near the Pole), he and Amundsen used sph trig equations of precisely the same form and computational difficulty.<sup>14</sup>

**B6** Since DIO 2.2 was printed, two more finds have only increased our wonder at those purported experts who have declared Amundsen's transverse data to be nonexistent:

[a] Ted points out that, in Amundsen's paper for the *Annual Report of the Smithsonian Institution* 1912 (pp.701-716), Amundsen states at p.713 (emph added): "During the last eight days of our march we had continuous sunshine. Every day we stopped at noon in order to measure the meridian altitude and *every evening we made an observation for azimuth.*"

[b] DR finds that Royal Geogr Soc Pres A.Hinks (whose views are uncomprehendingly promoted by the originators of the idea that Amundsen ignored transverse shots), in his learned 1944 article<sup>15</sup> on the Amundsen-Scott 1911-1912 S.Pole data, states at p.169 that the Norwegian edition displays "facsimile reproductions of observation books".

**B7** I believe future historians will be as puzzled at how these data were overlooked as they will be at the Pearyites' notion of how to steer at a geographical pole.

## C The Jekyll&Hide Defense: I Say, What's Astronomical HISTORY Doing in the Journal for the History of Astronomy?

**C1** In *J.HA* 1.2 & *DIO* 1.3, DR's "Muffia Orbituary" extensively admires the pioneering work of 1991 lead papers in the *Journal for the History of Astronomy* and *Isis*, who are: [i] lodging (& understandably promoting the originator of) Hist.sci's unprecedented discovery of the WINTER EQUINOX<sup>16</sup> — as well as [ii] rewriting the canons of gradeschool arithmetic,<sup>17</sup> in order to promote certain precious Hist.sci tenets.

[a] The prominent *JHA-Isis* articles cited contend that none of three surviving Hipparchos trios of solar observations (*Almajest* 4.11, 5.3&5) can be satisfied by trig-based (Greek style) solar orbits. So, *DIO* 1.3 helpfully supplied all three of the allegedly-impossible orbits<sup>18</sup> — and noted that one of these Nonexistent orbits (satisfying the Hipparchos solar

<sup>14</sup> In *DIO* 2.2 ‡5, compare eq.3 (Amundsen) to eq.10 (Scott). It is strange that the truth of Amundsen's steering method should have become lost and (nowadays) so universally contradicted, since (from working with the original Amundsen observationsbook), Mohr 1915 gave (verbally) Amundsen's computational procedure (*DIO* 2.2 ‡5 fn 17).

<sup>15</sup> *Geographical Journal* 103:160 (1944/4).

<sup>16</sup> *JHA* 22:101 (1991/5) p.119. Not every journal can boast of refereeing which comes up to the *JHA*'s rigorous standards. *Isis'* 1991/9 paper is squarely based upon the prior *JHA* 1991/5 delight.

<sup>17</sup> Readers who possess the advantage of an elementary school education may wish to check the arithmetic found in the govt-funded Muffia paper selected as lead article for the 1991/5 issue of the extremely handsome *Journal for the History of Astronomy*, whose highest-ranking Ptolemy defender is *JHA* co-Editor and Harvard Hist.sci Dep't head O.Gingerich. (And the Muffia author's followup paper led off the proud first Univ Chicago issue of the History of Science Society's *Isis*.) At p.117 of the *JHA* paper's development, we learn: [a]  $128^\circ 35' - 65^\circ 30' = 65^\circ 05'$ , and [b] the solar mean anomaly (increasing at Hipparchos'  $0^\circ .985635/\text{day}$ ) changes by  $67^\circ 2/3$  in  $67 2/3$  days! These adventures in Muffia New-Math are at the very core of the Muffia's attempt to prove that the DR-solved Hipparchan solar trios cannot be solved by Greek trig methods. But this is the *JHA*; so, expect no *DIO*-citing or *DIO*-quoting retraction — despite the aim-to-please publisher's-statement offer appearing at the end of each *DIO* issue.

<sup>18</sup> E.g., the EH orbit (founded upon Hipparchos' earliest adopted solar cardinal points), epoch Phil 1:  $Y = 365^d 1/4$ ;  $e = 228^\circ$ ;  $A = 44^\circ$ ;  $e = 3^p 1/4$ . This satisfies eclipse-trio B of *Almajest* 4.11. The same chapter's equally "impossible" eclipse-trio A is satisfied by a hybrid meld of the EH orbit with the famous PH orbit preserved in the *Almajest*. Also found in *Almajest* 4.11 are the Hipparchos lunar ratios, which have defied 2000<sup>y</sup> of attempts at explanation (from Ptolemy through Muffia capo G.Toomer):  $(327 2/3)/3144$  and  $(247 1/2)/(3122 1/2)$ . In *DIO* 1.3, it is discovered

position trio of *Almajest* 5.3&5) had already been published in *DIO* 1.1 (‡6). This issue had been cited in *Isis'* own sister publication;<sup>19</sup> however, the fact that DR has solved The Unsolvable remains unmentioned in any of Hist.sci's insecure captive journals. (By contrast, *DIO* has received probes from the highest Hist.sci levels, trying to find out if DR is continuing to unleash *DIO-J.HA* — journals which regularly expose Hist.sci archons' amusing attempts at pretending to scientific facility. Despite all this purported interest, not one of the inquirers has yet cited any result published by *DIO*, nor have they evidenced the slightest familiarity with its scholarly contents — meanwhile, they affect bewilderment at why DR isn't taking them very seriously . . . .)

[b] The Ancient Star Catalog of 1025 celestial objects was compiled (*Almajest* 7.5-8.1) by mathematician-astrologer C.Ptolemy (for epoch 137 AD), reported by him as if based on his own observations, though it has been knowledgeably suspected for centuries that Ptolemy stole virtually<sup>20</sup> the whole Catalog from Hipparchos (128 BC).

**C2** The Catalog issue became central when: [a] in the 1976/8/6 *Science*, loyalist O.Gingerich unconvincingly tried explaining-away Ptolemy's solar, lunar, & planetary fudgings by calling them "pedagogic", and [b] DR responded (p.362 of *Publ Astr Soc Pacific* 94:359 = DR 1982C) that this alibi was irrelevant to the Catalog, since over 90% of its stars aren't used in any *Almajest* computation. (Gingerich 1976, following Neugebauer *HAMA* p.284, deemed Ptolemy's star data: real, outdoor, and more accurate than Hipparchos' . . . .)

**C3** *DIO* 1.3 unsystematically savours some of the hilariously fouled-up analyses (still blissfully accepted by Muffia capos as perfectly valid . . . ) turned out by the Neugebauer-Muffia's two leading purported experts on the ongoing Catalog controversy: J.Evans & G.Graßhoff, who have by now wasted hundreds of handsome Muffia-publication pages, fruitlessly defense-lawyering Ptolemy against a passel of persistent proofs<sup>21</sup> of his theft. Note: since the shellshocked Muffia has lately begun uncertainly admitting that maybe some of the Catalog was taken from Hipparchos after all, Muffiosi now usually avoid telling their readers that Ptolemy insists he personally<sup>22</sup> observed the whole Catalog outdoors with his alleged armillary astrolabe.

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that: [a] These ratios were derived (to extremely high trig precision) from a simple trig development (showing that "Ptolemy's Theorem" was known to Hipparchos) based upon eclipses A2&A3 and B1&B2 (ignoring A1 and B3), with the presumed lunar elements: mean-anomaly-at-epoch =  $82^\circ$  and mean-longitude-at-epoch = Ptolemy's  $178^\circ$ . (Both values probably from Aristarchos.) [b] The trio A Moon distance is based on Aristarchos' famous  $3^\circ$  value for the half-Moon's angular distance from quadrature, combined with his school's use of an Astronomical Unit of 1000 parts, so that  $r_M = 1000^d \tan 3^\circ = 52^\circ 24' = 3144'$ ; trio B's distance is merely this same value, affected by one of the most common ancient scribal slips,  $r_M = 52^a 1/24 = 3122' 1/2$ . Further *DIO* 1.3 analysis is based upon Swerdlow's solid and original 1968-9 discovery of another Hipparchos value,  $r_S = 490$  Earth-radii. This DR article also begins *DIO*'s demonstration that all 3 of the astronomical distance estimates (surviving via pseudo-Aristarchos, Archimedes, & Poseidonios) — including the famous half-Moon experiment — of the school of Aristarchos (who wrote on light & vision) are based upon his correctly setting the limit of raw human ocular discernment at  $1/10000$  radians.

<sup>19</sup> *Hist Sci Soc Newsletter* (1991/7) p.35.

<sup>20</sup> *DIO* 11.3 ‡6 §C2 will contend that all 5 of those exceptional Catalog stars, whose longitudes end in  $15'$ , were fudge-tailored by Ptolemy when he rigged longitudes of Venus and the eclipsed Moon, longitudes which he pseudobased upon real astronomers' observed conjunctions of the bodies with these stars. (The 5 stars' longitudes therefore were not obtained merely by adding  $2^\circ 40'$  to Hipparchos' longitudes.) The 5 stars (& conjunctions):  $43\zeta$  Gem (Venus  $140^\circ 7/29-30$ ),  $15\eta$  Vir (Venus  $-271/10/11-12$ ),  $76\theta$  Vir (Moon  $-134/3/20-21$ ),  $49\gamma$  Vir (Moon  $-719/3/8-9$  &  $62/3/13-14$ ),  $29\alpha$  Psc (Moon  $90/8/28-29$ ). Three of these cases are attested (*Almajest* 10.1&4, Neugebauer *HAMA* 1975 p.295 n.23), and the  $720$  BC eclipse is a Ptolemy favorite (*Almajest* 4.6-9, 6.9). [See *DIO* 6 fn 107.]

<sup>21</sup> A list of proofs against Ptolemy's authorship of the Catalog is provided at p.236 of DR's 1987 paper (*Amer J Physics* 55:235), a paper not cited by NCS's 1992 *JHA* effort — even when elementary honesty requires it, e.g., at the p.181 discussion of the origins of the planetary theories' longitudinal & latitudinal parameters. See DR paper at p.236 item 5 & p.237 and fns 27, 30, and the entirely original remark on latitudes at fn 38, which directly bears on the Swerdlow discussion of the *Almajest* latitude theory at his p.181.

<sup>22</sup> I commend NCS for not omitting this. He fairly describes Ptolemy's account early on (at NCS 1992 p.174). However, he later fails to note the key consequence: if Ptolemy got the Catalog from Hipparchos, then calling Ptolemy a liar is not simply "impolite" — it communicates an important fact of astronomical history. I urge *DIO* readers to consult Swerdlow 1992 p.174 or *Almajest* 7.4 for the elaborate details of Ptolemy's Catalog pretense. See also *Almajest* 7.2 & 5.1.

**C4** Now, at *JHA* 23.3:173-183 (1992/8), comes forth our favorite Muffia comedian-entertainer-satirist, Noel Coward<sup>23</sup> Swerdlow (Univ Chicago Dep't Astron, & Advisory Editor for the extremely handsome *JHA*), to pronounce his judicious<sup>24</sup> quietus upon the Catalog controversy, in a pseudo-delayed paper called "The Enigma of Ptolemy's Catalogue of Stars". Indeed, NCS thinks his remarks are so smart and valuable that they should be the last word on the subject! (See NCS 1992 p.182.) And he calls *skeptics* demented?

**C5** NCS still hides from debating skeptics. But Mr.Hide is also Dr.Jekyll. Evidently sobered by *DIO* 1.1, NCS has muted his previous abusive style (e.g., calling dissenters' work "crazy", "garbage", etc: fn 23). Now, donning (publicly) the mask of Jekyllian civility, he even poses as an arbiter of academic etiquette! (Humor-wise, that's on the level of appointing Nixon to clean up dirty political campaigning — or placing *DIO* in charge of good taste & style in academe.) Who else but the incomparable Lord Hoskin-O Gingerich *JHA* Editorship would select NCS (whose Hyde-side applied to the late RN the unretracted & *Hist.sci-uncriticized* libels "liar", "crank", & "con-man": *DIO* 1.1 ¶3 §D) as the ideal *JHA* AdvEd to call RN & DR insufficiently polite?! (See it happen, at NCS 1992 p.176.)

**C6** Presumably to establish that there's a "New Swerdlow", NCS even admits — for the first time in print anywhere, folks — that DR exists. But we mustn't expect too many concessions; so it shortly transpires that, even though there is a DR, it is fortunately still true that DR, like fellow skeptic R.Newton, has made *no contributions whatever* to the field of ancient astronomy. (Understand: the maintenance of this principle has become a prime cohering tenet for Muffiosi. Remove it, and their comfortably insulated little mental world would splinter.)

**C7** Skeptics, starting with Tycho Brahe (who also faked a bit of his own justly famous 1598/1/2 star catalog),<sup>25</sup> have always suspected that Ptolemy observed no stars but instead copied Hipparchos' star catalog, merely adding  $2^{\circ}40'$  of precession (mistaken by  $-1^{\circ}.1$ ) onto Hipparchos' longitudes. In his Muffia-hated 1977 Johns Hopkins Univ book, *The Crime of Claudius Ptolemy*, physicist R.Newton revealed a startling fact: though the unaltered star latitudes exhibit the expected excess of  $0'$  endings, the longitudes' most common ending is  $40'$  — exactly what one would expect if an indoor astrologer had added

<sup>23</sup> It should be clearly understood that *DIO*'s nickname for Swerdlow is strictly based upon his Noel-Cowardsque talents as humorist — talents displayed, e.g., in *Isis & JHA* (sampled in *DIO* 1.1). His courage is unquestioned, since he and his Muffia friends have been brave enough to call Ptolemy-skeptics loony and incompetent for  $20^{\circ}$ , while avoiding face-to-face public debate: *DIO* 1.1 ¶1 §C7 & ¶3 §D3.

<sup>24</sup> At p.180, NCS 1992 affects neutrality (a repeat of his equally honest 1981 pose, cited at *DIO* 1.1 ¶3 §D7), by saying how glad he is that he has never previously published anything on the Catalog which he might wish to defend! Uninitiated readers are not told that Swerdlow has a huge stake in the larger Ptolemy Controversy, having for years told everyone within hearing that Ptolemy-skeptics were nuts, fools, & crooks. He cannot now admit his error without losing face disastrously. In this connection, NCS & the Muffia are so shackled, by a consistently repulsive past, that their commitment has become yet another limitation — upon persons who in some cases were limited enough to start with. (By contrast, DR has always written admiringly of valid Muffia scholarship, and so enjoys a resultant noninterest in automatic denigration of the output of these self-created Enemies.)

<sup>25</sup> *DIO* 2.1 ¶4 shows that Tycho's largely magnificent epoch-1601.03 catalog of 1004 stars (hitherto neither numbered nor even counted by *Hist.sci*) contains 10 fake stars: the first 6 stars of Oph, Tycho's stars D675-680 (entirely invented); and all 4 stars in Cen, Tycho's stars D1001-1004. (The Cen set was computed from fake longitudes & real declinations — the latter probably observed at Wandsbeck, not Hven). Tycho's method of fabrication was essentially the same one, which the Tycho catalog's preface accuses Ptolemy of "usurpation" for using: just add a precession constant onto the longitudes of a predecessor's star catalog, while not changing the latitudes. (Tycho: for Oph, add  $24^{\circ}$  to Hipparchos' longitudes; for Cen, add  $21^{\circ}$  to Ptolemy's longitudes.) Then, unlike heedless Ptolemy (whose data were invariably rounded to  $5'$  or  $10'$ ), Tycho tossed in a very few arcmin of scatter, so that the "observations" would look real. (Tycho always rounded to arcmin or simple fractions thereof.) These 10 stars' errors are gross by Tycho's standards but they agree very closely with fabrication — and they are from the sole 2 subsets of his 227 final-rush 1596-7 stars for which no underlying data survive. Curiously, Evans' 1987 *JHA* paper believed (p.168) that the 4 Cen stars were real, because he neglected to apply to them this same weighty *JHA* paper's own laboriously-developed extinction formula: Evans 1987 pp.259-260, 267-271. Use of Evans' formula produces magnitude 7.95 for 2g Cen at Tycho's Danish observatory! As further shown at *DIO* 2.1 ¶4 fn 65, the massive Evans-JHA attack on DR also includes (Evans 1987 p.168) claims which require naked eye observations of stars as dim as *tenth magnitude*, by Evans' own formula. Well, 10th magnitude may be dim; but, if we were to assign a magnitude to the brilliance of refereeing at the extremely handsome *JHA*, could a mere 2 digits do it justice?

$2^{\circ}40'$  onto Hipparchos longitude data originally having the expected excess of  $0'$  endings. **C8** NCS starts his attack on the famous fraction-endings proof with the astonishing claim that RN's argument had assumed (NCS 1992 p.176) "an instrument graduated to half-degrees". This statement is false. (Expect no *DIO*-citing retraction.) And it suggests why NCS has such passionate sympathy for the exposed plagiarist C.Ptolemy. The NCS misstatement that RN assumed half-degree division is identical to the same error originated by G.Graßhoff *History of Ptolemy's Star Catalogue* Springer-Verlag 1990 pp.85, 88 (n.172!), & 162 — a book which NCS (1992 p.180) deems "very acute" (a description NCS does not apply to any analysis by RN or DR). The truth is given at RN's *Crime* p.247, which argues from evidence that "the circle was probably graduated only in degrees, rather than in intervals of  $30'$ ." (Graßhoff read only the RN statements<sup>26</sup> on pp.246 & 252, missing those on pp.247 & 255.) Throughout the long history of this controversy, no one else (besides Graßhoff & NCS) has ever made such an error. (Not even a scholar of O Gingerich's intelligence & reliability.) For anyone who's at home with the English language (& one must mercifully note that Graßhoff is German), it is impossible to read RN's discussion and misunderstand the point — *central to RN's entire analysis* — that RN is proposing whole-degree graduation of the astrolabe's longitude and latitude circles.

**C9** Nonetheless, the section of NCS's paper containing his amazing, unoriginal false allegation (that RN assumes half-degree graduation) is *cited*<sup>27</sup> to RN's book, not Graßhoff's. This must be what O Gingerich has in mind when he speaks fawningly of NCS' "meticulous scholarship" (*JHA* 23.2:149, 1992). In an equally "meticulous" 1979 book review — appearing in the journal of Phi Beta Kappa! — NCS falsely imagined he had detected RN's use of an uncited source. So he of course sneeringly imputed dishonest motives to RN. (See NCS at *American Scholar*<sup>28</sup> 1979 p.528. The elementary fatal error underlying NCS' assault is exposed at *DIO* 1.1 ¶6 fn 6.) It is particularly ironic that this NCS attack on RN's integrity was published as part of his review of RN's 1977 book (*Crime*) — the very book we now find him unfamiliar with THE central novel argument of! This exquisite longterm serial-embarrassment not only enhances (in the Muffia's special way) the reputation of Phi Beta Kappa, but even more so the *Journal for the History of Astronomy*, where NCS is a proudly-displayed & perfectly-apt Advisory Editor — and where his equally strange review of another of RN's Johns Hopkins University books appeared, a review which showed (as an incredulous RN demonstrated in *DIO* 1.1 ¶5) that NCS did not even understand the purpose or the very *title* of the book. . . . (I don't believe that NCS has yet, in his standard genteel fashion, attacked some poor unoffending nonMuffia scholar for fake book reviewing. But we NCS fans are all looking forward to the day when he does.)

**C10** NCS next turns his evenhanded analytical ability upon DR's extensive statistical analysis (Rawlins 1982C) of the Ancient Star Catalog (which appeared in an astronomical journal,<sup>29</sup> not a historical one). With admirable succinctness — even before he finishes

<sup>26</sup> RN 1977 starts his argument by looking about for an explanation of the fractional distribution of the latitudes; in this spirit, merely for the sake of testing, he momentarily hypothesizes (p.246, emph added): "*Suppose* that a circle is graduated at intervals of  $30'$  . . . ." However, even before the next page is finished, RN has discarded the half-degree hypothesis (due to  $0'$  endings outnumbering  $30'$  endings in the latitudes) and repeatedly states subsequently that his analysis assumes whole-degree division.

<sup>27</sup> NCS 1992 n.14.

<sup>28</sup> Just as O Gingerich was a top Editor at the *JHA*, when it published NCS' insulting 1981 attacks on RN (sampled at *DIO* 1.1 ¶1 §C7); so also he was on the Editorial Board of *American Scholar* when it published NCS' 1979 diatribe. O pretends to abhorrence of "abusive" writing — but does O seriously believe that merely using others to carry out his intents will successfully divert intelligent onlookers from recognizing the midlevel archon responsible for the low nature of the gang attack on RN's findings? O is certainly not fooling DR, who knows from a private 1974/11/15 meeting with O (at Goddard) that, even that far back, O was personally acting as a Muffia agent — verbally diffusing the Muffia slander that RN, an internationally respected physicist & a section chief at the Johns Hopkins University Applied Physics Lab, was simply a crank. (This rumor was spread so aggressively by Muffia&clones, behind RN's back, that it was accepted for awhile even at *Scientific American*, from whose people I heard it in 1978-9. Why shouldn't Muffiosi duck open debate, if academe lets them get away with fighting opponents this way instead?)

<sup>29</sup> *Publ Astr Soc Pacific* 94:359; 1982/4.

the same page on which he revealed his unfamiliarity with RN's book — NCS has already demonstrated that he is touchingly deficient in the ability to trace the math of DR 1982C. The DR paper contained a hitherto-unsuspected crucial-experiment revelation: if Ptolemy observed the Catalog with his  $-1^\circ.1$  longitudinal mis-set, this would also cause<sup>30</sup> error-waves throughout the Catalog:  $\Delta\lambda = 29'\tan\beta\sin\lambda$ ,  $\Delta\beta = 29'\cos\lambda$ . But these (HUGE) waves are not found in the Catalog, thus (to an uncommitted critic) it is obvious that Ptolemy did not observe the Catalog. Predictably unwilling to accept that this simple test proves anything, NCS states (1992 p.176) that  $\Delta\lambda$  would "be undetectable for stars within, say,  $25^\circ$  of the ecliptic and [would] produce nonsense for stars within  $25^\circ$  of the poles." Hilariously false on both counts.<sup>31</sup> (By its account, the *JHA* had *a year* to referee NCS 1992: fn 59.)

**C11** Having alibied the longitude problem to his satisfaction, NCS next tries explaining away the latitude-error  $\Delta\beta$ , the expression for which is so much simpler (than  $\Delta\lambda$ ) that one might hope for a more capable outcome. Alas, even it defeats NCS (*Archive Hist Exact Sci* Ed Brd!); he states (NCS 1992 p.176) that this error "appears to be absent from the catalogue although Peters had already found a more complex error for zodiacal stars, very roughly of  $\Delta\beta = 20'\cos(\lambda+35^\circ)$ ." (This equals  $16'\cos\lambda - 11'\sin\lambda$ .) DR's comments:

[a] Peters has provided the Catalog's standard latitude error curve via 36 normals, spaced at  $10^\circ$  longitude intervals around the zodiac. But Peters does not state the formula<sup>32</sup> which NCS permits the reader to believe was Peters'.

[b] As a sinusoidal fit to the Peters error curve, NCS' formula is grossly mistaken.

**C12** NCS has simply eyeballed his  $\Delta\beta$  sinusoid from the graph of either DR or Evans — since both lack the full detailed grid of Peters' own graph. From the original Peters graph, it is obvious that the (misleadingly asymmetric) peaks of the Peters  $\Delta\beta$  curve are precisely  $40^\circ$  from the equinoxes, not  $35^\circ$ . (The difference is not large but it unambiguously reveals the effect of "neutral" NCS' desire to wrench the phase nullward, in order to exaggerate the cosine-term's coefficient, hoping to get it as near as possible to NCS' desired  $29'$ .) No one consulting the original Peters curve could possibly get this wrong. (Indeed, I don't see how anyone could get  $35^\circ$  even from the Peters-curves published later by DR 1982C p.366 & Evans 1987 p.252. Readers who wish to measure raw Muffia prejudice at work ought to check them or Peters' original graph — in order to appreciate fully the NCS mental effort

<sup>30</sup> See analysis at Rawlins 1982C p.361 & Fig.2 there.

<sup>31</sup> Regarding the first NCS claim: basic amplitude =  $1^\circ \cdot 1\tan\epsilon = 29'$  (where  $\epsilon$  = obliquity), so for  $\beta = 25^\circ$ , the wave  $\Delta\lambda = 29'\tan 25^\circ\sin\lambda = 14'\sin\lambda$ , which (with gt-circ amplitude  $12'$ ) is far from undetectable in this context, as the merest glance at the wellknown Peters error curves will make clear. (At least NCS does not go as far as Graßhoff, who, at p.167 of his 1990 Springer-Verlag book: [a] confuses Ptolemy's proposed  $2^\circ 2/3$  precession-since-Hipparchos with the real  $3^\circ 3/4$  precession over this timespan, and [b] claims that the waves produced by such a longitude mis-set — of amplitude  $3^\circ 3/4 \cdot \tan\epsilon = c.100'$ ! — constitute "a small periodical error . . . so small for both coordinates [ $\lambda$  &  $\beta$ ] that it cannot be significantly tested." Amplitude isn't Graßhoff's only Waterloo: his work also includes a key error in phase of *precisely*  $180^\circ$ . It doesn't get any better. What BrownU talent-scout did Springer-Verlag employ to locate such Muffia expertise?) As for the latter NCS remark: it is based upon National Science Foundation-grantee Swerdlow's untutored impression that, since  $\tan\beta$  of course approaches infinity as  $\beta$  tends to  $90^\circ$ , then the above  $\Delta\lambda$  wave expression will become impossible to deal with, as one nears an ecliptic pole; but anyone of experience in this work knows that the real error here is great-circle measure (see the weighting discussion at DR 1982C p.366) — and for this we have  $[\cos\beta]\Delta\lambda = 29'\sin\beta\sin\lambda$ , which merely goes to  $29'\sin\lambda$  near the N.ecliptic pole. (Opposite at the S.ecliptic pole, which is of course not included in the Mediterranean-based Catalog.) To a scientist with even modest ability in spatial relations or possessing merely undergrad familiarity with the peculiarities of a spherical coordinate system near its axis, this result would be self-evident. If I may offer a slightly shocking suggestion to the *JHA*: could it try to find reviewers who are capable of performing the math they are purporting to analyse? Is that really too much to ask? Well, in a Hist.sci community that takes Muffiosi seriously, maybe it is.

<sup>32</sup> C.H.F.Peters *Vierteljahrsschrift der Astronomischen Gesellschaft* 12:296-299 & 341 (1877), p.299. The translation at C.Peters & E.Knobel *Ptolemy's Catalogue of Stars* (PK) Carnegie Inst 1915 p.8: "the [C-O] curve of errors in latitude has a maximum near to [ $\lambda = 140^\circ$ ], and a minimum near  $320^\circ$ . (Peters' 1877 graph of the errors  $\Delta\lambda$  and  $\Delta\beta$  is reproduced on PK p.6.) Peters does not attempt to fit sinusoids to his curves; if he had, he would not have arrived at a latitude curve phase anything like NCS' — since, though the irregular curve happens to exhibit extrema about  $40^\circ$  from the equinoxes, nonetheless, the bulk of each billow of the latitude curve is more nearly  $60^\circ$  from an equinox.

that went into his purely invented  $35^\circ$  phase.) Yet, strangely enough, NCS 1992 n.7 cites<sup>33</sup> Peters-Knobel 1915 (where the original Peters graph, with grid, is reproduced at p.6) as a source for the NCS 1992 paper. But the most uncomfortable question here is: after *over a decade* of silence,<sup>34</sup> while now finally attempting to reply to *THE* most airtight argument<sup>35</sup> ever raised against Ptolemy's authorship of the Catalog, why is NCS — a member of Univ Chicago's Astronomy Dep't (and recipient of NSF & MacArthur-genius grants) — *eyeballing* a sinusoidal fit? Badly. One might expect such an eminent scholar to be capable of calculating his own least-squares fit to the Peters data. Had NCS done so, he would instead have confirmed DR's rigorously computed sinusoid:  $\Delta\beta = (18' - )\cos(\lambda + 58^\circ) = 9'\cos\lambda - 15'\sin\lambda$ . We recall (§C10) that the latitude effect which Muffiosi need (to salvage Ptolemy) is  $29'\cos\lambda$  — so the real cosine coefficient ( $9'$  instead of  $29'$ ) falls terribly short of the mark. Note that NCS' erroneous sinusoid replaces the correct<sup>36</sup> cosine-coefficient  $9'$  by NCS' false  $16'$ , which makes the amplitude appear much nearer the  $29'$  value needed to save Ptolemy's rep. (Though, even this inflated  $16'$  value is far short of what "neutral" NCS seeks.) The bottom line: NCS' sinusoid is a falsehood (expect no *DIO*-citing retraction) — born not of mathematical analysis but of NCS' Ptolemaic eyeballs.<sup>37</sup>

**C13** "No one else" has ever gotten anything like the  $\Delta\beta$  sinusoid which "meticulous" NCS claims is a valid rough fit to the Peters curve. (Different-context NCS aside at *JHA* 1981 p.61: RN "knows something about calculating syzygies that no one else knows." Like NCS on van der Waerden at *Isis* 64:239.) R.Newton<sup>38</sup> makes it  $15'\cos(\lambda + 60^\circ) = 8'\cos\lambda - 13'\sin\lambda$ . And J.Evans, whom NCS rates<sup>39</sup> as "acute" fellow, finds (*JHA* 1987 p.251)  $\Delta\beta = (0^\circ.31)\cos(\lambda + 63^\circ) = 8'\cos\lambda - 17'\sin\lambda$ , close to DR's result — and even lower in the crucial cosine coefficient, where the Muffia is looking for the desired  $29'$ , not a piddling  $8'$  or  $9'$ . Evans, not willing to misreport the latitude error curve, instead just pretends the correct one isn't much different from  $29'\cos\lambda$ , admitting only (*idem*) "the phase is not exactly right" — i.e.,  $63^\circ$  of phase difference is merely "not exactly right"! But, one cannot play with the phase. The cosine & sine terms are independent. And  $8'$  or  $9'$  is a far cry from the required  $29'$ . Thus, Ptolemy is convicted, and the Muffia has lost the Catalog controversy. (Expect no retraction — *DIO*-citing or otherwise.)

**C14** But, perhaps the funniest part of this travesty has not yet been discussed. Even while F. Lee Evans & NCS conjure up amazing *speculations* in order to depress the required  $29'$  amplitude (e.g., NCS 1992 p.177) and try to [a] inflate the actual error-curve's  $18'$  amplitude & [b] shift its Muffia-offending phase, they have inexplicably ignored a huge effect: an  $11'$  wave which is (by their hypothesis<sup>40</sup> that Ptolemy observed the Catalog) entirely *nonspeculative*, namely, the wave due to the Cataloger's error in his adopted obliquity. Ptolemy's obliquity [like Hipparchos'] was unquestionably the Eratosthenes value,  $23^\circ 51' 20''$  (e.g., *Almajest* 1.12&15), but the correct obliquity in his day was  $23^\circ 40'.7$ . This  $+11'$  error would introduce into the stars a latitude error wave  $\Delta\beta = -11'\sin\lambda$ , which — at a stroke — accounts for roughly *half of the amplitude found by all parties*. Subtracting

<sup>33</sup> Isn't this just the sort of bibliographical offense which NCS used to condemn as dishonest (see *DIO* 1.1 ¶6 fn 6), when he imagined RN had made such a revealing slip?

<sup>34</sup> After two decades of privately calling RN&DR nuts (which caused no public concern among the Hist.sci archons who honor NCS) and after refusing for over ten years to reply to the ironclad DR error-waves argument (snobbery which also caused no public concern among the Hist.sci archons who honor NCS), NCS has now been shown to lack an honest rejoinder to the toughest argument of those skeptics he has been slandering — a situation which continues to cause no public concern among the Hist.sci archons who continue to honor NCS.

<sup>35</sup> Sent to the *JHA* in 1976.

<sup>36</sup> If Ptolemy observed the Catalog, then  $|\Delta\beta|$  should equal about  $29'$  near the equinoxes. But the Peters curve's mean equinoctial absolute magnitude is slightly less than  $10'$  — in almost perfect agreement with DR's  $9'$ . This is perhaps a superficial point, but it should at least have given pause to those attempting to pretend that the Peters curve suggests the presence of anything like  $29'\cos\lambda$  in the latitude residuals.

<sup>37</sup> On the eyeballs of NCS' ancient mentor: see DR at *Vistas in Astronomy* 28:255 (1985) p.266.

<sup>38</sup> *QJRAS* 20:383, 1979, p.390.

<sup>39</sup> Also "judicious": NCS 1992 p.177.

<sup>40</sup> Yes, it's deliberate.

out this effect (in order to find out what errors still need explaining)<sup>41</sup> of course depresses the sine coefficient to virtual nullity. And it must be equally depressing to those attempting to pretend that large unexplained error waves may redeem Ptolemy. Once this *known* (not conjured-up) effect<sup>42</sup> is removed from the actual (Peters) latitude errors, the best-fit sinusoid is merely  $\Delta\beta = 9'\cos\lambda - 4'\sin\lambda = 10'\cos(\lambda + 25^\circ)$ ; this 10' amplitude is catastrophically far below the 29' amplitude that must exist if Ptolemy observed the Catalog.

**C15** Since Muffiosi cannot answer DR's error-waves argument, the frantic dears must ignore, miscompute, rearrange, and/or distort the actual latitude errors' inconveniently DR-confirming phase & cosine coefficient. (Even the pre-§C14 amplitude needs Muffia-massaging, since  $18' \neq 29'$ .) But, if we wish to unloose the alibi-power of preconception, there is no reason to limit the fun to Ptolemy. So I suggest that the *JHA* set these same charmingly programmed Muffia myopes upon the task of fiddling phase and amplitude of the effects observed by, say, Bradley & Bessel. When the sand-in-the-eyes settles, we'll find that Bradley discovered stellar parallax & Bessel beat Chandler to his Wobble....

**C16** A final note on the Peters graph of the actual zodiacal latitude residuals: NCS refers (§C11) to the "complex" shape of its curve. I.e., the curve's 2<sup>nd</sup> major peak (centered on  $\lambda = c.300^\circ$ ) is too broadly flat for a pure sinusoid. In typically sterile selective-agnostic Muffia fashion, NCS sees this situation strictly in a whew-we-barely-slipped-out-of-that-cleftstick light — instead of asking: how can we use this curve's peculiarities to find out whose solar theory is indicated as having been adopted by the observer of the Catalog's zodiacal latitudes? Inspired by NCS' comment (and I am happy to acknowledge the debt), it took DR a few hours (1992/10/19-20) to derive and check out the solution. So it will be fair to give the Muffia a month (1992 Nov) to work on the same problem.<sup>43</sup> The solution will be published in an upcoming number of *DIO*. Our offer: the Muffia capo (Toomer, Swerdlow, Aaboe, or B.Goldstein) who, during 1992 November, is first to call us up (phone: 410-889-1414) with the correct solution, and who is able to describe a valid math derivation of it, receives a free one year *DIO* subscription. (Just what every Muffie dreams of finding under his Christmas Tree . . .) Hopefully more attractive: a published note of admiration (acknowledging a Muffia share in this provocative discovery), to appear in the first 1993 issue of *DIO*. [Note added for 1995 reprint: Solution printed at *DIO* 1.2 (©1993) fn 152.]

**C17** Delambre (1817) and DR (1982C) emphasized the total absence of stars from the c.5° band of southern sky which is visible from Ptolemy's Alexandria (latitude  $L = 31^\circ 12'$ ) but invisible from Hipparchos' Rhodos ( $L = 36^\circ$ ). So NCS uncritically follows the alibi of Evans (1987 p.166) and says (NCS 1992 p.176-177): "the object of Ptolemy's catalogue was surely to list the stars in and near recognized constellations, and since these were formed around the Aegean there was no reason to include additional stars not then included within constellations even though visible near the southern horizon in Alexandria." DR's comments: [a] Ptolemy's version of the Catalog (*Almajest* 7.5-8.1) contains dozens of stars explicitly labelled by him as "unformed" stars not belonging (though loosely attachable) to the traditional constellations. [b] While most of the constellations Ptolemy adopts were established by the time of Aratos (c.275 BC), Ptolemy is perfectly capable of breaking old tradition and states that he has done so ". . . the descriptions which we

<sup>41</sup> While the large sine coefficient is primarily due to obliquity-setting error, the cosine coefficient's non-nullity merely reflects the fact that the sky moves a little during the few moments between the astrolabe-observer's setting of rings 5&3. (See *DIO* 1.1 ¶6 §G.)

<sup>42</sup> For Hipparchos, the real obliquity was  $23^\circ 42'.7$ , so (assuming *Almajest* 1.12 is correct in saying that Hipparchos also used Eratosthenes' obliquity,  $23^\circ 51' 20''$ ), his obliquity-related error wave would be  $-9'\sin\lambda$ . Instead of assuming that a given attested obliquity must have been accepted by the observer, DR 1982C instead used the actual error wave (Peters' curve) to *solve for* the observer's adopted obliquity — which came out as  $23^\circ 56' \pm 1'$  (DR 1982C eq.27).

<sup>43</sup> I am not asking the Muffia to assent to specific interpretations. But I am challenging Muffiosi in this sense: Muffies pretend that they reject DR findings — when the truth is that they simply lack what it takes to generate such discoveries themselves. So this offer (involving an easier-than-usual problem) will provide them a chance to improve their standing in *DIO*'s eyes.

have applied to the individual stars as parts of the constellations are not in every case the same as our predecessors" (*Almajest* 7.4, G.Toomer 1984 *Almajest* p.340). [c] Ptolemy was unquestionably willing to adopt a brand-new asterism, since the *Almajest* is the first extant work to recognize the "Antinous" section of Aquila (*Almajest* 7.5, Toomer 1984 p.357), named in honor of Hadrian's boyfriend, who had died<sup>44</sup> only 7 years previous to Ptolemy's 137 AD epoch for his rendition of the Catalog. [d] Having found it temporarily convenient to invoke Ptolemy's UNORIGINALITY at pp.176-177, Univ Chicago Professor Swerdlow then turns right around and argues (feebley) in favor of the possibility that Ptolemy observed the stars by calling him *unqualifiedly* "an astronomer and mathematician of the greatest ORIGINALITY and the greatest depth" (NCS 1992 p.181, caps added). If *DIO* readers happen to know any bright young chameleons seeking a useful education, do urge them to attend Swerdlow's University of Chicago. He could teach them a few tricks.

**C18** Obvious question (ridiculously so): since we possess only a few extant scraps of astronomy between Hipparchos & Ptolemy, how can anyone confidently measure how original Ptolemy was<sup>45</sup> More curious yet: while NCS pronounces his purely faith-based judgement-*certain* (§C17) on Ptolemy's genius, NCS also says on the very same page (NCS 1992 p.181) that — despite symptom after symptom after symptom telling him that the Catalog is Hipparchos' — NCS is still *completely unsure* of whether Ptolemy stole the Catalog or not. (Thus the "Enigma" of NCS' paper's title.) The contrast is provocative — and tells us that the "Enigma" here is (not the Catalog's origin but) the impenetrability of a certain cult's *a priori* mentality. (When an academic community utterly flouts & abandons all interest in falsifiability, it destroys the rôle of reason even in discourse — much less in the evolution of accepted ideas in its field. Such dementia is inevitable wherever cult-status has higher priority than evidence and truth.)

**C19** Despite Ptolemy's vaunted originality, all his modern flacks (including even NCS 1992 pp.180-181 & Evans at *JHA* 1992/2 p.66) admit that it begins-to-look-perhaps-a-little-like some stars "may have come from Hipparchos" (NCS 1992 p.181). (Admire the passive language — as if the stars did the walking.) It's fun watching defenders think up Nice words for this. Like: "dependence"; never Impolite words like: "fraud".

**C20** I think we need some Impoliteness here. DR holds that Ptolemy stole — yes, stole — virtually the whole Catalog from Hipparchos. Besides a range of specific evidences of plagiarism, there is the simplicity of that hypothesis' fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of the central purported "Enigmas" (§C22) of the case immediately evaporate. The Muffia contrarily keeps insisting (at great length) that the theft is not yet absolutely, positively, completely, utterly proven. (And the ever-mounting weight of evidence has reduced Muffiosi to this feeble last ditch). Thus has an unsubtly transparent intellectual inertia gradually sucked defenders ever-deeper into a hodgepodge of ad-hoc exercises in special-pleading (for each separate suspicious Catalog circumstance: §C22), a thicket of disconnected alibis — sorely in need of a mow-job by Occam's Razor.

**C21** E.g., to try answering RN's fraction-endings argument and (inadequately) deflate DR's error-wave amplitude to "merely" 20', NCS (1992 p.177) promotes Evans' inane pro-

<sup>44</sup> To quote from DR's *Queen's Quarterly* paper (Rawlins 1984A), p.973: "The reason that Ptolemy's stele [*Canopic Inscription*] was erected at an Egyptian miracle factory is: that's just where he worked — forty years at Canopus, an infamously licentious town which was an ancient combination-in-one of Hollywood, Lourdes, and Las Vegas. The ultimate enshrinement of Ptolemy may have hinged on a seemingly unrelated event: in 130 AD, the Emperor Hadrian was sailing on the Nile with his young Bithynian lover, Antinous, when the lad drowned . . . Hadrian was emotionally shattered: he established a cult and named towns . . . in the dead boy's honor . . . Immediately after the death, still in his grief, Hadrian visited the Canopus temple [of the god Serapis, to whom Ptolemy's *Canopic Inscr* is dedicated], and probably met Ptolemy in person. A copy of the temple was soon erected in the 'Canopic Vale' of Hadrian's Villa. A group of stars in . . . Aquila were named for Antinous. . . . (Some twentieth century star-guides — e.g., Olcott's — have carried Antinous as a minor constellation, an apt memorial for an Asia Minor minor.)"

<sup>45</sup> NCS 1992 p.182 concludes that the Catalog issue is a question that cannot ever be resolved; meanwhile, he has resolved that Ptolemy was a genius. I.e., NCS has perfectly inverted the actual situation as to how much we can know.

posed Ptolemy-observing scheme, which: [a] was a Velikovsky-style victim of Collective Amnesia (since neither Ptolemy nor any other ancient astronomer ever mentioned it — NOR DID MUFFIOSI, until recent RN-DR-proposed crucial-testing cornered them), and [b] is more wildly comic<sup>46</sup> than the surreal sobriety-test fantasy in the cinema-farce *The Man With Two Brains*. (Hollywood screenwriters have to use drugs to get this high. How does the Muffia do it?)

**C22** Perhaps we can attain some perspective on the Catalog matter by simply listing the features we would expect to find in the Catalog if it “came from” Hipparchos. [Test’s first proposer: in brackets.]

- [a] An utterly GROSS  $-1^{\circ}.1$  mean longitude error [Tycho].
- [b] Absence of large  $29'$  amplitude error waves in northern longitudes [DR].
- [c] Absence of large  $29'$  amplitude error waves in latitudes [DR].
- [d] Longitudes with more  $40'$  endings than  $0'$  endings [RN].
- [e] Longitudes with more  $10'$  endings than  $30'$  endings [RN].<sup>47</sup>
- [f] Absence of a near-quarter-degree constant error in celestial latitudes  $\beta$  [DR]. (Such an error is roughly entailed by Ptolemy’s false assumed geographical latitude<sup>48</sup> for Alexandria,  $L = 30^{\circ}58' = \text{atn}[3/5]$ , which he swiped from Vitruvius’ crude, 2-century-old equinoctial ratio, shadowlength:gnomonheight = 3:5. See DR at *Vistas in Astronomy* 1985 p.267 n.6 and at *Amer J Physics* 1987 p.236 & n.15. Alexandria’s actual  $L$  is  $31^{\circ}12'N$ .)
- [g] No stars in the  $c.5^{\circ}$  band of sky visible from Alexandria but not from Rhodos [Delambre].<sup>49</sup>

**C23** Fact: *all seven of these fingerprints are found in the Catalog*. Five out of the 7 tests are original with RN&DR and appeared only in recent years, *after* the Muffia, innocently heedless of all 5 of these tests, had publicly & totally committed its reputation for expertise to faith in Ptolemy’s greatness & originality (and his critics’ idiocy). As new test after new test came out against Ptolemy, Muffiosi stuck to their party line: we expert archons have learned nothing from RN&DR. And they’ll die stuck to that same unalterable principle.

**C24** Even for the tiny but indicative sample of stars where Ptolemy copies errors of *several degrees*<sup>50</sup> from Hipparchos, NCS still isn’t finally convinced. NCS 1992: “a few stars may have come from Hipparchos” (§C19), “but I think *this part of the analysis* should be carried further” (p.180, emph added). Note the remarkable coincidence that: the only evidence (bearing on whether the Catalog was stolen), which NCS desires Further-Research into, is that which (even he thinks) looks bad for Ptolemy as things now sit! This tactic is a faithful repeat of what DR found long ago was standard among the very Velikovskians<sup>51</sup>

<sup>46</sup> Let’s see, we start by setting ring 5 NOT on the chosen fundamental star’s ACTUAL longitude at ring 3 but rather at the nearest whole-degree value LESS than the original value; then, after sighting the stellar quarry with ring 2, we read where ring 2 meets ring 3 AND THEN ADD BACK, ONTO THIS READING, THE AMOUNT WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . Got it? (Evans at *JHA* 1987 p.243 — including his conveniently thesis-aiding datum-misreportage [ $303^{\circ}03'$  rendered as  $303^{\circ}05'$ ], in the NCS tradition admired at *DIO* 1.1 ¶5 fn 7.) Can RN-DR be accused of cruelty to dumb animals, given the tightness of the evidential vise they’ve closed on the poor Muffia? To watch prominent scholars thrashing about in such pathetic credibility-death agonies is akin to viewing Animal-Rights films of stoats caught in spring traps. Trying to weasel out.

<sup>47</sup> It is seldom noted that this contrast (182 stars with  $10'$  endings vs. merely 88 stars with  $30'$  endings) is even more overwhelming than item [d] (246 with  $40'$  endings vs. 226 with  $0'$  endings). The totals for all endings are given at R.Newton *Crime* 1977 p.245, followed by his brilliant & pioneering induction of the now-obvious explanation.

<sup>48</sup> *Almagest* 5.12-13. DR’s *Amer J Physics* 1987 paper (p.236) also notes that the same argument proves that the real-dozen *Almagest* 7.3 star declinations (null mean error) are also stolen, though Ptolemy naturally presents them as results of his own outdoor observations.

<sup>49</sup> First broached by the eminent astronomer J.Delambre in his 1817 *Hist Astron Anc*, this argument was extensively developed by DR 1982C, in order to determine (statistically) the observer’s latitude & epoch. Both results agreed neatly with Hipparchos, disagreeing violently with Ptolemy. Since (after a decade of silent hope otherwise) Muffiosi cannot tear down the math, they must try alibiing in other fashions: see above at §C17.

<sup>50</sup>  $\alpha$  Cen (Graßhoff’s discovery),  $\theta$  Gem,  $\theta$  Eri. See Graßhoff 1990 pp.189, 291-2, 307-8, 313-4, 326, 331, 333.

<sup>51</sup> See *DIO* 1.1 ¶3 §D2; also §B2 of DR’s 1972-4 paper, “Freudian Astronomy, or: Do Planetary Orbits, Bristlecone Pines, & Velikovsky’s Believers Suffer from Collective Amnesia”, published in 1990 in the anti-kook newsletter of Leroy Ellenberger, 3929 Utah Street, St.Louis, MO 63116, phone 314-773-0329.

NCS compares RN to. Recall also the comment at *DIO* 1.1 ¶2 fn 7 (which might almost have inspired NCS 1992): “Parapsychologists, UFOlogists, & Ptolemists . . . prefer unending data-collection, thereby [ducking] the shame of having pursued & promoted a false path for decades.” NCS’ conclusion: “After more than a century of serious [read: Muffia] and not-so-serious [read: RN-DR] research into Ptolemy’s catalogue of stars, the fundamental question of its originality remains unanswered” (NCS 1992 p.182). Or, as another humorist is about to express it for us (§C25): research on the Catalog is back to Square-One.<sup>52</sup>

**C25** The overwhelming array of evidence against Ptolemy ensures that skepticism on the Catalog will continue, so the loyal Muffia will stand ever vigilant to defend its weirdo hero, and — as part of that effort — Muffiosi will keep right on pretending (fn 24) to impartial scientific curiosity on the Catalog issue. The spectacle of the Ptolemy lobby struggling with the Ancient Star Catalog pseudo-enigma reminds me of another farce, Dave Barry<sup>53</sup> on the tobacco lobby:

It’s time somebody spoke up for the troubled US cigarette industry . . . [and] the fine research being done at the famous Tobacco Institute, which is staffed by leading tobacco-industry scientists using sophisticated equipment and wearing state-of-the-art leashes. These scientists have been researching for years, but they are darned if they can find any solid evidence that smoking is bad for you. Although naturally they are continuing to look just as hard as they can:

FIRST SCIENTIST: Well, Ted, for the 13,758<sup>th</sup> consecutive experiment, all of the cigarette-smoking rats developed cancer! What do you make of it?

SECOND SCIENTIST: Beats me, Bob!

FIRST SCIENTIST: It’s a puzzle, all right. Hey, look at this: These rats have arranged their food pellets to form the words “CIGARETTES CAUSE CANCER, YOU ZITBRAINS.” What could this possibly mean?

SECOND SCIENTIST: I’m totally stumped, Bob! Back to square one!

THIRD SCIENTIST (entering the room): Hey, can you two guys lend me a hand? I need to screw in a light bulb.

But not even the Tobacco Institute ever thought of proposing a moratorium on discussing evidence at all.<sup>54</sup>

**C26** We now step back to size up the general portrait of Ptolemy that has evolved from decades of Muffia apologia. NCS 1992 (p.175) adopts the excuse of Laplace<sup>55</sup> & Gingerich (*Science* 1976/8/6) for Ptolemy’s  $-1^{\circ}.1$  mean Catalog error: maybe it’s just caused by the similar mean error in Ptolemy’s solar theory. DR’s comments:

[a] This argument directly inspired<sup>56</sup> the DR 1982C absent-error-waves test, which definitively refuted the Laplace-Gingerich alibi. (Rawlins 1982C, eventually published by a real science journal, was originally submitted to *JHA* Editor-for-Life [EfL] Lord Hoskin in 1976 & 1977. His Lordship refused even to referee it. The *JHA* has now spent years — consuming scores of its extremely handsome pages — trying to justify its original 1976-7 mistake by vainly attacking this DR paper, using pseudoscience packaged as scholarship.)

<sup>52</sup> Muffia strategy at this desperate juncture resembles the tobacco lobby’s primarily in that there is finally no longer any hope of proving Ptolemy or cigarettes innocent — no, the approach now is simply: obfuscate (to keep the money rolling in) enough to maintain the sham of a continuing “controversy”, in order to pretend that one hasn’t lost. National Geographic long used the same damage-control ploy to protect its Peary N.Pole lie. And TV networkdom’s tactic regarding the patently deleterious effect of TV violence on young viewers is similar: nothing-has-been-proved — so let’s just keep on profitably peddling sadism to kids.

<sup>53</sup> *Orange County Register* 1988/6/5 p.G2. (We thank Steve Wooldridge for sending this item to *DIO*.)

<sup>54</sup> This is standard for frightened academic communities. See, e.g., DR *Peary at the North Pole: Fact or Fiction* 1973 pp.251-253, 289-294.

<sup>55</sup> Laplace was himself a notorious nonciting adopter of others’ work. See, e.g., G.Airy *Report*. . . [BAAS 1831-1832] London 1833.

<sup>56</sup> Noted at DR 1982C p.359.

[b] NCS' preferred vision of Ptolemy is of a scientist who spent years observing 1000+ stars outdoors with his astrolabe — yet never, during all this time, did NCS' ancient precursor-in-geniusdom manage to notice that his observatory's latitude  $L$  was off<sup>57</sup> by  $-14'$  — an amount virtually equal to the solar semidiameter. Nor did Ptolemy ever realize (during at least 8 years of alleged solar observations, 132-140 AD: *Almajest* 3.1&7) that the real Sun's position differed from his tables by  $-1^{\circ}.1$ ! This error renders the easily-observable equinoctial solar declination off by c.half a degree, an amount equal to roughly *twice the solar semidiameter*. (Such a fantastic error would instantly be revealed by transit circle or astrolabe, both of which Ptolemy claims to have regularly used. Heck, even an instrument as awful as an ancient astrologer's asymmetric gnomon can do a lot better than this.)

No one having the slightest familiarity with outdoor astronomical observing can regard the foregoing vision as anything but an indoor lawyer's fantasy.

**C27** Since 1987, the *JHA*, utterly captured now by the Muffia, has published at least 7 pieces on the Ancient Star Catalog (running over a hundred pages in all). *All seven have been from the pro-Ptolemy side*<sup>58</sup> of the controversy. So, now, the *JHA* publishes the capper to this 5 year demonstration of its idea of equity, by suggesting (NCS 1992 p.182) a "moratorium". (And one notes that neither of the 1992 *JHA* papers cites DR's 1991 analysis at *DIO* 1.1 ¶6, which provides yet more novel evidence, positively attaching Hipparchos' solar work to the Catalog's zodiacal longitude error curve, with an ordmag 1'-precision match of amplitude.) I.e., now that the *JHA* has fired its last (for-as-long-as-We-feel-like-it) pro-Ptolemy shot on the Catalog, just in under its own welltimed moratorium wire,<sup>59</sup> the *JHA* decrees it would be best to just end the Catalog controversy right here. Megalomania rarely achieves such heights of unreality.

**C28** Unrealer yet: NCS unreels a proposal for more "research" (§C24) — even while calling for his moratorium. (It doesn't take a linguist to translate: [a] NCS wants a moratorium on the chaos of conflicting Muffia claims — which he is now himself so brilliantly augmenting! — that has left the Muffia looking about as convincing as Ptolemy. But NCS wants no moratorium on Muffiosi continuing to try to figure out new alibis for Ptolemy. [b] Given its tenuous hold on reality, the *JHA* perhaps even imagines that DR will submit a paper directly to the *JHA* in response; so, while it has left open the possibility of publishing some more of its own incomparable Muffia research on the Star Catalog, *JHA*'s "moratorium" is now in place, in print, as a pre-set official-excuse for rejection of a [believe me, PURELY] hypothetical direct<sup>60</sup> DR submission.) Isn't the *JHA* a treasure?

**C29** After 5 years & dozens of pages of failed *JHA* attacks on RN-DR's Star Catalog analyses, the *JHA* is now suddenly struck — like St.Paul on the Damascus road — with a New Awareness of The-Meaning-of-It-All. NCS 1992 (p.182): "life is too short to waste on questions that cannot be answered." Especially a silly nothing like: did the Muffia's Greatest-Astronomer-of-Antiquity merely steal Hipparchos' most precious heritage? So NCS 1992 concludes (p.182, caps added) by downgrading the issue — via the most original reasoning ever to grace a historical journal: "Is it really such an important question? [DR: NCS used to rate Ptolemy's integrity a very high-order question: *Amer.Scholar* 1979 p.525.] The interest in the catalogue is now ALMOST ENTIRELY HISTORICAL."

**C30** Seldom has a party of "experts" been so utterly defeated (and by scholars it exiled as fools) — so bare of substantial, coherent retort<sup>61</sup> — that its ever-so-clever strategists

<sup>57</sup> See above at §C22 item [f].

<sup>58</sup> And, even if something skeptical were printed in the *JHA*, the author would be a safe, effete House-Skeptic — not from the frank DR-DIO mold.

<sup>59</sup> Following NCS 1992, we find: "Editorial Note: This article was received in June 1991, but was held over to permit publication of the *Essay Review* already commissioned from James Evans, which appeared in our February issue." (Evans, too, defended Ptolemy — & failed to cite *DIO* 1.1 ¶6, though the paper is known to Hist.sci: fn 19.) The timetable alleged (who asked?) pseudoxcuses noncitation of the very *DIO* 1.1 that triggered NCS 1992.

<sup>60</sup> However, see the *DIO* publisher's statement at the back of this issue.

<sup>61</sup> E.g., since Muffiosi have been damning skeptics for decades as incompetents, one would expect pages of examples of the purported incompetencies to be forthcoming. Muffiosi's occasional efforts to expose alleged errors

got tangled up in such almost-artistically disjunct babbling. Who but our peerless Muffia jesters could even imagine proposing that a subject be *ruled out of a historical journal on the ground that it is too historical*?

**C31** After the foregoing, it may be superfluous to attempt a brief review of Muffia sanity on the Catalog issue. But, anyway:

[a] In 1974, EfL-best-friend O.Pedersen disbeliefed that the Catalog was stolen, because Ptolemy was too honest; Pedersen added that Ptolemy's rep for "integrity would be *damaged beyond repair*" if the theft indeed occurred (Pedersen *Survey of the Alm* 1974 p.258 emph added; DR 1982C p.362). (Once the RN-DR proofs of Ptolemy's thievery appeared, Pedersen's self-evident conditional quietly slid down the Muffia Memory Hole.)

[b] In 1981, O Gingerich admitted (*QJRAS* 22:42) that the RN-DR analyses show that Ptolemy probably did take the Catalog from Hipparchos.

[c] In the 1987 *JHA*, J.Evans attacked RN & DR to the extent of dozens of (frustratingly inconclusive) pages, swinging Gingerich's *JHA* back to denial that the theft had been proven.

[d] Graßhoff 1990 ("edited" by Muffia capo G.Toomer)<sup>62</sup> concluded that much of the Catalog was based on Hipparchos' observations, after all. (RN-DR had long asserted this, but watch Graßhoff grab off all the credit for proving it, while painting RN-DR as fools.)

[e] Now, NCS 1992 says: Maybe. Yes, possibly the Catalog was taken from Hipparchos; but . . . No, nobody has proved anything — and it doesn't matter anyway.

So, the Muffia line on whether Ptolemy stole lots of the Catalog: [a] 1974 no, [b] 1981 yes, [c] 1987 no, [d] 1990 yes, [e] 1992 no-yes-but-either-way-we're-still-right.

**C32** Is this a community of scholars honestly seeking a credible, consistent vision of the truth? — or are we instead enjoying: Jekyll&Hyde-go-vaudeville? (Perhaps the reason there seems to be no direction is: the Muffia has let its conscience be its guide.) But there is coherence. Indeed, the best part of the show is that the foregoing seemingly inconsistent positions [a]-[e] have one glorious factor in common: *all five of these analyses are as one in swearing that Ptolemy was wise and honest*. (You might think there is a wee difficulty here — like, maybe you suppose that there is something slightly dishonest about swiping, without acknowledgement, the labor of an astronomer who observed 1000+ stars. If you believe this, you will never make the grade in Hist.sci. Happily, Harvard Hist.sci Dep't head O Gingerich 1981 p.43 & Graßhoff 1990 p.215 will set your ethics straight for you.)

**C33** Well, when a cult's sacred conclusion remains the same — regardless of  $180^{\circ}$  flipflops in cult-perception of the evidential situation — then, observers outside the fold are justified in supposing that: the conclusion was established before the evidence was examined. Just the way Ptolemy operated.

## D The Heartless Undead: Sail On, O Ship of Hate

**D1** An occasional nervous-neophyte Muffie may momentarily worry that the foregoing revelations could disturb grantflow. Seasoned veterans of the game know better: happily, Hist.sci grants have not the slightest (positive) correlation with the grantee's accuracy or genuinely expert original scholarship. (As *DIO* readers know all too well.) So, we can relax. (Likewise, professional astrologers' amusing inability<sup>63</sup> to compute horoscopes correctly has no effect at all upon their clients' generosity.) Further: by this time, so many Hist.sci archons' reputations have been invested into the glorification of Muffia scholarship, that the cult cannot be *permitted* to be seen as having erred catastrophically in anathematizing RN-DR. Therefore, our favorite Unsinkable cult will positively insist on

by skeptics have been so pathetically thin that it is by now all too clear that the Muffia klan has simply been bluffing in this regard. (Note the feebleness of Muffia attempts in this direction: *DIO* 1.3 fn 252.) See *DIO* 1.1 ¶5 fn 6 for Hist.sci (including Muffia) precedents for publishing lengthy error-lists to attack authors.

<sup>62</sup> Toomer was being convinced by Graßhoff at least as early as 1986.

<sup>63</sup> Rawlins *Skeptical Inquirer* (SkInq) 2.1:62 (1977) pp.73, 76-77; Rawlins 1984A pp.974-976.

keeping its course and will slide right past DIO's iceberg. With barely a sound or a shudder. On its part, the Muffia must wonder why, despite years of archonal conspiring to ostracize RN-DR's heresy, the hated<sup>64</sup> heterodoxy persists nonetheless. (Even Time-Life's popular *Hoaxes & Deceptions* p.108 accepts that the Rawlins 1982C analysis, of the Ancient Star Catalog's southern boundary, indicates that this "Ptolemy" Catalog was actually observed in Hipparchos' Rhodos, not Ptolemy's Alexandria. See §C17.)

**D2** The DR-Muffia double-tarbaby-fracas will continue indefinitely, because: [a] DR positively won't stop publicly admiring Muffia gyrations, so long as the Muffia insists on its snobbish & effectively censorial minimum-citation-practice, based upon its equally ludicrous WE're-the-only-experts-around-here pose. (I.e., DR is asking that the Muffia acquire some fundamental ethics and integrity. But who's going to fund the brain transplants?) [b] Muffia mout'pieces are irrevocably committed to forever clinging to their precious pretense that DR's historical scholarship is utterly worthless.<sup>65</sup> This point is so sacred to Muffiosi that, in order to maintain it, the Muffia will pay any price (primarily: internal rot) — and, in order to cloak its ineptitude with the trappings of Reputability, will woo into its muck just as many major academic institutions as it is able to con into sharing that price. (Terseness borrowed-with-credit from etiquette-authority NCS' §C5-sampled lexi-con.)

**D3** Given Muffiosi's invincibly-advocatory nature (and their own frustration at DR's unkillability), perhaps they will appreciate an apt lawyer-joke. Now, please understand: some-of-DR's-best-friends-are-lawyers. (And lawyers themselves — especially the classiest — tell the goriest lawyer-jokes. It pays to advertise?)<sup>66</sup> Also, my mother's father was a prominent Maryland lawyer. And she married my friend, advisor, & stepfather, John W. Avirett 2<sup>nd</sup> — widely known as one of the very finest & most respected lawyers in the United States. So, as a member of a family of lawyers, DR is delighted to contribute here an original DIO creation: *the lawyer-joke-to-crown-all-lawyer-jokes*.<sup>67</sup> Ready?

**D4** Question: why can't you kill<sup>68</sup> a lawyer?

Answer: what do you hammer the stake through?

<sup>64</sup> Yes, hated. See DIO 1.1 ¶1 §C7 & fn 20; ¶3 §D2-D3.

<sup>65</sup> Curiously, the Muffia's null evaluation of DR's scientific-history production is not shared by: the American Astronomical Society, *PASP*, *Amer J Physics*, *Arch Hist Exact Sci*, the Royal Astronomical Society of London, among others. Likewise, the prominent scientific historians: K.Moesgaard (U.Aarhus, Denmark), S.Goldstein (UVa, Charlottesville), B.van der Waerden (U.Zürich), Curtis Wilson (St.Johns, Annapolis). (Also the late R.Newton of Johns Hopkins & W.Hartner of U.Frankfurt, Germany.) Each has published or supported the publication of DR science-history researches. Thus, Muffiosi's 100% rejection of these papers implicitly accuses each of these institutions & scholars of incompetency.

<sup>66</sup> Are top lawyers who revel in lawyer-jokes retching at the low end's ethics? Or, is this strain of humor just a gruesome byproduct of the legalization of lawyer-advertising? (When a local lawyer was told that his TV ads were lowering the reputation of the legal profession, he pithily replied: that's impossible.) There's a famous agent (graduate of Bernie Cornfeld's School of Asceticism) whose gentility & generosity are so universally respected that a mere sighting of her has inspired colleagues to hum the *Jaws* theme in unison. Ashamed? Hell, she brags about it.

<sup>67</sup> DIO dedicates this joke to another joke: the Neugebauer Muffia — in honor of that cult's highly original notions of ethics and human decency, not to mention its unquestioned talent in sucking tax monies out of the system, to fund its defense-lawyer fantasies.

<sup>68</sup> An anti-lawyer line from [Marlowe] (*Henry the Sixth Part 2* Act 4 Scene 2) has become popular of late, but the delicious mobocracy-fantasy context is rarely reproduced. Jack Cade [haranguing revolutionary]: "Be brave . . . [I vow] reformation. There shall be, in England, seven halfpenny loaves sold for a penny . . . I will make it a felony to drink small beer . . . when I am king (as king I will be) . . . there shall be no money; all shall eat and drink on my score . . . that they may agree like brothers, and worship me their lord." Dick [butcher]: "The first thing we do, let's kill all the lawyers." Cade: ". . . that I mean to do. Is not this a lamentable thing, that of the skin of an innocent lamb should be made parchment? That parchment, being scribbled o'er, should undo a man? . . . I did but seal once to a thing, and I was never mine own man since."