Table of Contents

DIO & The Journal for Hysterical Astronomy: Page:

†11 Concise Chronology of Approaches to the Poles: by R. K. HEADLAND 107
†12 Richard Byrd, Bernt Balchen, & the North Pole: by RAWLINS 110
†13 Scraps & Bits 112
†14 Recovering Hipparchos’ Last Lost Lustrous Star 119
†15 Naked Came the Arrogance 120

Byrd’s Bravery & Balchen’s Math Vindicated:
[Note added to 1998 printing.] The two lead DIO articles of this issue both justly (as we now know) questioned Richard Byrd’s success in his 1926 flight towards the North Pole. After this DIO appeared, Byrd’s 1925-1927 diary surfaced at the Byrd Polar Research Center (Ohio State University). The diary showed that Byrd indeed fell somewhat short (as Bernt Balchen’s early data-analyses had accurately contended). However, contra widespread doubts (including those cautiously entertained within by DR), the diary made it clear that Byrd was a knowledgeable mathematical navigator, who courageously went most of the way to the Pole and kept his course remarkably well during this most dangerous of all his grand career’s flights. DIO’s private detailed analysis of the Byrd 1926 diary was the cited basis for the New York Times’ 1996/5/9 p.1 story on the diary and the flight. The full 1996 analysis will be published in a future DIO issue [DIO 10 (2000)].

† Shortly after the Ohio State University archives’ (long-hidden) evidence of the truth about Byrd’s 1926 “North Pole” lie-theft exploded into the world press (starting with the NYTtimes), OSU loyalists determined to attempt Lazaran resuscitation of the 1926 claim’s corpse. Turning down Cal Tech’s offer of refereeing assistance, OSU instead depended primarily upon amiable on-campus talent to question DR’s report. Though this report was written at white-heat in only 10 days (1996 Spring), OSU has wasted two years seeking its longed-for (but ever-elusive) refutation-chimera. OSU Press has finally (1998) published its commentary-laden photo-reproduction of the Byrd diary. This pseudo-neutral book’s catechismic advocacy is embarrassingly transparent: [a] It refuses to cite the Byrd-cult-resented NYT page-one story (scrupulously written by the NYT Science Dep’t’s top writer, John Wilford), or NYT’s simple, irrefutable final-paragraph proof of Byrd’s glaring triple-stage contradictions on his claimed Pole-arrival time. [b] It does not even tell the reader on which pages of the book one may find & compare Byrd’s spectacular (ordmag 100 miles) 1926/5/9 7:07:10 GCT sextant-data contradiction — diary (p.85) vs. published report (p.154) — the very DR discovery which triggered NYT’s 1996/5/9 undoing of its own Byrd-glorifying 1926/5/10 headline. [c] The book neither reproduces nor so much as mentions two key handwritten documents (both in OSU archives & both cited in DR’s report) which prove that Byrd was willing to compute backwards, deducing 1926/5/9 “data” from story instead of the reverse. [d] The book (p.57) cites two allegedly Expert reports disputing the 1996 DR analysis (though finding no nontrivial errors in it). However, neither author is experienced in the positional astronomy that Byrd navigated by. One of OSU’s two apologist-Experts has such difficulty with spherical trig that his analysis contains none, though it does sport a serious arithmetic error at the heart of its prime (grade-school-level) math argument; he also mis-spells “Coriolis” 3 times out of 4. The other report (dated 1997/7/9) dear to OSU Press is the glacial-pace-learning-experience preliminary production of an OSU astronomy prof whose specialty unfortunately has no relation to the astronomy of this case. (DR’s 1998/3/9 phone inquiries found that his private alibis for Byrd’s three plainest internal contradictions — two not even cited in the prof’s report — exhibit wildly imaginative & generous flexibility. [I hope not to be forced to publish the comic details.] OSU repeatedly refused DR’s requests for access to the prof’s report, even weeks after it was being publicly cited against DR. It was finally released only after Science pressed OSU about the incongruity.) The OSU astronomy prof’s 1997/7/9 report supplies the wrong sign for all twenty-one of its solar hour-angles — and mis-spells “sextant” twenty-three consecutive times. Bottom line: OSU Press is attempting artificial creation of a sales-boosting Controversy where there is not a shadow of rational basis for substantial dispute. Since DR’s 1996 analysis is variously ironclad, OSU shuns logical debate, preferring instead to trust that ambitious and-or merciful apologists will dream up motley purported refutations, so that OSU can innocently shrug: hey, The Experts Disagree. But, though the years keep passing, OSU still lacks [a] genuine experts who’ll fill its discreditable bill, & [far more germaine] [b] a noncaricatural defense of Byrd’s 1926 hoax.

A ANARCTIC

1603 Gabriel de Castilla (Spain), with a ship’s company, probably penetrated the Southern Ocean south of Drake Passage

Subsequently several merchant vessels reported being blown south of 60°S [66.67°S] rounding Cabo de Hornos in severe weather

1773 James Cook (Britain), with companies aboard HMS Resolution and HMS Adventure, crossed the Antarctic Circle (66.53°S [73.92°S]) off Enderby Land, 17 January

1774 James Cook (Britain) on the same expedition reached 71.17°S off Marie Byrd Land, 30 January

1820 Fabian von Bellinghausen (Russia), with companies aboard Vostok and Mirnyyy, sighted the Antarctic continent at about 69.35°S [77.06°S] off Dronning Maud Land, 27 January

1823 James Weddell (Britain), with company aboard Jane, reached 74.25°S [82.50°S] in the Weddell Sea, 20 February

1842 James Ross (Britain), with companies aboard HMS Erebus and HMS Terror, reached 78.17°S [86.86°S] in the Ross Sea, 23 February

1900 Hugh Evans (Britain) and 3 others sledged to 78.83°S [87.59°S] on the Ross Ice Shelf, 23 February

1902 Robert Scott (Britain) and 2 others sledged to 82.28°S [91.42°S] on the Ross Ice Shelf, 30 December

1909 Ernest Shackleton (Britain) and 3 others sledged to 88.38°S [98.20°S], 9 January

1911 Roald Amundsen (Norway) and 4 others dog-sledged to 90°S [100°S], 14 December
1912 Robert Scott (Britain) and 4 others sledged to 90°S [100°S], 17 January (all perished during the return journey)
1929 Richard Byrd (United States), with an aircraft crew, claimed to have flown over the South Pole from the Ross Ice Shelf, 29 November
1947 Richard Byrd (United States), with an aircraft crew, flew over the South Pole from the Ross Ice Shelf, 15 February
1956 John Torbert (United States) and 6 others flew across Antarctica over the South Pole (Ross Island to Weddell Sea and returned without landing), 13 January
1956 Conrad Shinn (United States), with crew of an aircraft, landed at the South Pole, 31 October; a permanent station was then established, sustained by aircraft

Subsequently many aircraft have landed at the South Pole

1956 Vivian Fuchs (British Commonwealth) and an expeditionary party reached the South Pole by motor vehicles and sledge dogs, 20 January, and continued to cross Antarctica (Weddell Sea to Ross Sea)

Subsequently several expeditions have crossed the Antarctic through the South Pole by surface and many have made one-way surface journeys departing by aircraft

B ARCTIC

1553 Sir Hugh Willoughby (England), with companies aboard Bona Esperanza and Bona Confidentia, reached 72°N [80°N] on Novaya Zemlya, 14 August
1587 John Davis (England), with companies aboard Elizabeth, Ellen, & Sunshine, reached 72.20°N [80.22°N] off Greenland, July
1594 Willem Barents (Netherlands), with a ship’s company, reached 77°N [85.6°N], rounding Novaya Zemlya
1596 Jacob van Heemskerck (Netherlands), with companies aboard 2 vessels, reached 80.18°N [89.09°N] off Svalbard, 17 June
1607 Henry Hudson (Britain), with company aboard Hopewell, reached 80.38°N [89.31°N] off Svalbard, 16 July
1676 Vasily Chichagov (Russia), with companies aboard Chichagov, Panov, and Babayev, reached 80.47°N [89.41°N] off Svalbard, 16 July
1773 Constantine Phipps (Britain), with companies aboard Racehorse & Carcass, reached 80.80°N [89.75°N] off Svalbard, 27 July

Subsequently many whaling vessels reached high latitudes

1806 William Scoresby (Britain), with company aboard Resolution, reached 81.50°N [90.56°N] off Svalbard
1827 William Parry (Britain) and party, with 2 sledge boats from Hecla, reached 82.75°N [91.94°N] off Svalbard, 25 July

This position is farther north than the area inhabited by the Polar Eskimo of Greenland

1876 Albert Markham (Britain) and 2 sledge parties reached 83.34°N [92.60°N] off Ellesmere Island, 12 May
1882 James Lockwood (United States) and 2 others dog-sledged to 83.40°N [92.67°N] off Greenland from Fort Conger, 13 May
1895 Fridtjof Nansen and Hjalmar Johansen (Norway) dog-sledged to 86.22°N [95.80°N] from Fram in the Arctic Ocean, 8 April
1900 Umberto Cagni (Italy) & 3 others claimed to have dog-sledged to 86.57°N [96.19°N] from Zemlya Frantsa-Iosefa, 24 April

1908 Frederick Cook (United States), with a sledge party, claimed to have reached 90°N [100°N], 21 April
1909 Robert Peary (United States) and an expeditionary party, dog-sledged to 87.75°N [97.50°N] from Ellesmere Island, 31 March; Peary and 5 others continued north and possibly passed 88°N [97.8°N]
1909 Robert Peary (United States), with a sledge party, claimed to have reached 90°N [100°N], 6 April
1925 Roald Amundsen (Norway), Lincoln Ellsworth (United States), and 4 others flew north from Svalbard in 2 aircraft, crash landed and drifted to 87.83°N [97.59°N], 21 May
1926 Richard Byrd (United States), with Floyd Bennett, claimed to have reached 90°N [100°N] by air from Svalbard, 9 May
1926 Roald Amundsen (Norway), Lincoln Ellsworth (United States), Umberto Nobile (Italy), and 11 others, crossed 90°N [100°N] by airship Norge (flying Svalbard to Alaska), 12 May

Subsequently one dirigible balloon and many other aircraft have flown over the North Pole

1937 Ivan Papanin (Soviet Union) and party landed at 89.43°N [99.37°N] by aircraft from Zemlya Frantsa-Iosefa, established the first Arctic Ocean drift station, 21 May
1948 Pavel Gordyiienko (Soviet Union) and 5 others landed at 90°N [100°N] from an aircraft, 23 April

Subsequently many aircraft have landed at the North Pole

1956 Conrad Shinn (United States), with crew of an aircraft, landed at the South Pole, 31 October; a permanent station was then established, sustained by aircraft

Subsequently several expeditions have crossed the Antarctic through the South Pole

1959 James Calvert (United States), with crew aboard nuclear powered submarine USS Nautilus, reached the North Pole while submerged, 3 August, on voyage from the Pacific Ocean to the Atlantic Ocean
1959 James Calvert (United States), with crew aboard nuclear powered submarine USS Skate, surfaced at the North Pole, 17 March

Subsequently many submarines reached the North Pole and some surfaced there

1968 Ralph Plaisted (United States) and 3 others reached 90°N [100°N] by surface (snow scooter) from Canada and returned by air, 19 April
1969 Wally Herbert (Britain) and 3 others dog-sledged to 90°N [100°N], while crossing the Arctic Ocean (Alaska to Svalbard), 6 April

Subsequently several expeditions have crossed the Arctic on the pack-ice through the North Pole and many have made one-way surface journeys departing by air

1977 Yuriy Kuchiyev (Soviet Union), with crew aboard nuclear powered icebreaker Arkтика, reached 90°N [100°N] by sea from near the Novosibirskiy Orestra, 17 August

Subsequently many surface vessels have reached the North Pole

1991 Anatoly Gorshkovskiy (Soviet Union), with crew and passengers aboard the nuclear powered icebreaker Sovetskiy Soyuz, reached the North Pole by sea while crossing the Arctic Ocean (Murmansk to Provideniya), 4 August

Some of these claims are controversial and have given rise to much discussion. There is a way of resolving the question of precedence at the poles by a simple modification of the question. A secure answer may be given to Who was the first to see both poles? On 14 December 1911 Roald Amundsen and Oskar Wisting were at the South Pole, and both were over the North Pole aboard Norge on 12 May 1926; thus they became the first to have seen the South Pole and the North Pole.
Byrd, Balchen, & the North Pole

by Rawlins

110 1994 December

DIO 4.3

A1 On 1926/5/9, 1 airman-lobbyist Richard Byrd claimed to have made the 1st flight to the North Pole, from Kingsbay, Spitzbergen, in the Fokker trimotor airplane Josephine Ford. Various oddities of the claim have caused wide skepticism of it. But too much2 of that doubt is based upon B.Balchen's long-after report3 of an alleged detailed confession by Byrd's co-pilot (& Balchen's very close friend)4 Floyd Bennett.

A2 After 3 crashes left Byrd sleepless (in 13) about getting off the ground at all (Byrd Skystard NYC 1928 p.183), he & Bennett finally "sneaked to the plane" (NYTimes 5/16:3:4=5) and lifted off on 5/9 at 00:50 GCT. They disappeared to the north, lost radio contact, & returned unexpectedly early (with a leaking engine) 15¹/₂ later. Debt-ridden Byrd profitably claimed they reached the Pole but, while filming the scene, forgot (Rawlins 1973 p.264) to drop their cargo of hundreds of US flags, potentially visible to the dirigible Norge at the Pole 3 later (during its pioneer flight across the Arctic Ocean).

A3 Byrd's sole companion Bennett (died 1928/4/25) told his closest friend, the great aviator & WW2 hero B.Balchen (1928 Feb, Chicago), that: "the truth about the North Pole flight . . . would shock you through your heels. It makes me sick to think about it." 

A4 So much is certain. But the same paragraph continues with a revealing fact: "By some kind of silent agreement we never mentioned this subject amongst us any more."

Thus, Balchen's much-later detailed embellishments (in 3) upon the brief statements of §A3 must be regarded as simply his own theory of the truth of the 5/9 trip (which he believed Bennett wanted to tell him), put into Bennett's mouth for effect.

B1 The JoFords' takeoff time from Kingsbay (78° 55′N, 11° 9 E) was reported as 00:50 by Wm.Bird, the NYTimes correspondent with the Byrd expedition.5 Byrd's 5/12 telegram (NYT 5/13:3:2) to SecNavy said he reached the Pole c.9:15. Curiously, he later6 altered both figures7 by –13th to: 00:37 & 9:02, resp. The problem: though Byrd says his drift-measures showed speed c.77 knots (BUR p.2), his reports (NGM 50:386; BUR p.3) have him at Amsterdam I. (79° 48′N, 10.5° E) at 1:22, a trip of 53 nautical miles (nmi) in 32h (if 00:50 isn’t memory-holed), mean speed c.100 knots (awful ft fuel & engine).

B2 Byrd says he left the Pole at 9:15 and then (BUR p.6) became so aided, by a conveniently sudden wind from the north (just-as-conveniently absent during the northward leg of the trip), that he averaged 92 knots for the 1st 6 hrs of the return, thus placing him at 80° 50′N (c.15° E) at 15:15 (a datum Byrd never published: see BUR p.6). His expedition's original version (W.Bird NYT 5/11:1:7=8, mapped 2:3=4) has him continuing south from there to Verlegen Hook (80° 04′N, 16.3° E), "thence west to Amst Island and home" to Kingsbay8 at 16:20 or 16:25. But 163 nmi in 70h is 140 knots; and, using the 65h between 15:15 & (the Byrd expedition's own NYT reporter's in 10 return-time datum) 16:20, it's 150 knots, twice the plane's ski-less cruising speed! (The JoFords' ski caused air-drug.) One can see why Byrd left: [a] forgot 91. Verlegen, [b] said he swerved towards Amst.I, en route reaching, & [c] privately alleged (BUR p.6) he reached Kingsbay at 16:34, another convenient Byrd falsehood (never published until Rawlins 1973 p.270).

B3 The implicit original-report §B1&B2 superman-speed-discontinuities only add to the unacceptability of a claim where: [i] radio contact was lost; [ii] neither pilot used celestial navigation on subsequent witnessed trips; [iii] photocopies of putative original 1926/5/9 raw-data sheets (not a polished typescript), promised to world geog socs (NGM 50:388), were never sent; [iv] NatGeogrSoc deleted (NGM 50:385) its own report's 1926/6/23-28 dates, to hide the embarrassment that it had publicly bemedalled Byrd on the first day (6/23) of its 5-day exam of its data! Before&after textual comparisons at Rawlins 1973 p.268.

C1 A theory consistent with the known data: Spitzbergen is too small for dr-navigation to surely hit it (on the return trip) from c.600 nautical miles away. So, unwilling to risk suicide (but equally unable10 to report failure), Byrd went far enough north to be invisible from Spitzbergen but not so far that the required distance. After about 13 hrs of this, an engine began leaking. So as not to chance being forced down upon rough, drifting ice floes, Byrd instantly sped for the nearest land, Verlegen Hook, and from there forthwith got back to Kingsbay coastwise (thus via Amst.I.). [This theory proved false. See DIO 10 in 3 & 4.]

C2 Though this theory fits all available evidence, that circumstance does not prove its truth. But neither did Byrd provide credible proof that he reached the N.Pole. And, in science,13 the burden of proof is on the claimant, not the critic.

D1 We thus, shall state the situation in a conservative, positivist fashion: the first nonpseudostate & N.Pole is unequivocally that of the Amundsen-Ellsworth-Nobile Norge expedition, which arrived there on 1926/5/12 en route from Kingsbay to Pt.Barrow, AK.

1 All dates here are 1926 unless otherwise stated. All times are GCT (Greenwich Civil Time).

2 DR's initial (1972) skeptical paper on Byrd (Norsk Geog Tidskr 26:135) was not based upon Balchen's testimony. DR's Peary ... . Fiction Wash 1973 Chap.21 used some late Balchen data, all of which should be treated with caution unless verifiable from contemporary records or an independent source. E.g., his decades-later report (Balchen Come North With Me NYC 1958 p.43) of the time of Byrd's return (16:07) is simply Byrd's own final-version (§B1) takeoff-time (00:37) plus 1½h. However, Balchen's report (e.g., NARS autobio Intro B p.3) that Byrd did no celestial navigation in the Antarctic is verified by US-born Dean Smith & Larry Gould (E.Rodgers USNavInst pp.188-189). And the slowness of the Barrier to surely hit it (on the return trip) from c.600 nautical miles away. So, unwilling to risk suicide (but equally unable to report failure), Byrd went far enough north to be invisible from Spitzbergen but not so far that the required distance. After about 13 hrs of this, an engine began leaking. So as not to chance being forced down upon rough, drifting ice floes, Byrd instantly sped for the nearest land, Verlegen Hook, and from there forthwith got back to Kingsbay coastwise (thus via Amst.I.). [This theory proved false. See DIO 10 in 3 & 4.]

3 But the popular arena is not scientific. Thus, without direct testimony to Byrd's nonsuccess, I anticipate that the easily-forcefed public may well end up half-re-accepting this rickety claim. In the short term, the issue will remain undecided, as are 00:37 & 9:02. (Interim version at NYT 5/11:2:8, transl. Linda Olsen) have

4 All the outward data were nally altered by Byrd himself (with the 578 knots-less-long-distance-speeds of its "sister" Fokker (piloted by the great Chas. Kingsford-Smith: see A.Fokker Flying Dutchman NYC 1931 p.281).

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6 Though this theory ts all available evidence, that circumstance does not prove its truth. But neither did Byrd provide credible proof that he reached the N.Pole. And, in science, the burden of proof is on the claimant, not the critic.

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8 All dates here are 1926 unless otherwise stated. All times are GCT (Greenwich Civil Time).
### §13 Scrawlins

#### A Squeezing Out Suicides

A1 If legislatures wish to stop Jack Kevorkian (“Dr.Death”) by outlawing assisted-suicides, then those laws should also apply to tobacco companies. Otherwise, we have a situation where: it’s illegal to help painstricken elders to die, but legal to help happy&healthy youngsters to die.

A2 [In the days before assisted-suicide became an issue, it is said that crusaders had tried to wipe out unassisted suicide by making it a capital offense. . . ]

A3 Another way of interpreting Kevorkian’s persecutors: it’s illegal to end agony, but legal to cause it. Just the kind of logical consistency one expects of a nation whose laws are written by “legislator”-puppets, whose actions are responsive to those whose cash pulls the strings. (See, e.g., [DIO 2.1 §1 §A5 & fn 5.])

A4 Why has a decent and selfless martyr such as Kevorkian been pilloried so often in the press—where lobbies can slant “news” so easily? Partial explanation: about 20% of all medical billings occur in the last 6 months of life. So Kevorkian is seen by medbiz as confronting them with a stark choice, i.e., do doctors want: “a) keep Hippocratically extending “life” when it turns into 1-way misery, or [b] take a 20% pay-cut and thereby endanger doctors’ well-being, where yacht-deprivation is a critical risk-factor?”

Mention of the suicidally-inclined permits a seamless segue to organized baseball

#### B Shorts

**B1 Gowdiamus.** David Halberstam reports that a condition for baseball player Joe DiMaggio’s appearance at any function is that he be introduced as: the greatest living ballplayer. This is presumably what triggered my favorite Gowdyism, when sportscaster Curt Gowdy blurted out that DiMaggio was: “the greatest living ballplayer of all time”.

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1 [Note added 1995.] A cute play by Susan Smith’s lawyer has been to generate court sympathy by saying she’s so depressed that she’s tried to commit suicide. I.e., don’t kill her because: she wants to die. [See DIO 6 §4 [A.]]

2 [Note added 1995.] Baseball and tennis are fading, evidently from insufficient violence to sate the boozoobs. [DIO 2.1 §1 §B3 asked readers to ponder the evolution of a network-TV ad that scotched at men who complain about soap-opera-addicted wives while they themselves watch 5 hours of wrestling. If the ad (as originally written) scotched at the situation of women who are widowed to football not wrassling, then it would probably be altered — for the simple reason that football has become so critical, to distinguishing network TV from smalltime TV, that it is beyond attack.] Thus, baseball & tennis are increasingly dependent upon celebs. (The celeb-factor makes baseball vulnerable to strikes. Audiences will watch anyone box or play football. Not baseball, which is dead without its big-draw stars.) The trend has reached the point where even stars in the audience are used. But this can backfire. While the Atlanta Braves were vainly fighting for their lives in the 7th game of the seemingly endless 1991 World Series, their owners-in-attendance (Tarzan & Jane Turner) were viewed by national TV audiences vainly fighting off sleep. So it shouldn’t have required the 1994 strike to dampen buyers’ enthusiasm for paying $100,000,000 for ball clubs. But all-day-set-obsessed men’s tennis has ingeniously turned its draining internormality to its ad: during the 1995 US Tennis&Tontis Open, CBS cameras kept sneaking up on players’ girlfriends at ringside (usually with a Sheldy) to broadcast them yawning or nodding off, evidently with the idea of thereby enlivening the proceedings with candid-camera surprise-entertainment — i.e., using the spectacle of sleep to keep the TV audience awake.

3 [Note added 1995.] In a city known for crawling charm & sprawling slums, Baltimore baseball shortstop & longstreaker (2131 straight games played), admirably-square Cal Ripken, is paid $40,000/hit: more than former-record-holder Lou Gehrig was paid in an average 1942. (World Series lifetime slugging averages: Rip 167, Lou 73.) If Rip played a full career at this rate, the cost would exceed $100,000,000, easily enough to attain major political office. Hmmm: the agent (R.Shapiro) who arranged Rip’s 5-year $30,000,000 contract is the financial power behind Baltimore’s mayor. What a clever way to grow money. If Rip’s conscience is twinged by Ripoffing a poverty-stricken urban kid (world’s champ in teenage pregancy rates), there’s just an unfreedom, entirely accidental byproduct. (Vat City is as checkablock with nails as with wealth.) Question: how can there be peaceful multi-culturalism where 2 or 3 cults are competing to outmultiply everybody else? [Note added 2000: See DIO 8.2 §15 (O.2.)]

4 [Mention of the suicidally-inclined permits a seamless segue to organized baseball]

5 The founder of the society that gave us ancient knowledge’s pinnacle (3rd cy BC Alexandria) was the general and other poles decree that interfere in the poor’s reproductive “rights” is immoral — though interfering-tax-forcing the endangered middle class to subsidize these human waves is not (fn 19).
welfare, & race-preference schemes), the real GOP platform would instantly be revealed nakedly for all it actually is, namely, a money-is-the-root-of-all-good program to make the super-rich even richer. (Not a crusade which, unadorned with anti-welfare-loafer whipped-cream, is likely to attract more than 1% of the vote.) So, for the GOP to ensure that welfare-related diversions will continue indefinitely, two prerequisites must be met: [a] abortion, RU 486, & Norplant (& thus women’s freedom) must be suppressed, & [b] enough whipping-boy-Dems have to be kept in office to make it seem credible that the right-thinking GOP is (contra fn 21) doing its darnedest to wipe out social degeneracy, but those sneaky Liberal incubi somehow9 keep foiling them. (Does the GOP really want Clintons out of the White House?) It’s theatre-mythology that will persist indefinitely until retired by electoral intelligence or (less unlikely) a violent coup.

B6 If Jesse Helms, the Republican Senator from Tobacco, is so much more upset at sex (e.g., the late R.Mappletonther)10 than at his lovable weed’s continuing enslavement of kids and torture-massacre of millions of adults annually worldwide, then: why is Helms not horrified at the vital support which tobacco-ad funds regularly provide the glossy skinmags? (Losing this support — perhaps 1/3 of the latter’s ad-billings — would be crippling.) Mappletonther doesn’t sully even a hundredth the number of innocent youngsters they do. (Which is exactly why the youth-targetting tobacco industry supports the glossies & not the artist.) So, we’re waiting for the kickoff of Jesse’s no-doubt imminent decency-crusade for an industry-wide tobacco-ad boycott of porn. . . .

B7 TV news-Balance Unbalancen. In the US, when it’s a matter of press-coverage or legal adjudication of cases such as those of TonyaH, MTLyson, or OSimpson (where, coincidentally, millions in profits are to be made), we’re treated to every side — even the scamousmost — on a fairly hearing and a tediously complex defense. (Which gives rise, open, just image to The System.) Question: why isn’t this same fine, noble principle12 applied to: [a] politically-incorrect heresies (persons & ideas), [b] modest-sized dissenting political parties, and [c] those who question the US’ religiosity?

B8 At the 1994/5/6-8 Dibner conference at M.I.T., I mentioned to Muffia capo Noel Swerdlow my delight at his entirely original solution of some Hippiecrates numbers. (See DIO 1.3 fn 277 & fn 280.) He replied with admirable modesty that it was a “lucky” hit. (See similar self-deprecation by Aubrey Diller at DIO 4.2 Q ualification Held Hostage #2 p.55. See also DIO 1.2 681 & fn 49.) But the comment reveals the very opposite of NS’ genial implication, and we may formulate a general principle regarding the process:

The cleverer a discovery is, the more the discoverer feels it was lucky.

10 This, even though the Libs are always hugely ousted by the GOP. Our rulers’ desirer for cover-maintenance of Plunkitteke’s (fn 17) electoral pseudo-choss is the only barrier that prevents Toles’ recentBuffalo News cartoon (reprinted: New Post Weekly Ed 1995/5/18) from coming true. (Out in public, that is. In private, it’s already true; though not literally, since neither party’s polls are running anything. Except errors. For forces even wealthier than themselves.) The cartoon envisions a near-future here’s-the-latest-from-business announcement on TV news: “[We have a new merger to report. It] makes good business sense, encouraging a creative synergy. We can keep our core areas of traditional revenue, but add potential for a new customer base. While in the past we have had periods of intense competition, in today’s climate we have to look at our common bottom line: money. That’s why this Republican buyout of the Democratic Party is such a terrific deal for both the players and the American public . . . . (Voter layoff notices will be mailed out shortly.)”

11 See DIO 1.7 91995 & fn 19 (on whose fractional wit, see 1995 Summer Covert Action (Quarterly #53 p.4) has a point regarding Mappletonther: if the US Art World wants publicly to hang photos of homosexuals m-s, it shouldn’t expect heat support. This is part of the wider question of whether such public support is appropriate for a private fraternity, advancement up which appears to depend on factors often not discernable in the artistic production — but where the suspicion thus lingers that mercantile & carnal talents have filled the void. (Lucky there’s none of that in het showbiz, . . . .)

12 [Note added 1995.] Similarly, creationists affect love of free speech when trying to push creationism into bio classes, and the Roman Church (forgetting for-now aloft of sacred history: [15 fn 33) loves freedom of religion (pope, Baltimore, 1995/108). So, will both cults be inviting representatives of dissenting organizations (including atheists) for open debate in classrooms?

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14 Thus, the establishment view is the inverse of the truth. I.e., no sane US city-dweller today seriously expects to be protected (by police or courts) from a street attacker. By contrast, we do (see similar self-deprecation by Aubrey Diller at DIO 4.2 Q ualification Held Hostage #2 p.55. See also DIO 1.2 681 & fn 49.) But the comment reveals the very opposite of NS’ genial implication, and we may formulate a general principle regarding the process:

The cleverer a discovery is, the more the discoverer feels it was lucky.

These are the only prison that can stop crime? Answer: A condom.

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E  Getting the Best Hope for Cutting the Poverty-Cycle

Margaret Sanger promoted birth control primarily in order to help the poor out of poverty. (No other social-improvement program can long succeed if population control fails.) However, modern political and religious leaders have outfoxed her (with ever-loyal media assistance): by

E1 Doling out welfare-pittances to subsidize poverty, barely enough to keep ever-abundant that mortal potential-scape cheap-labor-pool which US employers encourage, in order to bite down laborers’ wage-demands. (Coincidence: the US’ interest in spending gov’t funds on welfare only began shortly after birth control’s use became legal & effective.)

E2 Keeping RU 486 (mifepristone, aka “The Death Pill”) from US women for a decade regardless of whether the President is (purportedly) feminist or no. (How much longer is it going to be before women get suspicious of the depths of corruption that this issue?) Note the provocative circumstance that RU 486 has been systematically kept from the citizenry of only one top nation, the US — which also just-so-happens to be the nation most unremittingly targeted by the foetus-hugger anti-abortion lobby.

E3 Virtually banning from guvprop and from TV ’snews (excuse our redundance) discussion birth control and-or abortion in any context but that of individual rights — not as part of a social-repair strategy. ([See DIO 2.3] §7 §A6 & DIO 4.2 §9 fn 44. According to Willspeake and Newspeake, the only permissible policies for dealing with the US’


E4 The Making-Murderously-Inclined-Women-Into-Moms Movement [MMIWMM]. How can TV ’snews take seriously a GOParty that claims to be anti-welfare even while it fights abortion? — a crusade that effectively forces unwanted children upon unwilling parents (for-their-own-good and-or as punishment for carnality), and then affects perplexity at slums’ immortality. In this connection, we note that, by their own abortion-is-murder logic, intercorder-pro-lifers are insisting that pregnant potential murderers MUST become mothers, whether they like it or not — a highly efficient prescription for creating child abuse & one fine instance of massive brain-finesse-out on this issue.)

F We Shall Undercover

18 §B3. DIO particularly stresses societal, not just individual, birth control for this reason — and because arguing its need (& the social consequences of ignoring it) is now almost as banned from popular US media as it has been under explicitly theocratic dictatorships.

19 This has been the context where (E3) young men can now be held legally hostage for eighteen years at the whim of a deceitful sex-partner who wishes, e.g., to prey with holey condoms — whether she is seeking to bind the man (out of love) or to drop out of school motherhood-as-a-paying-job (out of stupidity). If the father-to-be doesn’t want the child, why can’t the gov’t tell the mother-to-be that it’s up to her to either. Support. Aport. Or if the society that didn’t want more-prospect children would entertain that approach. Instead, such an obvious & central idea isn’t ever mentioned (fn 25) in the US’ self-congratulatory Free Press. (And poverty continues to expand, as: “leaders” [D3] shake their heads in mock perplexity.) I note that one of the prime secular promoters of this almost priest-like letter-of-the-natural-law lack of perspective. ACLU (which never bother to US media thoughtfully in connection), regards promotion of such an option as an application of warranted force upon the woman. By contrast (DIO 4.2 §9 §R4), ACLU does NOT see point-of-IRS gun forced-taxation (to underwrite the growth of massive semi-literacy & poverty, which ACLU has been so instrumental in inflicting on the US) as applying unwarranted force upon the invaluable provident-securely-middle class — the monotonic shrinking of which does not alarm pseudo-clueless ACLU in the least, even though this is THE major longterm US threat to civil liberties (ACLU’s own middle-name): fn 25. ACLU’s no-bad-means-to-good-ends fetish is equally pristile, as is the ability to set that smokescreen aside one’s nation goes to war, & forthwith to acquiesce in the marriage of for-foreigners, even by the formerly scorned-middle-classones one then depends heavily upon to manage such dirty work.

20 At least Newt’s orphanage-proposal faces (& hopes to sever) the self-perpetuating & cyclical nature of welfare-subsidized poverty. But it still falls short of asking why we must tax (into [relatively] low-fertility) providently-loving middle-class parents, in order to support the issue of imprisoned parents? i.e., the welfare-moms-as-orphanages “debate” is (since neither option can stem the US’ ongoing massive tide of careless parents’ unplanned children) as phony as the parallel Dems-vs-GOP “debate”. Just as the US needs a [genuine: DIO 2.3 §6 fn 23] third party, so it also gets a serious 3rd option on poverty — simply placing the discouragement of unwanted-child-creation ahead of guaranteeing every for-teen’s birth: fn 17, 19, 25.

21 [Note added 1995.] According to 1995/93 SelBS-New’s, it was a “historic” occasion: the Senate changed everything about welfare. (E.g., time-limits.) Everything except, of course, the one unkillable item which, if not changed, will mean that nothing has changed: bastard-subsidization, the radical-revolutionary opponents of which were called “reactionaries”, while “moderate” was the label for the same-ol’ it’s-not-the-babies’ fault-so-gotta-fund-“our” kids bastard-lobby. (Starring the senator whose wisdom has been improving New York for decades now: DIO 1.1 §2 fn 7.) I.e., a national TV ’snews network characterizes those pushing continued tax-subsidization for the voter-breeder-mom profession as: “moderates”. Note the ironically perverse upshot of the GOP’s imparable marriage of concern between welfare-lover-haters & fortes-leguagers: if welfare for the jobless is now shrank, then even more tragically poor folk (than previously) will, in short-term-thinking depression, feel limited-option-cored into having kids just to get AFDC-pittance-cheques, hoping to fend off the wolf-at-the-door; thus, the slum-consumption of pre-doomed poverty-ients (with parents just that forward-looking, might even get even worse. This is “historically-moderate”?) (Well, the pol responsible for the final bill r named: Dole.)

22 Question that effectively wonders just who really designs US inner-city disaster programs: Why is it that the very leftists, who swiftly realized that pouring money into late 1940s China wasn’t helping the Chinese, have — even after 40 yrs of pouring money into inner cities — found that pouring money on inner-cities is a prime solution to the poverty cycle — by those upstandingly-religious conservatives? All right-thinking-Americans ardently await the international-satellite-tel- nation most unremittingly targetted by the foetus-hugger anti-abortion lobby. In both cases, so much of the money has systematically ended up in corrupt gangsters’ & druglords’ hands, that they live in splendor while the poor remain poor. Final question of our little quiz: who do you suppose has paid off Congressmen to continue such policies, [a] the allegedly-intended-beneficiary poor, or [b] the corrupt warlords?

23 [Note added 1995.] As a USA Network 1995/8/30 spot (attacking racism as an adult sickness) unconsciously says: “It’s much easier to build a child than to repair an adult.” Just as Christian cultists will often get selectively agnostic when asked about god’s post-life intentions for less-fortunate but morally-blameless folks who lived&died without knowledge of Jesus, so bleeding hearts will selectively face rehab’s limitations — when confronted with the prospect of rehabbing anti-rehabbers. (See DIO 2.1 §1 [HI].)

24 Question: how many of these purported “deadbeats” ever wanted the child in the first place? (See fn 19.) i.e., how many are actually victims of the bastard-lobby? — of those who wish to encourage accidental pregnancy because the gov’t is too cheapskate to arrange longterm rational encouragement for parenthood, and so depends upon the improvement to produce plenty of new kids regardless. (The effect of this upon the nation’s mean cultural level is too obvious to require detailed comment.)

25 True, the punitive approach may dampen rational folks’ fertility. But how does it deal with the very, very, occasional citizen who may be a trifle less than rational and forward-looking? What is the objection to (fn 19) the imposition of limits, upon numbers of children born to those members of the population that (even if they bothered to marry) aren’t anticipating how many kids they can afford? (Society nonguiltily sets limits upon these same citizens’ amounts of less important creations than children, e.g., food, housing, & automobiles.) A societal blind-eye (actually derejection), at this critical stitch-in-time juncture, is a prescription for either brutal mass-starvation or (more likely) an even higher shrinkage-rate for the intelligent fraction of the population than already holds. The disastrous effects (of this trend) upon the rationality & sanity of US politics are already oppressively ubiquitous — and indeed are now probably irreversible by purely democratic or laissez-faire means. This is, of course, one of those instances where (of this trend) upon the rationality & sanity of US politics are already oppressively ubiquitous — and indeed are now probably irreversible by purely democratic or laissez-faire means. This is, of course, one of those instances where...
F  Trillions vs. Peanuts

The US military officially spends about 1/4 trillion of our tax-dollars per annum. (And its favorite GOP pols unfailingly wish to increase this paltry sum, so that the poor Pentagon can do more than — as now — just barely scrape by.) Why this monster outlay? Facts:

F1  Nobody is invading the US. Except Mexico.
F2  Foreign spies (e.g., Russian, Israeli) can buy a billion-dollar US weapon’s plans for ordnance 1% of 1% of the funds we were taxed to pay for it. (i.e., $100,000.)
F3  And the US pols who rule the weapons’ use themselves be bought-controlled by foreign bribes, for similarly trivial outlays. (Ever wonder why Japan: [a] doesn’t bother with massive military junk, & [b] is more prosperous than those nations who do?)

G  Who Says Our Taxes Are Wasted? Three Trilling Adventures

It should comfort all of us to learn that three difficult US problems can be solved by annually spending, on each, merely about a trill apiece: $1,000,000,000,000.

G1  We know [DIO 6 §4 §7] that a defense-lawyer-subsidy trill can ensure that no one will ever go to jail again — so long as he’s good-looking. And, as part of the US’ medical trill (§G3), we can guarantee all citizens the plastic-surgery they need to get handsome enough to survive any jury’s fussiness in such a critical department. (How to pay for the needed surgery? Simple, since we’re not going to need prisons ever again, we can cancel all the current cruelly-anticrime era’s outstanding orders for prisons and instead devote the funds to the doctors-of-beautication.)

G2  And, of course, we ought to throw another trill/year at the Pentagon (§F). Look at it this way, wouldn’t you spend anything to ensure that Clinton not only rules us but the whole world?

G3  Finally: most medical plans the gov’t is now mulling over will cost several thousand dollars per year per insuree. Multiplying by the number of US citizens, we find that proper medical care will cost25 the US ordmig a trill a year. Now, finally, we can close in on the true unsponged goal (of expending ever-more-astroonomical portions of gov’t treasure upon medical care): no one will ever again die. Instead, at roughly age 100, we’ll just transform permanently (§A4) into immortal senility — wedded forevermore to artificial hearts & whatever.

G4  A striking addendum to §G3: the US is the most totally & aggressively religious of all technologically advanced nations. The great majority of its citizens will swear to any pollster that they do verily believe in the standard Christian bye&skypie:26 eternal life. The deal being that, as soon as they die (if suitably confessed&blessed by an establishment-religion priest), they join Jesus in heaven and become angels who live in eternal bliss forever after. Despite the purported attractiveness & much-confessed ethereal beauty of said vision, the US citizenry appears prepared to spend a trillion dollars/year to pay our doctors to forever bar us from entering this heavenly goal of our lives. Instead, we will linger on, for eternity, upon our soiled Earth. As ever-undead vegetables. Well, if the US is going to go bankrupt, at least it’ll be for an upbeat vision of the future.

G5  We can put this more succinctly and thereby face starkly what happens to logic when there is a conceptual conflict between two potent lobbies (in this case, medbiz & godbiz): the US is outspending all nations, in order to stay out of the Holy-Heaven paradise which it, above all nations, allegedly longs for.

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25 The medical establishment frequently gets rosted in these pages. However, [a] DR is not a proponent of loko “alternate” medicinal treatments. [b] He is well aware of the benefits of US medical research, which is the best anywhere in history. (In addition, only a very flush lobby can take on the tobacco industry — so even the fat medbiz has at least one key side-benefit. Note a bit of ironic justice: the more people tobacco slowly tortures to death, the richer its medical-lobby enemy gets.)

26 D.Hammett Maltese Falcon NYC 1929 Chap.19: “Spade laughed at fasttalking Gutman’s glittering promises. “I know you’ll give me millions later... but let’s stick to this now. Fifteen thousand?” “

†14  Recovering Hipparchos’ Last Lost Lustrous Star

In DIO 4.1 §3 fn 20, DR suggested that the Ancient Star Catalog’s star PK964 (listed magnitude m = 3) was either δ Cen (pre-extinction magnitude m₀ = 2.60) or μ Vel (m₀ = 2.69). I there reasoned that PK964 was probably the former, because that hypothesis required but one error by the cataloger.

I now realize that, while connecting PK964 to μ Vel indeed requires presuming 2 errors, these errors are not independent — to the contrary, they’re the very same error: an easy confusion of step-number with step-interval.1 (Similar modern confusion-oppportunity: an apartment’s 5th floor is usually only 4 floors above ground.) The sole difference in the 2 mistakes is the reference frame: equatorial in one case, ecliptical in the other. As previously noted (iden), δ Cen is already accounted for by star PK960, whereas μ Vel is the only star brighter than 3rd magnitude certainly missing2 from the Catalog. Details follow.

Zenith distance Z was pretty accurately recorded as 75°, giving (for Cape Prassonesi’s presumed 35°/56/6 latitude) declination δ = −39°1/6. The r.s.a.c σ was observed3 as 9°7/12 — and thus recorded as: 10° step plus 8°3/4. This was erroneously converted to pure degrees by addition: α = 10°15° + 8°3/4 = 158°3/4. Transformation to ecliptic coords (using Hipparchan obliq. c = 23°11/12) would rigorously produce longitude λ = 179°56° & latitude β = −43°44′. Anciendy, this was: λ = 13° step plus 0° & latitude β = −43°3/4. Conversion of λ’s steps into degrees involved the same mistake (as for δ), so λ = 13°15° = 195°. And Hipparchos’ position for PK964 is in fact: λ = 195° & β = −43°3/4.

This reconstruction provides a neat resolution of the last major identity-anomaly in Hipparchos’ great Catalog.

[Note added 1995.] Incidentally, it now goes without saying that the Ancient Star Catalog is Hipparchos’. (As proved in 1977 by R.Newton Crime of Claudius Ptolemy & DR in Sking 2.1:62 p.73 n.6.) The curious 1989 paper4 of Fomenko et al, which dates the Catalog to the Arabic period, is inexplicably cited (as a solid contribution) in the otherwise high-quality paper, van Dalen 1994 (n.1). The Fomenko et al paper’s incredible date is based upon several lapses of procedure, most notably the authors’ mistaken use (when going from their Table 1 to Table 3) of the Catalog’s 900 AD obliquity-extern (21°: cited at their p.225) as a constant in time. (Since the 900 AD obliquity was 23°35′, this error corresponds to the zodiac-cataloger’s astrolabe-obliquity-setting = 23°36′ — a result already derived by DR & published in 1982 at eq.27 of PASP 94:359.) Given the multiplicity of evidences (DIO 2.3 §8 [C22]) showing that the Catalog is Hipparchos’, it is now way past time that the Catalog controversy be regarded as concluded. To yet continue stubbornly flying in the face of these evidences is to carry unfalsiability to kook dimensions — and to raise the question of whether it is worth discussing historical issues at all. (Of course, one may easily understand why certain parties might wish to render reason and competence irrelevant to the evolution of ideas in this field.) For, if even the most logically & evidently one-sided controversies are to be decreed1 as indesirably irresolvable, then — why investigate anything?

1 In ancient spherical astronomy, a “step” equalled 15°. See idem.
2 Excluding stars whose light was either much dimmed by atmospheric extinction or totally blocked by the horizon.
3 This is about 13° high. There are other Catalog errors this large, especially in the south (e.g., the latter stars of Cen). But it must be noted that the δ Cen hypothesis requires presuming less α observational error (6° high). In g-circ measure, both errors are ordmag.1
4 A.Fomenko, V.Kalashnikov, & G.Nosovsky Acta Applicandae Mathematicae 17:203 (1989). (The paper fallaciously damns the same 1987 study by Yu.Efremov & E.Pavlovskaia, and ignores an independent nding [Rawlins 1982] of the Cataloger’s epoch. [And site. Higher precision at DIO 4.1 §3.]) I understand that this citation was printed not by van Dalen but by a Maltese-circle advisor.
5 E.g., by N.Swerdlow. (See DIO 2.3 §8 §§C20&C25.)
Naked Came the Arrogance

Shunfight at the 0G Corral: Portrait of an Archon

The Reality Behind Academe’s Free-Discourse Pose

The Muffia at the Dawn of the Ptolemy Controversy

A  The PRIMO Principle


A2  My inductive ability bears a debt to their wisdom which I cannot adequately measure, let alone express. But, remarkably, not one of these eminent thinkers’ publications ever mentioned the PRIMO truth-determination principle — a principle that over-rides ALL the refined-scholarship rules and guides which naïvely earnest university students (such as DR) have foolishly spent years of labor (and thousands of dollars) to learn.

A3  This precious principle is marvelously simple & clear — and it positively resolves any controversy. Instantly. No matter how complex the facts may be or seem to be. The PRIMO-principle is simply this: the side that’s telling the truth is the one with the most money.

(A Note that the PRIMO-principle is indistinguishable from the way TV ‘snewls decides truth for us: whoever’s selling Truth best is telling it best.)

A4  History-of-science’s Princeton Institute-Muffia-O. Gingerich [PRIMO] combine has been teaching this principle to DR for two decades. (A kindness which is due to the combine’s recognition that, in a soft field [DIO 4.2 ¶9 fn 46] — like History-of-science, the PRIMO-principle is the only principle.) But, unfortunately for this distinguished cult, it is dealing with an invincibly-unteachable pupil: a political moron.

A5  And Mufoshi have their own learning difficulties. Even aside from inability to recognize compelling discoveries by those who dissent from Muffathought, they also have yet to perceive the unattractiveness of gang-bully cohesiveness. There is an admirable Biblical saying (Exodus 23.2), long a favorite of Bertrand Russell’s family (& of DR):

Thou shalt not follow a multitude to do evil.

B  Ringo Newton, There Ain’t Room Enough on This Here Planet for the Two of Us

Few followers of the Ptolemy Controversy have the faintest idea just how long ago the Muffia began hating and slander ing the work of the highly capable and respected physicist

Robert Newton (RN), Supervisor of the Space Sciences Division of the Johns Hopkins University’s Applied Physics Laboratory (Laurel, MD). Fortunately for historians of this affair, RN is alleged to have written a report about his very first contact with Hist.sci.’s anti-heresy inquisition. (The inquisitor: O. Gingerich, later head of Harvard’s Hist.sci Dep’t.) It is entitled: “Notes on a Telephone Conversation with Owen Gingerich”. The (largely typed, partly script) document’s text follows, starting with RN’s handwritten marginal note:

Written on either Dec. 3 or Dec. 4, 1968. That is, either on the day of the call or the next day. This note was added Feb. 21, 1969.

Gingerich tried to call me on Monday, Dec. 2, 1968, the same day that I received the peculiar letter from Nature rejecting my paper about the eclipses of Caesar and Stikelstad. We did not actually make contact until Tuesday, Dec. 3. The conversation lasted about half an hour. I certainly cannot reconstruct the conversation precisely, however I am rather sure that the notes which follow represent the spirit of it faithfully.

B2  He started by finding out whether I was interested in extending my work on ancient astronomical observations beyond the paper that is now in progress, and it was established that I was, although I did not intend to extend the present paper itself, except for the completion of Part II.

B3  He then stated that the subject was very complicated and no business for an amateur. [Note by DR: on OG’s expertise, see ¶C12.] From this point, I shall not attempt to follow the conversation chronologically, but only to summarize the main points.

B4  My amateur status was shown by my ignorance of the literature. One cited instance of my ignorance was the work on the Babylonian “astronomical diary” by Neugebauer, which is not even finished yet, much less published. According to Gingerich, this contains a large number of observations, particularly of lunar occultations. The other cited instance was a doctoral dissertation on Ptolemy’s data done at Yale about two years ago. This dissertation has not been published and, according to Gingerich, almost surely never will be [vs. fn 4], because the author [J. Britton] has gone into stock-brokering or some such non-scientific field. Gingerich did not know the author’s name.

B5  Any attempt on my part to publish anything about Ptolemy will be fiercely resented by the Department of the History of Science at Yale, because of the existence of this unpublished (any chance it is unpublished?) dissertation. Yale and Brown (Neugebauer) cooperate closely, and my attempts to publish would certainly mean that I will be denied any access to the Babylonian work.

B6  [Note by DR. As at DIO 1.1 ¶1 fn 20, we pause to appreciate OG, who — despite his remarks, quoted there and here, showing his complete awareness of the Muffa cult’s suppressiveness — called DR “exceedingly paranoid” for “suggesting that a cabal has been suppressing the consideration of [R.N. Newton’s] work on Ptolemy. Despite DIO 1.1 ¶1 fn 20’s 1991 publication of OG’s demoted lie-slander, OG has not seen fit even to withdraw his libel, much less express regret for it. See here at ¶H10 item [1]. (Note added 2009. RN’s heryes [that Ptolemy lied] is now vindicated. Archon’s vanity’s hideously transparent antidote? See www.dioi.org/fft.htm#mhwx!)]
B7 (The force of this threat was largely negated later in the conversation. It appears, still according to Gingerich, that Neugebauer is feeding the Babylonian occultation data to a graduate student at Yale as fast as he establishes it. Thus I would not have any access to the data before the astronomical analysis of it is published in any case. Once it is published, I cannot be denied access, whatever access may be worth at that point. I refrained from pointing out to Gingerich that this information negated his threat.)

B8 In sum of this part of the conversation, the Gingerich-Yale-Brown axis intends to exercise control over my publishing in the field of ancient astronomical observations. It was not clear to me whether I was being forbidden to publish only about Ptolemy or whether I was being forbidden to publish at all.

B9 The conversation closed with reference to my claim that there are errors in the [D.Menzel-O.Gingerich] Preface to the [1962] Dover edition of Oppolzer’s Canon. In my opinion, he continued to show the same lack of comprehension [§C12] of the subject matter in conversation that he did in the Preface. He tried to maintain that Fotheringham’s big paper (1920) on solar eclipses is full of references to the Canon, and that he knew this because he had just finished rereading the paper. A look through Fotheringham’s paper shows that Gingerich’s claim is false. He tried to maintain that the whole line of work of Fotheringham and others on finding the secular accelerations was inspired and made possible by the Canon. He seemed to be ignorant of the work in the field before the Canon was published, such as Newcomb’s 1875 paper, and he also did not know that Martin at Yale re-analysing the occultation data used by Newcomb. He felt that the error about the saros has been made so often that it has become correct, and that it is beyond all question that he was correct in his usage of the word. [Note by DR: I side with OG on this one.]

B10 (According to the biographical sketch in American Men of Science (does this list any women?), Gingerich has been a lecturer in the history of science at Harvard several different years. This accounts for his feeling that he is an expert on the subject.)

B11 The timing of Gingerich’s phone call with respect to the [1968/11/25] letter from Nature, and the uncompromising nature of his call and of that letter, suggest an hypothesis: Gingerich, or a friend of his, blocked publication in Nature by writing a letter to them warning them that my papers on the subject are no good. He received notice of their action, and knew that I had received and been “softened up” by the rejection, and chose that time to call.

B12 I am trying to test whether Gingerich indeed speaks for the Yale department by a letter I have written asking for information about the dissertation.

B13 Since Gingerich is at SAO [Smithsonian Astrophysical Observatory], I would expect that he can and will block publication in the Smithsonian Contributions to Astrophysics. Further, Duncombe will almost surely turn to this axis (such as Clemence at Yale) for review of the big paper [see §B16], so there is a good chance that it will be rejected by the Naval Observatory. I must probably find an alternate source of publication and try to manage for review by [Sir Harold] Jeffreys . . . or some other charitable soul.

B14 RN later added the following comments, entitled “Explanation of the Accompanying Notes on a Telephone Conversation with Owen Gingerich”, dated 1980/11/17.

B15 I have just reread the accompanying notes of a telephone conversation I had with Owen Gingerich on December 3, 1968. As the handwritten note suggests, I did not date the notes originally, and when I did date them on February 21, 1969, I was unsure whether I wrote the original notes on December 3 or 4. In any case, they were written almost immediately after the conversation.

B16 The paper mentioned [at §B2], and the “big paper” mentioned [at §B13], both refer to what [eventually] became Ancient Astronomical Observations [R.Newton 1970]. At one time I expected to publish it as a paper in the Astronomical Papers Prepared for the Use of the American Ephemeris and Nautical Almanac, but that never worked out. I do not know whether that was for the reason hinted at or not; I never actually submitted the paper to the Naval Observatory, because I was discouraged from doing so although I was originally encouraged to. I prepared Part I of the book [R.Newton 1970] and distributed it in preprint form before I started Part II. I think this must be why the subject of Ptolemy came up, since I showed in Part I [R.Newton 1970 p.20 & Table II.2] that Ptolemy’s solar observations were fabricated.

B17 [At §B11] I suggest an hypothesis which I tested by writing to Yale asking about Britton’s dissertation. I did receive an answer identifying the dissertation and its author, and I subsequently got a copy from [University Microfilms]. I also did another test:

B18 The paper sent to Nature was originally rejected on the ground that it was so short that it was incomprehensible, and it needed to be lengthened before it could be considered. I revised it slightly, lengthening it by half a dozen lines in the process, and resubmitted it. Again it was rejected, this time because it was too long! At the same time, I got another call from Gingerich, with suspicious timing. He reminded me of the earlier call and wanted to assure me that he meant business. I consider that the hypothesis is established.

C God Save the Grand Inquisitor

C1 At Dio 1.2 §11, it was recounted that, on 1976/11/12, a few days after DR mailed Science the first précis of his least-squares analysis of the Ancient Star Catalog, a man phoned c.8PM for DR from Cambridge (Mass) and — finding DR not at home — grilled DR’s wife for details regarding his person & plans. The caller did not leave his name. However, in light of the foregoing and the common factor of post-submission timing (see §§B1, B11, & B18): is anyone now incapable of inducing the caller’s identity?

C2 Since there are those who wish to blame the unpleasantness of the Ptolemy Controversy upon DR, the reader should be reminded that the foregoing shunning-obscenity (§B) occurred several years before DR had even entered the Controversy. (See also the reception of DR’s occasional attempts at amiability: Dio 1.1 ¶4 fn 20 & ¶3 fn 7, Dio 2.1 ¶3 fn 20K31. [Note especially the 1st reference & here at §B6: the almost comical perversity of professedly religious OG’s record-setting apogee in vileness-space. No self-respecting fiction-editor would permit such behavior even in a storybook character.])

6 [Date: 1976/11/1]
Another pre-Controversy happenstance: around 1974-5, I became interested in the mentioned the *QJRAS* papers. At this, a visibly agitated OG burst out with an unqualified assurance that R.Newton's analyses of Ptolemy were worthless & disreputable scholarship, completely unaffected by the establishment — adding that a top Yale expert (unnamed at the time but clearly Asger Aaboe, Hist.sci., Yale Univ) was preparing a devastating response. (Which has never appeared.) Under Aaboe's name, at any rate.

C9 Later that day, I joined OG at a purely social gathering of scholars. During the conversation, I brought up some suppressive (& overly personal) behavior by Editor Jos. Ashbrook of *Sky & Telescope.* OG listened with obvious interest but said nothing about the matter in front of the group.

C10 The next day, we talked by phone, at which time OG only now (privately) volunteered that he had had a parallel experience with Ashbrook (who years earlier had for ordmag a year unjustly condemned OG as misinformed for having made certain “errors” which in fact were based on JA’s own use of an obsolete source). Naturally, I was interested in finding someone who knew that my account of an utterly weird JA encounter was credible. (I would not have believed it myself. Until it happened.) But when I asked what one could do about it, OG said that nothing could be done: JA is just “like that”. OG added that he presumed that I would not do nothing, but added that if I did react to JA: “You’ll only hurt yourself.”

C11 This translates: you can’t fight City Hall. What I didn’t know at the time was: Gingerich is City Hall. One is reminded that many scholars who cite Sherman’s war-is-all-hell appraisal fail to add that he made it so.10

C12 OG was a wellknown & (I then thought) highly respected figure. I inexcusably permitted these (at best) irrelevant factors to persuade me — and so stupidly accepted (for many months) what OG said about R.Newton’s charges against Ptolemy. (In retrospect, I am puzzled that OG’s opinion is so overrated. His errors [*DIO 6* \[1 fn 66\] are less telling than the fact that he remains virtually the sole regular participant in the Ptolemy Controversy who has never made an important original discovery in ancient astronomy. I doubt he even claims to have done so. This is quite aside from his judgements upon others’ discoveries, a department where he also exhibits little discernable talent, though RN suggested that he may serve as a handy reverse weathervane: if O.Gingerich takes a strong position, the contrary will usually turn out to have logic in its favor.)

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C3 The propertary nature of this guiding principle afforded useful later insight (\[G9\]) into the actual psychological mechanics underlying the purported reasons for Hist.sci resentment of the terrible bottom line of the Ptolemy Controversy: mere physicists, not Hist.sci specialists, discovering some of the prime secrets of ancient astronomy.

C5 [Always friendly in a personal way with the JHU Hist.sci crowd in those days, I later played alot of softball with Kargon on the Hist.sci team. In 1984, about the time I’d become a political untouchable in the eyes of the *JHA* Editor, Kargon suddenly concluded that my facile switch-hitting habits were “obnoxious”. (His newfound partiality was particularly weird since Kargon was the first Hist.sci person who had warned me of the peculiar bigotries of the Mufida, and had described Hoskin to me as just a power-operator who was trying to get his then-young journal off the ground.) Considering the sharpness of language Kargon had routinely used when discussing other scholars (e.g., Aaboe; and see *DIO 2.3* [5 \[F1\]]) was not especially in the course of the disapproval of DR. (Another possible factor in his alienation: I had arround this time informed Kargon that the Ptolemy Controversy was becoming a lessonic war of two worlds — scientists vs. Hist.sci. From this time, a one-sided chill settled into our relations.)]

C6 The first detailed R.Newton papers proving Ptolemy’s fakery were published (1973-1974) in the *Quarterly Journal of the Royal Astronomical Society.* (All Fellows of London’s Royal Astronomical Society receive its house journal, the *QJRAS*, as a perc of RAS fellowship.) This is how I first heard of the Ptolemy Controversy.

C7 Without reading the articles carefully at the time, I presumed that there must be something to the exposure. But about the only specific datum I initially absorbed from the material was that the information that R.Newton worked nearby. (I lived in Baltimore, less than an hour’s drive away.)

C8 On 1974/11/15, I met6 with the wellknown Harvard & Smithsonian astronomer & historian Owen Gingerich at Goddard right after he had delivered a talk there, and since JHU-A is very near Goddard, I asked OG (as we chatted in the right half of the audience area of the hall) whether R.Newton also had come to hear the talk. At the sound of R.Newton’s name, OG reacted with a strange look, replying: no, but-why-do-you-ask? I

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7 Astronomers’ geographical manuals are not (like, e.g., Strabo) concerned with descriptive geography, but are mere lists of longitudes and latitudes. Ptolemy’s “Geography” (Geographical Directory) is of the latter type: c.8000 sites’ longitudes & latitudes, by far the largest such compendium surviving from antiquity.

8 The criticism of Aaboe was that his *exegese* was inappropriately slim. This represents a failure to understand that Aaboe’s work is largely inductive (not merely slapping together a biographical piece on someone: the standard Hist.sci project); thus, a huge amount of mental effort will inevitably produce just a few pages of results, but such results may represent much more novelty & intelligence than the Hist.sci norm.

9 I had first met OG on 1974/2/11.

10 If it is under 20 pp, *DIO* would be happy to publish Aaboe’s attack on R.Newton. (This suggestion is not purely libertarian, I would expect Aaboe’s comments to contain worthwhile material & reflections.)

11 Still a sickness at *S&T*, I’m sorry to report. On the other hand, it must be said: whatever Joe’s oddities, he was a marvel of astronomical lore, and I miss his “Scrapbook” entries in *S&T.* (See Sam Goldstein’s 1987 appreciation in *JHA 16* \[147\]. I even agree with Sam’s choice of Joe’s best entry: on solar oblateness.) It has been a comfort to me that, at the time of his justly-lamented death, we were back on good terms. (Reconciliation was triggered by a funny incident — cited at *DIO 1.1* \[3 \[A3\] — which Joe took in just the right spirit. We remained friends from then until his death.)

12 DR had criticized *S&T* for its ludicrously-superfluous censorial policy of refusing ads for the nutty Velikovsky journal *Propos.* So an enraged *S&T* had tried to wreak vengeance by rushing into print a review (1973\[1\] 38) implying that DR’s new polar book, *Peary . . . Fiction* was unreliable. (To JA’s dismay, no one paid the slightest attention to his piece. *Fiction* is now regarded as a standard in its field, cited even in *Encyclopedia Americana*‘s Peary entry.) *S&T* then added a correction: [1] suppressing DR’s reply to the review, and [2] refusing (for all time subsequently) to besmear *S&T*’s pages by citing DR’s work. E.g., *S&T* has for nearly a decade suppressed the fact (provably known to *S&T*: its letter of 1987/3/2) that the famous compact refrac-correction format (now found in top astronomical & navigational manuals the world over) was invented & (1982 April) first published by DR. (See Rawlins 1982 eqn S&T. For improved *DIO* refraction & extinction formulas [of professional-level accuracy], see *DIO 2.1* \[4\] fn 17 & other fn there cited.) In 1986 July, *S&T* (whose Editor co-publishes with Gingerich) instead credited (S&T 72.1:70) the format to two later scholars. Apparently, in certain archonal circles (not exclusive to Cambridge MA), if one simply doesn’t like an inventor, no consciences are bothered when his due credit is stolen for a decade. (If now intended from appearances of DR’s priority, I suspect that agile *S&T* will go in another direction: losing interest in the inventor’s worth.)

13 Sherman also offered (1865/4/19) the most admirable generous peace in the history of warfare.

14 But see *DIO 1.3* fn 223 for citation of a key observation (which I believe should be credited to OG) that helps to narrow suspicions of anomalies of Ptolemy’s fakes. (See also where OG was right against an incorrect DR theory: *DIO 11.2* p.30, even if evidently by politically-inspired accident: *DIO 1.1* \[1 fn 9\].)
D Censorship Encountered

D1 Over a year went by with no involvement of myself at all in the Ptolemy matter. That changed (farther than) overnight due to an incident that finally smelled odd enough to alert me that something seriously wrong was going on. The 1976/2 *Sky&Telescope* carried a virtually ad-copy reviwe by CUNY’s Janice Henderson of *A Survey of the Almagest* (Pedersen 1974), authored by a prominent Hist.sci prof who then ran *Centaurus*, a journal which has carried a number of pro-Ptolemy papers but never one by the center of the skeptical contingent. (Still the case — 20 years later! Same for the equally captive reviewing journal, *Sky&Telescope*: fn 12. After 2 decades of such utterly one-sided promotion of Ptolemy, both journals now have accumulated an enormous face-investment in continuing to suppress the truth about the greatest faker in astronomical history.)

D2 What struck me right away was that the *S&T* review did not even mention the existence of R.Newton’s work, simply noting that there used to be doubts about Ptolemy but that “Pedersen’s book goes a long way toward reestablishing the reputation for integrity that Ptolemy deserves.”

D3 Henderson 1976 was not merely an ad for Pedersen’s book. It was an even more enthusiastic ad for Ptolemy’s *Almagest*: “the greatest astronomical treatise of antiquity, a compendium of all mathematical astronomy known at the time”, and (quoting Neugebauer 1957) “one of the greatest masterpieces of scientific analysis ever written.”

D4 When I learned that Henderson was a Yale protégé of Aaboe, I realized that a revealing re-write of OG’s 1974/11/15 confident assurance had since taken place: OG had said at that time that Aabobe would publish evidence showing R.Newton was wrong; but, over a year later, Aabobe had published nothing (& never has in the 2 decades since) in reply to RN; instead, his personal henchperson was scoffing at RN’s position while entirely ignoring RN’s evidence — not even telling *S&T*’s readers where they might consult it. I did not yet realize that noncitation had already for seven years been standard policy among Ptolemy’s defenders, being just about the only weapon they felt comfortable with. (Besides private slander of dissenters — which needn’t be accurate, since the party discussed is never on hand to correct errors.) Nor did I realize that attack by protégé-proxy was equally standard.

D5 Question in passing: would a clique with confidence in the evidence for its position behave this way? Would it for 20 years evade repeated suggestions of face-to-face panel discussion or debate? (The most public pre-DIO debate challenge is published right in the *AmerJPhysics*: Rawlins 1987 p.236.)

D6 Two decades ago, O.Gingerich was involved in the AAS scientists-vs.-Velikovsky bariasie episode (which, for courage, matched the US invasion of Grenada); thus, we conclude that he is not unwilling on principle to endorse or even arrange such debates. (Likewise, O.Neugebauer published, in *Isis*, a review of Velikovsky — but would not answer R.Newton.) Or, perhaps it would be more accurate to conclude (from OG’s approval of debate with V but not with RN & DR) that OG is not afraid of debate when he knows the law were openly saying that the Nixon defense lawyers’ strategy only made sense if the defense believed its client was guilty.) Another question is raised by this contrast: does OG believe that Velikovsky is worth more attention than the leading world controversy in ancient astronomy? (The Ptolemy controversy’s heatness-primacy was later publicly acknowledged by OG-sucophant B.Schaefer: www.dioi.org/fff.htm#rsgs.) Or is it rather simply that: until his opponents become prominent enough in the popular press (thus a palpable danger to orthodoxy) OG won’t debate them? (Similarly, Ptolemy’s top [Muffia] defenders — who do not include OG in their inner sanctum — did not publicly attack RN until the press began noticing him in 1977: §F4.) If so, then: [a] this implicitly makes the press a large factor — almost an arbiter — of what are important issues in academe; and [b] one must conclude that power-academics who attack scholars’ attempts to create press sensations are simultaneously & revealingly reacting only to the very same low-appeal approach which they profess to scorn; thus, they are effectively encouraging that proscribed behavior — and are penalizing those who prefer logical discourse to popular-media n.r.

D7 It was on 1976/2/25 that I first read & photocopied Henderson 1976. I was so perplexed by Henderson’s review that on that very day I made two unsuccessful attempts to reach her by phone. I then phoned R.Newton (same evening). We had never had previously any had communication. I asked him right off about the Henderson review’s noncitation of his papers. It didn’t seem to surprise him: he said this had been going on for 7 years, since R.Newton 1969! The stench of rottenness was getting stronger.

D8 He mentioned O.Gingerich’s having warned RN (B) that he would no longer be permitted to publish such charges. (This would not be the last time OG tried throwing his weight around: §H9.) So RN had had to send his Ptolemy-doubting papers to the QJRAS, because that journal was deliberately seeking opinion papers.

D9 This first (2/25) chat did not persuade me of much regarding Ptolemy, but the arrogant centrict treatment of heterodoxy made a powerful impression. I was on the verge of beginning a long experiment, which I have since found increasingly intriguing: testing the extent to which a small band of mutually backscratching scholars is able to control opinion in a specialty area — entirely regardless of incoming evidence’s strength. The secret of such success is the same as one of the keys to the Peary North Pole fake: just as Peary was in 1909 geographically isolated, far out on the Arctic Ocean ice (and thus protected from scrutiny), so any modern specialty academic establishment is intellectually isolated by its very specialization. What external sanctions on misbehavior can apply, when outside observers are given to believe that they cannot even understand the matter under discussion? The implicit insulation from checks & balances is the core ingredient of a recipe for corruption. Moreover, there is no deputed academic court of appeal for such matters (§3) — no recognized mechanism for dealing with cases of abuse. Isn’t anyone out there even a little concerned that modern academia — with all its genuine lofty intellectual merits & credits — has come to such a low, degraded state in the vital area of freesppeach? (See §4.)

E The Awful Stakes

E1 Decades ago, a small group of History of science scholars rejected centuries of knowledgeable astronomers’ consensus that Claudius Ptolemy — Mr.Geocentricity of antiquity — had faked data. They did so while slandering numerous prior scholars in this area as fools. (See, e.g., Neugebauer 1957 p.206. And Neugebauer 1975 pp.16, 274, 331 n.6, 334 n.10, 350, 734 n.14, 935 n.7, 937, 942 n.1, 959, 965 n.6, 976. And Toomer 1975 p.201 & *DIO* 1.3 [R2.] [For a happy contrast, see Neugebauer at his best at *DIO* 6 fn 100. Also the admirable remarks of Neugebauer 1975 p.vii.) Insofar as hypercriticism represents commitment to high personal standards of scholarship, this peculiarly rude approach isn’t a pure negative. But the Muffia is itself not quite genius-level. Thus, with respect to aggression-tactics: for this gang to savage others’ scholarly acumen is about as smart as starting a poison-gas war when your self-created Enemy is upwind. (Didn’t

15 From the discussion of Pedersen 1974 pp.255-258, this strikes me as an accurate inducition from the book (p.371 raises the thought of fraud but then backs off on p.372 by calling the evidence simply a mystery). However, Pedersen announced during a 1983/84 symposium that he had known all along that Ptolemy faked observations and felt that readers of his book ought to have understood this. I leave it to Pedersen & Henderson to work that one out. During a 1987/2 *JHA* review of Toomer 1984, Pedersen repeated in print this appraisal of Ptolemy’s observations. (He did not mention RN or DR.) So that is his position for now. [This suggestion (“for now”) was originally written in 1988. I refer the reader to *DIO* 1.2 fn 99.]

16 Whatever its pretensions, the *Almagest* is actually not a scientific work but a mathematical one: *DIO* 2.1 [2 fn 11.

WWI (Germany actually do this?) As a conspicuous tactic of the Neugebauer cult, attacking others’ alleged incompetence appears to be one of a number of manifestations of what the psychologists call “projection”.

E2 The attempted shift of opinion in Ptolemy’s favor has never really taken hold outside Hist.sci; but even the partial success was a definable Achievement of the Muffia, which made it feel important. Since there was never any strong evidence for Ptolemy, the cult went far beyond reasonable conclusions from evidence, in order to prove its case. It is possible that an early conscious realization of a key fiscal & careercr reality played a rôle in the sham this group has perpetrated upon the scholarly community. That reality: how can one raise funds for research in a field where the central document is a clumsy18 fake? And, even assuming the best initial Muffia motives, the ax problem lingers nonetheless: when a Cultleader & followers have for decades damned all dissenters as fools & knaves, the cult acquires a huge stake in making its position look valid.

E3 [Paragraph added 1995.] Thus, there is no way that such instinctively-turf-sensitive, gang-warring parties can independently choose to make peace. E.g., at the very moment when the JHA was making the potentially-fruitful (if insufﬁciently handled [DIO 6 §3 §C5]) gesture of creditably acknowledging DIO’s math-correctness on a single point, the Hist.sci Society re-opened hostility (in the HS’s Isis 86.2:309; 1995/6) by using 1 nonmathematician Muffioso Alan Bowen (who co-vested the very JHA 1991 elementary-school mis-math which the 1995 JHA was now correcting!) to pass off, as virtually worthless, Cambridge-trained mathematician Hugh Thurston’s valuable,20 gentle, open-minded, & highly capable Springer-Verlag book. (Which has won well-deserved praise from the Rom.Sci Societies, in F6:1 Nature 370:339. [We are happy & obliged to her thank Nature for listening to DIO 1.2 fn 12&96.]) Bowen’s unprovoked [F4] denigration of non-Hist.sci-cultist Thurston naturally has no relation to: [a] Hist.sci’s professional-survival-instinct desire [F4] to insist that scientists can’t do history as well as the Hist.sci cult. [b] Springer’s recent dropping of Muffia-capo G.Toomer’s valuable 1984 Almagest (an event which, despite the book’s flaws [F & Toomer’s seething, irrevocable loathing of DR], DIO genuinely deems regrettable). [c] Thurston’s tementy in deﬁning a private Muf­ fia warning never to cite DR [DIO 6 §1 fn 7 & §3 §E2]. (Understand: Thurston’s text only mentioned two DR results, while repeatedly and respectfully citing numerous Muffia achievements. But 99% assent is not enough. Just as no medical procedure can permit mere 99% sterilization. The book-burner mind won’t tolerate ANY heresy.) Not to mention: [d] Thurston’s JHA note [DIO 6 §3 §C] correcting a Bowen-verter’s error.

E4 The unbreakable barrier that prevents any progress towards compromise here is that, at this late date, if Ptolemy’s fraudulence is even seriously considered21 by academe (much less generally accepted), the loss of face by the Muffia cult would humble its precious pretense to expertise. And it knows it. Indeed, the effect on the viability of the whole highly insecure field of Hist.sci could be unpleasant. And Hist.sci knows it.

E5 Reality: if a couple of rogue physicists (RN & DR) could be right after all, while the cream of Hist.sci experts could have been (even might have been) adamantly & slanderously wrong for decades, well — Hist.sci archons don’t need to be told what questions will immediately surface. Each of the several (very few) major universities that have established Hist.sci departments could start asking why the university’s reputation need be dragged down by association with such embarrassments.

E6 Therefore, if Hist.sci promotion of Ptolemy as “the greatest astronomer of antiquity” is a horrifying ironic mistake (whether originally by well-intended folly or no), the awful truth cannot be admitted (at least on the record). The stakes are too high. Important faces are too deeply into potential egg. No matter how clear & potent the still-coming evidence may get, it cannot be allowed that the very astronomer repeatedly praised by the Hist.sci establishment as The Greatest of all ancient astronomers is in fact the biggest faker of the lot. The irony is too gross. Too pervers. Too hilarious. Too damaging to the whole Hist.sci business, whose top22 Ptolemy expert decreed that R.Newton 1977 “tends to bring the whole top [of Ptolemy’s manifestion of his material] into disrepute” (Toomer 1984 p.xviii). But, the evidence has gained so unfriendly to the defenders that many now content themselves with an increasingly watered-down version of the former position: Evans, Wlodarczyk, & O.Gingerich have essentially, though not as explicitly as acknowledged homeopathes, been reduced-diluted just to arguing that the manifest evidence convicting Ptolemy is not utterly conclusive. That is a long way from the conﬁdent Gingerich 1976 assertion that Ptolemy was the best that ever happened to ancient astronomy. Such transformations are among the nuttier features of The Controversy — the nuttiest being that many Muffiosi will deny there has even been a transformation.

E7 This and indeed all of the dementedly illogical, inconsistent,23 & burgeoning dishonest history recorded here is the inevitable consequence of this nightmarish vise — this tarbaby commitment to a short-term-smart course of trying arrogantly to stonewall out for getting, it cannot be allowed that the very astronomer repeatedly praised by the Hist.sci establishment as The Greatest of all ancient astronomers is in fact the biggest faker of the lot. The irony is too gross. Too pervers. Too hilarious. Too damaging to the whole Hist.sci business, whose top22 Ptolemy expert decreed that R.Newton 1977 “tends to bring the whole top [of Ptolemy’s manifestion of his material] into disrepute” (Toomer 1984 p.xviii). But, the evidence has gained so unfriendly to the defenders that many now content themselves with an increasingly watered-down version of the former position: Evans, Wlodarczyk, & O.Gingerich have essentially, though not as explicitly as acknowledged homeopathes, been reduced-diluted just to arguing that the manifest evidence convicting Ptolemy is not utterly conclusive. That is a long way from the conﬁdent Gingerich 1976 assertion that Ptolemy was the best that ever happened to ancient astronomy. Such transformations are among the nuttier features of The Controversy — the nuttiest being that many Muffiosi will deny there has even been a transformation.

F The Muffia & its Godpop

F1 The above-cited notoriously contentious & turf-possessive Muffia clique was fathered by (& long godfathered by) the late Otto Neugebauer, of BrownU & the Princeton Institute for Advanced Study, who for decades pronounced his own talents & his colleagues’ reps to the curious cause of glorifying the most transparent faker of ancient science, C.Ptolemy. (The common factor in this & ON’s other odd ﬁxation, Babylonian math astronomy as source of Greek [DIO 6 §1 fn 27], is: self-convincing modern academy’s grant-dispensers that ancient astrologers’ texts are high-science, which thus merit high-funded, high-expert-Mufa analysis.) ON managed to breed & plant at prominent universities a small but virulent clique of clones: Asger Aaboe (Yale), Bernard Goldstein (UPitts), & Gerald Toomer (Brown, Harvard). Each of these breeding-reactionaries has in turn attempted to clone further grad students.

F22 [Note added 1995.] Those (incl Rawlins 1991W fnn 172&236) who are unawed by Princeton socialite-scholars & the MacArthur Foundation shouldn’t miss J.Hitt’s highly perceptive (& funny) inside look at MacArthur winners. The PU connexion (Hitt 1995 p.96): for years, “it didn’t hurt to be a professor at Princeton (where foundation president Adele Simmons once served as a dean). In fact, five of Princeton’s new MacArthurs guests: All 1338. (Note: there is no difference between JB & NS on the subject of this subject. Consequence: religiously-obsessive inability to compromise with either heretics or heresy; i.e., the Muffa’s years of unbending decrees (& cumulatively dishonest pretense that heretics contribute nothing) have forced it to continue striving for complete & utter expurgation of dissent (& all disobedient dissenters) on Ptolemy. (Such poison infects even a productive scholar like Britton: §13.) Meanwhile, Muffioi slanders (e.g., DIO 4.1 §1 §C7 & §3 §D2-D3) paint the skeptics as the rigidly nutty side of the Controversy.

F23 See DIO 2.1 §2 [H21.]

F24 [Note added 1995.] Those (incl Rawlins 1991W fn 172&236) who are unawed by Princeton socialite-scholars & the MacArthur Foundation shouldn’t miss J.Hitt’s highly perceptive (& funny) inside look at MacArthur winners. The PU connexion (Hitt 1995 p.96): for years, “it didn’t hurt to be a professor at Princeton (where foundation president Adele Simmons once served as a dean). In fact, five of Princeton’s new MacArthurs guests: All 1338. (Note: there is no difference between JB & NS on the subject of this subject. Consequence: religiously-obsessive inability to compromise with either heretics or heresy; i.e., the Muffa’s years of unbending decrees (& cumulatively dishonest pretense that heretics contribute nothing) have forced it to continue striving for complete & utter expurgation of dissent (& all disobedient dissenters) on Ptolemy. (Such poison infects even a productive scholar like Britton: §13.) Meanwhile, Muffioi slanders (e.g., DIO 4.1 §1 §C7 & §3 §D2-D3) paint the skeptics as the rigidly nutty side of the Controversy.

20 See AmerJPhysics 55.235 (p.236 item 4) or DIO 4.2 §7 §B3 item 2 it'

21 Designating a Muffioso to review Thurston is about as innocent as deputing Wilberforce to review Darwin.

22 See DIO 3 §3B & DIO 4.2 fn 1.
This seething clique has made genuine contributions to knowledge— including my own knowledge. For this I have for over a decade repeatedly published\(^{25}\) my appreciation (entirely unreciprocated);\(^{26}\) and I shall naturally continue a policy of evenhanded citation, since that is the heart of honest scholarship. It is on this point of policy (citation of those with whom one simply disagrees) that the Neugebauer clique most blatantly exhibits both its scholarly & temperamental shortcomings — and reveals a cohesiveness of purpose which I will unFashionably but quite justifiably refer to as conspiratorial. (Written confirmation of this assessment will appear below,\(^{27}\) from an unexpected source: the Neugebauer clan’s most loyal public toadie, O.Gingerich.) The group’s other characteristic is its innocence of the temper tantrum & demiennded attitude of modern science — a precious ferity which has led it into so many entertaining howlers, omissions, & muffs in matters of astronomy, mathematics, & statistics, that readers will understand why DIO regularly refers to this gang as: the Mufa. (See 45-item catalog of Mufa muffs at DIO 4.1 §4 A.)

I do not mean to imply by the foregoing that the Mufa’s historical accuracy is a whole lot better\(^{28}\) than its science. The clan’s greatest scholarly strength — especially Toomer’s — is probably bibliographical. But Mufa affinity for deliberate noncitation of heretics is obviously fatal to one’s trust even in that.

The Mufa currently explains its reluctance to cite heterodoxy by complaining of DR’s behavior. (Classic self-fulfilling prophecy: my criticisms are entirely in reaction to the censorial behavior now justifled thusly.) The problem with that alibi is obvious: R.Newton never reacted as forcefully as I have to Mufast. But a Mufa citation-blackout (Rawlins 1991W §D4) was applied to him for about a decade — abandoned only when R.Newton 1977 (The Crisis of Clam: Ptolemy, Johns Hopkins Univ) became widely noted in the popular press. (See §D6 & DIO 2.3 §6 [E3].) [[Remark added 1995.]] Likewise, gentlemanly Hugh Thurston is now attacked, shortly after he committed the heinous offense of publishing the irresistible [DIO 6 §1 fn 137] heresy that DR had solved the source of the Babylonian System B yearlength: §E3 & [DIO 6 §1 fn 16]. Earlier polite scholars, who became victims of Mufa anti-heresy assaults, are listed at DIO 1.1 §1 [C5].

So explaining years of Mufa noncitation of RN will require a different ad-hoc excuse, a situation that creates an Occamite problem which we will also encounter in the evidential pros&cons of the Ptolemy Controversy itself: Mufa attempts to answer the charges against Ptolemy (invariably appearing in arenas where reply is highly unlikely to be published — e.g., book reviews) always concentrate largely upon just 1 single sector of the evidence. Thus, a reader will not realize that the unlikely alibi generated for this special part of the Ptolemy problem is inapplicable to Ptolemy’s other fabrications. Each ad hoc scenario (e.g., Evans 1987) seems less implausible in isolation; for, only when one sees, at all once, the whole crazyquilt of untested Mufa hypotheses (applied to the task of resuscitating Ptolemy), does one discern just how unlikely they are in conglomerate.

The scholarly acumen of O.Gingerich (Harvard & Smithsonian) is not much respected by these Mufaicos capos, but OG’s unrequited loyalty [Gingerich 1976, DIO 2.3 §6 fn 17-18] is such that I will loosely speak of him as an adjunct member anyway. (Has OG imagined that backing Mufaicos might land him a permanent Princetitute post? [Contra Rawlins 1991W §B3’s jest: not even Inst critics think its standards have come to this.] Note in passing: those familiar with the hidden politics of the Peary affair will find a parallel between V.Stefansson’s rôlle in it, and OG’s rôlle in the Ptolemy Controversy.)\(^{29}\)

OG is #2 editor at J.Hist.Astr., whose Editor M.Hoskin (Cambridge Univ) & Hoskin’s best friend Olaf Pedersen (Univ Aarhus; long an Editor of Centaurus) have also aided the Mufa in destroying dissent on Ptolemy. Thus, I include them & theirs under the Mufa banner, as well. (That does not imply that the central Mufa exactly reveres Hoskin or Pedersen. See, e.g., DIO 1.1 §5 fn 6.)

Fear & Dissembling

On 1976/3/2, a few days after my first talk (§D7) with R.Newton, I again tried & failed to reach Henderson at CUNY (Queens). On the evening of 3/3, I phoned OG about the review. He told me that Henderson was a protégé of Aaboe, himself a protégé of the ultimo in these matters, Otto Neugebauer, whose name I knew quite well and for whom I never reacted as forcefully as I have to Mufosi. But a Mufa citation-blackout (Rawlins 1991W §D4) was applied to him for about a decade — abandoned only when R.Newton 1977 (The Crime of Claudius Ptolemy, Johns Hopkins Univ) became widely noted in the popular press. (See ibid & DIO 2.3 §6.)\(^{30}\)

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Later that evening, I finally reached Henderson by phone in Queens and asked her: [a] why she hadn’t cited the R.Newton papers I’d seen in QJRAS, and [b] how she accounted for the suspicious agreement which R.Newton had demonstrated between Ptolemy’s allegedly outdoor “observations” & his indoor tables, especially\(^{30}\) for the Sun. (E.g., was this correlation being explained away as due to Ptolemy’s data-selection?) We also discussed the fact first pointed out by R.Newton 1973-4 (P.14): that the framing instrument allegedly used by Ptolemy for measuring lunar zenith distances (Almajest 5.12-13) would instantly reveal that the Moon’s distance from Earth did not vary by a factor of nearly two (!), as the Ptolemaic lunar theory required. (Neugebauer actually believed of System B yearlength: §E3 & [DIO 6 §1 fn 16]. Earlier polite scholars, who became victims of Mufa anti-heresy assaults, are listed at DIO 1.1 §1 [C5].)

But it was equally obvious from Henderson’s replies that: she didn’t even pretend to know the answers; i.e., she didn’t know what the justification for her ignoring R.Newton was. Surreal. All she could say in response to questions of astronomy & statistics was that: her mentor Asger Aaboe knows the answer; let me give you his phone number! (It all reminded me of a longago conversation with a young nun whose defenses of the deity were totally healthy; and OG has never invited me to talk anywhere.)

It is worth adding that, given O.Gingerich’s good relations at this time with QJRAS, it is likely that he was involved in the selection of this cipher as a reviewer.

I spoke to RN again 3/3. When I brought up the possibility of a verbal debate, he was not sanguine about the idea, largely because he had been earlier invited to a gathering where he was to be the sole skeptic and his paper was to be shown to the other side without reciprocation! He also had been quite ill at the time. (Curiously, the next time he was invited by OG to speak, 1984/6, he also happened to be ill. Though he was in the event able to talk, no debate was scheduled — despite my 1984/46 urging of this to OG. By contrast, I am always healthy; and OG has never invited me to talk anywhere.)

\(^{25}\) See Rawlins 1991W fn 16 & fn 174 for extensive lists of examples of DIO’s recognition of & gratitude for Mufa contributions and discoveries. [See also ibid fn 2.]

\(^{26}\) [Note added 1995.] The 1995 JHA has finally acknowledged a few corrections [DIO 6 §3 §A1], but there is still (ibid §G4) no Mufa disclaimer of DIO contributions. Indeed, despite the collapse of lead-paper “proof” that Greek trig orbits couldn’t be fit to Greek solar data, the Mufa still coherently stands behind its incoherent (DIO 1.2 fn 209) Babylonian arithmetic purported solution of the trisos A&B data of the trigonometrician Hipparchos (ibid fn 75). The mental requirements here are as obvious as absent: common sense, testability, & generosity. I.e., nothing essential has changed.

\(^{27}\) See §§B5-B6, B8, GI3-G14, DIO 1.1 §1 §A8, DIO 1.2 §D4 & [DIO 6 §1 fn 7].

\(^{28}\) See DIO 1.2 fn 92 & fn 116.

\(^{29}\) But whereas VS wanted to be known as a freethinker, OG prefers posing as a practicing Christian.

\(^{30}\) See DIO 4.2 §17 §§B23 [B] or Rawlins 1987 p.236.
simply wished not to lose little things like: publication, grant-funds, & conference-invites.

The reality was now out of the bag. The course & nature of the modern Ptolemy Controversy was determined. I.e., an ancient scandal was irrevocably doomed to become a modern one.

### H Referee Anonymity Abuse: Backshooting-Slander as Peer Review

#### H1 Soon after, DR learned that JHA Editor MAHoskin had called, for early 1977, a meeting at the Royal Astronomical Society, where R.Newton’s work was to be attacked — but to which RN was not invited! Upon learning of this, DR immediately sent MAH (in two pieces) the full version of his analysis (§C1) of the Ancient Star Catalog. It was not read nor even noted at the meeting. (Nor did the JHA referee it. In fact, Hoskin did not even acknowledge receipt — until after DR inquired at the Roy.Astr.Soc.) Indeed, the high-handed nature of the paper’s treatment probably accounts for the stolidity of the PRIMO-clan’s demented, even dishonest* refusal (to this day) to acknowledge the force of its arguments. After treating the paper so badly, to now admit its value would implicitly reveal how untrustworthy the Maffia’s pretended expertise can be. (This is not to imply that the Maffia’s work & judgement is worthless. Far from it.35 But its suppressive policies are as unreliable as they are unethical & transparently motivated, particularly the skewed policy of citing heretics’ supposed errors, not their useful contributions [DIO 6 §3 in 61].)

#### H2 Next, DR — realizing that Hist.sci was simply incapable of evaluating his star-catalog statistical arguments — submitted the paper to a real science journal: the *Publications of the Astronomical Society of the Pacific*, which — after long delays due to OG&co — published it in 1982. (Meanwhile, the paper was strongly endorsed36 by the great Bart van der Waerden, author of a classic Springer-Verlag statistics text.)

#### H3 One of the anonymous *PASP* referees chosen was none other than O.Gingerich. Now, most scientists believe that a referee’s job is to analyse the technical aspects of a paper. But OG didn’t bother with any of that. What follows is the full text of OG’s referee report, which was expected to remain forever secret from DR & the rest of the academic community:

To: *PASP* 1977/7/8

From: Owen Gingerich
Professor of Astronomy & the History of Science, Harvard University

*It is really awkward for me to be a reviewer for the paper by Dennis Rawlins. I have had long telephone arguments with him about Ptolemy’s integrity as a scientist. He sent a version of this paper to be presented at a session of the IAU in Grenoble last summer, and because it was so badly written and unclear* I did not read it. *And as a result he believes that there is a conspiracy to suppress [sic] all criticisms of Ptolemy. I do not wish to be seen as part of a conspiracy, but I must agree with your previous reader that the message is almost incomprehensible. I think that his two arguments are sound as far as they go, but in both cases I believe that legitimate alternative interpretations could be proposed.*

**H4**

It is really awkward for me to be a reviewer for the paper by Dennis Rawlins. I have had long telephone arguments with him about Ptolemy’s integrity as a scientist. He sent a version of this paper to be presented at a session of the IAU in Grenoble last summer, and because it was so badly written and unclear I did not read it. And as a result he believes that there is a conspiracy to suppress [sic] all criticisms of Ptolemy.

**H5**

I do not wish to be seen as part of a conspiracy, but I must agree with your previous reader that the message is almost incomprehensible. I think that his two arguments are sound as far as they go, but in both cases I believe that legitimate alternative interpretations could be proposed. [DR: none mentioned; likewise at fn 41.] I think that it would be a disservice to his own attack on Ptolemy to publish arguments so obscurely framed.
Mr. Rollins [sic] has almost become paranoic about this, so if you turn it down in its present form I hope that you will be extremely careful in framing your letter.

Gosh, what a nice guy. So, even back in 1977, just as soon as JHA’s O. Gingerich saw DR’s horrible absent-error-waves test (later Rawlins 1982C) and before he even understood it, he was already stating privately in writing that (though he claimed to find no factual or technical error in it [note added 2009: its validity is not even controversial anymore]): there MUST be an alternate explanation. And, he of course helpfully added that DR was crazy — a desperate (failed) ploy to prevent DR’s critical discovery from ever being published.

DR’s private reaction follows.

This Referee Report uses libelous personal statements to cast doubt upon the validity of scientific conclusions which are embarrassing to the Referee’s own repeated public stance. In this way, he avoids dealing with the technical content at all — which is perhaps fortunate, considering that not one factual statement in the Report is accurate. . . . Literally. (Incredible? — not if you know the Referee’s record re fluffs, in both astronomy and gossip.)

[1] I have never had a verbal argument with 0. [Several friendly chats.]
[2] He has confussed two papers of quite different date, subject-matter, math-level, and method of analysis.
[3] The Star Catalog tests were conceived and executed entirely subsequent to the IAU section on Ptolemy at Grenoble, so their alleged obscurity cannot possibly have been the cause of the first paper’s non-presentation, which OG’s own 1976/9/15 letter to me ascribes to lack-of-time (he ought to get his stories straight). The earlier paper was 3pp, and its basic equation was arithmetic. No-time? Not-clear to OG? Well . . .

[4] OG’s failure to present the first paper at Grenoble (1976/8/31±1) can hardly have been the cause of my criticisms of ancient-astronomy-historians’ cohesive non-reply (to R.R.Newton’s demolitions of Ptolemy) for the very simple reason that my censuring of this behavior was right in the addenda to the Grenoble paper! (Mailed to OG, 1976/8/19).

[5] It is amusing and ironic that OG should now attempt to brand as mere Conspiratorial Dementia these conservative appraisals of historians’ systematic peculiarities re Ptolemy (and re RRN, whom they then refused to discuss in print, while regularly reviling him behind his back — still the case with Neugebauer and his disciples Aaboe & Toomer). The fact is that everyone familiar with this cult’s introverted ways was aware of the realities (and freely discussed them, privately). For example, the scholar who first (Science 1976/8/6) broke the Ptolemy-defenders’ 2 year public silence (re RRN’s 1973/12-1974/6 QJRAS papers) later wrote me (1976/9/15): “So far the Neugebauer camp has not been heard from. Perhaps my merely mentioning Newton in a review of Neugebauer has placed me beyond speaking terms.” The identity of this, my [fellow paranoid]? Owen Gingerich. . . .

I am reminded of the exasperation of a recent critic of other irrepressible Independent Thinkers who have plagued astronomy (generally from the outside) — he notes weary how much time, labor, and page-space are required to set straight only a paragraph of pseudoscience mis-statements. I [had] taken similar pains once previously with OG; his reply, unable to refute a single one of a long string of demonstrations of factual screwups, instead simply went off in other directions — and launched a whole new thicket of misinformation!

And I always thought it was we Paranoinds who were out of touch with reality.

About the same time, OG answered an invitation to debate DR by sending (to the party offering to publish the exchange) another private charge that DR was a nut. (Text quoted at DIO 1.1 11 fn 20.) When this gross letter was sent to DR, OG got mad at the transmitter! — and, to this day, he has not recognized any fault of his own in the matter. Yes, archonhood does affect brains. And their remains.

We end this section by citing two striking realities, each of which will usefully educate the uninformed, regarding the true current state of US academic freedom:

[1] O.Gingerich will never acknowledge his misbehavior in any of the here-noted episodes (§§B5-B6, H6, I1).
[2] Not one archon will criticize OG’s behavior. In public, at any rate. (Which explains the broad scope of one of this article’s sub-headlines. [See DIO 2.1 §3 fn 15.] Academe has finely-honed senses of proportion & propriety: it bans not its criminals but their exposers.)

Similarly: not one scholar has ever (publicly) criticized Hoskin’s 1983/3/21 banishment of DR. When I say that alot of modern academics live under a reign of terror, it’s realities like this that I’m thinking of.

These realities — as well as the fact that the most basic common sense can be suppressed for decades on end — recommend a mild warning to the public: one must be just a little bit wary of academe’s pronouncements. Even though I have no (or am not aware of having an) argument with most academic orthodoxies (especially in the science arena, which is by far the sanest of them all), the problem is that, when controversies arise, the public can be kept uninformed about even the very fact that there IS a controversy. So, once one cannot (without laborious private checking) know which of academe’s consensuses are the ones which are reared upon rotten foundations, it is wise to remain mildly-skeptical about all of them — until open-court mechanisms for settling controversies are finally established. (See §J3.)

I Ivy League Grab: Mean Motions & Spirit

On 1980/4/13, DR sent a fateful letter to OG, imparting the shocking information that all the Muffia’s top snobs had mis-stated the mathematical sources of the Almagest planet mean motions. (Full citations at DIO 2.1 §3 fn 38. See also DIO 1.2 fn 56. On Muffia amnesia about this: see DIO 2.1 §3 fn 47.) Those who had published mathematically incorrect equations: O.Gingerich, O.Neugebauer, & O.Pedersen. (See also G.Toomer’s misconception: ibid §C15.) I.e., all Muffia capos had mathematically screwed up the solutions of these 5 mean motions, and DR’s letter bore mathematically fitting solutions for all 5. [Two historically false: see DIO 11.2 (2003) p.30.] (Letter’s text at DIO 2.1 §3 §C5. Planet-by-planet Muffia-vs-DR fit-comparisons at ibid §C3.) Muffia’s pristine record: [a] no (explicit) retractions, & [b] 1984 publication (Toomer 1984 App.C) of most (the three undeniable ones) of DR’s five solutions (this after referee OG had suppressed 1983 publication by DR! — see DIO 2.1 §3 §C7) — but without the slightest credit to the discoverer, or to R.Newton 1982 (pp.103&108 n.11) where RN published the same three solutions for DR, two years ahead of Toomer 1984. (In the more than ten years since, not one Muffioso has acknowledged the undeniable publication-priority here. Is this the sort of scholarly integrity which Harvard Univ and the Princeton Inst wish to be involved with?) I.e., the very cult that for years consistently contradicted the solutions is now implicitly pretending to have invented most of them, while not crediting the scholar who actually did recover [most of] them. This is as extreme as academic brass gets.

Mercury, Venus, & Saturn — whose [valid] solutions’ identical factors are right in Ptolemy’s own preface (Almagest 9.3) to the tables. The other two [nonhistorical: §3] solutions were first published at Rawlins 1987 p.237 (Mars) & fn 29 (Jupiter).

But academic archons will do nothing about such sham. (See §I2.) Why? Because a scandal makes academe look dishonest. (If you ever questioned whether there are natural comedians, this almost perfectly perverse situation should dispel any doubts.)
12 On 1994/5/8, when publicly challenged by DR from the floor, at the M.I.T.-Dibner Inst meeting, neither Toomer nor OG would claim these discoveries for themselves. And DR proposed that, after 14 years, there should be public acknowledgement of the identity of the true discoverer of solutions important enough to be featured by Toomer in a special final Appendix C to the standard modern edition of the Almajest. This reasonable suggestion has met with silence since. DR has requested leading Hist.sci persons to inquire of Gingerich about the matter (to push OG to make up his mind as to whether he wishes now to claim nonreceipt or subsequent misplacement of the 1980/4/13 DR letter to him). None has reported doing so. [No Mulfosi asked; but in 2003, D.Duke asked for a copy, and OG sent mime. (To DR, not Duke: but we'll assume here that OG is now acknowledging receipt.)] This, in spite of DIO 2.1 § 3[b]'s extensive publication of the historical record of this appropriation.

13 OG's 1983/7/23 reaction to DR's §1 planet-mean-motion solutions? Same ploy41 (echoing §H5): there's-got-to-be-an-easy-alternate-explanation. (Question: why has Muffia output, e.g., §B4, not been suppressed on similar grounds? I.e., the alternate-solutions gambit is an argument for skeptical discussion, not for suppression. And this conveniently-broad alibi can kill any paper an editor dislikes: in the entire history of science, no one who found a solution to an empirical problem has ever been able to pre-guarantee that another solution [whether valid, plausible, or just an Occam-nightmare contraption] won't fit the same data. Should one have suppressed I.Newton's corpuscular optics just because the wave-theory provided an alternate explanation?) [Note added 2003. Though the OG & Moesgaard Mars solutions were indeed false (& grossly nonfitting). DR's perfectly-fitting Mars solution has now turned out to be invalid, and the Muffia claim is as follows: A, and Jones' true solutions at DIO 11.2 p.30 and §4 eqs.31&45. Jones' finds prove positively that all 5 motions are based on period-relations as was 1st to propose: 1980/4/13, so there is no longer any doubt (contra Toomer 1984 p.672) that the revolutionary 1980/4/13 DR solutions for the other 3 planets are correct. And, along with most scholars in the field, Gingerich appears (if DIO 11.2 p.30 item [a] is true) to have at least implicitly acknowledged that the above (§H5) alternate-theory alibi against DR's Ancient Star Catalog analysis is dead.] Comments: [a] Does occultist-level invincible-unfalsifiability alibi-wriggling-out get any funnier than the there-must-be-an-answer bedrock of OG's faith in the mere-

dissolution of all apparent contradictions in Ptolemy-worship? [b] If OG was certain from the outset that there MUST be equally convincing alternate explanations — even though he can't find them! — for all evidence that seemed to contradict his views, then he must have known positively from the outset that his scholarly position was correct. So we all await impartation of the precious secret (possessed also by hero Ptolemy — as well as by a fellow specialist in Christian apologia & other fiction, C.S.Lewis: fn 42) of how to KNOW the answer to a problem — a method that his scholarly position was correct. So

41 See DIO 2.1 ibid §[C7&C18-C19]; also DIO 1.1 §1 fn 9. Likewise, a.K.Moegsgaard 1983/12/15 referee report discouraged publication of DR solutions to a quite different Ptolemaic problem by adopting the automatic alternate-solutions-are-easy mantra. However, again, in the decade since, he has yet to produce any of these allegedly-easy alternatives. Instead, the "alternatives" proposed are either lousy fits (DIO 1.2 fn 129 & DIO 2.1 §3 [§C note: former ref misrendered at latter's fn 25 in 1st printing]) or solve a completely different problem. Moesgaard — an extremely able scholar, sadly trapped in a careerist world — has been the most irrepressibly-volunteering fount for such exercises in Muffia-suppression-alibi artistry. (So far.) See the amazing Moesgaard 1987 noncitation remarked at DIO 1.2 fn 56. He has made no comment on this or several other oddities: ibid fnm 126, 129, & 170, and DIO 2.1 fn 23. (Note 40% DR mis-fire here: DIO 11.2 [2001 p.31].) C.S.Lewis The Problem of Pain 1940 Chap.9 (“Animal Pain”) attempts to slither out of one of the several fatal internal contradictions of Christianity: why god visits pain upon animals who have no souls (to purify by deserved adversity) and who do not share Original Sin. Thus hideously self-contradictory, Lewis actually suggests that animal pain might be a form of punishment. This is where one ends up if one thinks about it. One finds oneself in one of the rare places where alone (if at all!) we know Him . . . then in other realms where we cannot know [much about] Him . . . then, despite appearances to the contrary, he cannot [DR: vs. above if-at-all] be a power of darkness. For there were appearances to the contrary in our own realm too; yet . . . they have somehow been got-over. To watch this theologian (the world-renowned “apostle to skeptics”) get so desperate as to seek refuge in semi-solipsistic agnosticism (in order

0.9) to be a power of darkness. For there were appearances to the contrary in our own realm too; yet . . . they have somehow been got-over. To watch this theologian (the world-renowned “apostle to skeptics”) get so desperate as to seek refuge in semi-solipsistic agnosticism (in order
encourage scholars to believe that one doesn’t have to cowtow to archons. An intolerable vision. The upshot is: overarching, ever-present fear of making high-placed enemies — thus, reign-of-terror-freezing of free discourse and equitable evaluation of scholarly output. 

J3 (The Wash Post’s prize-winning science reporter B. Rensberger notes that, for years, there has been discussion of the idea of establishing a court for settlement of academic disputations — but archons keep killing the proposal, claiming that academe is so honest that there is no need for such a body. OK, while we’re at it, let’s just scrap the US court system, too, since US society is so trustworthy & fair that: there’s just no need.)

J4 The close of Mill’s more-revered-than-read 1859 On Liberty (emph in orig):

The worth of a State, in the long run, is the worth of the individuals composing it; and a State which postpones the interests of their mental expansion and elevation . . . [and] which dwarfs its men, in order that they may be more docile instruments in its hands even for beneficial purposes — will find that with small men no great thing can be accomplished; and that the perfection of the machinery to which it has sacrificed everything, will in the end avail it nothing, for want of the vital power which, in order that the machine might work more smoothly, it has preferred to banish.

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DSB = Dictionary of Scientific Biography, Ed: C. Gillispie, NYC.
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46 [See DIO 6 1:1 [36.]

47 Despite numerous obvious non-parallelisms, I am nonetheless reminded of the impassioned closing pages of E. Byck’s History of the Weimar Republic (Harvard Univ 1963, Sci Ed 1967), summing up the suicidal events culminating in Hitler’s Chancellorship (emph added): “The attempt by the German people to rule themselves had failed. A time now came when Germany ceased to be a state based on law. This was the time when German judges allowed their courtrooms to be overrun by [the cul] who drove out the people whose noses they did not like; when the judges saw their independence and security abolished and their professional advancement become dependent on the way their decisions pleased the ruling party . . . .

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