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

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

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

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

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

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


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News note added 1993:

Impact

We were pleased at recent national-level attention to Ted Heckathorn's startling recovery of Roald Amundsen's 1911 longitude observations, a story first broken in DIO 2.2 (1992), where for the first time the spherical trig navigation of Amundsen's great South Pole journey was mathematically explained, including several sample computations.

A gratifying byproduct of this work has been complete vindication for the martyred British explorer Robert Scott's similar S.Pole navigation, which had lately been much attacked, quite unjustly.

Boyce Rensberger's prominent 1993/6/1 Washington Post story on these matters — explicitly citing DIO — was picked up by AAAS's Science (1993/6/11, Constance Holden). Both accounts rightly emphasize the negative impact of Heckathorn's discovery upon the Robert Peary 1909 North Pole claim (entirely lacking in steering observations), which is now more suspect than ever as the most successful science hoax of the century.

Informational note added 1993:

History-of-science & Dissent

DIO 2.3 showed (§8 in 17) that, in 1991, the preeminent History-of-science organs, Journal for the History of Astronomy and Isis, published handsome lead articles (gov't-funded Neugebauer-Mufa output, on ancient astronomical history), the central contentions of which depended heavily on astounding mismath, including two crucial errors [both refereed by Mufosi] in gradesschool arithmetic. (Full details in Journal for Hysterial Astronomy 1.2, esp. §G9.) The sole response from the History of science community is the incident described below.

In reaction to 1992/10/30 receipt of DIO 2.3, a wellknown Hist.sci archon — professor in Johns Hopkins University’s Hist.sci Dep’t & sometime boardperson at History of Science Society’s Isis — swiftly (without a word to DIO) contacted the Johns Hopkins University central Milton Eisenhower Library, resulting in the Library’s cancellation of its DIO subscription. These low doings were expected to remain secret.

DIO congratulates the Hist.sci center upon the brilliance, the clarity, & especially the characteristic dexterity of this latest demonstration of: [a] Hist.sci’s renowned dedication to free intellectual discourse, & [b] how unafraid Hist.sci archondum is of DIO.

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

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

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

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

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

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

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**My own attitude is uncomplex: fraud in science is to be denounced, not alibied. Even in Tycho’s work. (See Thoren 1990 p.293, excusing Tycho’s suspected plagiarism of a key astronomical discovery: quoted J.HA 1.2 [H2].) No, it’s not pleasant to acknowledge either the ten fabricated Tycho star-places — or the implications of data-faking having occurred at such a rarified level among astronomical greats. But these are the stark truths. And I do not propose to pretend otherwise.**

**References**

- DSB = Dictionary of Scientific Biography, Ed. C.Gillispie, NYC.
- Gerd Graßhoff 1990. History of Ptolemy’s Star Catalogue, NYC.
- Gerald Toomer 1984, Ed. Ptolemy’s Almajest, NYC.

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**1 Scrawlings**

**A Shorts**

- **A1** When Orson Welles pulled his 1938/10/30 invasion-from-Mars hoax, a terrified 1/6 of the US public found it credible that Martians had deliberately moved to New Jersey. When Russia genuinely took the first photograph of the Moon’s backside, 1/6 of the US public deemed it a fraud. At the lowpoint of Nixon’s Watergate disgrace, even after the tapes of his selfbuggery were exposed, 1/6 of the US public still believed in him. Question: Are these the same people?  

- **A2** Ever-degenerating US cities are now begging for special Dr.Feelgood vitamin-injections of extra federal money — funds forcibly tax-conscripted from productive citizens. Comments: [a] Cities are where most US money already resides (in, e.g., banks). [b] On the media (owned by forces which also hold plenty of real estate), one doesn’t hear calls for cities assisting their overweening power by confiscating US land, which is even more inequitably distributed than US money. (Always the media push is for more tax-money: i.e., soak-the-Middle-Class.) [c] The notion of curing US cities’ woes with more money is as bright an idea as curing an alcoholic with more booze. Disaster-cities (especially NYC) have in the past successfully begged more federal funds, and the inevitable result was not improvement but — just the same old disaster — only bigger.

- **A3** The outer Solar System is popularly regarded as spookily dim. Actually, the light and Neptune is ordmag 1000 times brighter than the light of the Full Moon falling upon the Earth.

- **A4** It is remarkable that the largest buildings in human history, the mysteriously non-utilitarian Giza Pyramids, were built at the dawn not the evening of civilization. (Possible partial explanation: §C. Also: Egypt was the most religious of early ancient cultures.)

- **A5** Paradox: how can it be legal for some city gov’ts to give away clean needles (so that the drug addict population won’t die off from AIDS?) — but not legal to give away free injectable drugs (in order to kill the profits that spread the drug blight of streetcrime & municipal corruption). The apparent contradiction eases as soon as one wonders if this is not precisely the double-standard policy which druglords would prescribe.

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69 Prof Emeritus, Classics, Johns Hopkins University. A generation ago, Jimmy’s sister and our old friend, Anne Poulney Taylor, made us a gift of her own sumptuous painting of our family’s longtime Ruxton house. It has now hung in our Baltimore home for over a decade.

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1 [Note added 1992/11: Despite Nixon-pal R.Perot’s retrograde-loop 1992 in-out-in pseudo-run for the White House, about 1/6 of the voters bought his act and voted for him. Comment: NOBODY who’s genuinely running for office — i.e., not running as a cunning spoiler — is going to drop out when leading the polls, which is just what Perot did. His subsequent attempts to justify & rationalize this patently inexplicable performance are even more disingenuous than the original show. We probably haven’t seen the last of Perot’s pioneering new business enterprise: dial-a-votesplitter.]

2 Question: Is it accidental that Charon’s nodal line is so nearly coincident with Pluto’s apsidal line?  

3 For DR’s speculation regarding the astronomical placement of these monuments, see his lecture at the 1984 Greenwich centenary celebration of the prime meridian’s establishment (Vistas in Astronomy 28:255; 1985).

4 If you like corrupt gov’t, police, & media, then: just keep drugs illegal. Another hitherto-unnoted paradox: the big profits (which purchase control of Congressmen by smarter criminals) connected to hard drugs are contingent on drugs not succeeding with most of the public. (So, black leaders’ suspicion — which I do not share — that drug-sales are injected selectively into ethnic ghettos, is not a priori incredible.) E.g., if a majority of the citizenzyn got hooked on cocaine, prohibition would be repealed (as for booze in 1933) — and mafia profits would plunge. In case the reader is imaging a personal stake here, it should be added that DR strictly avoids — and makes a pest of himself warning youngsters against — drugs or non-nutritive stimulants of any type. That includes tobacco, alcohol, & caffeine. The happiness-through-chemistry myth promoted by US media ads is a key element in setting up youth for drug-use. I am so turned off by this greedy propaganda that I don’t even take aspirin. (Whether for individual or societal depression, the media singularly promotes quick-sell band-aids, not stable-health longterm solutions.)
A6

The US public enemies believes it longs for truth. So, why does it systematically keep electing two-faced liars to public office?5

A7

Since certain public enemies of my late friend Robert Newton have regularly attacked triples (even spelling)6 in his work (e.g., DIO 1.3 fn 264), I cannot resist returning the favor — through the revealing little item that follows. The extremely handsomely Journal for the History of Astronomy (Editor-for-Life: Lord Hoskin, University of Cambridge, Churchill College) makes a point of prominently listing, on each issue’s inside front cover, its “Advisory Editors” and their uniformly eminent institutional affiliations. (See §4 fn 65.) For years (at least 1987-1992), this JHA list has rendered “Advisory Editor” N.Sivin’s (where Isis was edited until 1991) as the University of “Pennsylvania.”7 This despite several resettings of this special-press-page’s type. Again,8 we ask: how many scholars actually read, even experience, the journals we cut down forests to make paper for?9

A8

[a] In DIO 1.1 (§2 fn 7), we learned that US politicians (allegedly fighting poverty) have expressed astonishment & surprise that, when poor women (many of whom had virtually no other job prospects) were paid10 extra money for each child produced, they bore lots of children — and thus poverty expanded rather than contracted. [b] Similarly, the US cancer-industry has lately been fighting breast-cancer with X-rays11 (well-known to

5 Least any reader has failed to connect §A5 to §A6. I will add the item that: annual illegal drug profits in the US are ordmag 1000 times the combined (official) salaries of all of the 435 US Congressmen (who write “our” laws). So, does Congress rule vice? Or vice versa.

6 There are those who would imply incompetence merely because of a scholar’s unorthodox spelling of an ancient’s name. (Note also JHA 1.2 fn 92.) See a certain OG’s very first review of R.Newton’s output: Centaurus 17.2:173 (O Gingerich 1972). Though not entirely negative, it cites some odd alleged “flaws” or “inelegancies” in nonhistorian RN’s Ancient Astronomical Observations (Johns Hopkins 1919). Newton’s12 spelled “Ibn Yunus” as “Ebn Jounis”. Comments: [a] Revealingly (downright embarrassing) trivial. [b] R Newton 1970 p.304 notes 3 different spellings. [c] There is no uniformly accepted anglicization of this 11th century Moslem astronomer’s name. [d] Throughout, R.Newton 1970 uses “Ebn Jounis”, not OG’s alleged “Ebn Jounis”. [e] This review is in a History journal, Centaurus, O Gingerich. [f] Reviewer OG writes “Ebn Jounis” for Tycho’s et al. Hist. sci Dep’t. By contrast, OG casts aspersions on any specific scholar’s ability, during his current review of the work of the Mufia’s reputed satirist-entertainer Noel Coward Swerdlow and other Hist.sci volk, when he offers the weighty complaint (JHA 2.2.150, 1992): some Hist.sci scholars “no longer [choose] to distinguish between principle and principal.” (The fact that Hist.sci archons cannot distinguish between principled and unprincipled seems to be of far less interest to them than spelling the words correctly. Reminds one of [E. Haggins’ art aside, in My Fair Lady], on the Gally amatory legend: the French don’t really care what they do, so long as they pronounce it properly.) No imputation of nonbravery should be attached to DIO’s name for agile longtime debate-ducker Swerdlow, who has earned his rank as top Muffia humorist by his 1983 implication that the actual coward in the Ptolemy Controversy is R.Newton: see DIO 1.1 §3 SD7. [Correction: Original edition erred on the Gallic-aside-source.]

7 DIO’s mailing label brought the misspelling to Sivin’s attention (exclamatorily) in 1991.

8 See here at §3 §C16 & DIO-JHA 1.2 §B4 (the JHA’s “Winter Equinox”), or the previous issue’s “Royal Comitatus” (JHA 1.1 §B8).

9 What’s-left-of-the-left (WLL) keeps suicidally promoting this “cure” for poverty. Instead of having the next generation raised largely by stable middle-class couples, the WLL effectively insists it’s less demeaning to have much of the next generation instead raised by poor single mothers at least one of whose “jobs” is collecting child-welfare cheques, with gov’t-social-worker bureaucrats riding paternally hor in the snow. (The theory is: just pay enough welfare, and poverty will atrophy naturally. As wacky as the Reaganesque notions that if you cut taxes enough, the unburdened economy’s reborn tax-base will make up the gov’t revenue-loss. One is naturally impressed by the dementia of these theories’ creators! — but I reserve my particular awe for the straightfaced pols & press who peddle them to the public.) Well, we’ve now experienced generations of replacing real parents with bureaucrats, and the inner-city results suggest — to right-thinking observers’ shocked surprise — that the gov’t makes a lousy parent. (Nonetheless, all known US political parties refuse effectively to interfere in this eternal cycle with regressive, no-gas & inegalitarian bandit rules.) Note: Scandalized, but has shown (so far) that welfare can be made effective & noncyclical — and not a mainline subsidy to drugpushers. But the US media seems singularly uninterested in exploring what differences have made the NE-European approach work.

10 Breast X-rays are now aggressively marketed under the sales-sphinxm, “mammograms” (not “breast X-rays”). The term is called “risk-benefit”, which translates as: OG we may lose the fortune X-ray-caused breast cancer — Nonetheless, the claimed benefits of early-detection outweigh that (allegedly trifling) regrettable iatrogenic downside. Few patients are made aware of this ruling logic, in order that they can make an informed choice between options. Are mammograms simply another kind fake from theory and publication of it as real. The commission of that

11 self-pickels Evans. . . . [Further on pickell-suicide: DIO 1.2 fn 29.]

18 Thus, perversely, the abortive JHA attempt (Evans 1987) to save Ptolemy from the charge of faking his star catalog has not only made his theft much more believable (§H6) but has directly resulted in exposing the far greater figure Tycho, engaging in the very same utterly dishonest practice — and by exactly the same scheme RN&D (Tycho)’s had charged against Ptolemy (disbelieved by Evans & his Mufia pals): simply adding a precession constant to celestial longitudes. DIO must of course thank the ever-courageous JHA’s massive if ineffectual Evans 1987 sneak attack on DR, for handing DIO such an important discovery. But DR’s thanks to the extremely handsomely JHA will be as nothing compared to establishment archons’ eternal (& eternally silent) gratitude to JHA Editor-for-Life Lord Hoskin for assisting so generously with: [a] spreading the stain of proven science fraud even wider among famous astronomical figures than had been the case before the JHA’s Evans 1987 assault on DR (note that it was also a JHA paper — that triggered DR’s discovery of Hipparchos’ hoaxes: DIO 1.3 §N11), and [b] the Tycho case’s public demonstration of establishment-beloved DR’s inductive and fraud-detecting abilities. (Imagine the sheer bulk of Muffia chopper-enamel that’ll grind down, before admission of the latter item. Dentists will mass-vacation on the restoration-proceeds.)

II9 Of course, anyone outside the immediate Muffia, it is obvious that Tycho stands so far above Ptolemy as to be in virtually a different phylum. But a comparison of the ten Tycho stellar hoaxes to real astronomers’ work is not flattering to Tycho: [a] the proof is certain, & [b] there is no question of an innocent interpretation. It will only add more scholars’ disgrace to Tycho’s if modern Hist.sci archons attempt to brush aside the fact that a deliberate hoax (even if one of modest proportions, in context) has been discovered in the work of an astronomical: giant: data fabrication by indoor computation from note. From: The much of the much-publicized fudging, which modern scholars have demonstrated (as against speculation) to work of Galileo & L.Newton, involves, temptation to fake astronomical data from theory and publication of it as real. The commission of that ultimate scientific crime appears to be restricted to a very few astronomers, Ptolemy & Tycho being the most famous. (And astrologer-mathematician-quackster Ptolemy wasn’t even an astronomer.) Thus, the Tycho stars here exposed must now rank as the fakest data ever published by an astronomical Immortal. Nothing like the Tycho fabrications have ever been found in the work of such other premier astronomical observers as Hipparchos, Walther, Bradley, Herschel, Bessel, etc.) I must comment just as I have upon Ptolemy’s similar behavior, which is condemned by R.Newton 1977 & Rawlins 1982C, but defended by Muffinosi — who claim (JH10) that Ptolemy is not unethical when he plagiarizes hundreds of stars (in a few days of indoor arithmetic) and then instead explicitly pretends in detail10 to have himself made laborious, delicate outdoor observations of these stars (a task that would require years of time, plus more years spent acquiring the requisite expertise), DR’s view: if this is not science theft-fraud, then there is no such thing.

H10 I must add the warning: Hist.sci archons’ easy ethics (on such matters as plagiarism) suggest that the wise modern scholar will not leave unattended, within their reach or eyesight, any new discoveries of his own. Careful readers of DIO may come to suspect that this is a not-entirely-theoretical point. If such archons represent the sort of scholar that major universities wish to harbor and promote, then each of these institutions must share credit for the diffusion of their ethics — and such pioneering Hist.sci notions as: Ptolemy’s merely stealing ordmag 1000 stars from Hipparchos (without attribution) is not dishonest. (See Harvard’s Gingerich 1981 p.43; also [Muffia 1990] pp.215-216.)
appear\textsuperscript{63} at magnitude\textsuperscript{64} $\mu = 7.95$ for Tycho, while (again by the JHA formula), Tycho recorded nothing outside of Cen dimmer than $\mu = 6.42$ ! (For the dimmest real cat D stars, D194 & D234, DR finds $\mu = 6.19$ & 6.23, resp, while the JHA formula produces 6.38 & 6.42, resp.) Question: how could Evans 1987 take his putative readers through page after page of fancy-looking extinction math (Evans 1987 pp.259-260 & 267-271) — while not using his resultant extinction equation (p.260 eq.2) for any of the stars he attacks DR with on p.168.\textsuperscript{65} One partial answer: eq.2 (Evans 1987 Part 2 p.260, published JHA 1987 Nov) was not yet known to Evans when p.168 (Part 1) was written. (Evans 1987 Part 1 was rushed by OG into the 1987 Aug JHA, in hot rage that the Amer J Physics had just the previous March published DR’s exposure of Muffia misattribution. Because the JHA paper’s eq.2 is not known to Evans, only by the JHA paper’s eq.2, so neatly designed & massaged into readiness for attacking DR in Evans 1987 Part 2, unluckily entailed the destruction of Evans’ own key conclusions in Part 1 ! (The JHA costs institutional subscribers merely $134/year.) By the way, in the fine print, at Part 1 n.41, Evans admits\textsuperscript{66} that the extinction-attack on DR ultimately fails. Thus, besides bulking up the deliberate-thickness of the alleged JHA case against DR, Evans’ lengthy extinction excursion makes no gain — while, in Part 1, it instead unambiguously

63 Using the original formula $A = \rho \csc h \left( \frac{\pi}{2} + \frac{\rho}{\sigma} \right)$ (where $\sigma$ & $\rho$ are as in fn 17), and following Evans 1987 (p.168) in adopting epoch 1591 & latitude 55° 91, we find that the light from 2g Cen passed through $A = 19.1/2$ molecular atm before arriving at Hven. (Out A is Evans’ in his eq.2.) See above at fn 17. (Note that setting $T = 273 K$ in the above formula will produce $A$ values close to those given by Rawlins 1982 eq.6.)

64 And we have not included sky-brightness, which, even on the best nights, can add ordna 0.1 magms of visual difficulty.

65 More hilarious post-extinction magnitudes yet are demanded by other Evans 1987 p.168 statements of possibility-visibility, the wildest being the claim that in 1591, 1 CMA = 475 (star #475 of the Tycho cat C which Evans is testing to have been similar (through 55.5 atm . . .) from being similar (through 35.5 atm . . .) by Berger’s own extinction formula (his eq.2), this star would appear at mag $\mu = 10.1$ in Berger! Finally, by the same formula, Fomalhaut (through 16.8 atm) would have post-extinction magnitude $\mu = 4.54$ from Hven, whereas Tycho should have seen $\mu = 3.84$ according to this massey JHA eq.2 (eq.2) pre-Cent Ptolemy “extinction” magnitude & extinction assessments purport (Evans 1987 p.275) to have been turned out with the meaningful assistance of an impressive flock of prominent scientists. (Curiously, the JHA’s top-ranking Ptolemy adulator, O.Gingerich, who saw this astonishing article all the way through from its 1st draft to its handsome journals? (Keeping in mind that similar Experts took most of this century to finally figure out that painful — & expensive — radical mastectomies were no better than the modest “modied” exposure here as incurably insane? — thus undercutting the attack which caused cancer) — while affecting bewilderman that net US breast-cancer cases ( & deaths) are rising instead of falling. But, not to worry: the good news is that trends $\{a\} \& \{b\}$ have increased,\textsuperscript{11} not decreased, the business of ( & need for) $\{a\}$ our pols & $\{b\}$ our docs.

A9 In our obviously Lucifer-founded world, the solution to Satanists’ vexing Problem-of-Good is simple: the anomalous existence of goodness must be blamed upon rebellious man’s free will. (Let’s nock what created man & the good in him . . . .)

A10 When the CIA was deputed ( & granted vast unseen sums) to protect us from no-doubt-imminent foreign invasion, one of the cuter facets of this prank on the public was: persons (e.g., ex-CIA chief Bush) running the US. (Suggestion: try imagining the extent of the secret funds the CIA has at its disposal, whenever it wishes to affect domestic politics.)

A11 Tons of tomes have been written on govt’s solutions to human problems. Yet the most important solution is: how can one place these solutions into power? This question is a reminder that in gov’t matters, as with inductive problems in physical science: there is no lengthiest extinction excursion makes no gain — while, in Part 1, it instead unambiguously

B Doubletakates

B1 S.Allen (1955): I’m scared that if I think about religion, I might become an atheist and go to hell.

B2 Life magazine (1936/12/7 p.63) explained the troubles, between the notorious show-buckler ( & sometime actor) Errol Flynn [1909-1959], and his then-wife, starlet Lili Damita [1901-1994], as partly due to a lifestyle contrast: “Mr.Flynn, it seemed, liked a quiet outdoor life and Miss Damita liked night clubs and parties.”

B3 Late-Autumn 1991 Jenny Jones promo for her new housewife-catering network talkshow (debuting 1991/9/16): Why do men call our soap-opera dumb, and then watch 5 hours of wrestling? JHA solicits inductive comment upon the evolution that produced the final version of this ad. [See DIO 4.3] 13 fn 2.

B4 How deeply the media is — yes, singular — in the totalitarian grasp of buckmasters may be measured by the equally singular fact that: it never even identifies (much less bestows long-overdue praise upon) those decent celebrities (e.g., Vidal, Brando) who refuse to prostitute their names by doing commercials to sell products whose payoffs (“ad billings”) feed the media. How can TV ‘news’ condemn bribe-taking by public figures — when the media’s own incessant fiscal-lifesblood ads are performed in return for fiscal bribes?

C Germs

C1 The more time-saving devices are invented, the less spare time we have.

C2 Ever heard a druglord (Philip Morris, Busch, or mafia) bemoan tax money flushed down the poverty-welfare cycle?

C3 If God existed: humans, politicians, theology, & $\tau$ would all be rational.\textsuperscript{13}

\textsuperscript{12} Can be dressed up, but the bottom line is a startling, publicly-undiscussed contradiction: [a] thanks to mass mammograms, early detections of breast cancer are up so [b] why haven’t death rates fallen just as dramatically? Or fallen at all, for that matter. Keep in mind that similar Experts took most of this century to finally figure out that painful — & expensive — radical mastectomies were no better than the modest “modied” version. Halsted, the Johns Hopkins doctor-addict who established the radical’s primecessum, got rather rich. It is said that he used to send his shirts to Paris to be pressed.)

\textsuperscript{13} The US medical establishment’s spectacular failure regarding the spread of AIDS (the worst record of any technologically advanced nation) has likewise created a fiscal bonanza for that very establishment (fn 19). Inset US business CEOs’ gross salaries (especially the golden-parachute-fad) have rightly been attacked as merely reward-for-failure. Why has there not been equal recognition of the AIDS-mediz lobby’s similar dynamic?

\textsuperscript{11} [Note added 1992/11: see fn 1.]
D Heritage

D1 Given the state of the world (which justifies massive political cynicism), I am occasionally asked why I remain happy and optimistic.

D2 Simple: for no cost beyond merely getting born, we not only partake of the beauties of nature (simple blue of sky and smell of grass&flowers, the visual grandeur of terrestrial clouds or Mirandan topography), but additionally we become beneficiaries of the art&genius of men (or their schools) such as: Homer, Aristarchos, Archimedes, Lucretius, Michelangelo, Marlowe—“Shakespeare”, Tycho-Kepler, Newton-Halley, Voltaire, Lagrange-Laplace, Beethoven, Turner, Berlioz, Darwin, Lizst-Wagner-Mahler-Strauss, Russell, Einstein.

D3 For the same admission price, we video-visit the Moon, Halley’s Comet, and even — thanks primarily to Ed Stone & Gary Flandro — the gorgeous swirling blue giant planet Neptune (my 2nd favorite celestial body). In this respect, even Neptune’s discoverer, Leverrier, was not so fortunate as we.

D4 Dramatic entertainment abounds. Uplifting music, deeper and far more varied than that available to the wealthiest king of 2 centuries ago (& even then only upon his prearranged occasion) now floods the humblest US home, at the merest flick of a switch.

D5 How can anyone stay depressed or blanket-misanthropic, for even a few consecutive minutes, when humanity has made such riches an inheritance-in-common to all?

E A Puzzle for the Ages

E1 Greg tells me that his twin brother Chris was born 2 minutes after him. But, on Greg’s 8th birthday, Chris had yet to celebrate a birthday.

E2 Question: what is Greg’s age?

E3 The answer will appear in a later issue of DIO. (Hint for those attempting to solve this puzzle: it helps if you aren’t a Vice President of the Royal Astronomical Society.) The first person to send the correct solution to DIO receives: [i] a free DIO subscription, and [ii] mention in our next issue.

F Educational Ironies

F1 Every time you bet on a sports hero’s performance, you contribute to creating an economic motive14 for him to: [a] privately bet against himself, [b] throw the contest you bet on, & [c] thereby walk off with a piece of your gambling loss.

F2 Likewise, every time you answer a pollster’s political question, you are telling politicians exactly how to lie to you.

F3 When evaluating polls, citizens feel protected by their imagined ability to see through most con-men — a confidence which fails to account for sample-filtration, by naively assuming that polls are like most of the breed. After all, it’s obvious that: the con-artist who least appears to be, will be the most successful — and thus the most toobiquitous.

F4 Wait ‘til a fraction of the money, stolen (largely by real-estate-speculators) during the Reagan-era Savings&Lootings deregulation&kickback orgy, starts filtering into the 1992 election process, paying for mass-befuddlement advertisements,15 to help elect most of the very politicians who made it all possible. Lucky botomy is still legal.

14 The most prominent person ever to warn the public of this reality was: Ronald Reagan (1986/1/26 HyperBowl halftime chat). An admirable act — and done before the most apt audience possible.

15 Also: “news”-spot propaganda, massaged “polls”, & the jokes of whoreling media “comics”.

H5 So, as in the Peary case: once the capacity for fraud is established beyond doubt, discussion of other suspicious cases can proceed without undue shyness of frankly pointing out (if the evidence warrants) the likelihood of dishonest scholarship. Another Tycho-Peary analogy: both men were among history’s best at their specialty (& both worked 23rd at it); but, when they were forced by circumstances to fall slightly61 short of a goal62 that promised fame and riches, they were willing to resort to deliberate exaggeration in order to make up the difference between hope & reality.

H6 A comparison to Ptolemy: most of the Tycho school’s work was genuine. E.g., while Ptolemy’s star catalog was up to 99% faked, Tycho’s was about 99% real. (And we know that Tycho’s epochal discovery of new lunar terms was entirely original with him.) It is curious that my attention was first drawn to the Tycho star catalog by Evans 1987, which attempted (pp.167f) to defend Ptolemy’s authorship of the Ancient Star Catalog by mock-demonstrating that the RN-DR arguments against him could just as readily be used to prove that Tycho’s catalog was also suspect. Indeed, it was Evans 1987 (p.168) which specifically brought up the four Cen stars as a bothersome (but nonetheless surmountable) glitch in his otherwise magnificent strawman-pseudoargument for Tycho’s apparent-but-of-course-illusory-fraudulence! The ironies here are so extreme as to require no comment. But there is additionally an economical, if smooth, implication: if so faithful a mathematician as Tycho could publish indoor-fabricated data (not merely fudged but simply computed wholecloth from theory, for the Oph half-dozen), then it is hardly incredible that a professional astrologer of Ptolemy’s ilk was capable of doing so.

H7 Evans 1987 (p.168) even notes how strange it is that Tycho gives the four Cen stars in exactly the (curiously irrational) order in which they appear in the Ancient Star Catalog (Almajest 8.1). In fact, Evans 1987 p.168 remarks that the lowest of the four Cen stars (2g Cen) would be only c.2 magnitude high at Hven transit, which makes its magnitude sensibly low (6 1/2) even presuming correctly computed extinction (& null dust). Yet, it never occurs to Evans that these stars might be fabrications. (Is it possible to come any closer to a discovery without making it?) What renders Evans’ oversight particularly inexplicable is that, if we use the over-opaque extinction formula which Evans himself develops at such length & with such seeming authority in this very article — the same formula which is used to attack DR throughout Evans 1987 — we find that 2g Cen would

61 Tycho’s cat D (1598) lacked only about 1% of his announced 1000 star dream. Peary’s final (1909) sledging-trip fell roughly 20% short of his longsought N.Pole.

62 I notice that Noel C. Swerdlow 1979 p.529 heaps scorn upon R.Newton for the latter’s then-most suggestion that Ptolemy faked data to agree with previously existing round-number parameters. (Curiously, WCS himself later learned that Ptolemy did precisely that! — see JHA 1.2 123 in 123 on Ptolemy’s use of Pliny’s integer-degree max elongations. The integrity-required apology to RN in this NCS paper was no doubt accidentally printed with invisible ink.) Yet we see that Tycho’s goal was an even thousand stars, so his fake stars were brought on by his desire to seem to have attained this round number. (Recall that, as early as 1595, Tycho had fatefully boasted that he already had 1000 stars in hand: §C9.) Also, DIO 1.3 (§N1 & fn 235) showed that Hipparchos twice faked a math proof to assure that two parameters came out to traditional integral-degree values. Therefore, though I do not myself accept the particular RN theory which NCS attacks, it is not an inherently unreasonable one. Thus, NCS’ lordly sarcasm is as apt as ever.
G Contradiction?

G1 US leftists believe that the ruling gov’t-media combine is a creature of business, and thus their enemy. Yet this ruling combine agrees with the Left’s support of: “education”-rebab, gun-control, treating every Sin (but homosexuality) as illness, the holy mission of affirmative action, massive Latin immigration, anti-racism, homosexuality-is-just-another-lifestyle, & opposition to using birth control for social engineering or to using capital punishment as crime-deterrent or justice-symbol.

G2 US rightists believe that the same gov’t-media combine is a creature of Liberals, and thus their enemy. Yet this combine agrees with the Right’s support of: anti-socialism, anti-communism, the sacred virtues of capitalism, the anathematization of suck-the-rich schemes, real estate development of every square mm of US turf, [mass-paranoid belief in an invisible deity], the iniquity of any foreign leader who won’t salaam to the US State Dep’t, & (the mafia’s Eleventh Commandment) the eternal illegality of drugs.

G3 No one who wishes to understand who rules the US and how, can ignore these strikingly persistent apparent contradictions. Both sides explain such anomalies by presuming mere-pretense media-support for its own side’s above-cited sacred tenets. But, a hypothesis which may facilitate resolution: are the foregoing “Left” & “Right” media positions really contradictory?57

H The Rehab Perpetual-Emotion Mirage

H1 Every TV ’snews discussion of possible solutions to continuing guerilla-war-level US city crime is a variation on the same error: seeking the chimera of “rebab”. Whatever the TV ’snews-approved-panacea (social programs, in-prison education, etc.) — it’s always the same delusion: if we get smart enough to throw the right lever, we can rehab the social misfits who commit crimes.

H2 But the sad truth is: we will never get that smart, because we aren’t even bright enough to realize that: [a] no such salvation is feasible here at present, without intolerable rehab-industry costs16 to the noncriminal majority; [b] a smart society’s 1st salvage-priority is to decent citizens (not to criminals), who deserve immediate (not someday) relief from street terrorism. For decades, “permissible” public discourse on preventing crime has implicitly presumed that, if we just tinkier a little longer with the rehab-machine, we’ll finally turn-the-corner on crime. (Remember the Vietnam War morass caused by light-at-the-end-of-the-tunnel propaganda?) Why does the media not ask if such a cure isn’t simply an emotional-wish delusion,16 as wise observers (such as George Bernard Shaw) have long

1598, Tycho dedicated the catalogue to Rudolph as a New Year’s gift”. Later (B1), Longomontanus published cat C (while Keplor published cat D: B11). Perhaps Longomontanus knew something. However, though Longomontanus spent years at Hven (Oph fakes), he did not arrive at Wandsbeck (Cen fakes) until late 1598 (Dreyer 1980 p.272), well after cat D had been completed & distributed. The different additive constants, used for the Oph & Cen fakes, might suggest 2 hands at work. (The implication is discouraging, namely, that fakery is more ubiquitous in science than most of us wish to accept is the case. If a small group of assistants felt pressed to produce data, the result might be: Table 6, the which hard-earned data cannot pass over in silence. Tycho’s arguments at Thoren 1990 pp.250 & 304 are comparable in folly to Polyné’s, though Tycho credibly went as far as having the planets revolving around the (Earth-circuiting) Sun: Thoren 1990 p.252. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C1.) And other: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes 1697 pp.250-252.9. See also DIO 1.17 [C3 & fn 2.) And-or: [b] The faker just added 24” to Hipparchos’ Longitudes
suggested? Face it, rehab is statistically no more effective now than it was, 30 years of criminologist-B/S. In fact, it is probably about less so. Why depend on receiving the long-falsely-promised benefits of progress from a field that is actually in regress?

Moreover, even if we assume (purely hypothetically) that criminologists, by studying murderers instead of fanning them, finally did discover a treatment which seriously diminished criminal behavior; the entire effort would turn out to have been wasted — because this treatment would instantly be attacked & gutted (by ACLU & co) as over-invasive and degrading to persons’ & groups’ dignity: who-can-play-God in complete or part) 4 dim Cen stars, when brighter real quarry beckoned? — e.g., the criminologist-B/S ago. In fact, it is probably a lot less so. Why depend on receiving the

Lessons & Phags

H1 Some concluding thoughts about the Tycho fabrications. Against Tycho’s personal involvement (in the Oph starfaking here exposed) is the following important point: Tycho, well before he produced cat D, argued evidentially for the obliquity’s secular shift — indeed he claimed this as his own discovery.23 So one would expect a faking Tycho to alter the Oph stars’ Hipparchan latitudes, to account for this precessional effect. Yet no such alteration is found.

H2 Apologists tend to excuse embarrassments such as the foregoing (Tycho’s fake stars) as perhaps due to underlings.24 But this alibi betrays frying-pan-to-fire smarts, by (in whole or part) 4 dim Cen stars, when brighter real quarry beckoned? — e.g., the 3 “middling”, easily-visible Sagittarius stars (γ Sgr, δ Sgr & ω Sgr), which Evans 1987 p.168 thought (§2) redeemed Ptolemy, since Tycho missed them. (Evans here uses the Journal for the History of Astronomy’s brilliant & novel abbrev of Sagittarius: “Sag”)

DIO urges astronomers’ general adoption51 — as a prominent reminder of JHA’s rightful guardianship of refereeing standards for the rest of us: J.Historical Astron. 1.2f n5.) The likely explanation for their absence from cat D is: timing.22 (See §F5 & fn 50) During the very end of Tycho’s pre-cat D observing period53 at Wandsbeck, the north tip of Cen culminated near night’s end, while Sgr was below the horizon all night.

G2 There are several reasons why I will speculate that the four Cen fakes (Table 2) originated at Wandsbeck, and may indeed have been produced just a few days before the 1598/12/2 date of cat D’s release. (Fn 10 contains a hint that only 2 days earlier, on 1597/12/31, these four Cen star positions were not yet entered into cat D.) [a] From Hven, the (null dust) culmination post-extinction magnitude of the most difcult of the four Cen stars (2g Cen) was μ = 6.48. (§B3); from Wandsbeck, the most difcult (4h Cen) had μ = 5.81 (fn 34) — almost twice as bright. Tycho’s normal outer-limit for μ lies either between these two features or near the latter. (Not one star of the frantical Final Fifty, cited in fn 42, had μ as dim as 5.81.) [b] The crudity of the underlying declinations (1601.0 rms error 14°) is not typical of Tycho’s output when he used his mounted instruments at Hven. But, when cat D was issued, Tycho had yet to make observations at Wandsbeck with anything but his primitive cross-staff (or “radius”: Ræder & Strømgren 1946 pp.96-97; Dreyer 1890 pp.19-20; Thoren 1990 p.18-19). [c] The Cen foursome would not be visible except near transit and in complete dark; but such conditions did not exist at Wandsbeck until virtually the date of cat D. On 1598/1/2, when the last of the Cen foursome transitted (LST = 134°30°, a bit past 6 AM Local Mean Time), the Sun’s altitude h = −18° (total dark). Only a week earlier, at LST = 13°30°, solar h = −14° — which would probably wipe out any possible naked-eye observations of the four dim Cen stars. So near the horizon (at Tycho’s latitudes) these objects would have been difcult enough even in complete darkness.
just call γ Sgr), δ Sgr, & ζ Sgr. So, we will next examine the exceedingly unmysterious causes for Tycho’s passing over these three stars (at OO 11:401 & OO 12:77-78).

F3 First off, it is obvious that these are summer stars — and Tycho reminds one (Ræder & Strømgrens 1946 p.113) that dim stars cannot be taken in the summer (since it never gets completely dark then in Denmark). Thus, capturing Evans’ three Sgr stars would require special, felicitously timed post-sunset autumn observations or (§F4) pre-dawn spring observations.

F4 Further possible mundane causes of Tycho’s omission become equally obvious when we reconstruct his record of the suspect 13 star Oph addendum (OO 3:364) — according to the simple hypothesis that all 13 were recorded during the last panicly observing night on Hven: 1597/3/15-16. The 1st quarter Moon (near the Summer Solstice & a north anti-node of the lunar orbit) was up most of the night, dampening prospects for capturing dim stars. As the Moon descended, observations of easy stars commenced (by sextant,48 thus no need for precise transit): 10A Oph, 53ν Ser, 55ξ Ser, 56ο Ser, 57μ Oph (D683-687). The six eventually-faked Oph stars (§C2) may have been vainly attempted, after initially-innocent consultation of the Hipparcos-Prolemy Ancient Star Catalog (Almajest 7.5-8.1), to rultle up some new stars (i.e., ones not already recorded by Tycho). But the six were mostly dimmer and lower than D683-687. For the faked six (D675-680), the records were: [a] never made, [b] poor (from moonlight), or [c] unrelatable to the highly erroneous Almajest positions expected: see §C3-§C5, & fn 28. (Against any option but [a]: it’s hard to make any record any worse than D681’s: §E4. But Tycho retained & published it anyway.) For D675-680, Tycho presumed that the Almajest places better-represented-reality (12 fn 28 or Gradhoff 1990 p.33) than he had received or record he might have made of them. So he faked all six stars.

F5 On 1597/3/15-16, Tycho’s last Hven night, the Moon was beginning to set at about the start (Sun 18° below horizon)49 of astronomical twilight — about Local Sidereal Time 15:50 (shortly after 3:30 Hven Local Mean Time). Only a few minutes of total dark remained, of Tycho’s epochal 2 decade observing career at Hven. In this final, pathetically narrow slice of time, the stars 45δ Oph, 3 Sgr, and 10γ Sgr were all about 6° magnitude (post-extinction), if we assume a perfect36 sky. (Approximate altitudes h at this time: 3°1/2, 4°1/2, & 1°, respectively. Pre-extinction magnitudes: 4.29, 4.54, & 2.99, respectively.)

F6 The attempt at low 45δ Oph led to the very rough place, D681; but higher 3 Sgr was pretty well recorded, so D682 became Tycho’s last creditable star place. The failure to record the lowest star (of the 3 stars suggested at §F5), 10γ Sgr — an omission which [see §F2] so furrowed the brow of Evans 1987 (p.168) — was obviously just due to: [a] twilight’s onset (after time had been taken to record D681-682), [b] star not noticed at altitude barely 1°, and-or [c] low haze or clouds at the horizon.

F7 Thus ended Hven star-work — with a 13 star record so sloppy and so salted with fakes, that the entire night’s data-sheet was destroyed, to cover (successfully for nearly 400°) the scientific sins here revealed for the 1st time since their commission.

G Last-Minute Centaur

G1 Presumably the four Cen stars were later added onto the previously-assembled 1000 star catalog, as insurance against a miscount (or detection of repeats). But why fake

G2 In the real (dusty atmosphere) world, the order of visibility-ease at this time would obviously be: 3 Sgr, 45δ Oph, & 10γ Sgr. And this order is reflected in these stars’ rôle in cat D, namely: good (before un-signed), crude, & nonexistent, respectively. The actual coordinates of these three stars follow. (The 3rd is the one, 10γ Sgr, whose absence from cat D so worried Evans: §F2 & §G1. Its place is found here via the excellent ecliptical star tables of K.Moergaard & L.Kristensen, Centaurus 20 129; 1976.) The A & β (E&E 1601.03) were, respectively: 257°19’ & −6°34’ (45δ Oph), 261°40’ & −4°22’ (3 Sgr), 265°42’ & −6°55’ (10γ Sgr).

1992 April DIO 2.1 ¶1 ever-deeper into debt) as it would have cost to stay solvent if the debt had never been incurred in the first place.

F4 Been wondering why interest rates have stayed so low for so long in the post-Carter era? Suggestion: if interest rates now suddenly doubled, the cost of paying the annual interest on the (ballooning Reagan-Bush)55 US National Debt would double. And thus (§I3) the annual budget deficit would double.

F5 Before 1968, the GOP was regarded as unprogressive. But since 1968, it has given us (besides the foregoing):

[a] the first demonstration of simultaneous inflation & recession (Nixon), and
[b] our first booted President (also Nixon).

The unerring trustworthiness of their approach is testied to by the examples that follow. These enable us to predict those verities which equally brilliant future historians will induce (from the ruins of our putative civilization), by depending upon the most widespread (& thus survivable) accounts:

J3 The most common portrayal of the use of alcoholic beverages in the US media was in TV advertisements. In these snapshots we learn that humans never drank alcohol. They held la-de-da parties to celebrate selecting wines that were either upscale or cheap or allegedly expected: see DIO 1.1 ¶1. & nonexistent, respectively. The actual coordinates of these three stars follow. (The 3rd is the one, 10γ Sgr, whose absence from cat D so worried Evans: §F2 & §G1. Its place is found here via the excellent ecliptical star tables of K.Moergaard & L.Kristensen, Centaurus 20 129; 1976.) The A & β (E&E 1601.03) were, respectively: 257°19’ & −6°34’ (45δ Oph), 261°40’ & −4°22’ (3 Sgr), 265°42’ & −6°55’ (10γ Sgr).

25 Lots of elder USmen prefer Bush because they believe he is more likely to keep the currency stable. (I.e., no Carter-style-Dem rampant multiplication-inflation of the money supply.) The catch: as the US sinks ever deeper into debt, the only way to pay off — or escape the crushing burden of — that debt (without even further ensnaring the US public at peak, zero-savings post-tax wages) will be to inflate the currency and thereby render the debt trivial. (Though, Japanese green hypnotism of the US Congress will block that route as long as possible.) Thus, Republican budget policy is guaranteeing the eventual election of the very bunny-money Dimocrats whom Republicans allegedly abhor. Maybe that’s why an (officially) Dem-majority Congress has so gleefully signed onto the Reagan-Bush there’s-no-tomorrow, schizo-economics squandering — which has become symbolic of the modern GOP White House.

26 See DIO 1.1 ¶1, 157 ¶3, DIO 1.1 ¶31, 157 ¶4. DR’s essential ¶3 logic was set forth in Queen’s Quarterly 91:4 969 (1984) pp.984&985.

27 For confirmation, just ask any modern defender of the faker C.Ploemey, whose magnificent Almajest so dominated publicly-accepted astronomy that it finally drove out of existence virtually all of the data & astronomical treatises of Aristarchos, Apollonios, & various other merely-honest scientists.
The most Christian man in the history of the US (the most Christian of nations) must have been that exemplary scholar whose name was chosen (from all the hundreds of millions of US citizens) to be first placed in celestial preservation, for time eternal, on the first plaque left (1969) on the erosion-free Moon. This saint’s name: Richard Milhous Nixon.

The thirteen Oph stars are indeed precisely those that close our gap between the very last stars (of US citizens) to be first placed in celestial preservation, for time eternal, on the first plaque left (1969) on the erosion-free Moon. This saint’s name: Richard Milhous Nixon.

Further Inductions

K Evolution of the Specie: From our records of late 20th century academe, we conclude that nothing refined a scholar’s creativity and ethics better than: touching and counting money. Or being a publisher: usually the same thing. Academic publications in such emotionally secure fields as History of science (Hist.sci) were filled with scholarly articles and reviews which went out of their way to extol the omniscient wisdom and exalted character of the businessman-scholar archons who peopled review committees and-or ran these very journals — or otherwise controlled the financial wellbeing of the scholars writing the articles that worshiped archons. The only mystery here is why these archonial paragons were not canonized to a man, since, according to the consistent Hist.sci journal record: [a] No editor or society officer ever did anything more sinful than misspelling. [b] They were the brightest of the bright, not to mention generous, inspirational, rigorously fair & neutral. (We know that all such characterizations were true, because — being self-confident models of no compromise — these journals would not embarrass themselves by publishing works that were otherwise inferior.) [c] No society officers or editors were ever censorial or vindictive. Or even cross. (Though they bore one. See under DIO, below: §K2.) It is still unclear what chemical was increasingly being added to 20th century currency, that made the much-touching of it so salutary to character.

K2 Near-Misses: Sad to say, at the very dawn of the Third Millennium, the above-cited otherwise-unruffled chorus of praise was — obstreperously and always erroneously — interrupted by the persistent dissent of an odd & trifling journal calling itself DIO. Happily, all intelligent scholars (i.e., those that spent time counting money, in reality or in dreams) agreed that DIO was never actually right about anything — and should be renamed Diatribe. (That the publisher was beyond all reason was notorious: e.g., if power-archons suppressed & secretly slandered which sacred duty is, after all, their privilege & prime earthly mission — well, DIO would actually criticize this. Out in public, mind you. Who can fathom the folly of it?) Happily, no copies survive; and DIO is now almost exclusively known through the sparse remains of a flood of refutations, which — by a remarkable coincidence — burst forth immediately after the scurrilous publisher’s sole archonally-approved feat. (Death was presumably hurried by his fanatical refusals to partake of the wellknown health benefits of cigarettes: §§J7.) These refutations are almost exclusively by lower-echelon castrati. But their accounts of his work, being as accurate as the rest of the output of the handsome reputable Hist.sci journals of that day, are to be trusted implicitly, and the refutations built upon these accounts are completely convincing. Another amazing coincidence: we now know that the positions taken by the publisher (whose very name

References:

28 We thank our childhood friend, Richard Lee Smith, for suggesting this item.
29 DR aside: O Gingerich has made the unintentionally-revealing observation that certain Hist.sci volk are now confusing the words “principle” and “principal”. (See fn 6.) Ah, these revealing capitalist slings. I note that, after explorer-loafer R.Pearcy became a millionaire & stock investor c.1910, his correspondence occasionally uses the word “cheque” when he means: verify.
30 See, e.g., the Van Helden review cited at DIO 1.2 fn 3.
31 Another theory has it that the DIO publisher’s demise was related to a repute motif of his, a printable version of which is: “Never kiss a jackboot, especially if it’s trying to neck.”

45 See fn 14 & §§D5. Modern scholars are all too familiar with this phenomenon. But few are doing much about it — besides shoultering briskly for a firm personal place at the trough. Most institutions handle the attendant stink (not by reforms but by suppressing & denouncing reformist criticism. Classic public relations priorities.
46 Tycho’s principal stars’ errors averaged less than half an arcmin. See Dreyer 1890 pp.351-358, 387-389; also Wesley 1978. [Note added 1993: See also DIO 3.] The worst of the 5 add-on Crt stars was D993 (24c. Crt). Though its null-dust post-extinction magnitude was dim (μ = 5.82), its position error was less than 5′.
D3 I have searched through the Tycho star observation records later\(^1\) than early 1596. The search turned up 54 of the 71 extra stars,\(^2\) accounting for all the stars in 6 of the 8 post-cat C sets of \(\text{JD2} — \text{but none of the stars in the remaining 2 sets: in Oph and Cen. I.e., of Tycho’s last}\(^3\) (post-cat C) 227 stars, these 17 stars (the 13 Oph supplemental stars & all 4 stars in Cen) are the only ones for which no empirical records of any sort appear in Tycho’s surviving mss materials. I need not add that these are the very 2 sets (17 stars in all) which contain all of the 10 faked star places. And, looking beyond the final 227 stars: in the entire cat D, almost no other stars lack supporting data.\(^4\)

D4 We have already fully discussed the four Cen stars, noting that they are actually additional to the 1000. (Recall that the Cen quartet brought the total to 1004 stars: \(\text{§B3.}\) Thus, if we fix our attention on the 1000 Tycho stars, we should temporarily set aside the Cen foursome and scrutinize just the Oph set of 13 star-places — which has now been isolated here as virtually (fn 44) the sole set of the 1000 stars for which no records have been found in the Tycho mss.

D5 In order to reconstruct how this Oph supplemental set of 13 came into being, we start with a simple question. When Tycho was informed of his full loss of royal patronage (in a letter of 1597/10/20; Dreyer 1890 p.231) and realized his days on Hven were numbered, just how many more stars did he believe he needed to make his desired 1000 stars? The answer is explicitly stated in a note written right on the manuscript of the final series of star-position data ever taken at Hven under Tycho (1597/2/4-3/10). At the beginning of these last star data is written (OO 13:98; \(\text{§B1.}\) “Desiderantur 60 pro compleendo millenario.” Just 60 more stars would do the trick. Tycho had claimed he had about 1000 stars as early as 1595 (a claim his astronomer-biographer rightly regards as a lie: \(\text{§B1.}\) It seems that Tycho was expecting (\(\text{§B1.}\) a commemorative medal for the 1000 stars (which would of course enhance his prospects for future funding).

D6 To qualify for this honor, he needed 60 more stars. But a count of these last Hven data (OO 13:98-100) shows that he only acquired 47 new listed stars before leaving Denmark. So, how did he get the last 13 stars (all Oph)?

\(^{1}\) I’ve examined previous data as well in OO vols.10-13, finding none of the 10 suspects listed in Tables 1&2. The seeming 1591/10/10 observation of D678 at OO 12:171 is just a scribal error: for Tycho’s reference to Hipparchos’ 16th star of Oph, instead read 16th star of Ser — cataloged in the “1588” Appendix (complete until much later than 1598) to the 1592 observations; see OO 12.254. The original Tycho mss reside in the Royal Library at Copenhagen; but the full observational records are printed in OO vols. 10-13.

\(^{2}\) All 54 of the real data are found in OO vol.13 (note added 1993: they will be traced in \(\text{DIO 3} \)) Cyg, pp.59-60; UMa, pp.59-76; B4, p.74; UMi, p.76; Per, p.99. The total number of outdoor stars (added to C D by these data) shrinks to just 49 stars, after certain repeats & slippages are accounted for. (\(\text{DIO} \) will round & henceforth refer to this set as the Final Fifty.) [Note added 1993: See Tables 19&20 in \(\text{DIO} \)’s upcoming contribution: the 1st critical end of cat D.]

\(^{3}\) The actual chronology of cat C’s compilation was not simple; see Thoren 1990 p.297 n.133.

\(^{4}\) (Note added 1993: Of the 777 stars (cat C) recorded by Tycho before his final gleanings (the 227 stars we are now scrutinizing), only 4 stars’ observational data are missing. These are: D370-371 & D379-380. (In cat C: C528-329 & C337-338.) These were evidently viewed accurately (with Tycho’s rarely-used semicirculus (Räder & Strömgren 1946 p.96, Thoren 1990 p.177), using Sintus & Algebr as reference stars. The data presumably got mislaid due to the highly exceptional (probably experimental) mode of observation. Detailed analysis in \(\text{DIO 3} \).)

\(^{32}\) \&c. 1992 April \(\text{DIO 2.1} \) \(\text{§4} \)

E Stars Without Sources

E1 We now turn to the set of 13 supplemental Oph stars, for which no records exist. These 13 are listed at the top of OO 3:364. They are separated from the rest of the constellation by the heading “sequentes pertinente ad Ophiuchum et eius Serpentem”.

Longomantanus, when editing the Tycho star catalog, intended to use D701 (as the cat D order indicates), but noticed that D701 was just an identical repeat of D699. Noting that both D701 & D675 were labelled “in dextra tibia”, he substituted the (nonrepeat) data of D675 into the slot for C553. The result is that C553 is probably the sole faked star in cat C. (No real 3rd magnitude star is close to C553’s coordinates — which surely makes it a rarity in the generally eficient — so the whole Earth became a one-newspaper town. (TV ‘s news had already effectively made the US a one-newspaper nation even before 2000 AD.) This was obviously not regress but progress. Like unto the triumph of monomethion in the 4th century AD.

\(^{33}\) \text{\textcopyright} 1992 April \(\text{DIO 2.1} \) \(\text{§4} \)

L Collective Shamelessness

L1 The reader may suppose that \(\text{§J7} \) is overstrong; however, it should not amnesia-forgotten that Hollywood films have been used for most of this century to glamorize smoking. Did this occur by chance?

L2 When it began to be widely rumored c.1950 that smoking was medically suspect, we could count upon certain softhearted film producers to help out the poor misunderstood tobacco lobby. (Only the hardest cynic could suppose that a film’s kindness to weed-interests was performed in return for generous under-the-table scal support.)

\(^{34}\) \&c. 1992 April \(\text{DIO 2.1} \) \(\text{§4} \)
always been this lobby’s strong suit, as we see from a particularly precious cinema scene, which I have extracted from our Doubletakes Dep’t, in order to display it, unabridged, in a special niche here.

L3. In the highly-promoted 1951 sci-fi film *When Worlds Collide* (mixing Velikovskyan & Noah’s-Ark Biblical themes), we encounter the following immortal exchange (between Velikovskyan & Noah’s-Ark Biblical themes), in the office of an astronomical observatory, 10° into the film:

Dr. Drake: You smoke? [Offers Mr. Randall an American cigarette.]

Mr. Randall: Thanks, I’ll try one. [Just in from S. Africa, Randall pulls out a cig of his own & offers it to the Doctor.] I have a South African one. You — ah — part of this deal, Doctor, this stargazing?

Dr. Drake: No. No, I confine my gazing to eye, ear, nose [lights Randall’s cig], and throat. I’m an M.D. [Immediately lights own cig.]

M Masterpiece Theatrical Integrity

M1. As tentatively predicted, the Royal Astronomical Society of London has so far printed no correction in response to the *Journal for Hystorical Astronomy*’s recomputations of the magnificently innocent RAS-published mismath (*QJRAS* 1985 p.514) of RAS Vice President David Hughes (J.H.A centerpiece of *DIO 1.1*: §8 §B-§E & Table A).

M2. As for the same RAS Vice President’s alleged calendric proof (*Journal for the History of Astronomy* 1986 p.189) of alleged British priority in seeing Comet Halley (*DIO 1.1*: §8 §G): the J.H.A’s esteemed Editor-for-Life has indicated no interest in correcting that hilarious bit of typically unrefered nonsense. (The J.H.A & *QJRAS* annual subscription fees are each only $50.)

M3. These journals’ contempt for the truth is exceeded only by their contempt for their own loyal subscribers’ intelligence & independence. (The implicit whatayagonadaboobit presumption is that no scholar will complain. Or even inquire. Is the presumption correct?)

M4. And the Brits put on such convincing airs about honour and all that. Well, didn’t you *Masterpiece Theatre* fans ever wonder how Britain keeps producing the world’s very best actors?

scrunching permitted advertisers to get in more commercials during a film.) Recently, having noticed discrepancies in the actual-vs-official times of Cinemax (cable) films, I became curious as to the cause and started by comparing Cinemax versions to rental versions. I was amazed to find that Cinemax is scrunching films, commonly (but not always) at a 6/5 ratio, thus shortening them by precisely 4%. But why? (Not to get in more ad-time: Cinemax doesn’t even have ads.) Answer: mere scheduling convenience. While The Movie Channel starts films at odd times (like 9:25 PM), its competitor Cinemax starts films in prime time (like 9:30 PM). Thus, if a film is so inconsiderate as to start, say, 93° over 90° — all so that the following film can start at the round time, 9:30 PM. (The intent is obvious, since the only films which Cinemax scrunches are ones that are just a little bit over 90° or a similarly round time.) Classic films are less likely to be mistreated thus. However, even so well-known a film as *Road Warrior* has suffered the Cinemax scrunch. In any case, I find the practice almost as offensive as its sneakiness. No notice is given by Cinemax. Or the media’s film-promoters aka “critics”. (One recalls these same parties’ years of obsessive attacks on Ted Turner’s colorization of old black&white films — which is harmless since most TV sets have a black&white button. The anti-colorization hysteria was obviously just the anti-Turner crowd’s way of sniping at him, since the far more serious scrunching-vogue has barely been noticed by these folks.)

40 The book (on which the film is based) was co-authored by Edw.Balmer & (the famous) Philip Wylie: *When Worlds Collide 1933 & After Worlds Collide 1934; rebound & published together in 1950, the same year Velikovsky’s *Worlds in Collision* appeared. (A neat instance of mutually-benefiting publicity.) The film’s special effects are occasionally quite effective, though nothing in it is as otherworldly as the gosseynicity at the start and end — completely out of place today.

41 *DIO 1.1* 18 fn 29.

23°31’1/2), using the simple relations:

\[
\begin{align*}
\sin S &= \sin \epsilon \cos \lambda \sec \delta \\
\tan x &= \sin \lambda \tan \epsilon \\
\tan(x + \beta) &= \tan \delta \sec S
\end{align*}
\]

C8. If the declinations were really observed, then: [a] for 2g Cen (even assuming zero atm dust), the Hven’s post-extinction magnitude \(\mu = 6.48\) (§B3), which was the dimmest \(\mu\) of the entire 1004 stars, or [b] the Cen observations were made after 1597/10/20-21 (Dreyer 1890 pp.258, 390), at Wandsbeck,35 where cat D was issued36 on 1598/1/2.

C9. Dreyer 1890 p.227 (§B1) says that Tycho deceived as early as 1595 regarding his having allegedly already recorded 1000 stars (though p.259 presumes that 1000 star places were actually taken before Tycho left Hven in 1597): Tycho mentions his prior 1000-star promise in cat D’s preface (OO 3:340: “quas pollicemur”). But before he could quite complete his taking of 1000 stars, Tycho (having utterly lost royal favor) left Hven. (All Hven celestial data cease37 1597/3/15, shortly after the Vernal Equinox.) During the winter of 1597, just before eviction, he raced frantically to record enough stars to comprise 1000. So, at this desperate time, there was motive for somebody faking a few stars, to kick the total up to the sacred number 1000.

D Tracing the Post-cat C Stars

D1. After coming to the above conclusions & writing the foregoing (largely as it now stands), I inspected Tycho’s raw observations, published in OO vols.10-13. I learned that most of the 227 stars supplementing cat C to bring it up to cat D’s total are found in the Appendix38 to Tycho’s 1596 data. This 1596 Appendix contains 156 of the 227 supplemental stars. (The Appendix contains 157 stars; but one star, 24ω Her, was omitted by cat D, as noted at OO 13:76 n.1.)

D2. This leaves 71 (227 minus 156) star places that appeared subsequently. (It is known that the observations of the supplemental stars began in 1595: [§B1].) The 71 extra stars occur in 8 constellations; taking these data (fn 42) roughly in chronological order, the cat D numbers are: D565-569 (Cyg, 5 stars), D399-411 (UMa, 13 stars), D481-482 (Boo, 2 stars), D675-687 (Oph, 13 stars), D345-355 (UMi, 11 stars), D596-614 (Cas, 19 stars), D644-647 (Per, 4 stars), D1001-1004 (Cen, 4 stars).

value \(\epsilon = 23°31’1/2\) in numerous reductions. Moegaard’s finding was adopted for some of DR’s detailed analyses here. [Note added 1993-1997: the \(\epsilon\) values actually adopted for Tycho’s work are investigated at *DIO 3* (1993) fn 190.]
C4 Another long-shot fluke helps cinch our exposure of the fraudulence of one of the entries, D675: this star (36A Oph = D675) has the fastest shift of latitude. The star’s actual $\beta = -2^\circ 35'$ in Hipparchos’ day, disagreeing with Hipparcos’ recorded $\beta_H = (-2^\circ 1/4)$ by $20'$, which is about equal to his mean $\beta_H$ error (R. Newton 1977 p.216); however, by Tycho’s time, the star’s actual $\beta$ had shot up to $-3^\circ 22'$. Thus, even aside from its wrong sign, D675’s $\beta$ was off by over a full degree: an error of $1^\circ 10'$. This because the position of D675 was plagiarized from data (PK 247: $\Sigma$C2) which were far more obsolete than the swper had reason to suspect.

C5 A final slip by the plagiarist: Hipparchos’ star PK251 (from which D680 was copied) does not even exist. It is simply 390 Oph with $\lambda = 231^\circ 1/2$ accidentally written as $234^\circ 1/2$ by Hipparchos. Tycho just copied the error.

C6 Having established this background, we now return to consider, in Table 2, cat D’s four add-on Cen stars (D1001-1004; OO 3:373). To our astonishment, we find that the longitudes have been faked, in almost exactly the same way the Oph fakes were accomplished: $21'$ of presumed precession (fn 56) added onto Ptolemy’s longitudes for stars PK935-938. (These stars are listed at PK p.71, Toomer 1984 Ptolemaic p.394.) Note: the Cen mean $\lambda_T$ error exceeds $1'$. Keep in mind the context: Tycho’s positional errors were normally about $1$ or $2$ arcminutes. [Note added 1993: See §E5, and DIO 3’s forthcoming full edition of cat D; standard-deviation $= e.2'$.] Another way of putting it: the Cen $\lambda_T$ in Table 2 are roughly 30 times nearer the fabrication values $\lambda_T + 21'$ than to real $\lambda_T$.

C7 However, the latitudes $\delta$ are no closer to Ptolemy than to reality. Evidently, declinations $\delta$ were genuinely observed somewhere (poor rms error: $1'$) and then combined with the fabricated longitudes $\lambda$ (§C6), to yield latitudes with Tycho’s obliquity $\epsilon = \ldots$  

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1992 April DIO 2.1 ¶4

‡2 Correspondence: Reaction to DIO 1.1

A Fantasies

A1 Absolutely delighted with DIO. I’ve long said that what this backward nation needs is a good three-letter abbreviation for junk-mail. You have my warmest congratulations for finally supplying it. [Signed] The Malignant 1

A2 I lost no time reading your magazine. [Signed] DMcC.

A3 DIO is inimitable. I simply cannot put it down. Having not chosen to pick it up. Ahead of the garbage man. [Signed] N.Coward

A4 You are a genius. I love you & long to bear your child. [Signed] Spike

A5 DIO makes the libellous implication that I am a litigious dictator, which is a damned lie. Retract or I shall sue. [Signed] Lord Thinskin, Deity-for-Life.

B Realities

B1 OK, now to subside from dreams-of-gory to: the real letters. Before dipping into that barrel, though, I must note that numerous unwritten verbal reactions were received. Several professional sociologists expressed gratitude for the raw data that DIO is placing onto the public record. And, while a number of readers said the footnotes’ print was eyestrainingly tiny, at least one DIO subscriber reads mainly the footnotes. (See fn 10.)

B2 DIO 1.1 drew encouraging missives from so many quarters that not all can be included here. Some are too kind to quote, such as the letter from my Gilman School physics teacher Bill Porter (who, at the instigation of DR’s lifelong friend & advisor Ludow Baldwin, introduced me to the wonders of calculus & Newton’s Laws, when I was only 16’ old). But I will always treasure them privately.

C Honesty & Belief

To: DIO

1991/5/1 From: Louisa Fitzgerald Huber, Harvard University

C1 glad to find someone who among other things makes a case against stupid or downright damingly dishonest reviews.

C2 It’s funny: the people who at the bottom of it all don’t believe in much of anything are the ones who care the most for honesty and accuracy.

D Alkaline Reaction

To: DIO

1991/5/14 From: Kristian Peder Moesgaard, University of Aarhus, Denmark

D1 I hope that soon you may even succeed in diluting the acid component of the ink and thus keep the innocent paper free from contamination.

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1 This was a favorite response of poet David McCod (Harvard Alumni Fund), longtime genial boss of my dear mother-in-law, Sylvia Reynolds.

2 Now living within easy driving distance of BrownU, Bill is available for tutoring. Mufosi take grateful note.

3 A delightfully artful reference to DIO’s back-inside-cover statement.
D2  [Your] Hipparchos paper [DIO 1.1 §6] clearly falls within my field of interest and competence. It reveals an important new stage-setting for the interplay between Babylonian and Greek-Hellenistic astronomy.

D3  I find a lack of (consistency) in using [Yj] ... for the [UH solar] mean motion instead of [Yh]. Also, the [Meton-Hipparchus solstice-pair] already includes epoch-longitudes, so why speculate further on this as [at ... §7]? ... the argument for the reconstruction of the three Hipparchian positions of the Sun would gain by employing consistent elements, even if the results should miss the mark by a minute of arc.

D4  My central question is concerned with the interpretation of the year-length Yh, tropical or Metonic, i.e., 235/19 times synodic month? ... [DR] takes it to be tropical and accordingly “the best of a rather poor lot of surviving ancient estimates” [§88]. But interpreted as Metonic it is clearly inferior to 365;14.48' [Yj] which the author (therefore?) brings into play for the demonstration of the success of the UH orbit in imitating eclipse events.

D5  DIO believes no extended responses are necessary, for those who have read the original DR paper with care. Three brief comments: [a] The on-the-agreement displayed in §55 of the DR paper shows that the proposed UH mean motion indeed matches Hipparchos’ final mean motion tables to Y. [b] Though Yh (eqs. 1&4) revealed [to DIO] Hipparchos’ raw recorded time for the 135 BC SSolst, it is obvious from Almajest 3.1 that Hipparchos’ contemporaneous solar tables instead used yearlength Yh (DR eq. 7). [c] The same Babylonian tablet ACT 210 contains both Yh and MAx (ibid eq. 10), and these two numbers are obviously not in the ratio 235:19 — though Yh/MAx does very closely approximate 235/19 (as Moesgaard has often justly emphasized, contra R. Newton). Therefore, we know that Yh was (wisely & boldly) not founded in any way upon lunar data or considerations. (I.e., the tropical year was not falsely identified with 235/19 months — Meton’s 432 BC scheme, subsequently followed in some degree by Kallippos, Aristarchos, Hipparchos, & Ptolemy.) Which is precisely why Yh was the most accurate tropical year we have inherited from antiquity.

D6  I warmly commend the broadmindedness of Moesgaard (an internationally respected expert in this area) in acknowledging the essential validity & watershed import of the DR paper. [Note added 1993: Unfortunately, Moesgaard has failed to put this encouragement into public effect. See J.HA 1.2 fn 170.]

E  Ringside Chuckles

To:  DIO

From: David Fowler, Math Inst, Univ Warwick, Coventry CV4 7AL, UK

E1  Many thanks for DIO & J.HA . . . . I’ve wondered how to thank you . . . . am enclosing one of my last offprints (for they were fabulously expensive) of a recent obscure article [Archive Hist Exact Sci 41.3:189 (1991)] on continued fractions — a topic close to your heart . . . . Also a little preprint (“Unit Fractions Again”) in which I loose off a few strident opinions. Had I enjoyed your journal less, I might have sent more reprints. (The old chestnut: “First prize: a week at Blackpool. Second prize: two weeks.”)

E2  I work outside most Muffins, cliques, schools, and the like . . . . So I stand way outside most of the issues that you are dealing with, and treat these arguments and fights as high class entertainment.

E3  My central question is concerned with the interpretation of the year-length Yh, tropical or Metonic, i.e., 235/19 times synodic month? ... [DR] takes it to be tropical and accordingly “the best of a rather poor lot of surviving ancient estimates” [§88]. But interpreted as Metonic it is clearly inferior to 365;14.48' [Yj] which the author (therefore?) brings into play for the demonstration of the success of the UH orbit in imitating eclipse events.

E4  I warmly commend the broadmindedness of Moesgaard (an internationally respected expert in this area) in acknowledging the essential validity & watershed import of the DR paper. [Note added 1993: Unfortunately, Moesgaard has failed to put this encouragement into public effect. See J.HA 1.2 fn 170.]

Table 2: Tycho’s Fake Centaurus Stars

<table>
<thead>
<tr>
<th>Cen</th>
<th>PK#</th>
<th>D#</th>
<th>(\alpha_p+1^J)</th>
<th>(\beta_p)</th>
<th>(\lambda_T)</th>
<th>(\beta_T)</th>
<th>(\lambda)</th>
<th>(\beta)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2g</td>
<td>935</td>
<td>1001</td>
<td>211'30&quot; -21'40&quot;</td>
<td>21'27&quot; -21'49&quot;</td>
<td>212'29&quot; -21'34&quot;</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4h</td>
<td>936</td>
<td>1002</td>
<td>211'00&quot; -18'50&quot;</td>
<td>210'59&quot; -19'08&quot;</td>
<td>212'15&quot; -18'56&quot;</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1l</td>
<td>937</td>
<td>1003</td>
<td>210'10&quot; -20'30&quot;</td>
<td>210'12&quot; -20'51&quot;</td>
<td>211'10&quot; -20'32&quot;</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3k</td>
<td>938</td>
<td>1004</td>
<td>211'00&quot; -20'00&quot;</td>
<td>211'03&quot; -20'12&quot;</td>
<td>212'23&quot; -20'02&quot;</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

21 [OO 3.337; Neugebauer 1975 p.280 n.1]

22 For PK250 = D679, Tycho found discrepant [\(\beta\)] in the source catalogs for his Oph fakes: 0°1/4 in Almajest 7.5, as against 1°4/4 at Copernicus 1543 p.50A. And the 2 main latitude variants on the ancient ms are 0°1/4 & 0°3/4. (These are noted on PK pp 99 & 187. Both values have been prominently printed. The former is the more common.)

23 Evidently, Tycho judged (correctly, I believe) that Copernicus had adopted the 0°3/4 option (with an inadvertent +1 error in the degree units place). So Tycho felt it safest just to average the two fractional endings, yielding \(\beta_{UH} = 0°1/2\) (italicized in Table 1). For the next star, PK251, there are 2 prime ancient variants: 233°1/2 & 234°1/2. The latter is more frequent — and was Hipparchos’ own value. (See fn 29.) But the former is that of Copernicus 1543 p.50A, and it was the value used in the Tycho fabrication.

24 Except in the cases cited at fn 56 [note added 1993: & see DIO 2.3 §8 in fn 20], the \(\lambda\) are just 2°2/3 less than the explicit Polynomials values \(\lambda_p\) found in the unreconstructed Almajest 7.5-8.1 Ancient Star Catalog. [Note added 1993: For a brief review of the abundant evidence that Ptolemy got his Catalog’s vast number of purported outdoor “observations” by indoors arithmetic (adding 2°2/3 onto the ordnag 1000 \(\lambda_y\) in Hipparchos’ catalog), see DIO 2.3 §8 (C)].

25 This star is D676 = PK246. Of the group of six, it was Hipparchos’ 1° (Tycho’s 2°). Thus, presuming Hipparchos used a ditto system (something like PK’s), a sign error for the \(\beta_{UH}\) of PK247 (perhaps triggered visually by the equality of its absolute value with that of PK246’s \(\beta_y\)) would carry a single sign error into all the following stars. (At PK252, we finally encounter a correctly-signed positive \(\beta_{UH}\).) That the sign-confusion problem here is partly Tycho’s own is suggested by the positive \(\beta_T\) sign wrongly given (E4) to D681-D682. (Neither position was adopted.)

26 Tycho’s word) of Hipparchos’ Ancient Star Catalog was Tycho! — right in the preface to the very star catalog (cat D) containing the 10 stars here exposed as faked by just that same method, and using data preserved by that same ancient astronomer. . . .

27 I will designate stars in cat D with the prefix D. The fake Oph stars are D675-680 (top of OO 3.364; see also OO 3.416); in Hipparchos’ Catalog, the same stars are, resp, PK247, 246, 248-251 (listed at PK p.79 & Toomer 1984 Almajest pp.354-355). We set out these six stars in Table 1, using \(\lambda = \) real celestial longitude & \(\beta = \) real celestial latitude, 22 with subscripts H for Hipparchos, 23 C for Copernicus, & T for Tycho.

28 An unlikely chance factor that unambiguously exposes the pretension: it is well known that, in numerous versions of the Almajest (obviously including at least some of those used by Tycho), the negative latitude signs of 5 of these stars were accidentally lost. The last Tycho’s latitudes \(\beta\) (listed as positive \(\beta_{UH}\)) are: 90°4/4, 90°3/4, 90°0/4 (ibid), 8°1/4. (1984 Almajest p.354 n.151), though only the 2°96 had originally been recorded (by Hipparchos) 22 with positive \(\beta\). (PK p.99 quotes astronomer C.Peirce’s opinion that the stars PK247-251 were “one of the greatest perplexities of the catalogue.”) 25 And cat D faithfully copies all of these sign

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* Moesgaard’s helpful observation is not mere carping. He & I are aware that the UH orbit fits the data regardless of whether \(Y_j\) or \(Y_h\) is adopted.
Table 1: Tycho's Fake Ophiuchus Stars

<table>
<thead>
<tr>
<th>Oph</th>
<th>PK#</th>
<th>D#</th>
<th>( \lambda_{H} + 24\beta_{H} )</th>
<th>( \lambda_{C} + 28\beta_{C} )</th>
<th>( \lambda_{T} )</th>
<th>( \beta_{T} )</th>
<th>( \lambda )</th>
<th>( \beta )</th>
</tr>
</thead>
<tbody>
<tr>
<td>36A</td>
<td>247</td>
<td>675</td>
<td>254'20' 2'15&quot;</td>
<td>254'20' 2'15&quot;</td>
<td>254'23' 2'12&quot;</td>
<td>254'29' -3'22&quot;</td>
<td>254'29' -3'22&quot;</td>
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<td>40C</td>
<td>246</td>
<td>676</td>
<td>255'00' 2'15&quot;</td>
<td>255'00' 2'15&quot;</td>
<td>255'01' 2'16&quot;</td>
<td>255'19' 2'06&quot;</td>
<td>255'19' 2'06&quot;</td>
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<tr>
<td>41C</td>
<td>245</td>
<td>675</td>
<td>255'40' -1'30&quot;</td>
<td>255'40' -1'30&quot;</td>
<td>255'50' -1'37&quot;</td>
<td>255'50' -1'37&quot;</td>
<td>255'50' -1'37&quot;</td>
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<td>44B</td>
<td>249</td>
<td>678</td>
<td>256'20' 0'20&quot;</td>
<td>256'20' 0'20&quot;</td>
<td>256'23' -0'20&quot;</td>
<td>256'26' -0'54&quot;</td>
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<td>51C</td>
<td>250</td>
<td>679</td>
<td>257'10' 0'30&quot;</td>
<td>257'10' 0'30&quot;</td>
<td>257'12' 0'29&quot;</td>
<td>257'14' -0'38&quot;</td>
<td>257'14' -0'38&quot;</td>
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<tr>
<td>39A</td>
<td>251</td>
<td>680</td>
<td>258'30' 1'00&quot;</td>
<td>258'30' 1'00&quot;</td>
<td>258'36' 0'58&quot;</td>
<td>258'33' -1'08&quot;</td>
<td>258'33' -1'08&quot;</td>
<td></td>
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</tbody>
</table>

(1 Cen), 6.48 (2g Cen), 6.13 (3k Cen), 6.23 (4h Cen). It's hard to believe Tycho could record all 4: even assuming null dust, two of them would have the dimmest \( \mu \) of any stars in the entire cat D — indeed, 2g Cen would be off in a class by itself.

C Isolating the Fakes

C1 Our next discovery is a startling jolt to me, as a fervent admirer of the Tycho school’s genuine discoveries. I have found that the first 6 stars of Ophiuchus (Oph) in cat D are fakes. Simple recipe: [a] just add 28° onto Copernicus’ longitudes \( \lambda \), which is almost (fn 56) equivalent to adding 24° of precession onto Hipparchos’ \( \lambda \); [b] sprinkle a very few arcmin of random scatter 15 onto these results and onto Hipparchos’ presumably-little-changed longitudes,20 also adopted by Copernicus. (It is ironic that Tycho was a pioneer in precession, showing that prior astronomers’ equinoctial “trepidation” was chimerical — indeed, 2g Cen would be off in a class by itself.

F Penguins Under the Ecliptic Pole

To: DIO

From: Keith Pickering, 10085 Country Rd. 24, Watertown, MN 55388

F1 . . . I was struck by your remark2 that Aldebaran and Antares “can never be seen simultaneously from the Mediterranean area”. This led me to investigate the conditions under which Aldebaran & Antares are above the horizon at the same time.

F2 The [great circle] angular separation of the 2 stars is 169°.96, just over 10° from complete opposition. This implies that it would be possible for each star to be a comfortable 5° above the horizon simultaneously, provided that the median point of the 2 stars is at the zenith. The median point turns out to be (epoch 2000.0) . . . less than 0°5 from the south pole of the ecliptic, which makes perfect sense when you realize that the 2 stars have opposite ecliptic longitudes and each is at about —5° ecliptic latitude.

F3 But now we are faced with a problem: if we stand with the median point overhead (near the Antarctic Circle), . . . the [solar center on the ecliptic] can be no more than 0°5 below the horizon. Correcting for refraction, this means that at least half of the solar disk would be visible at all times of the year for any observer who has the median point of the 2 stars at his zenith.

F4 The way to save the situation is to rotate our horizon frame around the Aldebaran-Antares axis. Or, more precisely, around the axis joining [the two points on the ecliptic at the 2 stars’ longitudes]. Each star will be slightly “above” this axis for our hypothetical southern hemisphere observer. The rotation will tilt both stars simultaneously from the ecliptic well below the horizon, while lowering the altitudes of the 2 stars only slightly. The tilt angle must be at least 6° (which would put the earth at the middle of the night at the right time of year). . . . This tilting creates what we might call a [fairly narrow] “line [focus of] zeniths” under which the Aldebaran-Antares-Antares line, under which we must stand to see both stars simultaneously.

F5 Projecting this line on the equatorial-coordinate sphere, we find that it passes through its lowest declination at —68° 1/2 . . . . This implies that Aldebaran and Antares are both visible above the horizon on at least one day a year for all observers in the southern temperate zone, but observers travelling north of the Tropic of Capricorn will find it increasingly difficult to see both simultaneously. If we extend the tilt all the way to 90°, the line of zeniths reaches declination 8°, which latitude may be regarded as the absolute northern limit from which the simultaneous observation can be made [ignoring refraction]. Observations from the Antarctic [south of c.74°S] are precluded by Aldebaran’s northerly declination.

F6 Now’s a tougher test: is it ever possible to observe Aldebaran, Spica, Antares, & Deneb Algedi simultaneously above the horizon? These four stars all lie just south of the ecliptic in four different quadrants of the sky. Putting the south ecliptic pole near your zenith would allow all four to be above the horizon, but only during daytime. The horizon tilting strategy used above will not work, since at least one of the four will be rotated below the horizon along with [half] the ecliptic. The 4-star simultaneous observation is therefore impossible.

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15 This question arose because DIO 1.1 (6 fn 30) had noted that the latitudes of Aldebaran & Antares differed by exactly 180° (within 1') from c.300 BC to 1200 AD — and that the Ancient Star Catalog (whose longitudes are generally expressed to precision 10') placed these 2 bright stars exactly 180° apart. Neugebauer 1975 p.960 notes that this same stellar opposition was stressed by Kleomedes (4th century) & Rhetorius (6th century).
16 A crude check on Lorentz (from arctan^tanh cos(\lambda)) = 8°1/2. For about 300 BC, the same calculation would find that the limit was nearer to the Tropic of Cancer than to 8° N latitude.
F7 There is, however, one “miraculous” exception to this: for an observer at the correct latitude (the Antarctic Circle), and at the correct Local Sidereal Time (6°), the south ecliptic pole will be overhead and the [refracted] sun will be just above the horizon. If at the same time there were a total eclipse of the sun, all four stars could be visible simultaneously.

F8 The enterprising photographer who uses an all-sky lens to capture Aldebaran, Spica, Antares, Deneb Algedi, the eclipsed solar disk, and up to 5 bright planets, all ringing the horizon in a single frame, would certainly qualify for the Astrophoto of the Millennium Award. Such a photo would best be taken from a high-flying aircraft, because of: [1] lack of suitable land along the Antarctic Circle; [2] generally bad weather conditions in the region; [3] dip of the horizon due to altitude; [4] reduced atmospheric extinction.

F9 Questions for DIO readers with eclipse-predicting software: When and where is the next time a path of totality will cross the Antarctic Circle at 06:00 LST? And when was the last time?

F10 P.S. I keep seeing references to “DR” in your magazine. Does this stand for “Dennis the Renegade”? . . .

Comments by D-the-R:

F11 I enjoy the footnote-to-a-fooote note aspect of the foregoing. (Various DR research finds have started out as such.) So the following thoughts are 3rd order footnoting.

F12 The scene painted by Keith reminds me of one of my old favorite astrologer-hassling plays: ask a horoscope-caster to do a birth for the Arctic Circle at Local Sidereal Time (LST) 18° or the Antarctic Circle at LST 6°. (Any longitude will suffice.) Slight unstaed problem: in either situation, the ecliptic is coincident with the horizon, so the astrologer’s critical “Ascendant” point becomes nonexistent and thus uncalculable.

F13 Forgetting relatively dim Deneb Algedi (the Seagoats’s tail), we note that all the 14th-magnitude zodiac stars’ longitudes fall in one precise semicircle: Aldebaran, Castor, Pollux, Regulus, Spica, Antares. When the Sun is well into the opposite zodiacal semicircle, there will be at least a few minutes every night when all 6 of these famous stars are visible simultaneously: in the southern hemisphere’s temperate zones, something that was never possible for the northern hemisphere ancient astronomers who first cataloged these objects.

F14 I note that the current celestial latitude of Pollux is +6°.7 (and slowly increasing); this is well outside the Moon’s usual (5° amplitude) range of wander off the ecliptic. In ancient lunar-planetary observational work, Pollux was traditionally (though see Almagest 9.7) not regarded as being quite of the import of Aldebaran, Regulus, Spica, & Antares, the famous 4 stars which were occultable by the Moon. However, a lunar occultation of Pollux is almost possible from the deep southern part of the Earth, since the Moon’s latitude can be as great as +5°.3, and the gap of 1°.4 (between this figure and Pollux’s latitude) is nearly covered by 1°.3, the maximum possible sum of the Moon’s horizontal parallax & semidiameter. But not quite. So such an occultation is now impossible to see from the Earth’s surface. However, in ancient times, the latitude of Pollux was less. (It was +6°.4 in King Tut’s time.) So occultations of it were occasionally visible from the Antarctic regions. The only catch is that no lieke persons were there in those days to see the event. (Yahgans & penguins may have been luckier.) By the time civilized man had penetrated to the deep southern reaches of the Earth, the latitude of Pollux had so increased that the shadow (of Pollux’s light), cast by the Moon, grazed the Earth no more.

14 Tycho’s eventual 1598 distribution of the padded “thousand star” catalog D is excused by Dreyer 1980 (p.266) thusly: “The handsome manuscript volumes . . . were chiefly intended as advertisements”. Thoren 1990 p.383: “they were good enough for advertising purposes”. (See DIO 1.6 fn 266.) Tycho was embarrassingly short of money in the early months of 1598.”

15 Dreyer’s notes to cat D (OO 3.414, 416, 417) speak of the following regional star sums: 335 zodiac (correct), 480 north (vs. 481 actual), 185 south (vs. 188 actual); the stated total being 1000 stars (vs. 1004 actual).

16 Number of dust atmospheres \( D = \text{csc}(\phi + 34/65 + 54 h^2/3) \), or \( \text{csc}(\phi + 51/29 + 67 h^{-1/3}) \), for (exponential-model) dust scale-height 1.0 km (or the standard value 1.2 km, respectively. For moderately different adopted scale-heights, one may extrapolate the 3 constants.) These are DR’s original formulae for \( D \) as a function of a celestial body’s apparent angular altitude \( h \) in degrees. (For an observer well above sealevel, these formulae may be attenuated in an obvious crude exponential fashion. Both load & scale-height of atm dust are notoriously variable; thus, the formulae here are merely to be taken as rough estimates, not as rigorous representations of a fixed atmospheric model.) DR’s extinction-calculation procedure here will (unless otherwise stated) use zero atm dust, because: [a] even in modern times, dust-extinction is flaky, & [b] in ancient (pre-pollution) times, dust’s effect on clear dry winter seeing was probably small — perhaps nearly negligible on water-surrounded sites such as Rhodes & especially (near-arctic) Hven. (Objects transiting south of Hven are seen over c.100 km of water.)

16 DIO’s procedure for computing atmospheric refraction: set \( 1/c = 51/1 + h^2 \), and compute \( a = p^2 + q^2 + r^2 \), where \( p = T/1013 \) and \( t = T/283 \) (in millibars, \( T \) in K); then, using either \( h = 61/1 + h^2 - 1/94 \) or \( h = H + 2[(11 - H^2/3) - 1/121] \), atmospheric refraction \( r = 58.6 \text{arcsec}/p^2 \) (with \( h \) apparent altitude, \( H = \text{true altitude} \) — both taken literally in degrees). (For DR formulae to compute molecule atm mass \( Z \), & dust atm mass \( D \), see fn 63, fn 18, & fn 16, respectively.) These new DIO equations are easily handled by pocket calculator or computer; they more than suffice (at the professional level) for all situations, 0° to 90° altitude, at all likely Earth-based heights above or below sea-level. Given the recent findings of Schaefer & Libby (PASP 102:796; 1990), on refraction’s vagaries near the horizon: the precision of DR’s formulae here is, if anything, overdone.

16 Standard mean ozone cover, adopted here throughout. We use: \( R = \text{sealevel radius of curvature of Earth’s surface} (6378 \text{km is close enough}) \), \( z = \text{observer’s height in km (above sealevel)} \), \( k = 2 \), \( h_2 = h \), \( g = \text{hght of laminar ozone layer (22 km,} \text{r = (r + z + z^2)(R+h)/2} \), & \( X = \text{arccos}(r, \text{cos h}) \). Then, ozone atm mass \( Z \) (normalized, like \( A \), for unity at zenith), as a function of apparent altitude \( h \), is here found via the DR approximation (efficient for \( z < h/2 \)): \( Z = c\times X \). For the magnitude calculations of this paper (unless otherwise stated) using Vilnius
Tycho’s work never fully recovered, after his 1597 downfall. Not a single accurate Tycho star position was recorded subsequent to his leaving Hven. On 1598/1/2, he issued his “Thousand Star Catalog” (supposedly based entirely upon data observed before departure from Hven), which I will call “cat D” throughout this paper. Cat D’s stars are grouped into 46 constellations; the entire catalog is printed in the Tycho Opera Omnia (abbreviated henceforth here as “OO”) at vol.3 pp.344-373. Tycho died at Prague in 1601.

B Starstress

Tycho’s astronomer-biographer briefly provides 11 (Dreyer 1890 p.227) the relation between Tycho’s traumatic 1597 eviction and his “1000-star” catalog (cat D), which was first distributed in 1598: “Most of his observations for determining accurate places of fixed stars were made before the end of 1592, and the results were embodied in a catalogue of 777 stars. Henceforth here, this 777 star catalog will be called “cat C” (The complete cat C is published at OO 2:258-280.) Both cat C and cat D were published after Tycho’s death. Each catalog’s star places were precessed to epoch 12 1601.03 AD. Dreyer 1890 (p.266) notes that cat C was the edition preferred & published by Tycho’s assistant, Christen Longberg, aka Longomontanus, while the full cat D was published in 1627 by another Tycho assistant, Johannes Kepler (Dreyer 1890 pp.266, 371) — whose perseverance & genius had by then transformed Tycho’s planetary observations into the three groundbreaking Kepler Laws13 (any one of which would entitle Kepler to first rank among astronomy’s pioneers). Dreyer 1890 (pp.227-228) continues

In 1598 observations of fixed stars were resumed in order to bring the number of stars in the catalogue up to 1000, and even in the first two months of 1597, immediately before leaving Hven, some observations were taken in hot haste to make up the thousand (pro completo millenario [D5]), mostly only depending on a single measure of the declination and/or the distance from one or two known stars . . . . It must therefore be taken [with a grain of

8 Fn 35; Dreyer 1890 p.259, Thoren 1990 p.372f.
10 Heretofore, it has always been assumed (in 35) that all the cat D stars were observed at Hven. In his 1598 Astronomiae Instauratae Mechanica (dedication 1597/12/31: Dreyer 1890 p.261), Tycho said (op cit p.262 or Räder & Strömgrens 1946 p.112) that his people had observed 1000 stars, not 1004, which (assuming the former figure was intended to be taken precisely) leaves open the interpretation that the final four Cen stars were observed at Wandebbeck and written into cat D only a few days before cat D was distributed 1598/1/2 (which in fact was very probably the case: [G2]). However: [a] Tycho never stated that any star places were observed at Wandebbeck, and his Wandebbeck records (OO 13.105f) include no such data. [b] The preface (OO 3:340) to cat D says it contains 1000 stars, not the actual figure: 1004.
11 See Dreyer 1890 p.265. Ibid p.266: “Tycho sent magnificently bound copies of the star-catalogue to . . . influential men in Austria & Germany, to the King of Denmark”, etc. Thoren 1990 pp.383f speaks (contra Räder & Strömgrens 1946 p.112) of Tycho hesitating about distributing these handwritten copies and suggests Tycho didn’t print the Catalog because he had doubts about the “quality” of the post-cat C stars. Another possible explanation of apparent hesitancy is: Tycho himself knew on 1598/1/2 (or learned a little later) that cat D had been slightly fleshed out with fake stars.
12 It is generally stated (e.g., Dreyer 1890 p.266 or Moesgaard 1983 p.311) that cat D’s epoch = 1600 AD. Actually, Tycho says (OO 3:340, 344) that his epoch is for the end (not start) of the year 1600 (Julian cal). In the Hist sci literature, I have not seen any mention of this, though Dreyer & P.Ryba understood Tycho’s convention (e.g., Dreyer 1890 p.387). Operating out of a Protestant country, he was working in the Julian calendar. Thus, his 1600/6/23 is Gregorian 1601/1/10, so the Besselian epoch of cat D is 1601.03. (Besselian dating was officially discontinued 8 ago; but, for its attractive traditional simplicity of expression, I will use it anyway throughout this paper — and in any future articles it seems apt for.)
13 [Note added 1993: For a detailed and novel argument in favor of the consistency of Kepler’s physical astronomy, see A.Davis Centauras 35/97f (1992).]
trivia, while excusing Ptolemy for every kind of cheating — and Huber is “neutrally” (§H21) undisturbed by Mufa suppression of discourse. The implicit double-standard is evidently so unconscious that Huber probably won’t be aware of it — until [a] seeing it dissected in these DIO pages, or [b] looking at his responses in a mirror.

H3 It is false & mass-defamatory to suggest (as at §H13, below) that ancients approved of plagiarism more readily than moderns. I have elsewhere cited Pliny’s blunt opinion (modern in 77 AD, anyway) that plagiarism is theft. (See also Synesios: J.H.A. 1.2 fn 154.)

To: DIO
From: P. Huber, Dep’t Math, M.I.T., Cambridge, MA 02139

H4 Bill Tuman has shown me a copy of DIO[1.1] and I would appreciate getting a copy of my own.

H5 Incidentally, while I deplore the mud-slinging between Swerdlow and RR Newton, I believe RRN is wrong on several points. First, his statistical analysis of medieval eclipses is affected by a hidden selection bias bad enough that his conclusions cannot be trusted. When I tried to confront him on that issue at a meeting, I believe in the early 1970s, he shut up and refused to discuss as soon as he realized that I was a professional statistician. How should one interpret such a behavior? I then felt it would be a waste of time to read his works!

H6 Second, it seems to me that RRN approaches Ptolemy with a thoroughly post-Gauss frame of mind. Ptolemy did not have least squares! . . . I know that it was accepted practice among Babylonian observers to report observations not as they were, but as they should have been under good weather conditions, sometimes (but probably not always) with a footnote stating that the event had not actually been observed.

To: P.Huber, MIT, 2-334, Cambridge, MA 02139-4307
From: DR

H7 You may not realize that I have no opinion on R. Newton’s eclipse-discrimination, having long ago opted for a method (of determining mean Earth-spin-deceleration) which quite side-steps the need for selecting among the motley ancient eclipse reports. (See DIO 1.1 §5 n.11 & §6 n.5.) Nonetheless, I will be glad to receive your evidence that RRN’s work in this area is infected by a “hidden selection bias”. I know that our mutual friend van der Waerden also rejects RRN’s eclipse sampling, but this never prevented him from appreciating RRN’s considerable contributions to the scientific history of ancient astronomy.

H8 You speak of mud-slinging “between” Noel Seraph [Swerdlow] & RRN, implying shared responsibility. The record is sufficiently clear on the point that you may wish to reconsider this sentence’s expression. (Seraph has attacked in repulsive fashion even so gentle and esteemed a scholar as van der Waerden. See DIO 1.1 §6 n.6 and the 1973 Isis 64:239 Seraph review there discussed.)

H9 The enclosed offprint (American Journal of Physics 55:3:235; 1987) reveals a wholly new, crucial-experiment argument showing that Ptolemy already possessed the Almanjast mean motion of Mercury before he fabricated (obviously using that very mean motion!) the 1395/17 “observation” which

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1 See [C].
2 See, e.g., the list at Dreyer 1890 p.262.
3 For years, there have been speculations regarding early use of the navigational method of lunar distances for longitude-determination. Only by post-exploration comparisons of observed (home&away) times of lunar conjunction would any such method be feasible before Tycho — because of the gross inaccuracy of the celestial tables then available, e.g., those of Regiomontanus.
4 Frederick II having died in 1588 with Christian only 11½ old, Denmark was ruled by a board of regents until 1596 Summer. It didn’t take long after Christian’s official “maturity” for Tycho to detect waning interest in astronomy at the Danish court. Christian IV is now curiously well-thought-of in Denmark, even though he lost Tycho & turf — to Germany & Sweden, respectively.
5 Hellman 1970 pp.404-405. Thoren 1990 p.145 displays a romanticized image of Uraniborg (based on the contemporary woodcut reproduced at p.110): H.Hansen’s 1862 painting, now hanging at Fredericksborg Castle, Denmark (Zealand). (Perhaps the Hansen was felt to atone partially for the loss of an original portrait painting of Tycho in the 1859 fire at Fredericksborg: Dreyer 1890 p.264.) Paying for such projects as Uraniborg may have been related to Christian’s cutting the formerly generous flow of royal funds to Tycho, an act which led directly to the fake stars here revealed. The castle is named for Christian’s predecessor, Frederick II, who (fortunately for posterity) had a greater interest in astronomy. Evidently, the horoscope (partly empirical — a rarity indeed!) which Tycho had produced at prince Christens’s 1577/82 birth (Thoren 1990 p.120) did not predict the eventual schism nor Christian’s archaelical treatment of Tycho (Thoren 1990 p.380). Incidentally, throughout this paper, I have attempted to keep dates in the Gregorian calendar which is more times accurate than the Julian. However, wouldn’t we & Maffia capo G.Toomer [see DIO 1.2 fn 24] be less confused about historical dates if the world had just stuck to the Egyptian calendar Ptolemy preferred? It is no coincidence that the superior Gregorian calendar and Tycho’s superior observations occurred just before (not after) the telescope’s debut. All 3 advances were concurrent phases of an age in which original intellect, unleashed by printing & the decline of faith, was bursting forth in varied directions.
6 Though correctly regarded as a Dane, Tycho was born in Skåne, which is now part of Sweden.
7 On the buildings’ rapid post-Tycho decay and dismantling: see Dreyer 1890 pp.375f.

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‡4 Tycho 1004-Star Catalog’s Completion Was Faked

Summary

It is demonstrated that Tycho Brahe’s famous “Thousand Star Catalog” (1598) — largely a genuine, hard-wrought marvel of its creator’s perfectionism — contains 10 last-minute-added stars which are faked entirely (6 stars) or in part (4 stars). The method of these frauds is essentially Ptolemy’s time-dishonored one (which is exposed right in the Tycho catalog’s preface!): simply add a constant (for precession) onto a previous star catalog’s celestial longitudes, while leaving the latitudes unchanged.¹

A Tycho’s Vice

A1 Tycho Brahe is properly ranked as one of the handful of genuine immortals in the history of astronomy. His school is responsible for a string of epochal advances especially in the lunar motion theory, which before Tycho had suffered from rapidly varying errors of c.1°, and which he left with accuracy spectacularly improved:² by an ordmagn. The sad (if intriguing) tale that follows may alloy, but certainly cannot still, our gratitude for Tycho’s invaluable heritage — a monument to the adventurous best in humanity.²

A2 In 1597, having lost the royal patronage of willful¹ teener Christian IV, Tycho was kicked out of his longtime home-observatories.² A Tycho’s Vice

A2 In 1597, having lost the royal patronage of willful¹ teener Christian IV, Tycho was kicked out of his longtime home-observatories.² — a fate depressingly akin to that of the now-utterly-lost instruments of his great ancient star-cataloging predecessors Timocharis (c.300 BC) & Hipparchos (c.130 BC), whose (entirely speculative) portraits were hung, with Tycho’s own, in StarCastle’s warming room (Thoren 1990 p.184).
that you cease dabbling in shrinkoanalysis until you’ve mastered skills closer to home, such as arithmetic?

C18 When challenged to make good on the suspected non-uniqueness allegation (i.e., to produce your fantasized alternate solutions), you can’t cut it and are finally (11/14 — 10 months and stacks44 of computer readouts after your receipt of the paper in mid-Jan) reduced to pleading that you have not had enough time (!) and that you need my help. . . .

C19 A review of your review’s shiftness:

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

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

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

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

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

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

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

C23 OG’s suppression of this paper (which ended all OG-DR relations) was simply the dowry for a convenient political marriage, as QJRAS “Editor” D.Hughes45 and the JHA joined forces — and shut down any further (skeptical) discussion of Ptolemy’s fakery at either journal. The trifling cost: the inception of DIO. To this day, OG refuses — along with the entire Mufﬁa — to admit the slightest mishbehavior in this matter. And the miscalculations — implicit and-or explicit — of Pedersen 1974, Neugebauer 1975, Toomer 1977, & Gingerich 1981 (fn 38, §C17, or J.HA 1.2 fn 56) have never been publicly acknowledged.46 Isn’t being a power-type wonderful? Reality is so: Arrangeable. . . .

he claims13 this mean motion was computationally based upon. See item [5] (pp.236-237). (Note also item 4!) Curtis Wilson, van der Waerden,46 & others regard this as an important contribution to the Ptolemy Controversy. (Your friends in the Neugebauer clique, including its satellite O Gingerich, have not deigned to take note of it.)17 I look forward to your comments [upon this & the enclosed DIO 1.1].

To: DIO

1991/9/6

From: P. Huber, Dep’t Math, M.I.T., Cambridge, MA 02139

H10 . . . About data faking. Consider the following:

H11 At present, most statistics46 texts use fabricated or fudged data, thinly disguised as real data . . . . The usual reason given is that the focus is on methodology, not on the subject matter, and that it is much easier to teach the methodology by using “clean”, small data sets, with distracting details13 stripped away. . . . I myself do not like fudging and think one should use it only as a last resort, if one cannot find . . . real data sets to make the point without adulteration.

H12 It is standard practice today in the physical sciences not to report raw observational data.

H13 Customs can vary widely. Compare for example our current attitudes with regard to copyright and plagiarism to those prevailing among medieval authors and, more close to our days, among Singers of Tales . . . .

H14 I do not contest that Ptolemy fudged his observational data [DR’s §H9]. I suspect that the following two forces were acting: first, as above in [H11], his emphasis is on methodology: to explain his theories and so show how to derive parameters from the data. He did so very well and comprehensively . . . . If you illustrate the method of deriving parameters with a data example, you either have to fudge the result . . . or to fudge the observational data. Doing either explicitly detracts from the argument. Please note that it took Gauss and the Method of Least Squares to give a rational basis to dealing with “slightly” [pp.238-239] inconsistent observations. If Ptolemy had seen anything wrong in what he was doing, I guess he would have added slight perturbations21 to his fake data (as present-day students do in lab courses . . .).

Second, I surmise that Ptolemy reported the observations not as they were, but as he wished them to be. He would have added perturbations to hide his sins? In fact, he did precisely that. See §4 in 19 for this and a later historical example of a similar ploy.

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44 Shown to DR 1983/6/4: DIO 1.1 §1 fn 9.
45 Compact DR footnote in original letter: “No rule of law — as revealed by contrasts: [a] comparison to treatment of other papers, and [b] shifting (make-up-the-alibis-as-needed, as-we-go-along) criteria for [rejecting] this paper.”
46 Featured in J.HA 1.2 fn 18. (O had previously been a severe critic of Hughes’ “editing”: loc cit [B2].)
47 Toomer 1984 App.C admits Ptolemy’s solutions are wrong & cites RRN (never DR); but forgets the long history of Mufﬁa-supported solutions: §C15. DIO always alerts readers to & publicly corrects every one of its occasional errors. Some of the above turned out to be unhistorically erroneous. In 2003, A.Jones found better solutions for Mars & Jupiter: see DIO 11.2. This doesn’t excuse suppression of the other 3 solutions (& the rest of the 1983 paper, whose revolutionary cyclic thesis Jones establishes firmer than DR), but it vindicates Gingerich’s caution on 2.)

13 [Fn in orig letter:] Almajest 9.10; Toomer’s 1984 Almajest p.467. (Toomer n.104 says a discrepancy of ording 10’ isn’t negligible, obvious to Ptolemy’s 5” blunder for the same four-century interval!] See also the prejudice-inspired,undeniably-false Neugebauer-gang claims (regarding Almajest planetary mean motions) listed at n.30 of the enclosed AJP offprint. I suggest that you [Huber] request from O Gingerich a xerox of my 1980/4/13 letter to him (discussed in DIO 1.1 §1 n.9), which revealed for the 1st time the math behind all five planets’ Almajest mean motions. [Details of this math are here given under §3 §C3. Text of original letter: §] C6.)

16 [Fn in orig letter:] See DIO 1.1 n.37.
17 Swerdlov 1989 (JHA 20.29) makes no mention of this new argument, though its truth is obvious, and it is highly relevant to the subject of Swerdlov 1989.

18 Huber’s massive adds: “RRN has fallen prey to the old Emperor-of-China paradox (I believe it is due to the physicist Kapteyn): You are supposed to estimate the height of the emperor. Unfortunately, he lives in the Forbidden City, and the few people who see him won’t talk to you. But there are 600 million Chinese. So you simply go and ask every one of them what he or she thinks the height is. Then you average and get a fantastically accurate estimate (with a standard error of the order of 0.001 mm)!”. Reminds one of the wisdom & trustworthiness of [a] ESP research results (see pp.79-80 of D.Rawlins Skeptical Inquirer 2.1.62-83, 1977); and [b] US democracy’s elective choices.

20 This is less than relevant to Ptolemy, who piled on such distracting details as: [a] descriptions of instruments, and [b] repeated assurances that he had personally used these instruments to make the dozens of alleged outdoor observations we now know were actually faked indoors. See discussion at R.Newton 1977 pp.350ff.
21 Slightly? See R.Newton 1977 p.188.
22 To this day, OG refuses —

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but as they SHOULD HAVE BEEN: you calculate predictions, and if your admitted­imprecise observations agree within reasonable bounds, you stick with the theory. [DR’s added caps. Muffia’s most­avoided question: from whom came the theory by which Ptolemy pre­calculated what he “SHOULD” observe?! — i.e., what was the original empirical base for the theory which gave the “right” answers?!) Especially so if you do not trust your own talents as an observer! All the available evidence seems to indicate that Ptolemy was not a first rate observer, so his course of action may have been the prudent one. The same argument applies to the fixed star catalog. — As I mentioned in my previous letter, I have some antique evidence from Babylonian sources for the second part. For example, it is well known that the dates of solstices and equinoxes always agree with the known computational scheme (which was not very accurate), but they are reported just like observations. [DR: reported with Ptolemy­style discussions of the instruments allegedly used?! Hardly.]

H17 I was especially intrigued at this last piece of implicit logic, which is: no matter how strong the evidence for fraud by historical figure P, if scholar N mentions it, then scholar S is (according to Huber: §H16) no worse (than N) if S then accuses scholar N of fraud — no matter how nonexistent the evidence for the latter allegation. Another lesson in Muffia Morality. (No wonder fraud is so common in science — if those who object to it must endure Muffia­style­vendetta.) DR understandably kept his next letter brief:

To: P.Huber, MIT, 2­334, Camb., MA 02139­4307

1991/12/1

From: DR

[DR thanks yours of the 6th.]

H19 I have a question: are you sympathetic to the idea of promptly scheduling a 2­sided symposium on the Ptolemy Controversy? — to be held before an audience of professional astronomers, the panel comprising: myself & preferably (but not necessarily) another skeptic or two, plus yourself (if you like), O Gingerich (in case he’s willing), presumably the cynosurea Swerdlov and­or Toomer, backed up by as many other leading Ptolemy­apologist scholars as the Neugebauer­cult is willing to exhibit for live crossexamination before competent scientists.

H20 The obvious expectation that the proposed panel’s composition will give Ptolemy’s defenders a strong advantage (at least numerically) creates no deterrent on my side.

To: DIO

1991/10/1

From: Huber, Dep’t Math, M.I.T., Cambridge, MA 02139

H21 ... I am neutral with regard to your Ptolemy symposium. In 1974, I participated in a Velikovsky panel. ... neither side in any emotionally loaded controversy will ever bother to look at the evidence. The scientists

C16 The longstanding Muffia mean­motions farce raises a question (see also §7 at) that goes to the heart of why centrist academic publications exist: is anybody actually reading the papers published in these handsome, extremely expensive journals? (No wonder Hist.sci publications are not famous for extensive correspondence columns: DIO 1.1 [§5 fn.24] Note §4 fn 65 or J.HA 1.2 §2 [item# 2].) [Far too often, the authors aren’t checking their own work. [2] The journal “editors” aren’t reading much of anything. [3] Invisible alleged referees are letting the most obvious blunders go to press. (E.g., the J.HA’s Winter Equinox: DIO 1.2 §B4.) And [4] the readership isn’t noticing them either.]

To: Gingerich A­209, SAO, 60 Garden Str., Camb., MA 02138 1983/11/25

From: DR

C17 ... For years, and you have published (1974­1981, including in QJRAS solutions for the Alm. planet mean motions which are not only non­unique (your unsupported charge against my paper’s solutions), they are false. (Simple division. Awesome experts here ... ) Yet on a limo suspicion of non­uniqueness (your [1983] 7/23 QJRAS referee report’s crux), you are prepared to deny my right to publish,39 the first admittedly accurate solutions anyone has produced in 1800 — & you then have the cake­topping gall to suggest I’m a mental case when I remark that this screaming contrast indicates that there is something inequitable about the refereeing process. (May I ask of Muffia botched math, it has lavished dozens of its extremely handsome pages attacking the most trivial imagined shortcomings in RN­DR’s work — including special entertainment abuse­obbligato, sung for us by Noel coward Swerdlov. Contrast: if RN or DR had ever in their lives, even once, pulled a fudge­sticton stunt of the sort repeatedly exposed here as Muffia currency, the JHA would devote an entire issue to applying standard Muffia libels, sampled at DIO 1.1 [§7]. Note: the JHA Editor­s­Life and #2 Editor OG are both highly regarded at the Royal Astronomical Society, the Historical Astronomy Division of the (otherwise rather levelheaded) AmerAstronSoc, & the IAU rather diffuse Astronomical Union. Indeed, the IAU gave the JHA its written blessing at birth: JHA 1.1:3:3:1970/2. Thus, in the absence of the slightest subsequent public disclaimer, one assumes that JHA’s renowned editorial balance meets with the approval of these bodies.

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

40 DIO:J.Hystr Astron 1.1 §8 [E8]. As predicted at fn 29 there, the Royal Astronomical Society isn’t talking publicly about it, but its childish QJRAS Comet Halley disaster (exposed at op cit [C18]E1) seems not to have been picked up by 5 years of purposed reading by any of two thousand RAS Fellows, all of whom automatically receive the QJRAS. The most superflous scanning by an astronomer would immediately find the paper (written by RAS Vice President David Hughes) to be entirely miscomputed. The RAS has solved its embarrassment with Royal schiz­integrity: it refuses to acknowledge DIO’s existence. Score this one: childish­squared.

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

42 Sources: [C15, fn 38; J.HA 1.2 fn 56.]

43 Thanks to OG’s barriers, his pal Toomer published the 3 most unreadable of these 5 solutions before DR (though not before RN). Isn’t there another Tom Lehrer lesson? — on the secret of success in mathematics ...
helps suppressors of his stripe explain their behavior to colleagues, if they give it out that the object of their connings is not actually engaged in principled resistance to tyrannical censorship, but is a nut who subconsciously invites mistreatment. The implicit amorality (internal & projected) is suspiciously typical of Hist.sci archons’ general approach (§1C0) to all scholarship, past & present.

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

From: DR

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

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

C15 My final letter to OG on this matter started from one of funniest pinacles of the entire improbable history of the Ptolemy Controversy, namely, that a flock of Hist.sci’s leading imbeciles had for years been publishing false (non-fit) solutions of the very same Almagest items, the institutions & publishers whose page charges or page times often have promoted this extended pretense include: Princeton Institute, BrownU, Harvard, Springer-Verlag, Royal Astronomical Society, JHA, Centaurus, Arch Int Hist Sci. In several prominent cases (fn 38), including OG’s own Mars solution (fn 19), the reader was given to believe that careful mathematical check-matchings (more arithmetic I must emphasize) had occurred, when this was not true. The point is crucial to the Ptolemy debate since the Mufa solutions were based on Ptolemy’s purported method of computing the mean motions, which were lies in all 5 cases. (Even Toomer 1984 p.672 now agrees that Ptolemy was “not of course justified in concealing the mean motions’ sources” from his readers’). Yet Toomer conceals from his own readers that the accused DR discovered the perfect-fit solutions which Toomer grudgingly reviews on his previous page!) The modern imposition began with pseudo-checked statements that Ptolemy’s exemplifies power-insulted archonhood, that the charge of insanity merely reflects. And: 0 slanders others as “paranoid” (DIO 1.11 fn 20), while believing part of Beethoven conspired against the rest of him?

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

35 Apropos, I could mention the alleged observations reported to the Almagest items [fn above in §C3].

36 This strongly indicated that Ptolemy’s observations (allegedly the source of the mean motions) were in fact faked or fudged from those very mean motions. For a new and irrefutable proof that Ptolemy possessed the mean motion of Mercury before his alleged 139/917 “observation” (which he supposedly founded this motion upon) see Ralwins Amer J Physics 55:236 item [5]: Mufa-circle assert here at §12 [§H14 (ref to §H9).]

37 Another ancient-identical analogy (on suppression) has been remarked at DIO 1.17 §G4-§G5 & 17.

38 Pedersen 1974 p.308.10 eq.17 (Venus), p.270.eq.9.5 (Saturn), p.296-297 eqs.10a (Venus) & 10.3 (Merkury); Neugebauer 1975 pp.151-152 eq.6 (Saturn) & n.25 (Saturn), p.157.eq.2 (Venus). See DR comments here at §C15, also at DIO 2.1 §37 & at Americ J Physics 55:235 n.30 (1987). DR has exposed the leading Mufaists at precisely this sort of Ptolemaic fake-math, time after time after time after time . . . . The reader is urged to examine also the similar Mufa math-forcings exposed at DIO 1.15 fn 17 (NCSwendlow, UChicago), JHA 1.2 §J4-§J5 (Neugebauer, BrownU & Princeton Institute), DIO 1.3 fn 199 (ditto), & Americ J Physics 55:235 n.35 (Gingerich, Harvard). [Note added 1993: see also DIO 2.3 fn 18 §C12 (NC-Swendlow, UChicago).] This record presumably explains Muflono’s peculiar sympathy for the more long-term-successful prankster, Ptolemy. (Both parties’ chief thrill: forcing preconception upon uncooperative data.) For still further Mufa struggles with simple arithmetic, see JHA 1.2 fn 24 §G9. [Note: back when Muflono imagined they had mastered arithmetic, that field was favored as a presumably safe area for Mufa holy wars. E.g., Swendlow’s assault on E.Rosen, quoted at J.HYSTERICAL ASTRON 1.2 §G8: “Even addition & subtraction pose problems.”] Yet, while the prestigious JHA ($134/yr to institutions) remains silent to all examples were as bad as the Velikovsky fans. Thus, a Ptolemy symposium might be fun, but I am not at all sure whether it would achieve any useful purpose. . . . At present, Toomer is the expert on Ptolemy, and he simply must be on the panel, whether willingly or by coercion, if there is a symposium.

H22 . . . I am pretty sure that the ancient astronomers . . . must have derived their parameters by trial and error from rather inadequate sets of observations. But I am equally sure that for a self-respecting Greek scientist the only legitimate scientific paradigm was the deductive one. This adds still another facet to the Ptolemy controversy: how to publish results found by unsavory methods?

H23 I must add that I happen to be sensitized to such issues, because back in the 1960’s I had made myself a name in statistics by legitimizing the field of robust statistics in terms of the then prevalent paradigm. That is, I had been able to invent and solve a mathematically rigorous optimality problem in a murky area shunned by most academic statisticians, and I had done so in full awareness of the fact that the mathematical rigor I was injecting was nothing more than window-dressing, needed to make robust methods acceptable.

DR has several reactions to the foregoing:

H24 If we defend plagiarism by pointing to other instances, this is effectively just broadening the indictment of the academic community. Curious line of defense.

H25 Huber begins his rejoinder to DIO by attacking RN for being a bad statistician (§H5). Yet, when it transpires (§H9) that the case that Ptolemy faked & plagiarized is airtight anyway, Huber does not cede confirmation to RN or DR — but instead increasingly shifts (§H11-§H15) to contending that these are not such grave sins!

H26 Huber’s comparison ([H23]) of his 1960s behavior to Ptolemy’s is irrelevant. (Was L.Newton elevated for dressing up calculus-attained results in geometric garb? Come now.)

H27 Huber condemns a single verbal nonresponse. Hell, I’ve been dealing with 15+ of repeated nonresponse to write (e.g., 1984/4/30 to O Gingerich), even published (Rawlins Amer J Physics 1987) suggestions of the very item (face-to-face discussion at a scientific gathering) which Huber damnizes RN for allegedly evading on one solitary occasion. (Further details: fn 24.) The Mufla’s long & consistent evasion Huber “neutrally” ignores — arguing that encounters wouldn’t do any good. (Why is Huber not excusing RN’s alleged longago silent encounter by supposing that his responding to Huber wouldn’t do any good?) Hmm. Huber refused to read RN’s work after their noncommunication. So, will Mufla avoidance of debate cause Huber to cease reading Mufla output? . . .

H28 Huber’s long letter of 1991/6/6 manages (during pages of opinions, guesses, generalities, and analogies) to avoid responding to all the parts of DR’s 1991/9/23 letter which show his apologia for Ptolemy & the Mufla to be inadequate, e.g., [a] Noel C. Seraph’s vicious, unprovoked attack on gentle BvdW (brought to Huber’s attention at §H8), and [b] O.Gingerich’s laborious, persistent, devious attempts to deny credit to DR for finding the numbers behind the Almagest planet mean motions. (See fn 25; §3 §C3 & §C15; Rawlins Amer J Physics 1987/3 n.30; DIO 1.1 fn 9, DIO 1.2 fn 56. [And DIO 11,2 cover & p.30.])

H29 From Huber’s 1991/9/6 letter we learn that: [a] Nothing at all reprehensible has been done by Ptolemy (who Huber says MERELY deceived his readers and faked data)
or by the Neugebauer Muffia (whose sole possible indiscretion is Swerdlov’s regrettable-but-of-course-purely-defensive countermudslinging). [b] No, it is R.Newton who is to be damned; he is unhistorical & evasive, and it would be a waste of time to read his works.24

H30 All of this surely will seem perfectly reasonable to any gathering of competent scientists. (I can’t imagine that anyone could have a moment imagine that there are double standards operating here, or that such hypothetical bias could have anything to do with Huber’s friendship with O.Gingerich.)25 Therefore, I urge that a Poltemy symposium be held (as proposed at §H19), to ensure that R.N.’s & D.R.’s vile position be given its thoroughly deserved evidential quietus. If the Muffia continues to remain silent to this proposal, I can only quote the 1991/8/12 words of someone O.Gingerich admires (§H5: appraising an alleged RN evasion of confrontation): “How should one interpret such a behavior?”

H31 Stepping back from trees & twigs here, to discern the forest: we may, by analogy to the foregoing, envision the path down which Hist.sci ethics promise to take academe. During an exam at a large university, a hypothetical jock freshman copies answers from an adjacent honor student. The professor, insufficiently enlightened by Hist.sci morality, reports the incident. The jock-cheater responds by wisely hiring Mufa legal talent and submits, to the President of the university, a beautifully typed letter, from which we excerpt the following wisdom:

H32 . . . Our firm [Otto, Zero, Seraph, Tumor, Hooberdamm, & Cloneez] has drafted the following statement and transmitted it to the accusing professor:

Given that the said Mr.Jock had, previous to this test, never heard of a certain Kraut-4-eyes named Karl Gauss, and given that the said Mr.Jock is accused merely of copying all of Mr.Honor Student’s answers faithfully and without alteration (we know that a dishonest student would deliberately get a few answers wrong in order to cover his tracks), and given that Mr.Honor Student’s answers were (if, for the sake of argument, one admits the charge) evidently regarded by Mr.Jock as ‘theoretical constructs’ which Mr.Jock — with admirable intellectual modesty — had good cause to believe were more accurate than his own work, we allege that no dishonest intent has been proved. We therefore ask for our client’s immediate relief from imputations of unethical conduct. Until further evidence is produced than mere moralizing, dressed up with sociologically-inappropriate application of currently-fashionable postGaussian paradigms, we continue to regard Mr.Jock as the Greatest Intellectual on Campus.

24 RN allegedly made a statistical misjudgement regarding medieval eclipses, and Huber claims that RN refused to respond after receiving certain verbal information (Huber’s statistical expertise) at a meeting c.20 ago. Comments: [a] Huber raises this to me only many years later (now that RN is not able to comment). [b] Even trusting Huber’s recollection of this encounter, other interpretations (than fear of his brilliance) are fairly possible. (This is, after all, just a sample of one. Is Huber proud of damning a scholar’s entire corpus on such a basis? I hope Huber will re-read & re-think his astonishing 1991/8/12 claim.) [c] Huber dislikes RN’s nonreaction to him on a single ephemeral occasion. By contrast, the Muffia’s avoidance of DR is consistent over 357 of the written public record; and it is not subject to misinterpretation, especially since its reality has been directly attested through multiple sources (DIO 1.1 §A8). I have repeatedly urged a public discussion or debate; see, e.g., the 1978 challenge cited in DIO 1.1 (§1 fn 20) or the suggestion in Amer J Physics 1987/3 (an offprint of which was sent Huber 1991/8/23). No takers. None of this Muffia behavior (documented in detail in DIO 1.1) appears to lead to any conclusions or misgivings, while a single instance of RN’s shyness (and he was shy in person) causes Huber to reject all RN’s work — and to portray RN to Tuman and to me as a coward, which is a particularly amazing tag to attach to such a bold explorer & scholar. (In fact, RN did not avoid contrary scholars’ work but replied in extensive detail in his 1979 book.)

25 My §H5 letter previously suggested (see above, fn 15) that Huber ask O.G. a xenor of DR’s solutions for all the Almajeet mean motions, sent to OG on 1980/4/13. (Math details of letter quoted below at §J (C6).) Instead of giving credit to the discoverer, 0 and his colleague Toomer have done all in their power to deny it. (See also DIO 1.2 §D4.) If Huber is going to maintain that his friends have done nothing especially amiss, he should not be afraid to see relevant raw evidence. (Silent OG has now had years to work up an alibi for his behavior. Let’s urge him to try it out. In the open.)

20 RG 1983/7/23 referee report concluded (p.4) with a delightful slip of phraseology (emph added): “I have . . . put in an inordinate amount of work in order to convince myself that . . . DR’s conclusions are still???? largely unfounded.” Intentional or unintentional confessions of referee bias (whether conscious or not) are so unusual that this deserves preservation.

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

33 See J. Hysterical Astron 1.2 to 2. Gingerich had the brass to treat the party, who gave DR this libelous letter, as if he were the offending party in the matter & OG had done nothing amiss! The hypertensivity so perfectly
contrasts between the DR & Mufia solutions (§C3). An optimist will see the positive side here: cases as egregious as this are useful, since there are neophyte power-operators-to-be who (before matriculation) may doubt the blanket-censorship power of academic archons. E.g., fledgling censors may be downcast by the demise of the premodern legendary models (starchambers, etc.). So, when youngsters need inspiration, it takes incidents such as this to get them out of the doldrums into brighteyed anticipation for what can be accomplished by determined manipulating — especially when a juggling 0 is trying to save several of his faces from the wringers they’re caught in.

C5 Some excerpts of DR-O.Gingerich correspondence follow. We begin with the DR 1980 letter that transmitted the Almajest planet mean motion solutions to Harvard’s O.Gingerich, second-top editor of the world’s most conspicuously prestigious astronomy history journal, the JHA. (The reader may well wonder just how it came about that most of the DR solutions ended up being published, unattributed, in the 1984 Almajest App.C of OG’s friend, G.Toomer of BrownU: §C5. [Note added 1993: Toomer has now become officially attached to OG’s Harvard Hist.sci dept.’ Snug!])

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

C6 . . . Regarding the Almajest mean motions: . . . I uncovered the actual distance/time ratios on which all the Almajest planetary mean motions (synodic) are based, . . . Results [using $1^e = 1$ Egyptian yr $= 365^e$];

- Mercury, 725$^e$/230$^e$ + 62$^e$;
- Venus, 15$^e$/24$^e$ - 14$^e$.

- Mars, 39$^e$/4$^e$/$165^e$/155$^e$/14$^e$/34$^e$/40$^f$; [found invalid in 2003: fn 24]
- Jupiter, 325$^e$/335$^e$/63$^e$/01$^e$/04$^f$; [found invalid in 2003: fn 23]
- Saturn, 285$^e$/295$^e$/5$^e$/12$^e$.  

[Sidenote in origin: . . . the time-spans for Mars & Jupiter are 224630$^e$/82/135 & 129638$^e$/2/45, respectively . . .] Comparing prime-constituents of numerators proves a connection to the ratios given in Ptolemy’s Preface (Alm.9.3) for all but Mars. A similar check of Ptolemy’s distance/time figures he alleges were the observational basis of the tables’ mean motions: failure for all 5 cases. [This is the late R.Newton’s discovery.] So Neugebauer’s scenario (HAMA 1975, p.152f) is plain wrong. And Crime’s charge [RN] that the mean motions are pre-assumed is verified.

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

as “disreputable” scam (G.Toomer’s 1984 Almajest p.viii), even while selling, for considerable coin: [a] superfaker C.Ptolemy as “The Greatest Astronomer of Antiquity” (Princeton Institute’s O.Neugebauer & Harvard’s O.Gingerich, DIO 1.1/5 fn 24, 16/31 [7.2]], [b] a flock of Babylonian astrobors as the “sophisticated” (17/2 [32]) mathematical astronomers behind high pre-Ptolemy Greek astronomy, and [c] its own Mufosi as the sole reputable, trustworthy sources of wisdom on these subjects. (I recall a broker who used to brag that his highly- Reputable firm didn’t sell to clients that it had recently underwritten & aggressively promoted a stock that, within a few months of issue at $20, had plunged to ordmag $1. So a more accurate boast would have been: the firm indeed peddles penny stocks, but its good breeding forbids selling them for anything less than blue chip prices: fn 11 & JHA 1.2 fn 172.) As I was saying, Mufosi have exhibited a brilliance which DR wholly lacks.

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

A Malignitas Gloriosa

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

A2 DR will return the favor by now quoting a letter to DR (1985/5/25) from an unnamed A2 of OG’s friend, G.Toomer of BrownU: §C5. [Note added 1993: Toomer has now become officially attached to OG’s Harvard Hist.sci dept.’ Snug!]

Neugebauer’s sycophantically eulogised H.A.M.A 2

1 It should be pointed out that an exception to the near-ubiquity of starchamber refereeing in modern academe is the policy of C.Truesdell’s valuable Archive for History of Exact Sciences. This is pointed out by Truesdell during a sharp exchange at Isis 62.1: 90 (1990), where CT also claims, incredulously, that “No paper is ever rejected by the Archive.” Craig Fraser (Univ Toronto) notes (iden) that neither this statement nor the nonsecrecy of refereeing appear in the statement-to-authors published in each issue. I can add that the information that CT has unofficial ways of discouraging authors, such as calling about style and putting material through uncomprehending (but not prejudiced) editorial rewrite. (E.g., AHES will insist that “trig table” be rendered as “table of trigonometric functions”. And Truesdell cares that Johns Hopkins University be called The Johns Hopkins University. Does the AHES’s extreme concern with style have any relation to the novel contributions to the English language found in the 1991/10/29 number, in the来看看Vale’s communication article at AHES 43.2:133-144? — such creative expressions as “invisible”, “detailsment”, “appears”, “equation”, & “the many reference to”. And, at p.134 n.3, a citation to Neugebauer p.895 has been mysteriously rewritten as p.869.) In addition, CT has been known to (very politely) kick unapproved scholars out of his office. (And his Editorial Board now exhibits the same virtually-exclusive attraction to the institutionally Eminent as, say, the JHA. By contrast: at the AHES’ 1961 inception, half of those able scholars, who joined Truesdell on his then-noncentrist Editorial Board, listed street addresses. [Note added 1993: Of the 24 Editorial Boardmembers, 23 now list institutional addresses.]) However, for those who can swallow these endearing eccentricities, CT will honor the equitable and intelligent AHES rules he has set down — which is more than I can say for other journals in the Hist.sci field. But I must also regrettably add that Truesdell takes the Neugebauer Mufia amusingly overseriously — possibly because both parties share a traditional contempt for rules he has set down — which is more than I can say for other journals in the Hist.sci field. But I must also regrettably add that Truesdell takes the Neugebauer Mufia amusingly overseriously — possibly because both parties share a traditional contempt for the entire Neugebauer Mufa — and indeed the entire Neugebauer Mufa has done more good than harm. Curiously, if his Mufa’s attempted blackout of heresy is successful, the answer will be: No. This is a critical misperception (DIO 1.2 fn 73), as DR stressed at length to the DSB & Toomer 1978/5/18 pp.31-H1; H15. (Some of DR’s 1978 reasoning has now been published at p.140 n.10 of J.Bergren’s useful article at AHES 43.2:133; 1991.) Mufa stubbornness on the point is still inhibiting proper evaluation of ancient work: DIO 2.1 fn 73 item [b]. However, it would be improper & ungrateful for DR not to acknowledge his many debts to Neugebauer & Toomer. DR’s discoveries have often been triggered by their wisdom & folly, their stubborn drive & equally stubborn inflexibility. Another Mufia plus: the cross-referencing in Neugebauer 1975 is astonishingly full. This elevation further confirms the trend of AHES-Mufia merging — and further melts my former very high estimation of the AHES.

2 History of Ancient Mathematical Astronomy (usually cited by DR as Neugebauer 1975). A highly useful, encyclopedic work, marred here&there by ON’s occasionally irrational pet theories. As with the Almajest, there remains an unresolved question of whether HAMA — and indeed the entire Neugebauer Mufia — has done more good than harm. Curiously, if his Mufa’s attempted blackout of heresy is successful, the answer will be: No. This is a critical misperception (DIO 1.2 fn 73), as DR stressed at length to the DSB & Toomer 1978/5/18 pp.31-H1; H15. (Some of DR’s 1978 reasoning has now been published at p.140 n.10 of J.Bergren’s useful article at AHES 43.2:133; 1991.) Mufa stubbornness on the point is still inhibiting proper evaluation of ancient work: DIO 2.1 fn 73 item [b]. However, it would be improper & ungrateful for DR not to acknowledge his many debts to Neugebauer & Toomer. DR’s discoveries have often been triggered by their wisdom & folly, their stubborn drive & equally stubborn inflexibility. Another Mufia plus: the cross-referencing in Neugebauer 1975 is astonishingly full. I accomplish such tasks (a habit Neugebauer 1975 helped inspire) by computer, but my guess is that for HAMA, a lot of work was achieved by lapdog. While disapproving of his nongenerosity towards dissenters, I must also credit the massive labor & care that went into this feature of HAMA & his own 1984 Almajest which reflects the expenditure of much of his own time (arranging, by hand, thousands of crossreferences) in order to save that of his readers.
lap-dog) that enables him to suppress or ignore other scholars’ work that does not find favour with him — see especially the introduction to his [1984 Springer-Verlag] translation of the Almagest which positively reeks of his unpleasant egotism. With people like these at the head of the history of science establishment in the U.S.A., who in his right mind would want to have anything to do with it?

B Forked Pen Dep’t: the Mind-Behind

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

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

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

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

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

B5 I look forward to publication of your researches. Yours sincerely

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

26 Moesgaard is not blood-Muffa, though the JHA & co. have attempted to use his work to obscure DR’s. (See also DIO 1.2 fn 126: I have learned from Moesgaard’s papers. Indeed, he has contributed serious improvements to DR’s own papers. . . .) However, this able scholar’s alternate explanations for DR-solved ancient mysteries are rather cleverer than the ancients: nothing to be ashamed of! (DR also once [twice -- see fn 23 & 24]) committed the error of imposing more structure on data than actually resided there: cited DIO 1.1 fn 15. And his mistake was worse than anything Moesgaard ever did or will do.) Moesgaard is a protégé of a top committed archival detractor of RN-DR (O.Pedersen: see JHA 1.2 fn 11 & 126; & KM was among those scholars who circulated (starting 1980) continued-fraction-style solutions of the origin of Eratosthenes’ obliquity (11/83 semicircle) — shortly after DR transmitted the first continued-fraction solution (for Eratosthenes’ precision) to various archons here & abroad (1979/80 & days following). But, while KM had highly original twists to his solution & was innocently open about DR’s [partial] success in this regard. One can only stand in awe of the political savvy & persistence such an achievement has required, given the stark simplicity of the numerical

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27 In 1980 Moesgaard & Muffa had failed to include any published work (even mailing DR a pre-publication copy without being asked to). A more important reason for DR’s failure to submit to the Journal for the History of Astronomy (JHA) is his concern that the Editors reject his paper on the grounds that it is not sufficiently new. (Recall: Lord H did not even send the 3 alleged referee reports on RN’s refused paper! Similarly: see JHA 1.2 fn 6.) So His Lordship’s conveniently-conjured-up criterion represents a neat ploy that simply means meets the eye. It would seem to most of us that journals have another function than publishing new research, which is (DIO 1.2 fn 20) to permit (nay, encourage) all sides of a controversy to argue their already-published cases in detail in its pages. But the Editor-for-Life is ruling that out. In this case, anyway. (Compare to van der Waerden vs. the JHA’s D.Pingree, around the same time: JHA 11/50-58; 1980.) Of course, the EIL did, understandably, not wish to squander precious JHA pagespace upon the “a-historical” (JB3) nonsense of RN & DR — e.g., the blind paper, Rawlins 1999 (entirely new research, the very commodity EIL claims to

Venus:

Ptolemy mean motion =
[a] 0;36,59,25,13,12,28 degr/day
DR solution: 1800°/2919°40’ = 540°/875°45’ = [b] 0;36,59,25,13,12,28 degr/day

Ptolemy-Muffa solution: 29138°/149452° = 11056’/1793424’ = [c] 0;36,59,24,08,51 degr/day

Moesgaard solution: 360°-84’(1661 Mₜ) = 326592000’/529744391’ = [d] 0;36,59,52,07,12 degr/day

Mercury:

Ptolemy mean motion =
[a] 3;06,24,06,59,35,50 degr/day
DR solution: 52200°/168802°24’ = 2175°/7001°4 = [b] 3;06,24,06,59,35,50 degr/day

Ptolemy-Muffa solution: 456726’/147013’ = 109614452’/529744391’ = [c] 3;06,24,06,58,39,48 degr/day

Moesgaard solution: 360°-329’(1291 Mₜ) = 1279152000’/411739921’ = [d] 3;06,24,05,90,50 degr/day

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

Ptolemy mean motion

\[ [a] \frac{57.07,43.41,43.40}{2220} \text{ degr/day} \]

DR solution: \( 20520^0/21551^d/18 \) = 205200^0/21551^d

Ptole­Muff solution: \( 127611^d/27^h/133079^d/3/4 = 2543229^f/2661595^d \)

Moesgaard solution: \( 360^-880/11267^h/M_3 = 3421440000^f/359339557^d \)

Jupiter:

Ptolemy mean motion

\[ [a] \frac{54.09,02,46.26}{2200} \text{ degr/day} \]

DR solution: \( 23400^f/2592^d/137/225 \) = 658125^f/729214^d

Ptole­Muff solution: \( 124305^d/45^h/103732^d/2^b = 2983338^f/3305591^d \)

Moesgaard solution: \( 360^-199/2688^h/M_3 = 2014875^f/2232517^d \)

Mars:

Ptolemy mean motion

\[ [a] \frac{27.41,40.19,20.58}{2200} \text{ degr/day} \]

DR solution: \( 103684^f/224630^h/82/135 \) = 152145^f/329621^d

Ptole­Muff solution: \( 69181^d/43^f/14988^d/2/3 = 4159093^f/8992900^d \)

Moesgaard solution: \( 360^-90/2377^h/M_3 = 349920000^f/758098897^d \)

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

To: Dr R.R.Newton, Johns Hopkins U, Applied Physics Lab

1983/6/21

From: O.Gingerich, Harvard College Observatory, Smithsonian Astrophysical Observatory

B7 . . . I have just returned from a small conference in Aarhus, Denmark. Dennis Rawlins was there, and his [1983/6/4] announcement⁹ that Ptolemy had reported [DR: reported observing] the same greatest elongation of Venus for two separate dates created quite a debate. [DR: OG’s exact 1983/6/6 term was “brawl”]⁹ Dennis hoped that this would convince us all finally that Ptolemy was indeed a fraud, whereas he promptly discovered¹⁰ that while everyone thought his discovery to be fascinating, the question of Ptolemy’s moral uprightness or turpitude was ruled out of bounds for legitimate history of science, given our inadequate understanding of Ptolemy’s intentions¹⁰ in writing the [Almajest] or the expectations of his age.

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

seek assiduously for the JHA, which would have run about 7 JHA pages in the originally-accepted version (about 4 pp, in the censored version belatedly sent DR). The Editor-for-Life was justifiably anxious to leave lots of pagespace for solid, coherent work — such as J.Evans 1987 (64 pp!) and A.Jones 1991H (25 pp). Well, that explains DIO 1.1 fn 65, and at DIO 1.2 fn 64, fn 152, & fn 288. The even more amusing — but undeniably original! — scholarship of Jones 1991H is also displayed in DIO 1.2.

9 See DIO 1.2 §13; also DIO 1.1 fn 37 on continued Mufa alibis, lately persisting (less adamantly) in Swerdlov 1989. The alleged Venus observations (136/125 & 11/18) are reported at Almajest 10.182.

I notice that this letter makes no mention of another important DR discovery: that the [CanInscr] & the [Almajest] Ptolemy altered the daily lunar nodal motion by \( 1^\circ /311784 \). I had mentioned to OG (across the Aarhus conference table 1983/6/4) my having made a remarkable find, expressing a concern that, if Toomer had already found evidence that the CanInscr period-relation [still valid], see fn 21.

The details of the discovery were specifically imparted to OG on 12/1/83. Yet, there is still no visible interest in an HAD debate. (Indeed, an earlier DR suggestion of debate, to HAD chief DIO 1.2 fn 205 (containing this equation). Again: no comment. (Mufa interpretation of the equation was better than DR’s: DIO 1.2 fn 35.)

10 Of course, when it’s convenient, schlemiel on 0 takes the very opposite tack! — as in Gingerich 1981 p.44, which was “brawl”, so long to accept DR’s repeated challenges to arrange a public debate of the Ptolemy Controversy? (See §3 fn 24.) Everyone attending the Aarhus exchange knows why. The OG-founded Historical Astronomy Division of the AAS invited N.C.Swerdlow to speak at the 1992/6 HAD meeting, this after a member of the committee handling the meeting specially requested of DR (and received) 3 copies of DIO 1.1 which: [a] set forth details of technically amateurish (though professionally priced: fn 28) and ethically repellant NCS behavior (see list at DIO 1.2 fn 30). But, as yet, there is still no visible interest in an HAD debate. (Indeed, an earlier DR suggestion of debate, to HAD chief E.Krupp, was unanimously turned down by the HAD rulership.) The President of the History of Science Society (when DIO 1.1 appeared) is a member of the HAD. He hasn’t revealed any efforts to alter this situation.

11 The proud sense of redemption OG displays here is too precious: imagine boasting of being able to depend upon equally brilliant colleagues to join hands in an unfalsifiability-exercise. For the simple integral-sidereal grandfather Mars period-relation [still valid], see fn 21.
C The Doctor Is In

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

C2 As described in DIO 1.1 (11 fn 9), the QRoy Astr Soc in 1983 appointed Ptolemy-pr. man O Gingerich (whose decades of assiduous catering to the right archons has definitely rendered him: In) as sole referee of a paper by DR (who is proud to be Out, with the very same archons). This DR paper traced all 5 of Ptolemy’s hitherto-unsorted planet synodic daily mean motions (Almajest 9.3) back to simple integral period relations (all 5 listed here in fn 17). In each of the 5 cases, the DR solution explained the Ptolemy daily mean motion down to the last sexagesimal place displayed. — i.e., about one 1/50th of a degree precision for 4 planets; down to the 5th place for Jupiter’s motion (only attested to the 5th sexagesimal place: see fn 23), merely a billionth of a degree precision. Most. [All! See fn 23&24] of the degree/days ratios’ components turned out to be numbers found right in the Almajest 9.3 discussion! (Unknown until DR sent17 solutions [3 of them historically correct] to the Journal for the History of Astronomy’s #2 Editor, O Gingerich, on 1980/4/13: see letter’s text, below at [§C6].) The degrees/days components which produce precisely the longitudes of these Almajest 9.3-4 tabular mean motions are given below (for comparison) the Neugebauer-Muffia’s15 uniformly nonfitting solutions, which are simply those (falsey) stated by Ptolemy16 to have been the sources of his mean motions.

13 I well recall my own pre-DIO difficulties. But now, serial-disapointment at archival shortcomings is counterbalanced by: [i] the bliss of publication without censorship, [ii] the pleasure of my continuing good fortune in making serious & contributory discoveries (DIO 1.3 constitutes a precious cluster), and [iii] the educational playtime spent chronicling the archonts whose irresponsible antics make possible our readers’ enjoyment of the JHA.

14 The longterm grandfather period-relations (found 1982) were then unknown, but the prime factors of all 5 planets degrees/days motions were given in the 1980/04/13 letter, and those which related to the Almajest 9.3 attested numbers were (16C6) the same. The identified table (288 4+ 4), later appearing in Rawlins 1987 p.237, was also provided, though I’d not yet noticed (16C6 here) the seeming confirmation (noted in 1982) that the Mars denominator (22463082/2135) also exhibited a 4° excess, over 615 ancient tropical (Metonic) years. [This misled DR to suppose Mars’ period-relation in the geocentric bible Almajest was integral in heliocentric not synodic revs. Oddly, heliocentric turn out to be the basis after all: see Alex Jones’ (correct) inversion of DR’s miscue at DIO 11.2 fn 20.]

15 The Muffia cartil was published to DIO readers at DIO 1.1 171 [§C5-§C13 etc. It comprises, e.g., O.Toomer (BrownU), A.Aaboe (Yale), N.C.Swerdlow (Chicago), B.Goldstein (Pitts). Its satellite includes O.Gingerich (Harvard) & G.Ölafsson (Hamburg). See also JHA 1.2, e.g., fn 13, Mulfio’s attitude of unfriendly contempt (usually entailing just nonsensical) towards the ancient astronomic work of R.Newton-DR is largely peculiar to the Mulfia itself. RDR historical research has long been published in leading academic forums (see JHA 1.2 §114); it has been respectfully ( & far from critically) cited by, e.g., W.Hartert (Frankfurt), K.Moegsaa (Aarhus), B. van der Waerden (Zürich), C.Wilson (St.Johns), V.Thoren (Indiana), S.Goldstein (Va), J.Carlson (Md), etc. The discreditable process, whereby the Hist.sci community has assented to the elevation of an arrogant & intolerant cult to the status of Dominant Experts — who lend an air of current pseudounanimity to their cultish views — is less logical than sociological. I suppose one could be depressed by the spectacle of academe effectively saying: either it doesn’t mind, or it actually approves, or it’s not aware of, or it’s being corralled, or it’s being slanted towards — as well as suppressing DR’s 1983 paper presenting them, while OGC’s corespondent Toomer proceeded to publish (most 3/5 of them) first: 1946 Almajast App.C. (Luckily, RN had gotten all three of these DR results into print before then.)

C3 I provide, below, for each planet: [a] Ptolemy’s tabular synodic mean motion; [b] the DR solution;17 [c] the Ptolemy solutions long promoted by the Neugebauer-Muffia’s (§C4), helpfully tabulated18 by Toomer 1984 (now realizing at last that the Ptolemy solutions are false, Toomer 1984 p.672 says the tables’ origin is probably unknown); [d] the K.Moegsaa solution (for source & JHA’s promotion of it, see DIO 1.2 §H3), based on hypothetical ancient adoption of an unattested month M(29.31'50° = 20° = 318931/10800) I draw particular attention to the Ptolemy-Muffia Mars solution [c], since Gingerich 1981 pp.41-42 falsely insisted it matched the tabular19 value (Mars motion [a], below) even after R.Newton had specifically warned OG it didn’t check arithmetically (the late RN was the very first to point this out! and after DR had twice sent OG [a perfectly-fitting (though not historically true)] solution ([§C2 & DIO 1.2 §D4] to help20 keep him from blundering in his upcoming paper. (Gingerich 1980. 0 had sent DR a prepublication copy for comment.) OG has (publicly) stuck to this story for years. (Also key to maintaining the pretense, OG & the QJARS’s Editor David Hughes later — as noted at [§C2 & §C4 — suppressed DR’s much better-fitting [but also false!] solution: below, Mars item [b].) The comparisons for all 5 planets follow. (Every attested21 number is italicized.)

by Toomer 1984 p.672 (though naturally with no credit to DR, who first proved this truth, via the math of [§C3 here]. But Toomer treated us: one smile at 1991 OGC ([ii] 1991 Almajest, p.579, note: ‘computed or tabulated’ — but merely asks that our public be permitted to have the appropriate paternal and moral satisfaction of being thanked, for grants which are largely charity, not reward.) [c] The ancient scholars who genuinely observed, computed, and created the intellectual advances that are our scientific forebears of today’s archons. (i.e., certain modern scholars are suppressing the very research which exposes ancient suppressions of appropriate credit. It would be hard even to invent a more ironic situation.)

17 The grandfathers of the DR solutions are merely integral sidereal period relations. Saturn: 11 heliocentric revolutions = 313 synodic revs = 324 sidereal years. Jupiter: 36 heliocentric revs = 391 synodic revs = 427 sidereal yrs. Mars: 344 heliocentric revs = 303 synodic revs = 647 sidereal yrs. Venus: 803 heliocentric revs = 309 synodic revs = 494 sidereal yrs. Mercury: 901 heliocentric revs = 684 synodic revs = 217 sidereal yrs. Each but the anciently assumed (see Neugebauer 1975 pp.906, 390, 605, 466.) For samples of the precise development of Almajest 9.3-4 planet mean motions from these period relations, see Rawlins 1987 p.237 (Mars, in 27 Jupiter. [Though perfectly-fitting mathematically, both solutions are historically false. See brackets at fn 2423, resp.) All 5 planets’ developments, and their empirical sources, will be thoroughly detailed in an upcoming DIO 12.1 (2003.)

18 The five values are correctly computed in App.C of Toomer 1984 (pp.669-670). But, of the other fifteen App.C calculations of Toomer 1984, 60% are wrong in the last place displayed (DIO 1.2 fn 264) — this despite his highly expert use of “computer programs” and “modern mechanical aids” (Toomer 1984 p.vii, no assistant cited). By contrast, the ancient’s figures for such divisions, as well as reducing (16C6 here) the sidereal number is italicized.)

19 [a] Blufng this grossly deserves the extended verbatim attention it is here receiving. (See C.Wilson (St.Johns), V.Thoren (Indiana), S.Goldstein (Va), J.Carlson (Md), etc. The discreditable process, whereby the Hist.sci community has assented to the elevation of an arrogant & intolerant cult to the status of Dominant Experts — who lend an air of current pseudounanimity to their cultish views — is less logical than sociological. I suppose one could be depressed by the spectacle of academe effectively saying: either it doesn’t mind, or it actually approves, or it’s not aware of, or it’s being corralled, or it’s being slanted towards — as well as suppressing DR’s 1983 paper presenting them, while OGC’s corespondent Toomer proceeded to publish (most 3/5 of them) first: 1946 Almajast App.C. (Luckily, RN had gotten all three of these DR results into print before then.)

20 In return for DR’s effort at assistance, OG has been party to over a decade of suppression of crediting DR’s original results, as well as suppressing DR’s 1983 paper presenting them, while OGC’s corespondent Toomer proceeded to publish (most 3/5 of them) first: 1946 Almajast App.C. (Luckily, RN had gotten all three of these DR results into print before then.)

21 The unattested Mars solution’s ancestry is traced (back to the same sort of simple period relation which underlies each of the other 4 DR motions) in fn 237 of DIO. The pertinent passage is p.579 of Almajest, 55.3: 1987. The founding relation (303 synodic revs = 647 sid yrs) is the most accurate possible sub-millennial period-relation for Mars.