DIO

&

The Journal for
Hysterical Astronomy

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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 fn 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-Muffia output, on ancient astronomical history), the central contentions of which depended heavily on astounding mismath, including two crucial errors [both refereed by Muffiosi] in gradeschool arithmetic. (Full details in *Journal for Hysterical 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*.

‡1   Scrawlins

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.[1] 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-confiscated 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 overteeming poor 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 (of our Sun) falling upon Pluto[2] 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,[3] were built at the dawn not the evening of civilization. (Possible partial explanation: §C1. 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).[4] 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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[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 conniving 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 ghettoes, is not *a priori* incredible.) E.g., if a majority of the citizenry got hooked on cocaine, prohibition would be repealed (as for booze in 1933) — and mafia profits would plunge. In case the reader is imagining 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 believes it longs for truth. So, why does it systematically keep electing two-faced liars to public office?[5]

A7   Since certain enemies of my late friend Robert Newton have regularly attacked tri-fles (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 handsome *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" & 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 school (where *Isis* was edited until 1991) as the University of "Pennsylvannia".[7] This despite several resettings of this special-preen-page's type. Again,[8] we ask: how many scholars actually *read*, even superficially, the journals we cut down forests to make paper for?

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 paid[9] 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-rays[10] (wellknown to

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[5] Lest 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 *J.HA 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 work, taking the space to remark that R.Newton *Ancient Astronomical Observations* (Johns Hopkins 1970) spelled "Ibn Yunus" as "Ebn Jounis". Comments: [a] Revealingly (downright embarrassingly) 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 Iounis", not OG's alleged "Ebn Jounis". [e] This review appeared in the prominent Hist.sci journal, *Centaurus*. [f] Reviewer OG Gingerich now chairs Harvard's Hist.sci Dep't. By contrast, OG casts no aspersions on any specific person's scholarly ability, during his current review of the work of the Muffia's deputed satirist-entertainer Noel Coward Swerdlow and other Hist.sci volk, when he offers the weighty complaint (*JHA 23.2*: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 [Enry Iggins' 'tart aside, in *My Fair Lady*], on the Gallic 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 §D7. [Correction: Original edition erred on the Gallic-aside source.]

[7] *DIO*'s mailing label brought the mis-spelling to Sivin's attention (exclamatorily) in 1991.

[8] See here at ‡3 §C16 & *DIO-J.HA 1.2* §B4 (the *JHA*'s "Winter Equinox"), or the previous issue's "Royal Cometians" (*J.HA 1.1* ‡8 §E8).

[9] What's-left-of-the-left (WLL) keeps suicidally promoting this "cure" for poverty. Instead of having the next generation raised largely by loving, 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 paternalistic herd on the show. (The theory is: just pay enough welfare, and poverty will atrophy naturally. As wacky as the Reaganomics-notion 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 several 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 anything but disapproving gas & ineffectual bandaids.) Note: Scandinavia 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 N.European approach work.

[10] Breast X-rays are now aggressively marketed under the sales-euphemism, "mammograms" (*not* "breast X-rays"). The implicit medical strategy is called "risk-benefit", which translates as: OK, so we may lose a few patients to 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 fad (like tonsillectomies, a generation ago) which provide a robust steady income to medbiz? And: is or is not the cancer industry just juggling statistics, in its TV terror-tactic mammogram-hustling ads' claims that mammograms improve survival odds? The statistical details

cause cancer) — while affecting bewilderment 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,[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 nobody ask 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: good-old-reliable Congress was deputed to protect us from the CIA. Result: CIA-connected 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.)[12]

A11   Tons of tomes have been written on gov't 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 systematic method of solution. (If there were, we would have quickly eliminated all civil woes and solved all physical mysteries.)

B Doubletakes

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 slosh-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-operas dumb, and then watch 5 hours of wrestling? *DIO* 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 hucksters 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 'snews 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, & π would all be rational.[13]

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can be dressed up, but the bottom line cannot. And that bottom line is a startling, publicly-undiscussed contradiction: [a] thanks to mass mammograms, early detections of breast cancer are way 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 "modified" version. Halsted, the Johns Hopkins doctor-addict who established the radical's preeminence, got *rather* rich. It is said that he used to send his shirts to Paris to be pressed.)

[11] 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). Inept 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?

[12] [Note added 1992/11: see fn 1.]

[13] And Gozzi & Puccini would have lived to immerse themselves in *Turandot*'s 1926/4/25 La Scala opening.

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, Liszt-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 motive[14] 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 pols, citizens feel protected by their imagined ability to see through most con-men — a confidence which fails to account for sample-filtration, by naïvely assuming that pols 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 lobotomy is still legal.

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[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".

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"-rehab, gun-control, treating every Sin (but homosexuality) as illness, the holy mission of preventing overcrowded jails,[16] preserving the sanctity of AFDC, welfare, forced integration [except for the rich], 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?[17]

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 "rehab". 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 costs[18] to the noncriminal majority; [b] a smart society's first 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 tinker 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,[19] as wise observers (such as George Bernard Shaw) have long

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[16] By contrast, the overcrowding of neighborhoods (fn 22) is not discussed.

[17] One steady thread of consistency: whichever position TV 'snews adopts ("Left" or "Right"), the opposing viewpoint is systematically slighted.

[18] We tend to think of "costs" just in terms of dollars, which permits the rehab-industry forever to portray skeptics as calloused cheapskates. So let's get to the reality: yes, we might (might) be able to cure most violent criminals — *IF* half the noncriminal citizenry were converted to fulltime social workers. The question is: can a functioning society tolerate such a drain on its labor force? Obviously not, unless we wish to sink — even more rapidly than now — toward the level of a Third World nation. Question: given the tens of thousands of hours of TV 'snews time expended on coverage of the crime issue during the last generation, why are these simple social realities (just laid out here in a few lines) *never* mentioned?

[19] TV 'snews also pretended there might be a cure for AIDS, and only began to suggest otherwise much later — after the time had passed when some version of a quarantine might have been most effective. (For the record: DR has held from the outset that there'd never be a cure. But the key question should not have been will-there-be-a-cure, but rather: why risk the national health on the arrogant *assumption* that there will be? I.e., where are we headed if this assumption turns out to be wrong? Question: can you think of other national problems where similar arrogance — and unwillingness to admit the possibility of Noble-Experiment error — is ensuring permanent policy failure?) The US medical establishment designed the strategy that has ensured a more rapid spread of AIDS here than in any other comparably sophisticated nation. This establishment has been punished by reaping billions of dollars from the AIDS epidemic (fn 11). Incidentally, the main argument (of our genius-experts on AIDS) for "education" rather

suggested? Face it, rehab is statistically no more effective now than it was, 30 years of criminologist-BS ago. In fact, it is probably alot less so. Why depend on receiving the long-falsely-promised benefits of *pro*gress from a field that is actually in *re*gress?

H3 Moreover, even if we assume (purely hypothetically) that criminologists, by studying murderers instead of frying 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[20] by tampering with human individuality? . . .

H4 Obviously, prison-rehab is not the solution to crime. But the only efficacious long-range social solution (see *DIO 1.1* ‡2 §D) is not politically allowable, while the sham of allegedly rehab-educating The Botched[21] is the only discussable gradual solution (§M1). So US streetcrime cannot be seriously rolled back.

H5 Regardless of longterm social panaceas: what of the millions of career criminals *already* on the street? Even if one lacks the realism to recognize rehab as an eternally-out-of-reach mirage, it is undeniable (on the past record) that it is very far from a sure thing. So, does an intelligent nation *gamble its future stability* on the thoroughly, repeatedly self-discredited rehab-approach — or do we finally demand that another option be tested, at least in some sections of the US? What other option is available? (And why is it not seriously discussed on TV 'snews?) The short-term answer is: spend what it takes to lock up violent criminals PERMANENTLY.[22] Lock them up *with each other* (which shouldn't be so bad, if rehab is turning them all into pacifists), instead of regularly paroling or springing them back among those of us who aren't wealthy enough to live way out in relatively streetcrimeless posh suburbs — as do the genius architect-profiteers of the current US streetcrime-situation: judges, lawyers, shrinks, as well as network 'snewsanchors & owners.[23]

I Elefantasia

I1 Question: What's the most expensive movie ever made? Answer: the Reagan-Bush Presidency. US budget overruns are now a billion dollars per day. (Normal cinema's definition of Boxofficebombdom, *Heaven's Gate*, lost less than a mere $1,000,000/day.)[24]

I2 Think of it in terms of sheer achievement. Under Reagan-Bush, the Republican Party's Presidency spends money faster than *even a Democratic Congress' tax-fangs can suck it out of us*. (Again: a billion dollars PER DAY faster.) No one previously thought such a feat possible.

I3 The annual cost of servicing the Reagan-Bush debt now about equals the entire annual budget deficit — i.e., it is now, annually, costing the US as much to go broke (circularly

than some degree of quarantine has been that it's supposed to be so difficult to catch AIDS, since it usually spreads sexually. Just the sort of bright reasoning you pay Experts millions of tax dollars for. About on the order of opposing population planning by claiming it's difficult to get pregnant. (One other common AIDS-related illusion: the media repeatedly plugs celebrity AIDS-benefits as allegedly raising money for research. Actually, AIDS-lobby celeb-events are raising money largely not for medical scientists but for lawyers & agents, to influence politicians to have *you* the taxpayer fund the medical research. Whether this is wise or not can be argued. But the implicit deception is indefensible.) Finally, the "education" cure has been defended by arguing: "even if it saves just one life, it's worth it." Perhaps. But no one on TV 'snews has ever suggested the same defense for any variant or degree of quarantining.

[20] Woody Allen has the best reply (to the old who-are-you-to-play-God line): "somebody has to".

[21] Credit: my stepfather's old friend, Henry Mencken.

[22] TV 'snews will predictably rebel at the hideous indignity of Crowded Jails, which it regards as a social blight far more intolerable than nightly (ratings-booster) crowded-neighborhood street-shootings of innocent citizens.

[23] More apt yet: why not arrange that all paroled criminals will live in (or nextdoor to) the homes of the same judges, shrinks, *et ilk* that spring them?

[24] See H.&M.Medved *The Hollywood Hall of Shame* 1984 p.184 ("Passing the Megabuck") for a funny-repulsive account of the *Heaven's Gate* culprits' attempts to blame each other for their disaster. This edifying spectacle provided a pilot script for the eternal White House-Congress budget-responsibility fingerpointing nitcom.

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

I4 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)[25] US National Debt would double. And thus (§I3) the annual budget deficit would double.

I5 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).

Let's admit it: we all underestimated the Republicans.

J Historians' Inductive Logic

J1 Inducing the astronomy of the ancients from the paltry leavings we possess of their writings is a favorite DR intellectual pastime. Critical to DR's approach is the working theory that the best known and most enduring ancient science writers are not the most central or reliable figures.[26]

J2 Fortunately for posterity, my historian-opponents in the attendant ancient-astronomy controversy are much smarter[27] than I in such matters, and will set the record straight. The unerring trustworthiness of their approach is testified 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 widespead (& 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 both; beer ads showed unpudgy youngsters posing with, twirling, & fondling beer cans — and laughing healthily at the spectacle of opening them and pouring the contents, lingering over this inexpressibly beautiful experience with closeup slomo clips. But the participants in these advertisements *never* drank the stuff. As a result, they were all youthful, potless, & nonstop-smiling. Curiously, the alcohol industry paid billions to network TV for the incessant visual portrayal of this connection between healthful happiness and nondrinking.

J4 While propane tanks were widely rumored to be intended for supplying heat, we know that their actual purpose was: blowing up buildings. Virtually all press accounts mentioning propane tanks describe explosions, not heating.

J5 Over 99% of film of the sex act surviving from US civilization is what were called "X films". From viewing these (we're told . . .), one learns that the normal sex act always ended in male withdrawal from the vagina, followed by the expression of seed upon some other part of the female. Since this was clearly the standard reproductive act, we conclude that sperm fertilized the human female's body anywhere but through the vagina. Anywhere.

[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 enserfing the US public at peon, 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 §B1, ‡7 §G3, *DIO 1.1* §O1, §O4. DR's essential §J 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.Ptolemy, 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.

J6   The most Christian man in the history of the US (the most Christian of nations) must have been that exemplary person 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.[28]

J7   From cinema "entertainment" dramas, we learn that almost no ugly wrinkled people smoked — though smooth, healthy, young, attractive people smoked incessantly (with nary a cough) in popular films. Just like in the other cigarette ads.

K   Further Inductions

K1   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.[29] 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 archonal 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,[30] not to mention generous, inspirational, rigorously fair & neutral. (We know that all such characterizations were true, because — being selfconfident models of academic competence — these journals would not embarrass themselves by printing mere flattery.) [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 him — 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[31] by his fanatical refusal 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

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[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 slips. I note that, after explorer-hoaxer R.Peary 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 reputed motto of his, a printable version of which is: "Never kiss a jackboot, especially if it's trying to neck."

is lost) were quite frequently "almost correct",[32] e.g., he (almost) discovered such now-accepted positions as: [a] heliocentrists' work underlay all sophisticated ancient astronomy, [b] sph trig existed by the 2nd century BC and differential sph trig by the 2nd century AD, [c] Tycho faked ten stars of his Catalog, & [d] R.Amundsen was first to both the Earth's Poles. However, in *every* case — yet another amazing coincidence — the reasoning produced by *DIO* was found wanting,[33] and so the discovery-credit very properly[34] went elsewhere.[35] According to a controversial scholium (incompletely-erased, unfortunately), which for a time mischievously confused several naïve idealists (each happily enlightened since), *DIO* is said to have claimed that, in all such cases, those parties now receiving credit for the discovery had published material ignorant-of or (usually)[36] outright-opposing it — right up until *DIO* produced evidence which changed opinion. But the admittedly fragmentary now-extant record supports not a bit of this transparent grumbling. Discovery [a] has long since been assigned to its lifetime supporter, N.C.Swerdlow; [b] went to math genius G.M.Toomer, [c] to O Gingerich & J.Evans (who pointed out alternate spellings[37] of Hven & Wandsbeck, sloppily ignored by *DIO*), and [d] to National Geographic, which (according to the newspaper[38] of 2100 AD) had always held that Amundsen was first to the N.Pole in 1926 and that R.Peary had just innocently dreamed his 1909 claim.

L   Collective Shamnesia?

L1   The reader may suppose that §J7 is overstrong; however, it should not be 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, one 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 fiscal support.)[39] But subtlety has not

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[32] ‡3 §C15.

[33] See likewise at *J.HA 1.2* fn 58.

[34] See *Journal for Hysterical Astronomy 1.2* §H2, fn 127, & fn 148.

[35] E.g., *DIO 1.1* [‡3 §D1] mistakenly-if-understandably made Muffia singer-entertainer Noel Coward Swerdlow's middle initial "T" (presumably for "ToeDeLine": best pronounced with extreme care) instead of the correct "C" [fn 6]. Thus, due to this grievous error, the credit for discovering the final Hipparchan (UH) solar theory [*DIO 1.1* ‡6, *DIO 1.2* §G10] naturally fell in due time to the renowned orbit-authorities of the Muffia.

[36] E.g., ‡3 §C15.

[37] For a certain OG's attention to spelling convention, see fn 6.

[38] Throughout the 21st century, newspapers' numbers had varied in roughly inverse proportion to the number of people on the planet. And, by 2100 AD, it was realized that, given the commendable unity of viewpoint in newspapers, no one would lose anything of value if the world (which was now so packed that it *was* one big city) got efficient — so the whole Earth became a one-newspaper town. (TV 'snews 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 monotheism in the 4th century AD.

[39] [Note added 1993: While here on the subject of corruption in filmdom, I'll flog a pet beef of mine. When Public Television runs an archival film of the Kaiser or the Czar, I object to seeing them strutting about like Charlie Chaplin — an all-too-common desecration. Cause: films of that time were made at 16 frames/sec, instead of the 24 frames/sec which became standard once talking pictures came on; but the old ones tend to be run at the modern speed, thus 50% too fast. Remedy: television uses 30 frames/sec, so if historical films were videoed at two film frames for each TV frame, the match would be within about 10% of fidelity — about as accurate as the hand-driven cameras of that era achieved, anyway. However, instead of taking such care, most producers just sloppily convert old film footage at the standard modern 5/4 ratio (30/24), which is used to convert current 24 frame/sec films into video. The 1925 scifi film, *The Lost World* (based on a 1912 A.C.Doyle novel which DR has long believed was triggered by M.Conway's from-the-audience comments upon P.Fawcett's 1910 RoyGeogrSoc lecture, reprod at p.530 of *GeogrJ 35.5*:513) has been modernly released after conversion at a 3/2 ratio — which is better than 5/4, but still insufficient for faithful reproduction of what original audiences saw. It is obvious that the technology for accurate conversion exists, but it is instead being used for less noble ends. At least since 1987, some broadcasters have been quietly scrunching (speeding up) modern films by having them specially pre-converted at a frame ratio higher than the correct 5/4. (Noted in some media at that time; then largely forgotten outside the trade press.) The original alibi for this tampering was that

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 scifi film *When Worlds Collide* (mixing Velikovskian & Noah's-Ark Biblical themes),[40] we encounter the following immortal exchange (between 2 of the main personae, each later vying for roundfirm scientist Barbara Rush's affections), in the office of an astronomical observatory, 10[m] 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.] 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,[41] the Royal Astronomical Society of London has so far printed no correction in response to the *Journal for Hysterical Astronomy*'s recomputations of the magnificently innocent RAS-published mismath (*QJRAS* 1985 p.514) of RAS Vice President David Hughes (*J.HA* centerpiece of *DIO 1.1*: ‡8 §B-§E & Table A).

M2   As for the same RAS Vice President's alleged calendaric proof (*Journal for the History of Astronomy* 1986 p.189) of alleged British priority in seeing Comet Halley (*DIO 1.1* ‡8 §G): the *JHA*'s esteamed Editor-for-Life has indicated no interest in correcting that hilarious bit of typically unrefereed nonsense. (The *JHA* & *QJRAS* annual subscription fees are each ordmag $100.)

M3   These journals' contempt for the truth is exceeded only by their contempt for their own loyal subscribers' intelligence & independence. (The implicit whatayagonnadaboutit 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?

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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 prefers (especially in primetime) to start on the whole-hour or half-hour (like 9:30 PM). Thus, if a film is so inconsiderate as to last, say, 93[m] (which, if it started at 8 PM, would cause it to bump the next film's starting time to c.9:35 PM), Cinemax will scrunch it down to 89[m] — 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[m] or a similarly round time.) Classic films are less likely to be mistreated thus. However, even so wellknown 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-boosting publicity.) The film's special effects are occasionally quite effective, though nothing in it is as otherworldly as the gooie sanctity at the start and end — laughably out of place today.

[41] *DIO 1.1* ‡8 fn 29.

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‡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.[1]

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.

A6   Kill it before it metastasizes. [Signed] The BrownU Crew.

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 Ludlow Baldwin, introduced me to the wonders of calculus[2] & Newton's Laws, when I was only 16[y] old). But I will always treasure them privately.

C   Honesty & Belief

To: *DIO*   1991/5/21
From: Louisa Fitzgerald Huber, Harvard University

C1   . . . glad to find someone who among other things makes a case against stupid or downright damagingly 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[3] and thus keep the innocent paper free from contamination.

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[1] This was a favorite response of poet David McCord (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. Muffiosi 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 [Y_J] . . . for the [UH solar] mean motion instead of [Y_B] . . . . Also the [Meton-Hipparchos solstice-pair] already includes epoch-longitudes, so why speculate further on this as [at . . . §D7f]? . . . 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 Y_B, 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" [§B8]. But interpreted as Metonic it is clearly inferior to 365;14,48^d [Y_J] 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-nose agreement displayed at §H5 of the DR paper shows that the proposed UH mean motion indeed matches Hipparchos' final mean motion tables to 1′. [b] Though Y_B (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[4] *used* yearlength Y_J (DR eq. 7). [c] The same Babylonian tablet ACT 210 contains both Y_B and M_A (*ibid* eq. 10), and these 2 numbers are obviously not in the ratio 235/19 — though Y_J/M_A does very closely approximate 235/19 (as Moesgaard has often justly emphasized, contra R.Newton). Therefore, we know that Y_B 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* Y_B 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*   1991/10/13
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 Muffias, cliques, schools, and the like —
E3   So I stand way outside most of the issues that you are dealing with, and treat these arguments and fights as high class entertainment.

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[4] 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_B is adopted.

F   Penguins Under the Ecliptic Pole

To: *DIO*   1991/9/12
From: Keith Pickering, 10085 Country Rd. 24, Watertown, MN 55388
F1   . . . I was struck by your remark[5] 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 the lowest point of 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 sky at the end of civil twilight at the right time of year) . . . . This tilting creates what we might call a [fairly narrow] "line [locus] of zeniths" perpendicular to the Aldebaran-median-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[6] 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 here'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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[5] This question arose because *DIO 1.1* (‡ 6 fn 30) had noted that the longitudes 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) & Rhetorios (6th century).

[6] A crude check is obtained from arctan(tanε cosλ) = 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^h), 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 in the region; [3] dip of the horizon due to altitude; [4] reduced atmospheric extinction.[7]

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"?[8] . . . .

Comments by D-the-R:

F11   I enjoy the footnote-to-a-footnote 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 ploys: ask a horoscope-caster to do a birth for the Arctic Circle at Local Sidereal Time (LST) 18^h or the Antarctic Circle at LST 6^h. (Any longitude will suffice.) Slight unstated 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 Seagoat's tail), we note that all the 1stmagnitude 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 eclptic. In ancient lunar-planetary observational work, Pollux was traditionally (though see *Almajest* 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.)[9] So occultations of it were occasionally visible from the Antarctic regions. The only catch is that no literate 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.

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[7] Deneb Algedi would be dimmer than 6th magnitude for a sea-level observer under the S. ecliptic pole.

[8] See *DIO 1.1* ‡1 fn 23.

[9] See p.146 of the neatly arranged (also highly accurate & competent) tables of K.Moesgaard & L.Kristensen *Centaurus* 20.2:159. Pollux's secular increase of latitude since ancient times has been largely due to the decrease of the Earth's obliquity in the interim.

G   Footnote-Bifocalling

G1   Your kindly sending me a copy of the first issue of such an interesting journal as *DIO* & *J.HA* deserves an extended letter in reply, since you touch on many matters of interest to me; having recently invested in a pair of bi-focal spectacles to read cuneiform inscriptions, I may even be able to cope with the footnotes. . . .[10]

G2   I am currently working a little on the archival evidence for the adoption of the Metonic cycle in Babylonia — but even getting reliable data on that one point takes a lot of checking.

G3   Considering that Walker collaborates with Muffia scholars from time to time, I especially appreciate the good spirit of his letter.

H   The Looking Glass

H1   The following multiply-enlightening exchange occurred between *DIO* and MIT statistician Peter Huber. Huber's humor & high intelligence make his letters a good read, quite aside from the Ptolemy context. But the correspondence also usefully typifies the Through-the-Looking-Glass quality of Muffiosi effusions regarding Ptolemy's behavior. And it suggests an analogy to embryonic growth's rapid replication of evolution: in a few letters, we see Huber go through the same shifts & alibis which it had originally required years of Muffia ingenuity & sinuosity to refine into a bulwark-fence against admitting that an alleged scientific Immortal was dishonest. (Evaporating Myth One: Ptolemy was not a scientist at all. He was perhaps[11] a skilled mathematician. But he was, first&hindmost, an astrologer.)

H2   Huber is the sole scholar who wrote *DIO* to defend the otherwise silent Muffia (who have been publishing Huber's ancient-astronomy work, e.g., in the recent Abe Sachs memorial volume). We recall that Muffia don Otto Neugebauer calls Ptolemy "The Greatest Astronomer of Antiquity". Huber's good friend O.Gingerich on the other hand calls Ptolemy "The Greatest Astronomer of Antiquity" (‡3 fn 28). Ironically, as we will see below, Huber inadvertently — while attempting to defend both these promoters' logic — demotes Ptolemy (about correctly)[12] to the level of a mere textbook writer. Huber condemns R.Newton for

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[10] Another irresistible reaction to *DIO*: sociologist Scott Halupka (my Johns Hopkins softball-partner-in-ringerdom) says he skims everything *but* the footnotes. He finds the academic infighting the best part of the show. Suspecting that other *DIO 1.1* readers also got overmuch into our footnotes (much as seasoned *MAD* magazine readers get addicted to panel-backgrounds), DR will attempt to move some of the same sort of weird stuff into the main text in this & future issues. Besides integrity, fun, & novel discovery, I intend that the only predictable aspect of *DIO* will be: unpredictability. E.g., given the long timegap between past *DIO* issues, Muffiosi seem to have hoped that no response is necessary since *DIO* & *J.HA* have perhaps died. Several typically-subtle high-level Hist.sci probes have revealed alot more Hist.sci interest in this question than in citing the discoveries revealed in *DIO*. Standard. Pathetically standard.

[11] One interpretation of the Ptolemy scandal might be: the *Almajest* is what happened when mathematical ability overwhelmed scientific ability. (For a different slant on this question, see P.Huber's thoughts at §H22.) By this statement, I am not at all subscribing to the amusingly ubiquitous modern myth (e.g., Neugebauer 1975 pp.108, 367, 643, 667, 938) that mainstream Greek scientists were non-empirical. (See *DIO 1.1* ‡1 fn 24 & *DIO 1.2* §E3 and the highly recommended analysis of R.Newton *Crime of Claudius Ptolemy* Johns Hopkins Univ 1977 pp.350-354.) There are some gorgeous mathematics in the *Almajest*. But, since Ptolemy is a multiply-convicted plagiarist, it is hard to know how responsible he is for this. Similarly: much of the astronomy of the *Almajest* is of a higher level than what remains of, say, Hipparchos' work. But the same caution is recommended: we have no firm evidence that the *Almajest*'s astronomy is Ptolemy's personal contribution — while there is plenty of indication that large parts of it are due to others.

[12] The long-standard edition of the *Almajest*, that of K. Manitius, calls it a mere *Handbuch*. (Manitius' is still the preferable edition of the *Almajest* in some respects, e.g., sound judgement.) See DR *Queen's Quarterly* 1984 p.973.

trivia, while excusing Ptolemy for every kind of cheating — and Huber is "neutrally" (§H21) undisturbed by Muffia 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[13] quoted Pliny's blunt opinion (modern in 77 AD, anyway) that plagiarism is theft. (See also Synesios: *J.HA 1.2* fn 154.)

To: *DIO*   1991/8/12
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, Cambr, MA 02139-4307   1991/8/23
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[14] showing that Ptolemy already possessed the *Almajest* mean motion of Mercury *before* he fabricated (obviously using that very mean motion!) the 139/5/17 "observation" which

he claims[15] this mean motion was computationally based upon. See item [5] (pp.236-237). (Note also item 4!) Curtis Wilson, van der Waerden,[16] & 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 statistics[18] 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 details[19] 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*[20] [emph added] inconsistent observations. If Ptolemy had seen anything wrong in what he was doing, I guess he would have added slight perturbations[21] to his fake data (as present-day students do in lab courses. . .). Second, I surmise that Ptolemy reported the observations not as they were,

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[13] E.g., Rawlins 1982C and *DIO 1.1* ‡1 §B1; Huber had just written to thank *DIO* for receiving the latter.
[14] [Fn in orig letter:] Previous accusations that Ptolemy had computed his "observations" from his tables had to be defended in statistical fashion. But the *AJP* argument's unexpected confirmation of said charges: [a] is entirely independent of prior skeptical findings, and [b] is not statistical.

[15] [Fn in orig letter:] *Almajest* 9.10; Toomer's *1984 Almajest* p.467. (Toomer n.104 says a discrepancy of ordmag 10′ isn't negligible, oblivious 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 given here at ‡3 §C3. Text of original letter: ‡3 §C6.]
[16] [Fn in orig letter:] See *DIO 1.1* ‡6 n.37.
[17] Swerdlow 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 Swerdlow 1989.
[18] Huber's missive 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.
[19] 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.350f.
[20] Slightly?! See R.Newton 1977 p.188.
[21] Ptolemy *would have* added perturbations to hide his sins? In fact, he *did* precisely that. See ‡4 fn 19 for this and a later historical example of a similar ploy.

but as they SHOULD HAVE BEEN: you calculate predictions, and if your admittedly imprecise observations agree within reasonable bounds, you stick with the theory. [DR's added caps. Muffia's most-avoided question: from whom came the theory by which Ptolemy pre-calculated 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 Ptolemy-style discussions of the instruments allegedly used?! Hardly.]

H15   I think RRN misjudges the cultural context. His claims of fraud presuppose malicious intent, and I fail to see any basis for that. [DR: Note that in the previous paragraph, Huber himself refers to Ptolemy's too-faithfully-computed "fake data". So "fake" data don't imply malicious intent, while "fraudulent" data do?? And, no less an authority than the Editor-for-Life of the *Journal for the History of Astronomy* says that a scholar who uses such terms as "fabrication" places "a barrier" between himself and "the writing of history" (‡3 §B2). What a show . . . .]

H16   . . . **Mud slinging**. I stand corrected: RRN slung at Ptolemy, and NS [Swerdlow] slung back at RRN. . . .

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 Muffia-style-vengeance.) DR understandably kept his next letter brief:

To: P.Huber, MIT, 2-334, Cambr, MA 02139-4307   1991/9/23
From: DR

H18   Thanks for yours of the 6th.

H19   I have a question: are you sympathetic to the idea of promptly scheduling a 2-sided 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 cynosurae Swerdlow and-or Toomer, backed up by as many other leading Ptolemy-apologist scholars as the Neugebauer cult 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: P. 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

were as bad as the Velikovsky fans.[22] 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 paradigms. 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 I.Newton a fraud 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^y of repeated nonresponse to written (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 damns RN for allegedly evading on one solitary occasion. (Further details: fn 24.) The Muffia'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?) Hmmm. Huber refused to read RN's work after their noncommunication. So, will Muffia avoidance of debate cause Huber to cease reading Muffia output? . . .

H28   Huber's long letter of 1991/9/6 manages (during pages of opinions, guesses, generalities, and analogies) to avoid[23] responding to all the parts of DR's 1991/8/23 letter which show his apologia for Ptolemy & the Muffia 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 *Almajest* planet mean motions. (See fn 25; ‡3 §C3 & §C15; Rawlins *Amer J Physics* 1987/3 n.30; *DIO 1.1* ‡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)

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[22] DR asks 2 questions: [a] Where has DR failed to listen to the other side? (Muffia works are cited almost nonstop in relevant *DIO* output — admiringly when appropriate. Indeed, DR has been converted by occasional able Muffia findings: see, e.g., *DIO 1.1* ‡6 fn 35. Meanwhile the Muffia refuses to cite *DIO* at all. Why is Huber equating these 2 behavior patterns?) [b] Is Huber seriously claiming that scientists are as impervious to evidence as the Velikovskians? Such a judgement is more extreme than anything DR has ever said or written.

[23] Huber's letter is responsive & specific regarding the attacks on RN's use of eclipses. (I am happy to note that Huber's adopted Earth spin-acceleration is almost identical to that independently deduced at *DIO 1.1* ‡fn 5.)

or by the Neugebauer Muffia (whose sole possible indiscretion is Swerdlow'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 will surely seem perfectly reasonable to any gathering of competent scientists. (I can't imagine that anyone could for 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 Ptolemy symposium be held (as proposed at §H19), to ensure that RN's (& DR'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 Huber 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 Muffia 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, Hooberdamn, & 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.

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[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[y] 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 faintly 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 15[y] 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* ‡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 Huber 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 1991/8/23 letter vainly suggested (see above, fn 15) that Huber ask OG for a xerox of DR's solutions for all the *Almajest* mean motions, sent to OG on 1980/4/13. (Math details of letter quoted below at ‡3 §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.)

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‡3 Referees Refereed

A Malignitas Gloriosa

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

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

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

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

[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 Muffia — has done more good than harm. Curiously, if his Muffia's attempted blackout of heresy is successful, the answer will be: No.

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

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

B   Forked Pen Dep't: the Mind-Behind

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

To: Dr R.R. Newton, Appl Physics Lab, The Johns Hopkins Univ   1980/3/6
From: M.A. Hoskin
B2   . . . It has taken far too long to come to a decision about your paper . . . . The [3 alleged referee] reports [DR: *NOT* enclosed] gave me no clear advice. . . . it is of the essence of history of science that one attempts to understand *why* [emph in orig] people acted as they did, and by using terms such as 'crime'[4] and 'fabrication'[5] you put a barrier between yourself and the writing of history. The great question is, what was Ptolemy's *intention*? [Emph in orig.] To act as counsel for the prosecution is not to write history as understood in this journal, even if the facts you submit as part of the case for the prosecution are established by you. . . .
B3   . . . the problem for me has been whether the ingenuity and penetration more than compensated for the a-historical[6] 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[7] of this journal.
B5   I look forward to publication of your researches. Yours sincerely[8] . . . .

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[4] See below at §C10.
[5] Compare to ‡2 §H15.
[6] For a glimpse of what the *JHA*'s Editors regard as worthwhile historical analysis, see *J.HA 1.2* fn 36.
[7] When DR 1st submitted a paper to *JHA*, response took 11 months. (See also *DIO 1.1* ‡6 fn 15.) What a card.
[8] Placing the word "sincerely", anywhere near Lord H's fervent wish for more RN researches, requires a special kind of nerve, which evolution has reserved especially for journal-editors. But, aside from questions of frankness, there are here a couple of Catch 22s here that are easily missed:
[a] All relevant Hist.sci journals have printed papers that allegedly show Ptolemy was honest. But if skeptical papers say he was *dis*honest, they are rejected as unhistorical. (More of Hist.sci's kaleidoscopic Bureau of Double Standards: *J.HA 1.2* fn 104.) What a neat way to hold a public debate on Ptolemy's integrity: tape one side's mouth shut.
[b] If the *JHA* refuses (§B4) to print anything except new research, then, in order to publish in the *JHA*, RN would have been obliged originally to send his novel results (not to the relative security of his own univerity's press, at Johns Hopkins, but rather) to an obviously unfriendly journal, whose editorship contains several of the leading enemies of his viewpoint — this while knowing that his discoveries would be privately circulated, unprotected, for perhaps a year of purported refereeing. (Recall: Lord H did not even send the 3 alleged referee reports on RN's refused paper! Similarly: see *J.HA 1.2* fn 6.) So His Lordship's conveniently-conjured-up criterion represents a neater ploy than immediately meets the eye. It would seem to most of us that journals have another function than publishing new research, which is (*DIO 1.2* fn 20) to permit (nay, encourage) all sides of a controversy to argue their already-published cases in detail in its pages. But the Editor-for-Life is ruling that out. In *this* case, anyway. (Compare to van der Waerden vs. the *JHA*'s D.Pingree, around the same time: *JHA 11*:50-58; 1980.) Of course, the EfL did, understandably, not wish to squander precious *JHA* pagespace upon the "a-historical" (§B3) nonsense of RN & DR — e.g., the banned paper, Rawlins 1999 (entirely new research, the very commodity EfL claims to

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

To: Dr R.R.Newton, Johns Hopkins U, Applied Physics Lab   1983/6/21
From: O.Gingerich, Harvard College Observatory, Smithsonian Astrophysical Observatory
B7   . . . I have just returned from a small conference in Aarhus, Denmark. Dennis Rawlins was there, and his [1983/6/4] announcement[9] 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".][10] Dennis hoped that this would convince us all finally that Ptolemy was indeed a fraud, whereas he promptly discovered[11] that while everyone thought his discovery to be fascinating, the question of Ptolemy's moral uprightness or turpitude was ruled out of bounds for legitimate history of science, given our inadequate understanding of Ptolemy's *intentions*[12] in writing the [*Almajest*] or the expectations of his age.

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

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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 *JHA* pagespace for solid, coherent work — such as J.Evans 1987 (64 pp!) and A.Jones 1991H (25 pp). Well, that explains everything. (In fact, little of Evans 1987 produces new research results: it is almost entirely — though, see ‡4 fn 18 — a fruitless, ineffectual, & not-overly-capable attempt to tear down the original work of RN & DR; i.e., just the sort of controversial material the EfL was banning from *JHA* in the letter quoted above. Some of the lowdims of Evans 1987 are highlighted here at ‡4 §G1 & fn 65, and at *DIO 1.1* ‡6 fn 15, *DIO 1.2* fn 144, fn 152, & fn 288. The even more amusing — but undeniably original! — scholarship of Jones 1991H is also displayed in *DIO 1.2*.)

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

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

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

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

C The Doctor Is In

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

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

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

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

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

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

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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 (§C4), helpfully tabulated[18] by Toomer 1984 (now realizing at last that the Ptolemy solutions are false, Toomer 1984 p.672 says the tables' origin is probably unknown); [d] the K.Moesgaard solution (for source & *JHA*'s promotion of it, see *DIO 1.2* §H3), based on hypothetical ancient adoption of an unattested month M_s ($29^d31'50''20''' = 318931^d/10800$). I draw particular attention to the Ptolemy-Muffia Mars solution [c], since Gingerich 1981 pp.41-42 falsely insisted it matched the tabular[19] value (Mars motion [a], below) even *after* R.Newton had specifically warned OG it didn't check arithmetically (the late RN was the very first to point this out for the planets), and after DR had twice sent OG [a perfectly-fitting (though not historically true)] solution (§C2 & *DIO 1.2* §D4) to help[20] keep him from blundering in his upcoming paper. (Gingerich 1980. 0 had sent DR a prepublication copy for comment.) OG has (publicly) stuck to this story for years. (Also key to maintaining the pretense, OG & the *QJRAS*'s Editor David Hughes later — as noted at §C2 & §C4 — suppressed DR's much better-fitting [but also false!] solution: below, Mars item [b].) The comparisons for all 5 planets follow. (Every attested[21] number is italicized.)

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

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

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

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

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

[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 planets' motions) on p.237 of D.Rawlins *Amer J Physics 55*:235; 1987. The founding relation (303 synodic revs = 647 sid yrs) is the most accurate possible sub-millennial period-relation for Mars.

Saturn:
  Ptolemy mean motion =
  [a] *0;57,07,43,41,43,40* degr/day[22]
  DR solution: *20520°/21551ᵈ18′* = 205200°/215513ᵈ =
  [b] 0;57,07,43,41,43,40 degr/day
  Ptol-Muff solution: *126711° 27′/(133079ᵈ3/4)* = 2534229°/2661595ᵈ =
  [c] 0;57,07,43,41,44,18 degr/day
  Moesg solution: 360°·880/(11267M_s) = 3421440000°/3593395577ᵈ =
  [d] 0;57,07,43,42,10,05 degr/day

Jupiter:[23]
  Ptolemy mean motion =
  [a] *0;54,09,02,46,26* degr/day
  DR solution: *23400°/(25927ᵈ137/225)* = 658125°/729214ᵈ =
  [b] 0;54,09,02,46,26 degr/day
  Ptol-Muff solution: *124305° 45′/137732ᵈ23ʰ* = 2983338°/3305591ᵈ =
  [c] 0;54,09,02,45,09 degr/day
  Moesgaard solution: 360°·199/(2688M_s) = 2014875°/2232517ᵈ =
  [d] 0;54,09,02,44,55 degr/day

Mars:[24]
  Ptolemy mean motion =
  [a] *0;27,41,40,19,20,58* degr/day
  DR solution: 103684°/(224630ᵈ82/135) = 152145°/329621ᵈ =
  [b] 0;27,41,40,19,20,58 degr/day
  Ptol-Muff solution: *69181° 43′/(149881ᵈ2/3)* = 4150903°/8992900ᵈ =
  [c] 0;27,41,40,19,28,07 degr/day
  Moesgaard solution: 360°·90/(2377M_s) = 349920000°/758098987ᵈ =
  [d] 0;27,41,40,19,51,55 degr/day

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

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

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

Venus:[25]
  Ptolemy mean motion =
  [a] *0;36,59,25,53,11,28* degr/day
  DR solution: *1800°/2919ᵈ40′* = 5400°/8759ᵈ =
  [b] 0;36,59,25,53,11,28 degr/day
  Ptol-Muff solution: *92138° 25′/149452ᵈ* = 1105661°/1793424 ᵈ=
  [c] 0;36,59,25,49,08,51 degr/day
  Moesgaard solution: 360°·84/(1661M_s) = 326592000°/529744391ᵈ =
  [d] 0;36,59,25,52,07,12 degr/day

Mercury:
  Ptolemy mean motion =
  [a] *3;06,24,06,59,35,50* degr/day
  DR solution: *52200°/16802ᵈ24′* = 21750°/7001ᵈ =
  [b] 3;06,24,06,59,35,50 degr/day
  PtMff sltn: *456726° 53′/(147013ᵈ13ʰ1/2)* = 109614452°/35283255ᵈ =
  [c] 3;06,24,06,58,39,48 degr/day
  Moesg solution: 360°·329/(1291M_s) = 1279152000°/411739921ᵈ =
  [d] 3;06,24,06,59,00,50 degr/day

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

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

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

[27] If "hate" seems too strong an expression, consult *DIO 1.1* ‡1 §C7 & ‡3 §D.

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

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

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

> To: O.Gingerich, 100 Avon Hill Str., Cambr., MA 02140   1980/4/13
> From: DR
> **C6**   . . . Regarding the *Almajest* mean motions: . . . I uncovered the actual distance/time ratios on which all the *Almajest* planetary mean motions (synodic) are based. . . . Results [using 1^E = 1 Egyptian yr = 365^d]:
> Mercury, $725^r/(230^E + 62^d)$;
> Venus, $15^r/(24^E − 1^d)$;
> Mars,[29] $(288^r + 4°)/(615^E 155^d 14^h 34^m 40^s)$; [found invalid in 2003: fn 24]
> Jupiter, $325^r/(355^E 63^d 01^h 04^m)$; [found invalid in 2003: fn 23]
> Saturn, $285^r/(295^E 81^d 12^h)$.
> [Sidenote in orig: . . . the time-spans for Mars & Jupiter are $224630^d 82/135$ & $129638^d 2/45$, 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. . . .

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

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

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

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

> To: DR   1983/8/26
> From: O.Gingerich, Harvard College Obs, Smithsonian Astrophys Obs
> **C9**   . . . a [PBS] lecture . . . on Beethoven . . . . gave a perceptive analysis of why [he] never managed to marry despite being passionately in love over and over again. Some quirk of his character caused him always to choose women who would be unattainable, and the analyst concluded that this was a deliberate subconscious maneuver by Beethoven rather than the repeated cruelty of fate. I am beginning to suppose that there must be an analogy in your own preference to play the martyr rather than getting published.[31]

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

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

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

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

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

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

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

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

> To: O.Gingerich A-209, SAO, 60 Garden Str., Cambr., MA 02138 1983/8/31
> From: DR
> **C13** . . . classic: abuse a dissenting scholar, increase the dosage if he doesn't agree to take it quietly; and then, when he reacts, accuse *him* of abuse — and of wanting abuse when he knowingly chooses the honorable route of not agreeing to go along with this political filth.
> **C14** . . . [yours is] the fiercest refereeing assault on a paper I've ever heard of (revealing in itself) . . . . Your 1981 *QJRAS* [paper (Gingerich 1981)] . . . and 1983/7/23 [rejecting referee-]report's explanation ([Ptolemy's] observations)[35] fails to reproduce these motions for all 5 planets . . . . So your explanation ([based upon] false mathematics: simple division[!]) is published in *QJRAS* [Gingerich 1981], and [now] you of all people recommend that my correct mathematics not be published [*in the same journal*]. That's fair.

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

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

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

[35] Arc/time ratios, based on alleged observations reported in the *Almajest*: items [c], 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 ironclad proof that Ptolemy possessed the mean motion of Mercury before his alleged 139/5/17 "observation" (which he supposedly founded this motion upon!) see Rawlins *Amer J Physics* 55:236 item [5]. Muffia-circle assent here at ‡2 §H14 (ref to §H9).

[37] Another ancient-modern analogy (on suppression) has been remarked at *DIO 1.1* ‡7 §G4-§G5 & fn 17.

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

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

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

> To: O.Gingerich A-209, SAO, 60 Garden Str., Cambr., MA 02138 1983/11/25
> From: DR
> **C17** . . . For years, you and yours have published (1974-1981,[42] including in *QJRAS*) solutions for the *Alm.* planet mean motions which are not only non-unique (your unsupported charge against my paper's solutions), they are false. (Simple division. Awesome experts here. . . .) Yet on a flimsy *suspicion* of non-uniqueness (your [1983/] 7/23 *QJRAS* referee report's crux), you are prepared to deny my right to publish[43] the first admittedly accurate solutions anyone has produced in 1800^y — & you then have the cake-topping gall to suggest I'm a mental case when I remark that this screaming contrast indicates that there is something inequitable about the refereeing process. (May I ask

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

[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 mean-motions pretense; thus, his negligence has never been owned up to, at least publicly.) So, having published such a criticism of DR, will our brave Hist.sci journals now print a criticism of OG for the very same lapse? . . . (Such criticism is hereby submitted to them, as one may see from our *DIO* publisher's statement.)

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

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

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

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

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

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

C19   A review of your review's shiftiness:

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

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

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

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

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

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

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

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

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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 1.1* ‡8. (0 had previously been a severe critic of Hughes' "editing": *loc cit* §B2.)

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

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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[2] — 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:[3] by an ordmag. 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.

A2   In 1597, having lost the royal patronage of willful[4] teener Christian IV, Tycho was kicked out of his longtime home-observatories,[5] "SkyCastle" (1580) & nextdoor "StarCastle" (c.1584), on his isle of Hven. (This island is in the channel between modern Sweden[6] & Denmark. Hven may be seen in the distance from our family's north Copenhagen home.) Hardly a stone of his observatories remains visible today on Hven[7] — 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).

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[1] See §C1.

[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 — this 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 11[y] 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 Christian's 1577/4/2 birth (Thoren 1990 p.120) did not predict the eventual schism nor Christian's archonal treatment of Tycho (Thoren 1990 p.380). Incidentally, throughout this paper, I have attempted to keep dates in the Julian calendar used by Denmark until 1700/2/19(J)=3/1(G) — an ideal time to switch calendars. (I was about to say "upgrade", since the Gregorian calendar is many times more accurate than the Julian. However, wouldn't we & Muffia capo G.Toomer [see *DIO 1.2* fn 24] be alot 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.

A3   Tycho's work never fully recovered, after his 1597 downfall. Not a single accurate Tycho star position was recorded subsequent[8] to his leaving Hven. On 1598/1/2, he issued[9] his "Thousand Star Catalog" (supposedly[10] 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 henceforce here as "OO") at vol.3 pp.344-373. Tycho died at Prague in 1601.

B   Starstress

B1   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 Laws[13] (any *one* of which would entitle Kepler to first rank among astronomy's pioneers). Dreyer 1890 (pp.227-228) continues

> In 1595 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 Hveen, some observations were taken in hot haste to make up the thousand (*pro complendo 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

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[8] Fn 35; Dreyer 1890 p.259, Thoren 1990 p.372f.

[9] Dreyer 1890 p.266, Thoren 1990 pp.383f.

[10] Heretofore, it has always been assumed (fn 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 Ræder & Strömgrens 1946 p.112) that his people had observed 1000 stars, not 1004, which (assuming the former figure is intended to be taken precisely) leaves open the interpretation that the final four Cen stars were observed at Wandsbeck 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 Wandsbeck, and his Wandsbeck 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 Ræder & 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.Rybka 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 1600/12/31 was Gregorian 1601/1/10, so the Besselian epoch of cat D is 1601.03. (Besselian dating was officially discontinued 8^y 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 *Centaurus 35*:97f (1992).]

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salt] when Tycho already in January 1595 wrote[14] to Rothmann that he had now finished "about a thousand stars," and when he writes in his *Mechanica* that the great globe was quite finished in 1595, exhibiting a thousand stars. It has been suggested that it was this completion of Tycho's star-catalogue which he wished to commemorate by the striking of a medal (or rather two, slightly different) bearing the year 1595. This is quite possible, and he may have wished . . . to have a memorial of the work carried on for nearly twenty years at Hveen.

B2   It is strange that, in the near 400 years since Tycho's publication of his catalog (well known to every astronomer), no one but F.Baily has attached a catalog number to each star (as we do here) — indeed, it is possible that no one has even just checked the total number of stars in the catalog. Tycho called it a 1000 star catalog in its preface (OO 3:340) and standard sources since agree: Dreyer 1890 pp.227, 259-266; *DSB 2*:471 (1970); Evans 1987 pp.167-9, Moesgaard 1989 p.313, Thoren 1990 p.296 n.130 . But the correct number[15] of stars in cat D is actually 1004. [Note added 1993&4: There are 14 star positions that are merely repeats of other stars already cataloged, 10 stars are listed twice and 2 more stars — 13θ Cyg & 57μ Oph — are listed thrice in cat D. Two more entries — D566&567 — are nonexistent-hybrid bungles. And there are 11 faked objects, 10 of them treated here. So the total number of genuine & distinct attempted objects is 977. Discounting 12 more places based on hybrid data — measures from two different stars, mis-taken as same — we have the final total of real single stars cat D recorded: 965. Details in *DIO 3*, 1993, §K4.]

B3   Cat D's four-star excess (over the stated 1000) suggests that Tycho tacked the southern constellation Centaurus (Cen) onto cat D at the very last minute before 1598/1/2 publication. (He lists only 4 stars in Cen. And they are the very last 4 stars in cat D.) When we also comprehend these 4 stars' *extreme* faintness (as seen from Hven, only c.50 m above sealevel), we have cause for caution. Even assuming null atmospheric dust[16] & humidity, the post-extinction visual magnitudes[17] μ from 1597.23 Hven would be (for the usual 3 mm of ozone at 22 km,[18] with mean Hven Jan-Feb sealevel $P = 1013$ mb, $T = 271°$K): $\mu = 6.02$

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[14] Tycho's eventual 1598 distribution of the padded "thousand star" catalog D is excused by Dreyer 1890 (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 1.2* fn 266.) The dominant factor here (*Ibid* p.388): "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 $D = \csc[h + 34/(65 + 54h^{1.1})]$ or $\csc[h + 51/(89 + 67h^{1.1})]$, 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 fluky, & [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 Rhodos & especially (near-arctic) Hven. (Objects transiting south of Hven are seen over c.100 nmi of water.)

[17] *DIO*'s procedure for computing atmospheric refraction: set $1/c = 5[1 + h^{1.5}/2]$, and compute $a = p^{c/3}/t^{7c/2}$, where $p = P/1013$ & $t = T/283$ (P in millibars, T in °K); then, using either $h' = h + 61/9[1/(h+4) - 1/94]$ or $h' = H + 2[1/(1 + H/4.5) - 1/21]$, atmospheric refraction $r = 58''.6a(p/t)$ cot h' (with h = apparent altitude, H = true altitude — both taken literally in degrees). (For DR formulas to compute molecular atm mass A, ozone 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 & Liller (*PASP 102*:796; 1990), on refraction's vagaries near the horizon: the precision of DR's formulas here is, if anything, overdone.

[18] Standard mean ozone cover, adopted here throughout. We use: R = sealevel radius of curvature of Earth's surface (6378 km is close enough), z = observer's height in km (above sealevel), $\kappa = 2$ km, h_Z = hgt of laminar ozone layer (22 km), $r_c = (R + z + \kappa)/(R + h_Z)$, & $X = \arccos[r_c \cos h]$. 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 $z << h_Z$): $Z = \csc X$. For the magnitude calculations of this paper (unless otherwise stated): using Vilnius

Table 1: Tycho's Fake Ophiuchus Stars

Oph	PK# D#	λ_H+24°	β_H	λ_C+28°	β_C	λ_T	β_T	λ	β
36A	247 675	254°20′	2°15′	254°20′	2°15′	254°23′	2°12′	254°29′	-3°22′
40ξ	246 676	255°00′	2°15′	255°00′	-2°15′	255°01′	2°16′	255°19′	2°06′
42θ	248 677	255°40′	1°30′	255°40′	-1°30′	255°42′	1°32′	255°50′	-1°47′
44b	249 678	256°20′	0°20′	256°20′	-0°20′	256°23′	0°20′	256°46′	-0°54′
51c	250 679	257°10′	0′30′	257°10′	-1°45′	257°12′	0°29′	257°54′	-0°38′
39o	251 680	258°30′	1°00′	257°30′	-1°00′	257°36′	0°58′	254°53′	-1°08′

(1i Cen), 6.48 (2g Cen), 6.13 (3k Cen), 6.23 (4h Cen). It's hard to believe Tycho would (or could) record all 4: even assuming null dust, two of them would have the dimmest μ 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 λ, which is almost (fn 56) equivalent to adding 24° of precession onto Hipparchos' λ; [b] sprinkle a very few arcmin of random scatter[19] onto these results and onto Hipparchos' presumably-little-changed latitudes,[20] also adopted by Copernicus. (It is ironic that Tycho was a pioneer in precession, showing that prior astronomers' equinoctial "trepidation" was chimerical —

$v_\circ$ weighting for eye-sensitivity, visible starlight (0.48μ-0.68μ) was broken into 21 spectral components, at 0.01μ intervals (intensity from Planck blackbody curve, with T_c from B−V via C.Allen *Astrophysical Quantities* 1976 p.197). Molecular extinction (fn 63) was applied to each through a Rayleighesque exponential formula fitted to the standard table of Allen (1976 p.126), with a further diminution for Inn-Tanaka ozone extinction. The post-extinction components were remerged to yield the final magnitude. For high h, results are close to those for Evans' *JHA* formula with his $A_\circ$ = 0.13 for null dust, & (Evans' choice) $A_\circ$ = 0.2 for standard Allen dust. Which is a credit to both of us. And the credit for initiating taking-account-of extinction's color-dependence, during analyses of older star catalogs, goes to Evans not DR, since Rawlins 1982C ignored this factor, while Evans 1987 [a] included it, & [b] computed it correctly — at least for stars not near the horizon. In this connection: the 1597.23 light from 2g Cen (dimmest Hven μ of any allegedly-observed cat D star) came to Hven through A = 19.6 molecular atmospheres but merely Z = 11.6 ozone atmospheres. I.e., the key implicit premis for the *JHA* extinction formula of Evans 1987 (namely, that there is a single atm mass number A), breaks down for the very stars the *JHA* is arguing about: those of low altitude h. The dust atm mass D (fn 16) also parts company from A. But in the other direction. (E.g., 1591 Hven D = 24 for 2g Cen, thus its μ for Tycho, correctly computed according to the *JHA*-fantasized ancient dust density, would be even dimmer than 7.95, by ordmag 0.01 magn. Only ordmag 10% of dust is higher than 4-5 km, and below that we may use an exponential dust density of standard scale height 1.2 km, with Allen opacity.) Indeed, for stars on the horizon at sealevel: A = 39, while Z = 13, and D = c.100 ! For close approx to horizon A & D: atm mass = sqr[$\pi R'/(2h)$], where R' = Earth radius times 1.2 (adjustment for lightray-curvature: Rawlins *AmerJPhys* 1979/2) & h = scale hgt (8 km for molecular, 1.2 km for dust). As to Z: see above. The drastic low-altitude ratios cited here (A/Z > 3 & D/A > 2 at horizon) appear to be known little if at all. For Allen's standard dust opacity (adopted by *JHA* for antiquity), atm extinction at the sealevel horizon is about Δm = 11 magnitudes! I.e., Venus μ = 6, Sirius μ = 9. Are these apt for clear nights?? By DR's idealized null-dust math, Δm = 4 magns; ancient experience was something between our results, obviously (on the best nights) much nearer the latter. Overprecise data for B−V = 1.8, 0.8, −0.2: null-dust Δm = 3.96, 4.17, 4.36, resp. Including Allen dust, Δm = 10.32, 10.65, 10.95, resp. The molec + oz + dust contribs: 3.57 + 0.39 + 6.36 = 10.32; 3.79 + 0.38 + 6.48 = 10.65; & 3.99 + 0.37 + 6.59 = 10.95, resp. (At zenith: 0.104 + 0.026 + 0.070, 0.110 + 0.024 + 0.071, 0.115 + 0.023 + 0.072, resp.)

[19] Some may take scatter as more damning than none. MIT statistician P.Huber to *DIO* 1991/9/6, defending Ptolemy's less subtle frauds (‡2 §H14): "I do not contest that Ptolemy fudged his observational data. . . . If Ptolemy had seen anything wrong in what he was doing, I guess he would have added slight perturbations to his fake data (as present-day students do in physics lab courses . . .)." So, the clumsier the fraud, the less indictable it is? Obvious implication for those moderns now contemplating science fraud: merely can the scatter, and eminent professors will excuse your behavior! (Actually, Ptolemy did sneakily perturb c.10% of his data by 5′: fn 56.)

[20] Ulugh Beg's catalog (epoch = mid-1437 AD) was then unpublished and unknown to Tycho. There survives no other massive original star catalog between Hipparchos and Tycho. (See Thoren 1990 p.297 n.134.)

Table 2: Tycho's Fake Centaurus Stars

Cen	PK# D#	λ_P+21°	β_P	λ_T	β_T	λ	β
2g	935 1001	211°30′	-21°40′	211°27′	-21°49′	212°29′	-21°34′
4h	936 1002	211°00′	-18°50′	210°59′	-19°08′	212°15′	-18°56′
1i	937 1003	210°10′	-20°30′	210°12′	-20°51′	211°10′	-20°32′
3k	938 1004	211°00′	-20°00′	211°03′	-20°12′	212°23′	-20°02′

since real annual precession was virtually unvarying.) The method of the fraud is (aside from Tycho's random-scatter embellishment) just that of Ptolemy himself: merely add a precessional constant to a prior observer's stellar longitudes while keeping the stellar latitudes as they were. Carrying irony to new heights: the astronomer who first published the fact & the method of Ptolemy's "usurpation" (Tycho's word) of Hipparchos' Ancient Star Catalog was Tycho! — right in the preface[21] 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. . . .

C2   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 λ = real celestial longitude & β = real celestial latitude,[22] with subscripts H for Hipparchos,[23] C for Copernicus, & T for Tycho.

C3   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. Thus, all 6 stars' latitudes β got listed as positive (see PK p.99; Toomer *1984 Almajest* p.354 n.151), though only the 2nd had originally been recorded (by Hipparchos)[24] with positive β. (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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[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 $1°3/4$ at Copernicus 1543 p.50A. And the 2 main latitude variants on the ancient mss are $0°1/4$ & $0°3/4$. (These are noted on PK p.99 & 187. Both values have been prominently printed. The former is the more common.) 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 $\beta_H = 0°1/2$ (italicized in Table 1). For the next star, PK251, there are 2 prime ancient λ 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.

[23] Except in the cases cited at fn 56 [note added 1993: & see *DIO 2.3* ‡8 fn 20], the λ_H are just $2°2/3$ less than the explicit Ptolemy values λ_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 ordmag 1000 λ_H in Hipparchos' catalog), see *DIO 2.3* ‡8 §C.]

[24] This star is D676 = PK246. Of the group of six, it was Hipparchos' 1st (Tycho's 2nd). Thus, presuming Hipparchos used a ditto system (something like PK's), a sign error for the β_H of PK247 (perhaps triggered visually by the equality of its absolute value with that of PK246's β_H) would carry a single sign error into all the following stars. (At PK252, we finally encounter a correctly-signed positive β_H.) That the sign-confusion problem here is partly Tycho's own is suggested by the positive β_T sign wrongly given (§E4) to D681-682. (Neither position was based upon Hipparchos' data. I point this out partly because the implication might be seen as contrary to the present paper's thesis.) Which suggests that this whole set of β_T was considered by Tycho to be of dubious sign. (A signless quantity defaults to positive.) I.e., I suspect that the sign miscue for D681-682 was merely a carry-over (via ditto), when the previous stars were stripped of their negativity. The six β signs, in later renditions or updates of the Ancient Star Catalog: Al Sufi (H.Schjellerup 1874 p.102) and PK have PK246&251 positive, remainder negative. Copernicus 1543 p.50A has only PK246 pos, rest neg.

[25] Dreyer (OO 3:416) ascribed D675-682 to negligence (bad computing) from Tycho's rushing to complete the desired 1000 stars. Dreyer could identify none of these 8 stars (OO 3:364).

blunders[26] — the plagiarist's plainest type of giveaway slip.

C4   Another long-shot fluke helps cinch our exposure of the fraudulence of one of the entries, D675: this star (36A Oph) happens to have a proper motion that is huge (especially for so dim an object), $-1''.1$ annually in declination. Of all the 404 objects in the Moesgaard-Kristensen 1976 catalogue of bright stars within $10°$ of the ecliptic, 36A Oph = D675 has the fastest shift of latitude. The star's actual β was $-2°35'$ in Hipparchos' day, disagreeing with Hipparchos' recorded β_H ($-2°1/4$) by $20'$, which is about equal to his mean β_H error (R.Newton 1977 p.216); however, by Tycho's time, the star's actual β had shot[27] to $-3°22'$. Thus, even aside from its wrong sign, D675's β was off by over a full degree: an error of $1°10'$. This because the position of D675 was plagiarized from data (PK 247: §C2) which were far more obsolete than the swiper had reason to suspect.[28]

C5   A final slip by the plagiarist: Hipparchos' star PK251 (from which D680 was copied) does not even exist. It is simply 39*o* Oph with $\lambda = 231°1/2$ accidentally written[29] as $234°1/2$ 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 Almajest* p.394.) Note the Cen mean λ_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 = c.2′. Another way of putting it: the Cen λ_T in Table 2 are roughly 30 times nearer the fabrication values $\lambda_P + 21°$ than to real λ.]

C7   However, the latitudes β are no closer to Ptolemy than to reality. Evidently, declinations δ were genuinely[30] observed somewhere (poor rms error: $14'$) and then combined[31] with the fabricated longitudes λ (§C6), to yield latitudes (with Tycho's obliquity[32] $\epsilon =$

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[26] So near the ecliptic, such mistakes by Hipparchos would be understandable, when using his ecliptical armillary astrolabe; see Włodarczyk 1987 p.184. (Though, see fn 24 for a more coherent explanation.) However, Tycho's main armillary was equatorial. (The Oph stars were not near the equator.) For those familiar with such instruments, this is a powerful independent argument against the genuineness of Tycho's Oph data.

[27] The declinational or latitudinal decrease during the intervening 17 centuries was: over $1°/2$ from proper motion, plus almost $1°/4$ more due to precession (largely obliquity-decrease). (In the Moesgaard-Kristensen 1976 catalog, 61 Vir has a higher β component of proper motion, but 36A Oph's greater precession in β overcomes this superiority.)

[28] Use of fabricated observations of 36A Oph perhaps contributed a little to keeping Tycho from realizing that the "fixed" stars had proper motion (a discovery which finally fell to Halley a century later). However, if even a prominent swift star such as Arcturus did not persuade Tycho to see the stars as unfixed, then it's reasonable to doubt whether dim 36A Oph would move him very much in that direction. (See also fn 47.)

[29] Confusion of 1 with 4 (alpha with delta) is the most common scribal error in ancient math (see PK p.9). Note that this simple explanation will not work if Ptolemy is regarded as the Catalog's observer, since that makes Catalog $\lambda = 237°1/6$ for PK251 (*Almajest* 7.5). Decades of Muffia failure (*J.HA 1.2* fn 66) to realize that the original Catalog was Hipparchos' *and was ecliptical* helped keep this obvious identification (PK251 = 39*o* Oph) from being previously discovered. (See under Muffia-fogjam: *DIO 1.3* §9 §P3.) Various scholars' prior conjectures are cited at PK p.99 & Graßhoff 1990 p.218.

[30] The ten fakes' magnitudes disagree in two cases (D676 & D1003) with those of the Ancient Star Catalog. However, all ten agree with Copernicus' values, rounded to integers: Copernicus 1543 pp.50A&61A. (Tycho uses only whole magnitudes, not the finer Hipparchos-Ptolemy system, also adopted by Copernicus.) The use of Copernicus star data is consistent with our earlier suggestion that whoever faked the Oph stars was primarily depending upon Copernicus, with simultaneous consultation of Hipparchos-Ptolemy. See §C2 & fn 22. Note that the verbal descriptions of all 10 fake stars are virtual copies of those of Hipparchos-Ptolemy — with the revealing slip that, when Tycho reversed the order of PK246-247, (see §C2) and even reversed the magnitudes, he forgot to reverse these 2 stars' verbal tags. (There is no doubt that Tycho constantly touched base with previous star catalogs. For identification purposes, he frequently used the Ancient Star Catalog's star-numbers within constellations. And Tycho's use of earlier verbal star-descriptions is also immediately obvious from a comparison of OO 11:383f or OO 12:231f to Toomer 1984 pp.360f or Copernicus 1543 pp.52A f.

[31] Note our similar finding for the origin of hitherto-mysterious star D681: §E4.

[32] Tycho's publicly adopted obliquity was $23°31'1/2$ (OO 2:85, 208), which was high by $2'$ in 1598. (This was an uncharacteristically large error for a Tycho parameter. His latitude estimate was far better: Thoren 1990 p.226.) But a K.Moesgaard statistical analysis has found (Thoren 1990 p.226 n.16, p.230, p.298 n.138) that Tycho used the

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

$$\sin S = \sin \epsilon \cos \lambda \sec \delta$$
$$\tan x = \sin \lambda \tan \epsilon$$
$$\tan(x + \beta) = \tan \delta \sec S$$

C8   If the declinations were really observed, then: [a] for 2g Cen (even assuming zero atm dust), the Hven[33] post-extinction magnitude $\mu = 6.48$ (§B3), which was the dimmest μ of the entire 1004 stars, or [b] the Cen observations were made after 1597/10/20-21 (Dreyer 1890 pp.258, 390), at Wandsbeck,[34] where cat D was issued[35] 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 cease[36] 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[37] (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 Appendix[38] 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 from 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),[39] D399-411 (UMa, 13 stars), D481-482 (Boo, 2 stars), D675-687 (Oph, 13 stars),[40] D345-355 (UMi, 11 stars), D596-614 (Cas, 19 stars), D644-647 (Per, 4 stars), D1001-1004 (Cen, 4 stars).

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value $\epsilon = 23°31'$ in numerous reductions. Moesgaard's finding was adopted for some of DR's detailed analyses here. [Note added 1993-1997: the ϵ values actually adopted for Tycho's work are investigated at *DIO 3* (1993) fn 190.]

[33] Uraniborg's geographical latitude $L = 55°54'25''$N, longitude $12°41'55''$E (OO 10:xxvi; & see OO 12:102-103), height (above sealevel) $z = $ c.50 m. (Dreyer 1890 p.93 & n.2 provide estimates of 49 m & 53 m. JOG's map puts a nearby point at 45 m.) For mean applicable sealevel $P\&T$, see §B3.

[34] Wandsbeck latitude $L = 53°34'$N, longitude = $10°04'$E, $z = $ c.30 m; 2g & 4h Cen magn = 5.66 & 5.81, respectively, for 1598 transit (assuming null dust), mean Wandsbeck-area sealevel winter $P = 1015$ mb, $T = 273°$K.

[35] Dreyer 1890 p.265-6; p.259 denies that any data for placing stars were taken at Wandsbeck (fn 10); and OO 13:105f appears to support this. See Thoren 1990 pp.372f. But see also here at §G2.

[36] Dreyer 1890 p.235.

[37] For the record: DR isolated the tiny class of ten faked Tycho star places well before confirming that they were from the only two (small) post-cat C sections in cat D for which no records exist. Indeed, in all of cat D, there are only 21 stars which lack data records: D370-371, D379-380, D675-687, D1001-1004. Which means that the fakes (1% of cat D) entirely reside in the dataless 2% of cat D. The coincidence leaves no doubt that these stars' origin was conscious fabrication. Another way of putting it: the odds that all 10 suspects would fall by mere luck, into the very fraction (21/1004) of cat D which happens to lack records, is: 21/1004 to the 10th power — i.e., about one chance in 60 quadrillion.

[38] OO 13:61-77. The mss are compilations of accurate raw equatorial data (with ecliptical places computed therefrom, using a 5^y precession correction of $4'1/4$) of observations taken near the end or 1595 & the start of 1596, of which the original daily log records are not now extant. See OO 13:61 n.2.

[39] [Note added 1993: D565-569 are actually just 2 stars, 13θ Cyg and 54λ Cyg. Full analysis in *DIO 3*.]

[40] The fake star D675 accidentally slipped into cat C as star C553 when (after Tycho's death) cat D was pruned of questionable objects to form cat C. From the scanty information given by Dreyer 1890 p.266, I reconstruct:

D3   I have searched through the Tycho star observation records later[41] than early 1596. The search turned up 54 of the 71 extra stars,[42] accounting for *all* the stars in 6 of the 8 post-cat C sets of §D2 — but *none* of the stars in the remaining 2 sets: in Oph and Cen. I.e., of Tycho's last[43] (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.[44]

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: §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 data 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 1597/1/20; 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; §B1): "Desiderantur 60 pro complendo 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: §B1). It seems that Tycho was expecting (§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)?

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 pertinent ad Ophuichum et eius Serpentem".

Longomontanus, 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 excellent cat C.)

[41] 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 1588) 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.

[42] All 54 of the real data are found in OO vol.13 [note added 1993: they will be traced in *DIO 3*]: Cyg, pp.59-60; UMa, pp.59&72-74; Boo, p.74; UMi, p.76; Cas, pp.98-99; Per, p.99. The total number of outdoor stars (added to cat D by these data) shrinks to just 49 stars, after certain repeats & slipups are accounted for. (*DIO* will round & henceforth refer to this set as the Final Fifty.) [Note added 1993: See Tables 19&20 in *DIO 3*'s upcoming contribution: the 1st critical edition of cat D.]

[43] The actual chronology of cat C's compilation was not simple; see Thoren 1990 p.297 n.133.

[44] [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: C328-329 & C337-338.) These were evidently observed (accurately) with Tycho's rarely-used semicirculus (Ræder & Strömgrens 1946 p.96, Thoren 1990 p.177), using Sirius & Algenib as reference stars. The data presumably got misfiled due to the highly exceptional (probably experimental) mode of observation. Detailed analysis in *DIO 3*.]

E2   The thirteen Oph stars are indeed precisely those that close our gap between the 1000 (pre-Cen) stars produced in cat D (just months later: 1598/1/2) and the 987 stars that existed (according to the extant mss record) when operations ceased at Hven (1597/3). By Tycho's own count (§D5): 1000 minus 60 = 940 total stars before 1597. So, add the 47 taken (§D6) between then & his exit from Denmark: 940 plus 47 = 987. Or, by our other accounting: 777 (cat C: §B1) + 156 (1596 Appendix: §D1) + 54 (subsequent data: §D3) = 987. Let us now review the 13 suspect Oph stars.

E3   The first 6 stars were (as already shown: Table 1) transparently and clumsily faked by adding $28°$ onto Copernicus' longitudes. (The original celestial records of these 6 "observations" will never be found since they never existed.) Perhaps this may be said to illustrate the corrupting effect of grantmanship.[45]

E4   The last 5 stars (of the Oph 13) are all inexact repeats of other Oph stars already listed in cat C. But the two stars listed prior to them have remained unidentified, until now. These are the stars D681-682. (They are sandwiched between the 6 fakes D675-680 and the 5 repeat-stars D683-687.) I have reconstructed their origins, as follows.

D681 is evidently a primitive position for the low star $45d$ Oph: the Tycho longitude (precessed to 1597.23) of Ras Alhague (D688), $\lambda = 256°47'$, grafted onto the observed-guesstimated crude declination $\delta = -30°$. (We have already encountered 4 other Tycho examples of real declination data merged with appropriated longitude data: §C7 & fn 31.) Using Tycho's obliquity $\epsilon = 23°31'1/2$ (fn 32), the eqs of §C7 yielded $\beta = -7°10'$. (Actual 1597.23 coordinates of $45d$ Oph: $\lambda = 257°15'$, $\beta = -6°34'$.)

D682 is 3 Sgr, which was observed (correctly within $0°.1$) at $\lambda = 261°42'$, $\beta = -4°20'$. (Actual 1597.23 coordinates of 3 Oph: $\lambda = 261°37'$, $\beta = -4°22'$.)

The β of D681&682 were both wrongly printed without their negative signs (fn 24).

E5   The upshot is that the 13 star tacked-on Oph set is a disgrace. I suspect that (at the very least) Tycho's assistant Longomontanus (the self-cited actual supervisor of the construction of cat D: Thoren 1990 p.297 n.133) was privy to inside gossip that some of the supplemental stars fattening cat C into cat D were obtained in discreditable fashion. (See §B1.) Perhaps not knowing which were faked (or, if he knew of some fakes, being unsure if these were all there were), he threw out the whole lot (§B1) — and later printed (as cat C) just the 777 stars he trusted. Most of the ejected star-places were probably perfectly valid (as a test, I have checked the post-cat C stars in Crt and found that they are largely good to $1'$-$2'$) — genuine data[46] lost to contemporary scientists, yet another cost[47] of scientific fakery (which Hist.sci orthodoxy continues to regard as of no particular account).

F   Final Hven Dawn

F1   The extreme roughness of the δ ($-30°$) underlying D681's published coordinates suggests last-gasp desperation (as does the null-dust post-extinction magnitude, $\mu = 5\ 2/3$). It is possible that D681 & 682 ($45d$ Oph & 3 Sgr) were the last Tycho observations at Hven, as dawn began breaking on the morning of 1597/3/16 (1597.23).

F2   One of the more amusing arguments to be found in Evans 1987 is his attempt to alibi Ptolemy's notorious (e.g., Rawlins 1982C) omission of all the low southern stars visible from 137 AD Alexandria but not visible from 128 BC Rhodos (Hipparchos' place & epoch). In extenuation of this, Evans 1987 (p.168) triumphantly points to Tycho's allegedly mysterious omission of the low but geometrically accessible stars γ^2 Sgr (which we will

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[45] See fn 14 & §D5. Modern scholars are all too familiar with this phenomenon. But few are doing much about it — besides shouldering briskly for a firm personal place at the trough. Most institutions handle the attendant stink (not by reforms but) by suppressing & damning 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 (24ι Crt). Though its null-dust post-extinction magnitude was dim ($\mu = 5.82$), its position error was less than $5'$.

[47] See also fn 28.

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 & Strömgrens 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 panicky 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): 10λ Oph, 53ν Ser, 55ξ Ser, 56*o* Ser, 57μ Oph (D683-687). The six eventually-faked Oph stars (§C2) may have been vainly attempted, after initially-innocent consultation of the Hipparchos-Ptolemy Ancient Star Catalog (*Almajest* 7.5-8.1), to rustle 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 a 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 (‡2 fn 28 or Graßhoff 1990 p.215) than whatever record he might have 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 45*d* Oph, 3 Sgr, and 10γ Sgr were all about 6th magnitude (post-extinction), if we assume a perfect[50] 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 45*d* 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^y) 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

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[48] Illustration at Ræder & Strömgrens 1946 p.72 or Thoren 1990 p.169.

[49] *Explan Suppl to AENA* (1961) p.399.

[50] In the real (dusty atmosphere) world, the order of visibility-ease at this time would obviously be: 3 Sgr, 45*d* 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.Moesgaard & L.Kristensen, *Centaurus 20*:129; 1976.) The λ & β (E&E 1601.03) were, respectively: 257°19′ & −6°34′ (45d Oph), 261°40′ & −4°22′ (3 Sgr), 265°42′ & −6°55′ (10γ Sgr).

(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 & ζ Sgr), which Evans 1987 p.168 thought (§F2) 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 adoption[51] — as a prominent reminder of *JHA*'s rightful guardianship of refereeing standards for the rest of us: *J.Hysterical Astron 1.2* fn 5.) The likely explanation for their absence from cat D is: timing.[52] (See §F5 & fn 50.) During the very end of Tycho's pre-cat D observing period[53] 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 1598/1/2 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 difficult of the four Cen stars (2g Cen) was μ = 6.48 (§B3); from Wandsbeck, the most difficult (4h Cen) had μ = 5.81 (fn 34) — almost twice as bright. Tycho's normal outer-limit for μ lies either between these two figures or near the latter. (Not one star of the frantic Final Fifty, cited in fn 42, had μ as dim as 5.81.) [b] The crudity of the underlying declinations (1601.0 rms error[54] 14′: §C7) 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 & Strömgrens 1946 pp.96-97; Dreyer 1890 pp.19-20, 258; 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 = $13^h 30^m$, a bit past 6 AM Local Mean Time), the Sun's altitude $h = -18°$ (total dark). Only a week earlier, at LST = $13^h 30^m$, 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 difficult enough even in complete darkness.

H   Lessons & Phags

H1   Some concluding thoughts on 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.[55] 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.[56] But this alibi betrays frying-pan-to-fire smarts, by

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[51] Yes, there is a problem about the constellation Sagitta, the celestial arrow (current IAU abbreviation: Sge). Easily resolved: contract the official name to 'Gitta, and IAU-abbreviate this lofty shaft as "Git" — in order to ensure celestial commemoration of the long & the short of the *JHA*'s attitude toward dissenting lowlife (*DIO 1.2* §B2).

[52] I doubt (§G2) that the four Cen stars were taken at Hven; but I'll mention that, during the final frantic Hven star-gathering period (1597/2/4-3/15): [a] Cen transitted every night; [b] Sgr didn't transit during a single one of these nights. (From Hven, the stars could be recorded at other times than transit; but, for stars this low, a time near transit would be preferable.)

[53] Tycho observed at Wandsbeck from 1597/10/20, and cat D's date was 1598/1/2.

[54] The four Cen stars' mean (not rms) δ error for 1601.0 is +12′±4′, which is about right for Wandsbeck (assuming refraction-correction neglected), but Hven is not statistically excluded on this account.

[55] See Moesgaard 1989 p.313, Thoren 1990 pp.290f. However, there is evidence — hitherto ignored — that the obliquity's gradual decrease was known by the 2nd century AD: see Plutarch *Moralia* 411A. And Tycho may have swiped even his later discovery of this: §H3.

[56] Thoren 1990 p.383: "At Wandesbeck, Tycho's assistants quickly incorporated [the last-minute stars] into the primary catalogue, to produce an elegant manuscript version suitable for presentation to the emperor. On 2 January

immediately raising an even worse question. When surveying the history of discovery, it is worth keeping ever in mind a certain caution: how often were labors & novel finds, now attributed to the chief of a team or of an academic school, actually made by lower-echelon figures?[57] There is no reason to suppose that this alternate form of "usurpation" is entirely a contemporary phenomenon. Moreover, the underling whipping ploy may not even apply here. Let's face the obvious: [a] Tycho was himself more desperate than anyone to complete the promised thousand stars, thus it is incredible[58] that he would not have supervised these proceedings. [b] At the start of the final push towards this completion, his records state (§D5) that he needs 60 more stars, but these same records contain only 47 stars (§D6), a deficit of 13 stars — thus, to excuse Tycho is effectively to say that he couldn't count to 60. (See the reflections of Thoren 1990 p.298 upon Tycho's conscious responsibility for the faults of his star cataloging.)

H3   For several reasons, I expect certain factions to downplay the import of the foregoing discovery of fraud in the output of one of astronomy's giants.[59] Indeed, I hope (other) chroniclers of the Hist.sci movement will keep careful record of the descent of the usual establishment-sycophant phagocytes, upon the open wound this article will represent to them, as they attempt the usual career-serving damage-control, allegedly to protect-the-reputation-of-academe — oblivious to the fact that archonal phags' own censorial behavior is far more substantially damaging to academe's integrity than what they hope to cover up. A tempting alibi for Tycho will be: the faked stars constitute merely 1% of cat D (fn 61). One might respond in the tradition of Samuel Johnson's legendary crack at David Hume. Instead, let's simply note that Tycho has been suspected previously[60] of plagiarism. (It seems that the idea of the obliquity's variation — fn 55 — came originally from C.Rothmann.) The charge has been generally regarded (e.g., Thoren 1990 p.293) as not finally provable. However, the matter of the faked cat D stars (examined here) is unambiguous as to the fact that plagiarism indeed occurred in Tycho's output.

H4   Thus, the revelation of Tychonian star-fakes has the same rôle for an evaluation of Tycho's integrity as the case of "Crocker Land" later had for explorer R.Peary. Peary's N.Pole claim is baseless & absurd, but much of the hard evidence of deliberate fakery in that

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1598, Tycho dedicated the catalogue to Rudolph as a New Year's gift". Later (§B1), Longomontanus published cat C (while Kepler published cat D: §B1). 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 1890 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 1 & Table 2.) However, there is a simpler explanation. (And the uniform taking of magnitudes from Copernicus for both sets of fakes also suggests a single fabricator — perhaps inventing the two fake-star sets ordmag 1^y apart.) This is suggested by the common crudity of the additive constant: whole degrees in both cases. The two constants are simply integral roundings of the figures Tycho himself gives at OO 2:254 for the mean difference between his longitudes λ and those of Hipparchos & Ptolemy: $24°$ for the former & $21°$ for the latter. To manufacture the Cen stars' λ, the faker added $21°$ to Ptolemy's λ (Table 2). For the Oph stars, either of two theories works (§C1 & §C2): [a] The faker simply added $28°$ to the λ of the Copernicus 1543 star catalog. (This constant is the integral rounding of cat D's $\lambda = 27°37'$ for D1 = γ Ari, the star which is the λ zero-pt of the Copernicus 1543 star catalog. Copernicus' λ are just $6°2/3$ less than Ptolemy's — and they are $4°$ less than Hipparchos', except for the ordmag 100 stars whose longitudes Ptolemy deliberately altered by $5'$, in order to hide his criminal plagiarism of Hipparchos' great Catalog: R.Newton 1977 pp.250-252. [See also *DIO 4.1* ‡3 §C1.]) And-or: [b] The faker just added $24°$ to Hipparchos' λ. (Note: option [a] fits D680's λ, while [b] doesn't, for most *Almajest* mss: fn 22.)

[57] The names of those persons attached to Tycho's observatories are listed at Dreyer 1890 pp.381-384.

[58] Rather like pretending that Reagan didn't know about the Iran-Contra fiscal shellgame, even though he cared more about it than anybody else.

[59] Despite my high admiration for Tycho's monumental achievements, his opposition to heliocentricity constitutes an offense which hard-whiggist DR cannot pass over in silence. Tycho's arguments at Thoren 1990 pp.250 & 304 are comparable in folly to Ptolemy's, though Tycho creditably went as far as having the planets revolving around the (Earth-circuiting) Sun: Thoren 1990 p.252. (See *DIO 1.1* ‡7 §C3 & fn 2.) And one may speculate (in total absence of textual support) that Tycho's high praise for "Incomparabilis Vir Nicolaus Copernicus" (OO 3:337) hints that Tycho may have harbored more doubts of geocentricity than he published.

[60] See Thoren's alibi at *DIO 1.2* §H2.

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case requires expertise and-or close attention to evaluate. By contrast, one needs no special wisdom or effort to understand the significance of his repeatedly published 1907 claim that he had on 1906/6/24 discovered his important "Crocker Land" (now definitely known to be nonexistent) — at the very moment & place for which DR discovered his 1906/6/24 diary states: "No land visible". (Details: *DIO 1.1* ‡4 §B1-§B2.)

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 23^y at it); but, when they were forced by circumstances to fall slightly[61] short of a goal[62] 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 his school. We have no such assurance regarding the discoveries sometimes attributed to Ptolemy, who in truth probably contributed nothing of value to astronomy, that was 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 awful implication-by-analogy: if so fine an astronomer 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.$2°$ high at Hven transit, which makes its magnitude suspiciously 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

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[61] Tycho's cat D (1598) lacked only about 1% of his announced 1000 star dream. Peary's final (1909) sledge-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-moot suggestion that Ptolemy faked data to agree with previously existing round-number parameters. (Curiously, NCS himself later learned that Ptolemy did precisely that! — see *J.HA 1.2* fn 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* (§N11 & 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.

appear[63] at magnitude[64] μ = 7.95 for Tycho, while (again by the *JHA* formula), Tycho recorded nothing outside of Cen dimmer than μ = 6.42 ! (For the dimmest real cat D stars, D194 & D234, DR finds μ = 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?[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 misarithmetic, including OG's. See the paper's nn.30&35 on p.239.) Thus, it escaped the ever-alert *JHA*'s attention that the formula (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[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

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[63] Using the original formula $A = pa \csc[h + (8/3)/(9/5 + h^{1.1}/2)]$ (where a & p 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 air before arriving at Hven. (Our A is Evans' ζ 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 1982C eq.6.)

[64] And we have not included sky-brightness, which, even on the best nights, can add ordmag 0.1 magns of visual difficulty.

[65] More hilarious post-extinction magnitudes yet are demanded by other Evans 1987 p.168 statements of possible-visibility, the wildest being the claim that in 1591, 1ζ CMa = C475 (star #475 of the Tycho cat C which Evans is testing) would have been visible (through 35.5 atm . . .) from Bergen, Norway (latitude 60°.4 N). By Evans' own extinction formula (his eq.2), this star would appear at magn μ = 10.1 in Bergen! Finally, by the same formula, Fomalhaut (through 16.8 atm) would have post-extinction magnitude μ = 4.54 from Hven, whereas Tycho should have seen it at μ = 1st (!) magnitude, according to this massive *JHA* paper's central (sole) pro-Ptolemy "evidence" (pp.258-267, based squarely upon its eq.2). This paper's 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 *JHA* publication, is nowhere thanked in the paper.) I wonder if any of these scientists will have the integrity now to publicly distance themselves from the many-ways entertaining paper they once innocently signed onto. They have lent entirely unmerited credibility to a paper aimed at killing and (*J.HA 1.2* fn 144) swiftly burying valid scholarship. Will any of these men even have the decency to write *DIO*? Regardless, perhaps they will learn, from this experience, the degree of enhancement of their prestige which may be effected by involvement with typically open-ended Gingerich-inspired *JHA* "research". (I should add that the here-remarked gross problems with Evans 1987 were apparent to DR upon a first reading of this article.) *JHA* double-*LEAD* paper Evans 1987 also wears (directly opposite both its massive sections' opening pages) the prestige of the window-dressing *JHA* Adv Ed board: Cambridge, Harvard, Polish Academy Sci, Edinburgh, Czech Acad Sci, Frankfurt, NYU, USSR Acad Sci, Tokyo, Groningen, Aarhus, BrownU, Leicester, Pennsylvannia [*sic*], Indiana, & London's Imperial College. Since none of these universities appear to have the slightest concern that their names are being attached to funny science, the effect on their reputations is fully merited. The *JHA* allegedly has 2 top Editors (officially attached to Harvard Univ, Cambridge Univ, Roy Astr Soc, & Intern Astron Union), over a dozen prominently displayed supposedly-expert Advisory Editors, and c.750 subscribers (many of them libraries, so there should be alot more than 750 readers.) And this paper's magnitude-extinction treatment specifically enjoyed consultation from no less than *six* experts here & abroad (listed in detail at p.275: Brigham Young, Battelle Pacific, Nat Obs Athens, Univ Thessaloniki, Israel's Wise Obs). I.e., an article could not be better dressed. So, how is it that, both before publication *and for the five years since publication*, neither the author, nor 2 distinguished Editors, nor 15 *JHA* Adv Editors, nor 6 extinction experts, nor ordmag 1000 evidently-silent *JHA* readers ever took TEN MINUTES to compute the crucial magnitudes which are exposed here as incurably insane? — thus undercutting the attack which *JHA* launched against Delambre & DR? (By contrast, the *JHA*'s contacting these experts must have required *weeks* — hundreds of times more labor & delay than it would take to compute a magnitude.) Again (as at ‡3 §C16): is anybody actually *reading* the handsome journals? (Indeed, are they so handsome & so festooned with inside-cover assoc-editor brag-lists precisely *because* the inside cover is all that gets looked at? And ‡1 §A7 suggests even *that* isn't being read. . . .) It would appear that only one scholar on Earth has ever carefully read Evans 1987. And DR isn't even a *JHA* subscriber.

[66] I suspect that Evans 1987 n.41 was hurriedly added in press, since its preferred extinction-constant ($A_\circ$ = 0.20 mag/atm) destroys his p.168 attacks on DR — just as thoroughly as does his later eq.2, where he prefers the very same $A_\circ$ = 0.20 mag/atm: Evans 1987 p.260. [Like preference also at Evans 1987 p.269 & esp. p.271.]

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self-piekills Evans. . . .   [Further on piekill-suicide: *DIO 1.2* fn 29.]

H8   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&DR (& Tycho!) had charged against Ptolemy (disbelieved by Evans & his Muffia 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 handsome *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 — by A.Jones — that triggered DR's discovery of Hipparchos' hoaxes: *DIO 1.3* §N11), and [b] the Tycho case's public demonstration of establishment-belovéd 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.)

H9   Of course, to 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 theory. Note: none of the much-publicized fudgings, which modern scholars have demonstrated (as against speculated) in the work of Galileo & I.Newton, involve actual wholecloth computation of 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-quackser 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,[67] 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 Muffiosi — who claim (§H10) that Ptolemy is not unethical when he plagiarizes hundreds of stars (in a few days of indoor arithemetic) and then instead explicitly pretends in detail[68] 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.)

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[67] Hipparchos' math hoax (*DIO 1.3* §N11) constitutes: pretense to a complex math method, when a simpler one was actually used. But Hipparchos did not fake empirical data. (Though he did indicate that he had: [a] used data which were actually left out of his math processes, and [b] empirically confirmed prior astronomers' parameters which were in truth pre-assumed at the outset.)

[68] [Note added 1993: See *DIO 2.3* ‡8 fn 22.]

H11   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.

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