A Fresh Science-History Journal: Cost-Free to Major Libraries

Vol. 2 No. 3 1992 October

DIO — The International Journal of Scientific History.
Deeply funded. Mail costs fully covered. No page charges. Offprints free.

- Since 1991 inception, has gone without fee to leading scholars & libraries.
- Contributors include world authorities in their respective fields, experts at, e.g., Johns Hopkins University, Cal Tech, Cambridge University, University of London.

- Journal is published primarily for universities’ and scientific institutions’ collections; among subscribers by request are libraries at: US Naval Observatory, Cal Tech, Cornell, Johns Hopkins, Oxford & Cambridge, Royal Astronomical Society, British Museum, Royal Observatory (Scotland), the Russian State Library, the International Centre for Theoretical Physics (Trieste), and the universities of Chicago, Toronto, London, Munich, Göttingen, Copenhagen, Stockholm, Tartu, Amsterdan, Liège, Ljubljana, Bologna, Canterbury (NZ).
- New findings on ancient heliocentrist, pre- Hipparchos precession, Mayan eclipse math, Columbus’ landfall, Comet Halley apparitions, Peary’s fictional Crocker Land.
- Entire DIO vol.3 devoted to 1st critical edition of Tycho’s legendary 1004-star catalog.
- Investigations of science hoaxes of the 1st, 2nd, 16th, 19th, and 20th centuries.

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

E. Myles Standish (prime creator of the solar, lunar, & planetary ephemerides for the pre-eminent annual Astronomical Almanac of the US Naval Observatory & Royal Greenwich Observatory; recent Chair of American Astronomical Society’s Division on Dynamical Astronomy): “a truly intriguing forum, dealing with a variety of subjects, presented often with [its] unique brand of humor, but always with strict adherence to a rigid code of scientific ethics. . . . [and] without pre-conceived biases . . . . [an] ambitious and valuable journal.”

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

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

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

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

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

DIO & The Journal for Hysterical Astronomy: Page:

6 Scrawlins 91
7 Unpublished Letters 97
8 Current Developments: Columbus, Amundsen, Ptolemy’s Jekyll&Hide Defenders 99
9 The Neptune Conspiracy: British Astronomy’s Post-Discovery Discovery 115

Upcoming

In Future Issues of DIO:

Warren Report Was Right: Lone Gunman, Not Conspiracy, Killed JFK.

Ancient Planet Tables’ Sources.

Ulysses of the Polar Seas: the Kane Mutiny — Oscar Villarejo vindicated.

Ancient Vision.

The Unslandering of Sloppy Pierre.

Ancient knowledge of the 781 year eclipse cycle.

In Future Issues of J. Hysterical Astron (Previews of Coming Detractions):

The Editors’ New Clothes.

Photographic proof: moonrise in the west.

Selected Short Subjects:


Hist.sci accepts, as genuine, famous ancient text putting Moon into retrograde!

Possible Greek Use of the 831 BC Feb 4 lunar eclipse.

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

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

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

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

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

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

Each issue of DIO will be printed on paper which is certified acid-free. The ink isn’t.

©1992 DIO Inc.
This printing: 2019\12\26.
ISSN #1041-5440
H.Jones 1947. The Discovery of Neptune, Camb U.
H.Jones 1947. The Discovery of Neptune

1800/01/01 174°08′ 225°07′ 230°54′ 235°29′ 236°08′ 264°46′ 235°58′ 236°40′ 228°27′
1810/01/01 220°45′ 246°47′ 250°36′ 252°19′ 252°59′ 280°07′ 253°35′ 255°26′ 247°02′
1820/01/01 264°53′ 268°28′ 271°04′ 270°28′ 271°09′ 295°28′ 272°42′ 275°10′ 265°36′
1830/01/01 306°09′ 290°14′ 291°51′ 289°48′ 290°31′ 310°49′ 293°08′ 295°30′ 284°12′
1840/01/01 345°40′ 312°07′ 312°27′ 310°00′ 310°43′ 326°10′ 314°21′ 315°55′ 303°46′
1845/09/22 008°09′ 324°45′ 323°59′ 321°45′ 322°28′ 334°58′ 326°35′ 327°26′ 313°24′
1846/06/01 010°52′ 326°16′ 326°22′ 323°10′ 323°53′ 326°01′ 328°03′ 328°49′ 314°41′
1846/06/25 011°08′ 326°25′ 326°29′ 323°18′ 324°01′ 326°08′ 328°11′ 328°56′ 314°49′
1846/07/29 011°30′ 326°37′ 325°41′ 323°29′ 324°12′ 326°16′ 328°23′ 329°07′ 314°59′
1846/08/31 011°51′ 326°49′ 325°51′ 328°40′ 324°24′ 326°25′ 328°35′ 329°18′ 315°09′
1846/09/23 012°06′ 326°58′ 325°29′ 328°48′ 324°31′ 326°30′ 328°43′ 329°26′ 315°16′
1847/01/01 013°11′ 327°34′ 326°31′ 324°22′ 325°05′ 326°56′ 328°19′ 328°58′ 315°47′
1850/01/01 025°07′ 334°11′ 332°25′ 330°31′ 331°14′ 341°32′ 335°38′ 335°54′ 321°21′

Table 2: Geocentric Longitudes Corresponding to Key Historical Dates

Date Neptun Levrer MemC HypG HypW HypH Hyp1 Hyp2 HypX
1845/09/22 323°38′ 322°57′ 320°39′ 321°24′ 334°20′ 325°36′ 326°30′ 312°09′
1846/06/01 328°10′ 327°04′ 324°52′ 325°35′ 337°33′ 329°49′ 330°32′ 316°16′
1846/06/25 328°00′ 326°54′ 324°41′ 325°25′ 337°30′ 329°41′ 330°24′ 315°59′
1847/06/25 327°20′ 326°17′ 324°02′ 324°47′ 337°03′ 329°06′ 329°50′ 315°16′
1848/08/31 326°27′ 325°29′ 323°14′ 323°59′ 336°23′ 328°17′ 329°03′ 314°29′
1849/05/23 325°53′ 324°59′ 322°45′ 323°29′ 329°53′ 327°46′ 328°31′ 314°02′

Acknowledgements: This is to express my longstanding (& long overdue!) gratitude to David Dewhirst (Cambridge Obs) for sending (1967/3/2) photocopies of [a] Challis’ 1846 Neptune-sweep zone records. [b] most of CON (which I believe David first organized), & [c] the latitudinal Adams Neptune mss. (All this despite David’s wide & overmodest disagreement with DR’s view of the Neptune affair.) Also: my thanks go to J.Bennett, former RAS Archivist (now, like David, an Adv Editor of the JHA), for sending (1975/12/1) 70 pp. of material from the R.Sheepshanks correspondence, and to Malcolm Pratt of the St.Johns College Library, Camb University, for airmailing (on very short notice: 1988/11/30, 12/16) xeroxies of a sizable part of the longitudinal Adams Neptune mss.

### 6 Scrawlings

#### A Shorts

**A1** In 1990, rich Japanese businessmen purchased1 a Van Gogh, a Renoir, and Peru. Peru was cheapest.

**A2** One of the foundations of the search for wisdom is so simple that it can be cruelly expressed in just a few lines: the most important -ism isn’t conservatism, leftism, Darwinism, theism, or whateverism. The central -ism is: Truthism. Place one’s prime loyalty there, and hold all else contingent on that.

**A3** Stanislavski & Strasberg may have been great acting-instructors, but our 2 most effective dramatic-talks clinics are: Congress and prison. They differ in that: good acting gets you into one — and out of the other.

#### B Son of Read-My-Lips: Those Bleeding-Heart Republicans

**B1** Before ’88 election: [a] Bush decries Dems’ weekend release of allegedly-reformed criminal Willie Horton (whose idea of reform was to turn rapist), and [b] Bush pledges that no tax-increases would rape US pockets (by billions/month).

**B2** Bush in office: raises taxes. Now, for the ’92 campaign, remorseful Bush swears: I-won’t-do-it-again! Social workers everywhere want to learn: how, in just 4 years, has Bush rehabbed Willie Horton from scorned Lib-symbol to fave speech-writer?

#### C Archimedean Santa

**C1** What theme do these items have in common: [1] a wishbone, [2] a B.F.Skinner superstitious pigeon, [3] a US election? **C2** Answer: just pull the little lever, and all your dreams will come true.

---

1 The Van Gogh cost over $80,000,000; the Renoir, over $50,000,000. The bill for winning Peru’s Presidency was not reported, but the (over-the-table) cost of a Presidential campaign in the much larger US is only a few hundred million dollars, so a Peruvian election probably costs just a few million. Japanese may occasionally have paid more for another of their favorite Western land-acquisitions: US golf-courses. The Latin American nation of Peru has an area of over a million square kilometers. A few hours ago, its population was 22 million people. No one seriously regards this as part of an anti-man conspiracy.)

2 I risk insulting convicts by estimating that Congress & prison are about equally honest arenas. They are also equally male: both c.95% men, though the US population is c.51% female. Thought experiment: imagine the media’s hysterics if a 95%­white Congress were representing, say, a 51% black general population. (See below: D.) On the other hand, feminist organizations have been equally unquick to complain about a society that is (by lobby­logic) so anti­male that it locks up c.20 times as many men as women. Perhaps we should apply the Affirmative-Action quota approach — spring men (& sentence women) until both sexes’ jail populations are roughly at par? (Similarly, young men are routinely charged higher auto insurance rates than women the same age. No one seriously regards this as part of an anti-man conspiracy.)

3 Nightline 1992/11/5. And then there was USA Today’s 1992/9/10 headline (in which the President of the US comes off like a repentant 10-year-old), BUSH: NO TAX HIKES AGAIN, “EVER, EVER”.
The Inequity Inequity: Rainbow MENu

The unspoken lesson of the 1991 Anita Hill-Clarence Thomas affair is that, in the US, the “race card” trumps the “gender card.” How many more decades must pass before TV newsmen permit discussion of the lethally-revealing question: why do ethnic groups rate higher priority than women? Why are gross gender-inequities (in Congress, the Church, etc.) of so much less urgent interest to the press than are ethnic inequities? The contrast is itself the worst prejudice-related inequity in the US. So, naturally, that very fact is publicly undisputed.

D1 Women got the vote decades after southern black men.

D2 The US elects to political office more male Democrats, male Republicans, male WASPS, male Irish, male Italians, male Episcopalians, male Methodists, male Baptists, male Catholics, male homosexuals, male blacks, male Hispanics than women — though all these groups have (even in combine) smaller numbers than women.

D3 There have been several Jewish justices on the US Supreme Court, 2 blacks, but only 1 female. Yet, in the general population, Jews represent about 1/40th of the US, blacks about 1/8, while women are slightly over 1/2. (Wasn’t the 1776 revolt against King George fought over representation?) Thus, compared to women, blacks have been 8 times better-represented on the Court; and Jews, roughly 50 times better-represented. (One finds similar proportions on most other influential boards, panels, etc. E.g., a typical committee, say, 2 male WASPS, 1 male black, 1 male Catholic, etc. — and 1 female. Hey, everybody’s represented, so everybody’s happy, right?)

D4 Curiously, women’s-issue groups behave as if they believe that their salvation lies in supposedly smartpolitics alliances with the very same rainbow spectrum of ethnic-polishing lobbies which are responsible for such outrageous disproportionalities. Central question (which, perhaps revealingly, has not been publicly asked): why bother with alliances when your own group already comprises over 50% of the electorate? Are women as innumerate as their detractors charge?

D5 Feminists also court political alliance with the male homosexual lobby (whose private attitude towards women is not abundantly respectful). Perhaps that’s why no feminist has yet gone public with an irresistible . . . query: is there any connection between a Bush non-inner-circle-British astronomers) Adams’ precise initial agreement with Leverrier’s longitude; [2] Adams’ failure to push for publication (which lost him the discovery) was primarily due to his own paralysis, caused at least in part by his sign-error in a term of the math producing the elements set forth in MemoC. [3] Adams switched solutions on the public by pretending (e.g., §F2) that MemoC and MemoR (whenever the latter was composed) were only slightly different, whereas he knew that they were so seriously discrepant that he had feared publishing anything until making further tedious checks. (If the switch extended to having belatedly substituted MemoR for Hyp G as the 1845/10 document given to Airy — the hypothesis tentatively suggested in §F6-§F7 — then that could additionally help explain much of the secrecy surveyed in §B; however, this is but one among various possible non-mutually-exclusive alternatives, e.g., §C2 §G9, fn 67, fn 70.) The most extreme irony of the Neptune affair is the fashion in which the Cambridge conspiracy back-fired: Airy thought that his privately knowing of Adams’ work gave him an advantage, but Adams’ unpreparedness about everything contributed to spreading the British effort over a huge piece of sky, while intrepid (Columbus-like) Leverrier’s not entirely justified confidence in his prediction’s precision inspired the Berlin Observatory’s Galle (not Hamletized by Adamsian vagueness) to find the planet at one poke.

I1 We conclude with Biot’s sage and too-kind comments on the British Neptune disaster, reflecting a diplomatically, procedurally, & providentially correct position which should have been adopted for good as the official view, just as soon as Adams’ late claim was lodged, since it would have instantly ended the necessity for investigations of the sad truths behind the Neptune Scandal (the following quotation is taken from The Athenæum 1847/4/3 p.371; minor DR alterations in that translation):

in . . . 1845 . . . eight months before M. Leverrier’s first announcement, the new planet was predicted by the figures of Mr. Adams, and he alone was in the secret of its celestial position. These calculations . . . were well worthy . . . of being communicated without loss of time to the scientific world . . . . Or . . . [steps] should at least have been taken to find the planet [by telescope in 1845] . . . I see . . . a young man of talent . . . I shall say to him . . . “The laurel which you have been the first to deserve has been merited also by another, who has carried it off before you had the boldness to seize it. The discovery belongs to him who proclaimed and published it to all, while you reserved the secret to yourself. This is the common, unwritten law, without which no scientific title could be assured. But, in your own mind, you are conscious that the new planet was known theoretically to yourself before any one else knew of it. This inward success ought to give you the consciousness of your power, and excite you to direct it to the many other great questions yet remaining to be resolved in the system of the world; and if my years give me the privilege of offering advice, I shall express it in one word — PERSEVERE.”

I2 The felicitous ending to our story is that Adams creditably followed Biot’s just & fatherly charge to him: his later hotly-disputed (now un/questioned) discovery of the correct gravitational lunar-acceleration (overturning Laplace) by itself places Adams in the front rank of history’s mathematical astronomers.

Partial Bibliography
J. Adams SP. Scientific Papers vol.1 (of 2), 1896, Camb U.

---

4 I have my private opinion as to who was telling the truth — but must say that it would set an intolerable precedent to upset an impending appointment, on the basis of an account of unconfirmed events (of 10 antigun) when not a scrap of written notes was kept by the accuser — and when it appears that she took the percs (that attached to tolerating alleged verbal abuse) as long as they lasted, and only went public after that well went dry.

5 E.g., at least the first version (vetted by Bush) of the recent Civil Rights Bill placed a fiscal cap on damages women could collect from gender-discrimination suits, but no such cap for race-discrimination suits. Who thinks up insults like these?

6 Lobbies’ insatiability regarding proportions reminds one of the argument 23° into the classic 1963 film b’s a Mad Mad . . . World. The US Senate’s bitty-state-placating disproportionate representation-math originated just so.

7 In the US a generation ago, labor unionism was as sacred a media cow as today’s familiar special interest lobbies (e.g., military, capitalism, AIDS carriers, etc). What went wrong? Perhaps it was simply numbers: it’s hard to exploit a society whose masses are aroused, informed, & fighting-mad. So, vis-à-vis rulers, the women’s movement has the same downside as labor: there are simply too many women. If they get riled, there’s trouble ahead; so, the numerically small lobbies are tolerated, but not the potentially massive ones.

8 Are most women still falling for the media-dangled short-term-easy escapist myth (implicit in ubiquitous ads for cosmetics, creams, & shampoos) of: seductress-wife as a career? If so, then feminism is faltering not because it isn’t using its numerical advantage, but because there is no such advantage — i.e., feminism unfortunately doesn’t represent most women. (And that explain why the feminist movement is embarrassed about admitting why it believes it must for now — supposedly temporarily — ally itself with those above-cited lobbies which merely sap its potential force.) One can easily blame that situation on men, but the decent heart of the feminist movement is that Adamsian vagueness to nd the planet at one poke.

9 The Athenæum 1847/4/3 p.371; minor DR alterations in that translation):
16 I believe we now are in a position at last to explain the Neptune scandal’s inexplicable: [1] why Adams didn’t reply to Airy’s 1845/11/5 letter (Sampson 1904 p.168 has some private Adams math work, also dated, from 1845/11/28 & 12/24 on the letter’s question); [2] why his 1845/12/5±1 meeting with Airy (Smart 1947 p.34 fn) produced nothing (which [like their mutual attendance at RAS’ 1846/2/13 meeting] shoots down Adams’ excuse that he didn’t reply to Airy’s letter because he preferred verbal intercourse to writing letters; Glashier 1896 p.xxix); [3] why he did not publish even after Leverrier’s 1846/6/1 paper.

17 The cohering answer to these anomalies is simply that, during Adams’ “lost” period, the first half of 1846, he was simply trying (as I have said for decades is the case, e.g., Rawlins 1969) desperately to de-errorate his massive perturbational calculations (at which task he was inevitably less experienced than Leverrier, who was already the 1843 author of the accepted theory of Mercury’s difficult motion; & see fn 4). If Airy was told this by Adams (say around 1846/6/25-26, after he presumably informed Adams of Leverrier’s paper), then Airy’s prediscovery secrecy about Adams’ work is rendered less blameworthy (though hardly blameless). Had this secret ever been revealed (before or after discovery), Adams’ claim would be virtually defunct.

18 I propose that Adams’ timidity after his long-suppressed math blunder is the core of the Neptune scandal — a secret hidden all these years by [1] Adams’ & Airy’s peculiar behavior & excuses, and now by [2] the disappearance of so many original records. On the latter point, note that secrecy has consistently marked the Royal Greenwich Observatory’s handling of Adams’ prediction: [a] Prediscovery secrecy towards Leverrier, Hansen, and the public, [b] Sending of Greenwich assistant Breen to Cambridge on diversionary basis ([B7]. [c] Post-discovery nonpublication of Adams’ elements until after the details of Leverrier’s math and the reality of Neptune’s orbit were known. [d] Key documents (mainly the RGO Airy Neptune file) unavailable for a century. [e] After that century’s passage, the file’s location is not published, and the file then disappears, including the key document on which Adams’ priority is based (MemoR; see §C6).

19 Note also the number of odd lacunae in the history (especially in continuous records, harder to fudge): [a] No dates on Adams’ mss during the key period 1845/12/24-1846/8/20, and we know nothing specific of his communication with Airy on Neptune during this time. (See [G3].) [b] Adams’ 1837-1844 diaries have been used by his chief modern biographer (Smart 1947 pp.12-18), but nothing from the Adams diaries has been quoted from 1845-6.

[c] No mention of Neptune in the minutes of the very RGO meeting at which the desperate search for it was launched (1846/6/29, §B1). [d] Likewise, no mention of Neptune in J.Herschel’s diary 1846/6/29-10/1. [e] Adams’ name does not appear in Airy’s diary from 1845 Summer until 1846 Xmas (Chapman 1988 p.123), this despite Airy’s ([B1] “almost desperate” drive to find Neptune due partly to Adams’ calculations. [f] We are left with a grossly unacceptable (nearly total) lack of knowledge regarding Adams’ activities during the most important and peculiar period (1845/10-1846/6) relative to establishing the reasons for his nonpublication, which are crucial to the credibility of his belated claim of priority ([C2]). [g] Add to all this the astonishing fact that when on 1846/10/1 Challis and Herschel first brought Adams’ name before the public (instantly after the discovery, and while Airy was abroad), both failed to claim Adams’ work had priority (§C1).

110 Thus, in my opinion, not only should the Adams claim be shelved after the “lost” RGO file is unsealed, but: given British astronomers’ demonstrated filtering of documentary material (e.g., §B2), there is reason to doubt that what claim can ever be fully restored to health. Our foregoing review permits us to rewrite the infamous Neptune history in 3 crucial and related ways: [1] There was undeniably a conspiracy to keep secret (from running the most anti-female9 US presidency of the century (whose prime obsession10 has been packing the US Supreme Court with men he’s hoping will return women to pure baby-factoydom),11 and [b] Bush picking that cute little boy for his Vice-Presidential mate? (Granted, such a question is inexcusably prejudicial, since the undoubted truth is that Bush selected Quayle for his mental depth & celerity.)

D6 Not all US jobs are prejudicedly perceived as male-preserve; e.g., in most US neighborhoods, male prostitutes are even less welcome than female whores. Thus, feminists, making their move for power, might well begin by attacking the nation’s top male prostitution ring. Congress is, after all, only about 5% female; thus, any woman who votes for largely male candidates (until the House is roughly 50% female)12 fully deserves the subservient role her vote invites.

E Robert Newton & the Muffia

E1 Physicist Robert Russell Newton died in 1991 June. He was the retired Supervisor of the Space Sciences Division of the Johns Hopkins U Applied Physics Lab. I am proud to have known him, in good times and rough ones. He is survived by his 2nd wife Gene Newton and several children by his late 1st wife, Doris. RN was (along with 0 Gingerich & Lord Hoskin) the scholar most responsible for unleashing the new journal DIO. (Happily, RN lived long enough to read issue#1 and see it widely distributed and well received.) His Ptolemy researches got vile treatment from the Otto Neugebauer-Muffia oldbyperson-clique (for reasons which were, at bottom, careerist and thus largely fiscal — which make them particularly repugnant in light of the pseudointellectual veils used to hide this reality). Nonetheless, he remained admirably jocular about his various archon-smooching enemies. (I shall never forget his gentle, goodnatured attitude towards them, nor their ugly13 conniving to harm him as much as possible, for selfish professional advancement. This while avoiding symposium-catechism while he lived. His death now renders impossible ever to make substantial restitution to him.) A person of high mental abilities and wide culture, Newton became in his later years a rare combination of physicist and intellectual archaeologist, which resulted in a series of books (published by Johns Hopkins Univ Press or Univ Md) which used pretelescopic astronomical observations to determine the history of the Earth’s spin and revolution. If I were to specify the single quality which I most admired in him, and which ensured a firm friendship (despite numerous disagreements), it would be: when mobs of inferior minds yelped at him in unison,14 and scrambled to get in line to attack him (in order to cover the shame of the exposed archons they kiss up to), he had not the slightest interest either in muting the boldness of his theories or in politically compromising with Nibelings. Now that he is gone, I realize all the more how rare are such
self-confidence and courage, in modern academe. Even some among his legion of craven detractors acknowledge that he was the subject of useful controversy. He will be missed.

E2 A critical distinction, commended here (as also that at DIO 1.1 §1 [§C12]) to the consideration of those following the Ptolemy Controversy: scientists generally reserve the epithet “Incompetent” for those who are simply incapable of performing procedures necessary for the work at hand. It should be understood that Neugebauer-Mufa use of such terms is instead based upon *interpretational* disagreement. That is, difference from Mufa orthodoxy is instinctively equated with incompetence. (See, e.g., DIO 1.1 §5 §D14f & fn 20, or *DictSciBiol* 11:201.)

E3 Noncitation: Robert Newton’s publications on Ptolemy’s fakes started in 1969. But the Neugebauer Mufa’s leading capos refused to cite them until 1977. Eight years. (See, e.g., even Mufa princess Janice Henderson’s evasive piece in *Sky & Telescope* 1976/2.) Why? Simple: only in 1977 did the general public become aware (through articles in *Time* and *Science*) of RN’s findings in this area. Now, if an honest academic critic sees what he regards as erroneous work by a respected scholar, does he handle the problem (until forced to do otherwise) just by sealing off mention of the work & by privately slandering the author? (See DIO 1.1 §1 [§C7].) Or does he instead regularly meet the allegedly-errant scholar in polite public discussion at academic gatherings, where the evidential & logical merits of the matter can be rationally discussed? — and where, if the offender is indeed wrong or foolish, this can be demonstrated in an open-adversarial setting, on valid academic grounds. The Mufa preferred the former, censorial approach until 1977, when publicity (temporarily outside its immediate control) gutted this approach’s efficacy, and only then did the Mufa shift tactics and go on the offensive (i.e., switching from private to public slander). This pattern is consistent with an approach (to controversy) which is guided by motives not of integrity and courage but of political power-operation.

F People Power

F1 To observe astronomer 0 Gingerich (now head of Harvard’s Hist.sci Dep’t — and an ideal choice for the post) calling96 Galileo a “scrambling social climber” is as entertaining as finding (1976/3/12) R.Kargon, sometime *Isis* boardperson, accurately describing a well-known astronomer-historian-politician as “one of the biggest —kissers in the business.” Either of these eminent professors puts me in mind of Montaigne’s observation: he who gossips to you will gossip of you. (I can’t imagine why the next paragraph’s theme should follow so immediately upon admiration of the present paragraph’s magnates.)

F2 The cause of the dreary paucity of original thought in certain scholarship areas’ public discourse is self-evident:

[a] One will not be listened to unless one possesses power.
[b] One cannot attain power without laboring mightily towards its possession.
[c] But this very effort so wipes out one’s time&energy, that there’s insufficient left over for original thought.
[d] Upshot: the power operator labors for decades to get into a position where he can put over his new ideas — and by the time he’s got the power to do so, he has no substantial new ideas.

F3 When an academic biggie-editor & a productive scholar clash: the funniest item, in the bag of standard tactics used to damn the scholar, is the canard that he’s inherently Impossible. (Which may be strictly translated: he won’t kiss editors’ hands, feet, or brains.

15 I have long made it a personal rule to try to avoid using this unpleasant label; the only exception I can recall in the last decade was in 1989, for a particularly egregious case. And, given the Mufa’s continuing risibility in astronomical calculations, I reserve the option to bring the term out of mothballs in future discussions.


but also of a philosophical mind, we have nothing which we can put in competition. My acknowledgment of this will never be wanting; nor, I am confident, will that of any other Englishman WHO REALLY KNOWS THE HISTORY OF THE MATTER.” (Caps by DR.)

I3 Such statements constitute, it seems to me, agreement (and by one in a position to know) that the key to British failure to capture Neptune was simply Adams’ own inability to correctly overview-gauge the reliability of his mathematical accomplishment — and that in itself eclipses his claim of priority. Airy granted similar public concessions to Leverrier even during the famous 1846/11/13 RAS meeting at which his filtered (§B2) version of events made its then-inauspicious (but ultimately triumphant) public debut (M16:411, emph added):

I cannot attempt to convey to you the impression which was made on me by the author’s [Leverrier’s] undoubting confidence in the general truth of his theory, by the calmness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, “Look in the place which I have indicated, and you will see the planet well.” . . . . [For centuries], nothing so bold, and so justifiably bold, had been uttered in astronomical prediction. It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician [DR: the reference to Adams is painfully self-evident — and overdue, as Airy himself later realized]; it is here that we see the philosopher. The mathematical investigations will doubtless be published in detail; and they will, as mathematical studies, be highly instructive; but no details published after the planet’s discovery can ever have for me the charm which I have found in this abstract [Leverrier’s 1846/9/8-submitted AstrNachr paper] which preceded the discovery.

14 The degree of sanctity attained by Adams (vs. Leverrier) may be measured by the fact that these eloquent expressions of Airy have been almost entirely neglected by historians (unless used to damn Airy), though they are printed right in the prime published source on the affair (M16). Not yet turned by still-mounting nationalist vitriol, Airy is here expressing an honest wonder (which we who read the story after the event cannot ever quite share) at the most dazzling public-prediction miracle in the history of astronomy: Leverrier’s math-deduction declaration of the place of a giant planet “which no one has yet seen” (to quote the incredulous press before 1846/9/23) — and the swift vindication of that courageous deduction declaration of the place of a giant planet “which no one has yet seen” (to quote the incredulous press before 1846/9/23) — and the swift vindication of that courageous gamble by the man “who discovered a planet with the point of his pen”, to quote from Paris Observatory chief Arago’s unforgettable poignant announcement of his colleague’s success, an event that will always remain unique in the history of the oldest science.

15 It is a matter not merely of appreciation but of the most elementary fairness that: Adams, who published after the fact, cannot justify the credit due Leverrier for his daring. Moreover, the actual discovery was made due to Leverrier’s 1846/9/18 letter to Galle, which Adams had no part in whatsoever. Even less can Adams be given higher credit than Leverrier — though this has often been the case. (Neptune’s 1846 predictive location has become commonly referred to as “the Adams-Leverrier discovery of Neptune.”)

78 This word is important in that it undercuts Airy’s & Adams’ later use of the detailed shortcomings of Leverrier’s theory (regarding mean distance) as a means of grabbing for Britain a 1/2 share in the discovery.

79 Since the facts of this case have long since led me to come down on Leverrier’s side in this controversy, I should say: [1] I am of U.K. extraction. [2] Everything I have seen regarding Adams’ & Leverrier’s demeanor tells me I would be preferred by his company. That Leverrier (longtime head of the Paris Observatory) was extremely unpleasant to his colleagues is amply testified to. (In a devilish play on Neptune’s symbol, Humboldt called Leverrier “the man of the trident.”) But it is also fair to ask: was being cheated (of his proper due regarding Neptune) a partial explanation of why Leverrier became nasty? Against this theory: Leverrier’s bad temper appears to have been reserved almost exclusively for his countrymen, not foreigners. E.g., his kindness to his equally brilliant US Naval Observatory counterpart, USNO chief Simon Newcomb (Canadian-born), extended even to his presenting Newcomb
We are told by Adams (fn 59) that the 1845/9 & 1845/10 solutions were effectively identical (his “nal values”); thus, his seriously improved Hyp 1 solution cannot be continued a point raised in DIO 1.1 (¶1 fn 12, ¶7, ¶12), the base reason that politically-motivated academic gangs systematically refuse to give ANY credit to an “enemy” is: every discovery, publicly assigned to that person, enhances his stature — thus makes him a more formidable opponent. So: truth & equity be damned — the sort of ethics & priority-perspective one used to associate with gutter-level mobsters, not scholars. But, in certain academic areas, the difference is increasingly blurred.19

When publicly asserting an unwelcome truth, it is tempting to try working Within-The-System, since this is more pleasant and implicitly optimistic.20 However, [a] The more receptive The System is, the less important the issue. [b] The most important issue is: The System itself.

For archons long spoiled by routine assent, flattery, & bended knee: nongenuflection is Rebellion. Common-sense-time: who gets his jollies, not from scholarly creativity, but through power games, fights, sadism, etc. — an editor or a scholar?

Continuing a point raised in DIO 1.1 (¶1 fn 12, ¶7, ¶12), the base reason that politically-motivated academic gangs systematically refuse to give ANY credit to an “enemy” is: every discovery, publicly assigned to that person, enhances his stature — thus makes him a more formidable opponent. So: truth & equity be damned — the sort of ethics & priority-perspective one used to associate with gutter-level mobsters, not scholars. But, in certain academic areas, the difference is increasingly blurred.19

When publicly asserting an unwelcome truth, it is tempting to try working Within-The-System, since this is more pleasant and implicitly optimistic.20 However, [a] The more receptive The System is, the less important the issue. [b] The most important issue is: The System itself.

For archons long spoiled by routine assent, flattery, & bended knee: nongenuflection is Rebellion. Common-sense-time: who gets his jollies, not from scholarly creativity, but through power games, fights, sadism, etc. — an editor or a scholar?

Continuing a point raised in DIO 1.1 (¶1 fn 12, ¶7, ¶12), the base reason that politically-motivated academic gangs systematically refuse to give ANY credit to an “enemy” is: every discovery, publicly assigned to that person, enhances his stature — thus makes him a more formidable opponent. So: truth & equity be damned — the sort of ethics & priority-perspective one used to associate with gutter-level mobsters, not scholars. But, in certain academic areas, the difference is increasingly blurred.19

When publicly asserting an unwelcome truth, it is tempting to try working Within-The-System, since this is more pleasant and implicitly optimistic.20 However, [a] The more receptive The System is, the less important the issue. [b] The most important issue is: The System itself.

For archons long spoiled by routine assent, flattery, & bended knee: nongenuflection is Rebellion. Common-sense-time: who gets his jollies, not from scholarly creativity, but through power games, fights, sadism, etc. — an editor or a scholar?
Two Party Ping-Pong Pocket-Plumbing

For your home’s plumbing needs, you call on plumber A. But he fails you, so you go to plumber B. When plumber B fails,2 you don’t try plumber C or D or whomever — but instead you go back to trying plumber A. Then, after plumber A lets you down again, you go right back to plumber B, etc.

If you actually did turn your everyday searches for talent into such boring & infantile table-tennis exercises, you’d create, with respect to plumbers A&B: [a] understandably low regard for your intelligence, [b] your rapid impoverishment to fund plumbers’ mansions, limousines, yachts, & tourist junkets, [c] behind-the-scenes cartel-collusion-merging of A & B, [d] a home perpetually agurgle with new demands for plumbers’ ministrations.

Yet, sheeplike US voters follow exactly this pattern in their recourse to the two political parties that are taxing them (and their progeny) into economic debtor-imprisonment21 — even as TV ‘sneaks punit-flunkies assure the plumbees of the sanctity and inherent wisdom of the “Two-Party System”.22

21 Unwonted logical exercises: dedicated Dem voters regard the GOP as ghastly. (And I won’t say they’re wrong.) But none ask: what party’s mismanagement so grossed out voters that millions retched and elected Nixon. Twice. (That’s an indictment that would drive any self-respecting party to suicide. Well, maybe it did, at least in the sense that one can hardly tell Dems from GOPers anymore.) And, instead of moaning about Bush’s 1988 Willie Horton ad, why not ask: what party’s policies made that ad so effective? (What party ran US cities while they decayed into crime zones?) Answer: the Dems. (And what party’s insensitivity to poverty & simple justice so enraged 1932 voters that they turned for decades to the Dems?) Answer: the GOP. (And what party’s insensitivity to poverty & simple justice so enraged 1932 voters that they turned for decades to the Dems?) Answer: the Dems. (Thus the average citizen’s ability to save money has been declining for decades in the US — even when salaries rose. Few US citizens have (in savings) more than they owe — especially if their share of the national debt is taken into account. (The national debt is now roughly $50,000 per 4 person family. And that debt is growing at ordmag 10% — every single year.) Is the US turning into a vast company-store town?

22 Even allegedly reformist 3rd Parties have become increasingly suspect, starting with L.LaRouche (1976) & J.Anderson (1980) — for the simple reason that 3rd Party C may merely be regular Party A’s catspaw, injected to split the vote of the (other) regular Party B. In the 1992 campaign, TV ‘sneaks ignored honest 3rd Party possibilities (e.g., Ralph Nader), while grossly rich insider R.Perot’s p.r. men & high press contacts have transformed him into an Outsider, a “maverick”, i.e., the sort of creature which only MadAve has the nerve to conjure up: The Littleflegy’s Jullianne. Perot has served as a useful Pied-Piper lightning-rod, to help keep the two regular parties in clover by diverting (until it was too late for a serious 3rd party to get organized in 1992), harmlessly & fruitlessly, the public’s outrage at both GOP & Dems. (Similarly, GOP insider P.Buchanan was sent forth into the GOP primaries as another pseudoMaverick, to drain the dreaded D.Duke vote away into oblivion.) Simple consideration: if TV ‘sneaks builds up a candidate (or, indeed, any Approved Leader of a wortisome lobby, e.g., women) to Credibility status, by providing her or him lots of airtime, then that person is as trustworthy as the benefactor-builder-media itself. (Yes, singular.)

The foregoing examples — of orchestration-talent that would tax a Rimsky-Korsakov — are reminiscent of rulers’ prime unspoken principle, which Tammany’s own Geo. Wash. Plunkitt revealed in 1906. (From his single sentence, I learn more about US democracy than from ten years of civics courses.) Plunkitt: “I don’t care who does the electing, so long as I do the nominating.”

---

H Dates

The question remains: when was the famous Hyp 1 list of elements (MemoR, allegedly 1845/10) actually transmitted to Airy? An examination of Sampson’s description of Adams’ solutions cues us. Sampson (1904 pp.165-8) finds that there were 4 distinct 1845-6 solutions:

1) an early inferior one, then
2) Hyp D (§F6), which (if the superficial corrections [a]&[c] of §F4 are applied) is fundamentally identical to Hyp G (§F7),
3) Hyp 1 (§F5), & finally
4) Hyp 2 (merely Hyp 1, repeated for slightly reduced mean distance: fn 5).

Dates appear on the Adams mss during the work for the early solution: 1845/4/28 & 5/19 (Sampson 1904 p.165); for Hyp D: 1845/9/18 (p.166); for Hyp 2: 1846/8/20 (p.167). (Adams then found Hyp X by linear extrapolation from Hyp 1 & Hyp 2, 1846/9/2.) But there are no dates given anywhere on the manuscript pages of the work.

Which suggests the possibility that the calculator of this work did not want its date to be known. Adams’ various memos (the famous solutions for the elements of the predicted planet), handed to Airy & Challis, are also all oddly undated by Adams, as remarked above. This is not a record to be accepted at face value.

We have never been told what the devil Adams was doing for the first half of 1846. Not a scrap of dated Adams perturbational calculations has ever been located, between 1845 Xmas Eve & 1846/9/20 (ibid p.167), less than 2 weeks before his final 1846/9/2 report to Greenwich. This is as credible a record as supposing that Adams went off with Santa Claus for 8 months. A related & clearly inexplicable item (see also §D4), one which suggests culling of files (e.g., CON): after seeing Leverrier’s communication with Adams where) and once [1846/7/2] when Hansen and I came for half a day to Cambridge and we were walking over St.John’s Bridge. The interview on each occasion might last two minutes [B6]. No other opportunity of seeing him.

---

24 And this 1845/12/24 material is not work on the longitudinal problem but is immediately concerned with the Uranus radius vector.

25 Airy to Sedgwick 1846/12/4 (Smart 1947 p.40 emph added): “My whole epistolary communication with Adams is printed in [M16] and I never saw him but twice; once [1845/12/5±1], somewhere with Challis (I totally forget where) and once [1846/7] when Hansen and I came for half a day to Cambridge and we were walking over St.John’s Bridge. The interview on each occasion last might two minutes [B6]. No other opportunity of seeing him.”
to 1845.750, we may look for the required scratch-work shifting Hyp 1 (MemoR) from epoch up to 1845.750 — or just the bare 1845.750 element-list (like MemoD in the Adams papers: §F2 & fn 68). But nothing of the sort has been found among Adams’ mss. (The Hyp 1 sections of the mss, E IV-V, contain no such figures — nor, as noted, Adams’ copy of MemoR.)

G7 Leverrier’s 1846/6/1 paper publicly placed the new planet at true heliocentric longitu­dine 325° at 1847/1/1. Airy stated (M16;398) that in 1846/6 he was struck by agreement “to one degree” between this figure and that given by Adams’ 1845 Oct orbit (Hyp 1). But for 1847/1/1, Adams’ Hyp 1 orbit gave 329°18’ (over 4° ahead of Leverrier’s position and about 2° ahead of the real planet’s 1847/1/1 heliocentric longitude), while the Hyp G orbit (which I suggest was the orbit actually given Airy by Adams in 1845/6/20) puts the planet at 325°05’ on 1847/1/1, only 1/2 degree different from Leverrier’s place69.

G8 No wonder Challis said that the 2 solutions agreed “almost precisely” (1846/10/21 letter, AstrNachr 25:101; reprinted Adams SP p.43).70

But perhaps the most direct piece of evidence here is an obscure 1846 July document in Adams’ own hand (CON #35) — the item we have been calling MemoW — in which he himself states (in the context of computing an ephemeris, where true not mean longitude is all that is relevant)71 that the heliocentric longitude of his perturbationally-predicted planet was “325° very nearly”, rather than Hyp 1’s 328°34’-329°18’ (1846/8/29-1847/1/1).72 This suggests (but does not in itself prove)73 that the famous Hyp 1 orbit (allegedly left at Airy’s home on 1845/10/c.21) did not exist until after 1846/6/30 (since that is the earliest reasonable date for the existence of the MemoW just quoted). (Hyp 2 is irrelevant here since it was not made until after 1846/8/20; see Sampson 1846/10/6 p.47. Again: Hyp 1 is the orbit upon which Adams’ claim of priority rests. The other only possible interpretation is that Adams — and Airy & Challis — were referring to mean longitude 325° (not true longitude), which is consistent with Hyp 1 (1846/10/6 mean longitude 325°: fn 71). But that is just as devastating to Adams’ claim (not to mention implying that he, Airy, & Challis

69 For 1846/8/29 or the epoch Adams used in 1846 (his perturbation-based solutions always used Uranus’ opposition as epoch: 1846/10/6 for Hyp 2), 325° is consistent with his real 1845 solution (Hyp G) which gives 324°23’ for 8/29, 324°16’ for 1846/10/6 — while the crucial purported “1845 Oct MemoR solution (Adams’ wellknown “Hypothesis 1”) gives values that cannot possibly be confused with 325°, no matter the rounding: 328°20’ for 8/29, 328°47’ for 10/6. For 1847/1/1, the 1845 Sept solution actually handed Challis, including the errors noted here, MemoC (CON #43), gives 324°22’ — also within 1° of Leverrier’s place.

70 Challis says at this point that Adams’ 1845 solution (MemoC) imparted the planet’s “heliocentric longitude” (which generally means true longitude), though in fact Adams’ known heliocentric solutions explicitly specify solely mean longitude. It is possible that this looseness merely reflects the fact that Adams implicitly determined true longitude. Or, there may have been confusion of mean & true longitudes (which is counter to part of this paper’s proposed switch-hypothesis), or it could simply indicate that Challis was following the verbiage of Adams’ MemoW.

71 However, since MemoW is for a circular orbit, and since Adams’ perturbational calculations were based on mean longitude, one may argue that MemoW is consistent with Hyp 1, whose mean longitude at 1846/10/6 was 325°. (M16:445).

72 See also Airy’s 1846/7/12 instructions to Challis (CON #4): “The investigations of Mr. Adams and M. Le Verrier having made it probable that the place of the supposed planet is not far from 325° longitude, I would propose to examine a zodiacal zone of which that point on the ecliptic is the center, with an extent of 15° in each direction from that point in longitude, and to 5° of latitude north and south.” Airy then blocks out a jagged parallelogram of this description, whose acute angles are at 14°20.48 & declination −24°12.44 & RA 22h 58m 51s. He also comments (wrongly) that the known completed Berlin Sternkarten (Berlin Observatory) cover “a small portion” of the area, adding (§72: p.51): “There is only one [Berlin Hour 22] which applies partially to the inquiry.” In any case it helps explain how Airy’s nonuse of the Berlin Starcharts, I will point out that Adams’ Hyp 1 planet was at this very time moving among the stars of Berlin chart Hour 22 (which Challis possessed; M16:421), while his Hyp G planet was not, being 4° farther west. The Hyp 1 planet was about 2° to the east of Berlin Chart Hr 22’s west boundary, which was 1° less than 22 hrs of RA. (The Hyp 1 body was in Berlin Chart Hr 22’s space until 1846/10/12; real Neptune, until 1846/8/20 — so, at the very point Airy was downplaying the Hr 22 chart’s utility, Neptune was slowly sidewayising its way through Hr 22’s stars.) The Hyp G planet was about 2° west of that boundary, and thus quite off the Hr 22 chart. An oddity: Berlin Hr 22 (1833, by top pre-photo starmapper F.Angelander) covers more (c.35%) of the Airy-projected search area than the famous Hr 21 (1844, by C.Bremiker, also renowned for his 1856 log-tables) that made possible the swift Berlin Obs 1846/9/23 discovery. (Hr 21 added less than another 20% to what Hr 22 already covered.)

73 See alternate possibility here following; also fn 71 & 72.

7 Unpublished Letters

A A Mostly-Unpublished Warning

A1 The subject of Time’s 1991/9/16 cover story was Lamar Alexander, the US Cabinet’s Education Sec’y, who was going on about how he hopes-to-reverse-the-degeneration of US education. The response leading off those printed in the 1991/10/7 Time “Letters” was the bold-faced-printed letter: “America is still looking for a gimmick to pull it out of an educational downturn.” Marion Gadberry, Oroville, WA.

A2 A glance at the letter suggested that Gadberry had written more of value than what was published. So, I instantly reached him by phone and learned that the published statement was indeed just a snippet and that its writer understandably felt his message had been virtually eviscerated. So DIO presents here the original letter, in full, with thanks to the writer for his trust in transmitting it to us:

To: Letters to the Editor, Time, Rockefeller Ctr, NYC 10017
From: Mr. Marion Gadberry, a teacher, P.O.B. 1429, Oroville, WA 98844; phone: 509-476-2306

A3 The politicians and the American people are still looking for the gimmick which will pull us out of our educational downturn. They are blind to the sociological facts that no gimmick can ever overcome, namely — a 50% divorce rate that traumatizes students, 40%Latch Key children that come home to unsupervised homes, 40% of American students being raised by single-parent families that have neither enough money nor enough energy to properly supervise their children, children participating in a plethora of extra curricular activities that are “more important” than coming after school for extra academic help, taking days out of school for family reunions, hunting trips, vacations to Hawaii, orthodontist appointments, etc., 5 to 10% of the students involved in drug usage, an educational system that pushes every student through the same educational curriculum regardless of their intellectual and emotional characteristics, and all the above compounded by the insidious [side] of America’s affluence.

A4 A computer in every student’s lap and every educational reform will never erase the negative effects of the above-mentioned sociological facts. Let’s face it, Americans are getting back exactly what they put into their families and schools — very little.

A5 I would be more blase about Time’s removal of the guts of Gadberry’s letter if it weren’t so de-rigorously typical. US media will not discuss “radical” (literally go-to-the-root) solutions to social decay: only band-aid “progressive” solutions are ever permitted in leading mags or TV’s news. Thus, the only thing that progresses is the decay itself. (See DIO 1.1 §71 & DIO 1.1 §72.

A6 There is a creepy resemblance between the search after cures for cancer and for the US’ educational collapse: both searches are expensive, lobby-ridden, seemingly endless & fruitless. Perhaps we can learn something about the latter moras (see & DIO 1.1 §1) from a DRI’son the former: the best cure for cancer is not getting it in the first place.
B Prediscovery Observations of Neptune

To: Letters, *Scientific American*, 415 Mad Ave, NYC 10017

From: DR

B1 The December *Scientific American* states (p.74) that, after Galileo's 1613 observations of Neptune (Kowal & Drake's recent astonishing find), and before Galle's 1846/9/2 Berlin Obs, optical discovery (directed by the mathematician of Leverrier), only "One observation of Neptune . . . was already known [1795, by] Joseph Lalande, a French astronomer who catalogued some 50,000 stars. . . . " As author of the article [AJ 75:856 (1970)] cited in support of this, may I mention two items? [a] The 1795 observer was actually Joseph Jerome Lalande's nephew, Michel Lalande. Of the 50,000 stars in J.J.Lalande's 1801 *Histoire Celeste*, not one was observed by the titular author. [b] There are in fact 7 known observations of Neptune between Galileo & Galle. A complete table of these has, I believe, never been published. Augmenting with the Galileo position:

<table>
<thead>
<tr>
<th>#</th>
<th>Observer</th>
<th>Date</th>
<th>Place</th>
<th>Recoverer(s) (Date)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Galileo</td>
<td>1613/01/28</td>
<td>Florence</td>
<td>Kowal &amp; Drake (1980)</td>
</tr>
<tr>
<td>2</td>
<td>M.Lalande</td>
<td>1795/05/08</td>
<td>Paris</td>
<td>Maunais (1847)</td>
</tr>
<tr>
<td>3</td>
<td>M.Lalande</td>
<td>1795/05/10</td>
<td>Paris</td>
<td>Walker (1847)</td>
</tr>
<tr>
<td>4</td>
<td>J.Lamont</td>
<td>1845/10/25</td>
<td>Munich</td>
<td>Hind (1850)</td>
</tr>
<tr>
<td>5</td>
<td>J.Challis</td>
<td>1846/08/04</td>
<td>Cambridge</td>
<td>Challis (1846)</td>
</tr>
<tr>
<td>6</td>
<td>J.Challis</td>
<td>1846/08/12</td>
<td>Cambridge</td>
<td>Challis (1846)</td>
</tr>
<tr>
<td>7</td>
<td>J.Lamont</td>
<td>1846/09/07</td>
<td>Munich</td>
<td>Hind (1850)</td>
</tr>
<tr>
<td>8</td>
<td>J.Lamont</td>
<td>1846/09/11</td>
<td>Munich</td>
<td>Hind (1850)</td>
</tr>
</tbody>
</table>

B2 The Challis 1846 observations were part of his famous failed secret Cambridge U. sweep, aimed by J.Adams’ math, 2 [the sky search having been] done at the request of Astronomer Royal G.Airy. The Lalande 1795 and Lamont 1845-6 data were chance byproducts of regular star catalog work; the 50 year gap separating them corresponds to Neptune's period of most southerly declination (making it less likely to be recorded in N.Europe sweeps — there is a gap of about 3 decades in the series of 23 Uranus prediscovery observations, also [due to] the planet’s southerly position).

B3 Given Neptune’s rapid northward motion in the 1840s, it was sure soon to be captured by accident (as so many fainter asteroids were: not a single year 4 since 1847 without a discovery). Had this happened, we might have lost one of the great tales of scientific prediction, Leverrier's discovery of the 8th planet “with the point of his pen” (in the grand [contemporary] phrase of F.Arago). 5

G4 The 1845/9 solution contains a small but fateful math bungle (touched upon at §F2) of which Adams was so ashamed that he never published it anywhere: the sign of a term was large discrepancy in eccentricity (0.16 vs. 0.14: §F2). Understandably believing that if this eccentricity-discrepancy were known, his claim to co-discovery would fail (& his 1846/9/2 extrapolated solution Hyp X would of course utterly collapse), Adams published only the corrected solution, alleging that it was transmitted to Airy in 1845/10. (One begins to see why post-discovery renditions are not to be quite trusted.)

G5 We now have the fact of Adams’ suppression of his 1845/9 note (MemoC), and a motive for Adams’ possible suppression of the 1845/10 document (Hyp G? or Hyp I?) if it is effectively the same — with Adams states that it was (see above quotes from M16:429 at §F2 & fn 59). The MemoC which Adams handed Challis in 1845/9 specified geocentric longitude (see either CON #32 or Sampson 1904 p.166) — i.e., here is the spot (in the outdoor sky) to search at. But, MemoR (allegedly the 1845/10 document) does not so specify. (Why bother, after the discovery?)

G6 Airy's 1845/11/5 reply to Adams' 1845/10 submission says that it displayed perturbations (as shown, e.g., in Adams’ 1846/11/13 paper, M16:454). But the purportedly “1845 Oct” MemoR provides only residuals (M16:395-6), not perturbations. A 1845/10/23 Adams letter to his parents (noting he had just failed to see Airy at RGO) is quoted at Smart 1947 p.19: “I left a note for him, however, containing a short statement of the results at which I had arrived.” This tells us nothing of the Neptune celestial place imparted. But “note . . short . . . results” sounds like brief MemoC or MemoD (Hyp D: see above quotes from Histoire Celeste). One notes that the 10 residuals of MemoC, his copy of the crucial MemoR (Hyp 1) has not been found in the Adams mss (Sampson 1904 p.167). In that connection: noting that Adams’ perturbational solution is for epoch 1810.328 and so must be precessed forward to 1846 (no such date is written on the mss pages for any of the Hyp 1 work.)

But see My 1973 paper was perhaps the rst to accent the 38 discoveries (planets, satellites, asteroids) from 1807 (Vesta) to 1845 (Ariel & Astraea), followed by the deluge: EVERY calendar year after 1844 has seen at least one such discovery. Equally odd: the similar Cassini-Herschel gap 1846-1781.

66 Sampson 1904 p.167 notes that a small alteration in procedure was made in the Hyp 1 method before its publication, but he judges that this had no effect on the previously derived solution. (Again, we have no dates written on the mss pages for any of the Hyp 1 work.)

67 By 1846/12/12 (after Hyp 1’s publication in M16), Challis has straightened out the story and speaks of MemoR as displaying residuals, not perturbations (SP p.1). As is so often the case in this history, forgetfulness is always a possible explanation of contradictory statements. (It is the context of secrecy, conspiracy, & missing documents that makes such conflicts of greater than normal interest.) Comparison of Airy’s & Challis’ accounts on this point among others reminds us: if Adams & Airy agreed to publish a cleaned-up solution (Hyp 1) in place of the solution perhaps actually submitted in 1845/10 (Hyp G), then there is no reason to assume that Challis ever knew anything about the matter. (Challis to Airy 1846/12/19, quoted by Glashier 1896 p.xxx: “It will hardly be believed that before I began my observations [1846/7/29] I had seen nothing of his [Adams'] in writing respecting the new planet, except the elements which he gave me in [1845] September written on a small piece of paper without date.” The piece of paper was MemoC, which survives as CON #32.) This situation would leave Challis so ignorant of the truth as to help explain the contrast of his now-neglected early generous championship of Adams (vs. Airy’s initially measured praise). E.g., Challis (SP p.4v; 1846/12/12): “the problem of determining, from perturbations, the unknown place of the disturbing body, was first solved here (Cambridge U.). It was, I believe, to help explain the contrast of his now-neglected early generous championship of Adams (vs. Airy’s initially measured praise). E.g., Challis (SP p.4v; 1846/12/12): “the problem of determining, from perturbations, the unknown place of the disturbing body, was first solved here (Cambridge U.). It was, I believe, an application of one of his papers (to MemoC) to the Adams papers (to MemoD) that was the first real explanation of the true position of all the planets, and that was the result of this. The discovery of the 8th planet among us, who has at once done honour to the University, and maintained the scientific reputation of the country. . . .

1 This amusing item was revealed by J.Delambre.

2 But see §9 fn 19.

3 Curiously, though we have 23 prediscovery observations of Uranus and 8 of Neptune, astronomers have so far recovered zero prediscovery observations of Ceres, Pallas, Juno, & Vesta. (Possibly, an enterprising researcher can alter that situation.) Since Vesta is sometimes a barely-naked-eye object (& I’ve seen it so) — far brighter than Neptune ever gets — this is an extremely odd footnote to astronomical history.

4 My 1973 *Astronomy & Space* paper was perhaps the first to accept the 38’’ dought of major solar system discoveries (planets, satellites, asteroids) from 1807 (Vesta) to 1845 (Ariel & Astraea), followed by the deluge: EVERY calendar year after 1844 has seen at least one such discovery. Equally odd: the similar Cassini-Herschel gap 1846-1781.

5 See §14.
whether or not Hyp G was ever written out by Adams, it is the correct rendition of the elements he deduced in 1845/9 — which include \( e = 14\% \). It thus represents what a capable astronomer, e.g., Airy or Breen, would have been using on that basis.) Having broached this novel speculation (which may help explain why the RGO Neptune file has never been made public), let me now turn to the various evidences that lend credence to it.63

G The 14% Solution: Did Adams’ 1845 Oct Prediction = Hyp G?

G1 Adams worked slowly & cautiously (which is why he lost Neptune); it is on the face of it unlikely that, in less than a month (the 4 weeks prior to 1845/10/21), he performed all the necessary calculations64 that would turn up his term-sign miscue — and then recomputed and re-checked this lengthy solution, to his notoriously-perfectionist satisfaction-certain that no more such errors lurked (and then with blithe what-me-worry?-confidence instantly frisked off to bother the Astron Royal). Of the Adams ms solutions, that in question here (Hyp 1: sections E IV-V of the mss; Sampson 1904 pp.166-7) is one of the longest (22 ms pages). A characteristic of Adams was his admirably scrupulous reworking of results: fn 59. (Which was perhaps a weakness in the Neptune race — but proved a strength that won him glorious & legitimate victory in his later lunar controversy: §112.) From Challis’ 1846/12/12 Report to the Observatory Syndicate (SP p.164): “It is to be regretted that Mr Adams was more intent upon bringing his calculations to perfection, than on establishing his claims to priority by early publication.” (DR italics for irony.)

G2 If we are to believe that the “1845 Oct” MemoR (Hyp 1) really existed at the purported time, we must also believe that even though Adams subsequently rechecked lots of the work (Sampson 1904 p.166-7), nonetheless: all 30 of the residuals finally presented to the world on 1846/11/13 (M16:406-7) were identical65 (down to the arcsec tens & hundreds displayed) with those Adams allegedly handed Airy 13 months previously (M16:396).

G3 Comparing Adams’ published version (of his Neptune work) to his mss, reveals occasional anachronistic or temporally-uncheckable dovetailing (e.g., Sampson 1904 p.162) of material from loose ms pages that are only rarely dated. Adams’ mss show that he was setting up the formulae for perturbations on 1845/11/28, 12/16, & 12/24 (Sampson 1904 pp.158, 168). The 1845/12/16 work rightly should have occurred at the beginning of the calculation of Hyp 1 (which indeed is where Adams places it in his published 1846/11/13 RAS presentation, M16:435) — not after its alleged 1845/10 submission to Airy.

63 Keep in mind: Hyp G is not a very farout speculation, since MemoC & MemoD physically exist, in CON (#32) & the Adams Neptune papers, respectively; and Hyp G is merely: their numbers corrected for the 3 small errors noted at §F4. (Since the positions for the MemoC planet given in the table at Sampson 1904 p.152 agree closely with those I get for Hyp G’s elements — given above at §F7 — it is clear that Sampson also essentially corrected the same errors in MemoC that I have noted: i.e., the small differences between MemoC & Hyp G.) The genuinely speculative part of the new theory here is: whether in 1845/10 Adams presented Airy with MemoR as he later asserted, or whether the solution given Airy in 1845/10 was actually Hyp G — i.e., effectively MemoC or its Hyp D.64 A priori, it is more reasonable to suppose that the sign-error involved was found not during the doublechecking of Hyp 1 (rather than during the parallel calculations for Hyp 2. mids. 1846). The 1690 residual disagrees by 0°: 44° 4 44° 5.5; but Adams mss section E IV p.16 p.44° 44° 45°, so this difference is merely a matter of discretionary rounding. By contrast to this unownted Adams-steadfastness, I note that just in the 10 weeks from 1846/9/2 to 1846/11/13, Adams altered by 0° 27 the 1843 residuals for Hyp 1 and Hyp 2. Compare M16:407 & 455. Note also that the ratio Adams used to extrapolate-compute his final (Hyp X) mean & true longitude of 315° 20’ (M16:407) is 14:11 (apt to U-N mean distance ratio 0.57), while his 1846/11/13 paper has 5.4 (apt to 0.575, the value cited 1846/11/13). (The corrected 1843 residuals would lead to an unsated distance ratio of 0.85.) Adams’ 1846/11/13 RAS paper states that this 5:4 ratio was sent to Airy on 1846/9/2, based upon the 1843, 1844, & 1845 Greenwich normal places of Uranus; however, the 1843 residuals are altered, as already noted, and the 1844 & 1845 residuals can hardly have influenced Adams’ 9/2 letter since that very letter states (M16:407) that he does not possess these normals and asks Airy to send them! I suspect that a good deal of selection of material (if not worse) went on before Airy’s 1846/11/13 publication of his version of the history. With the RGO Neptune file “missing”, we are conveniently protected from knowing all the details of this process.

65 GD 1.11.1 (Marinos 15° b 225°) vs. 1.14.10 (Polomy 12° b 180°).


8 Now rigidly defined as 1,852 m, the naut mi was originally designed to be 1° (gt-circ) on the Earth’s globe; and, naturally, C = 360° (60° 1’) = 21,600’. For the stade’s length (185 m ± 2%), see, e.g., E.Bumbry HussGeogr (1883) 1:209 & 624, EncBri 25:488H (1961), Random House Dict 1967: “stadium” = a.007 f (185 m).

8 Current Developments

A How Double-Sunssets Triggered the Discovery of America

A1 G.Corraface, star of the 1992 film *Christopher Columbus, the Discovery*: While making the film, “We were on those long boats for ten hours, before we finally set foot on the newfound land. And it was quite a relief, let me tell you, because everyone was getting sea-sick.”9 Ten hours. Mmm-Mmm, old Chris C didn’t know how easy he had it. A2 By the time Columbus-hype crested on 1992/10/12, every nonbelieving creature on Earth had learned that Columbus made his 1492 journey because he was confident that: [a] the globe was much smaller than it actually was, and [b] the Eurasian landmass wrapped much more around the globe than is the reality. His Earth-size estimate was low by about 25%.

1992 October  DIO 2.3  9

1992 October  DIO 2.3  99

1 Cinemas promo interview; aqed, e.g., 1992/10/13.

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

DIO 2.3 §8

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

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

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

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

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

B Heckathorn Scores Again

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

\[ \frac{15\text{th}}{1920} \text{October} \quad \text{DIO 2.3 \ §8} \quad \text{DIO 2.2} \]

F5 The elements of Hyp 1, as taken directly from the first section of the surviving photocopy of the document in question (MemoR):

According to my calculations the obs\( ^{62} \)-irregularities in the motion of Uranus may be accounted for by supposing the existence of an ext\( ^{7} \)-planet the mass and orbit of wh. are as follows:

- Mean Dist. (presumed nearly in accordance with Bode’s law) 38.4
- Mean sid\( ^{8} \) mot\( ^{9} \) in 365.25 days 1°30’.9
- Mean Long. 1\( ^{10} \) Oct’. 323°34’
- Long. Perih\( ^{11} \) 315°55’
- Eccentr\( ^{12} \) 0.1610
- Mass (that of Sun being unity) 0.0001656

F6 Hyp D is simply the 1845/9 solution Sampson found (on what I call MemoD) in the Adams papers. It is provided at Sampson 1904 p.166 (note the linguistic resemblance to MemoR):

According to my calculations, the disturbances in the Motion of Uranus may be explained by supposing the existence of a more distant planet, the mass, orbit, and position of which are as follows:

- Mean Dis. 38.4 (assumed nearly in accordance with Bode’s law).
- Eccentr\( ^{12} \) 0.1428.
- Long. Perih\( ^{11} \) 320°30’.
- Mean Long. about the end of Sept’, 1845 = 321°40’.
- Hence Geoc. Long. at the same time will be 320°30’ nearly, [dim.\( ^{7} \) about 1’ daily. Mass 0.000173, that of Sun being unity. J.C.Adams.

F7 The foregoing MemoD (which has MemoC’s discrepant \( e = 0.14 \)) corrects only error [b] (of the 3 Adams miscues listed in §F4). But perhaps all 3 errors were eventually cleaned up, leading to a (DR-specified) document bearing what I am calling “Hyp G”:

- Mean Dist. 38.4 AU (assumed nearly in accordance with Bode’s law)
- Mean Long. at end of Sept’ = 322°08’
- Long. Perih\( ^{11} \) = 321°00’
- Eccentr\( ^{12} \) = 0.1428
- Mass = 0.000173, that of Sun being unity
- Geo. Long. at end of Sept’ = 321°15’ nearly, dim\( ^{7} \) about 1’ per day

(\( ^{62} \) CON #32 or Sampson 1904 p.166 vs. M16:395 or SP 1. Note also the slip at M16:406 (or SP 3), accidentally substituting 101 for the correct 1846 date of Uranus’ opposition, 10/6 (M16:445, 453). Chalinos (SP p.) makes the earlier epoch explicit as 1845/9/30.)

\( ^{8} \) Thus, on the surface of Venus (where the atmospheric pressure is 90 times Earth’s), there can be no sunrises or sunsets. (Though, for solar altitude \( h = 90° \), atmospheric extinction would be stone-total.) This point is so unrecognized that W-Kaufmann’s well-known textbook Discovering the Universe (NYC 1989 p.135) innocently speaks of Venus sunrises.

\( ^{9} \) Such persons as E.Lehmann-Haupt & C.Sagan depend upon fiddling with the length of the stade, in order to make 252,000 stades seemingly equal the correct figure. (To their great credit, D.Dicks & O.Neugebauer wisely avoided and explicitly rejected following that evidence-twisting precedent.) However, it now turns out (as shown above) that the answer to the ancient mystery of these disparate \( C \)-estimates is not metrological but physical.
which genuinely first reached the South Pole on 1911/12/15, 10 weeks after the martyred loser of the S.Pole race, Britain's R.Scott. (Scott died on the return trip, late the following March — just short of a depot.)

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

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

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

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

59 Adams (M16:429, 1846/11/13, emph added): “After obtaining several solutions differing little from each other, by gradually taking into account more and more terms of the series expressing the perturbations, I communicated to Prof. Challis, in September, 1845, the final values which I had obtained for the mass, heliocentric longitude, and elements of the orbit of the assumed planet. The same results, slightly corrected, I communicated in the following month to the Astronomer Royal . . . .” Sedgwick’s 1846/126 account of an interview with Adams relays a similar story (Glaser 1896 p.xxviii): “I wanted [in 1845] to send my papers in good order to the Astronomer Royal. I went over all my calculations three times. I added a few [perturbation] terms, without changing my results. I was much interrupted, so it was my vacation [between Sept’s MemoC-MemoD and Oct’s Hyp 1 on MemoB] before I could finish my last revision”. Nowhere does Adams [a] admit the serious difference between the Sept & Oct solutions, or [b] discuss his Neptune-related activities between 1845/10 and 1846/6.

60 Sampson 1904 pp.152, 165. The other longitudes cited here (e.g., Tables 1&2) are DR’s calculations.

61 Section E III of the Adams Neptune mss at St.Johns College Library (Cambridge Univ); Sampson 1904 pp.165-6.
with Amundsen’s own field calculations shows that he used sphen trig. This should interest the Peary contingent, which has insisted for a decade that Amundsen did not “waste time” with sphen trig and that the variously-inert R.Scott was a fool for having done so. (Scott’s critics seem to imply that he virtually deserved his ghastly death, for the crime of having used sphen trig navigation! However, what killed Scott was not overprecise math but [a] censurable failure to anticipate adverse travel conditions, and [b] creditable determination genuinely to reach a Pole — instead of faking it, like some.) Indeed, Scott’s lately-much-lampooned navigation is utterly vindicated by the new findings, since (though both explorers would have been better served by cartesian navigation near the Pole), he and Amundsen used sphen trig equations of precisely the same form and computational difficulty.

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

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

[b] DR finds that Royal Geogr Soc Pres A.Hinks (whose views are uncomprehendingly promoted by the originators of the idea that Amundsen ignored transverse shots), in his learned 1944 article16 on the Amundsen-Scott 1911-1912 S.Pole data, states at p.169 that the Norwegian edition displays “facsimile reproductions of observation books”.

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


C1 In J.HA 1.2 & DIO 1.3, DR’s “Muffia Orbital” extensively admires the pioneering work of 1991 lead papers in the Journal for the History of Astronomy and Isis, who are: [i] lodging (& understandably promoting the originator of) Hist.sci’s unprecedented discovery of the WINTER EQUINOX16 — as well as [ii] rewriting the canons of gradeschool arithmetic,17 in order to promote certain precious Hist.sci tenets.

[a] The prominent JHA-Isis articles cited contend that none of three surviving Hipparchos trios of selenological observations (Almajest 4.11, 5.3&5) can be satisfied by trig-based (Greek style) solar orbits. So, DIO 1.3 happily supplied all three of the allegedly-impossible orbits18 — and noted that one of these Nonexistent orbits (satisfying the Hipparchos solar

14 In DIO 2.2 7, compare eq.3 (Amundsen) to eq.10 (Scott). It is strange that the truth of Amundsen’s steering method should have become lost and (nowadays) so universally contradicted, since (from working with the original Amundsen’s Mohn 1915 vowel) Amundsen’s computational procedure (DIO 2.2 15 fn.17).


17 Readers who possess the advantage of an elementary school education may wish to check the arithmetic found in the gov-funded Muffia paper selected as lead article for the 1991/5 issue of the extremely handsome Journal for the History of Astronomy, whose highest-ranking Ptolemy defender is JHA co-Editor and Harvard Hist.sci Dep’t head 0 Gingerich. (And the Muffia author’s followup paper led off the proud first Univ Chicago issue of the History of Science Journal for the History of Science, whose development, we learn: a) 128/128, & 68/30 = 65°05’, and [b] the solar mean anomaly (increasing at Hipparchos’ O): 98565355a days by 67/25 in 67.25 days! These adventures in Muffia-New-Math are at the very core of the Muffia’s attempt to prove that the DR-solved Hipparchos solar trio cannot be solved by Greek trig methods. But this is the JHA: so, expect no DOI-citing or DOI-quoting retraction — despite the aim-to-please publisher’s statement offering at the end of each DOI issue.

18 E.g., the EH orbit (founded upon Hipparchos’ earliest adopted solar cardinal points), epoch Phil: 1 Y = 365°14; e = 0.0226; A = 44°; e = 3°14’. This satisfies eclipse-trio B of Almajest 4.11. The same chapter’s equally “impossible” eclipse-trio A is satisfied by a hybrid meld of the EH orbit with the famous PH orbit preserved in the Almajest. Also found in Almajest 4.11 are the Hipparchos lunar ratios, which have defied 2000+ attempts at explanation (from Ptolemy through Muffia capo G.Toomer): (327 2/3)/3144 and (247 1/2)/(3122 1/2). In DIO 1.3, it is discovered during 1806 & 1846.57 Thus, Leverrier’s final prediction was superior to Adams’ (Hyp X) regarding not only longitude but distance.

F Speculative Reconstruction of Adams’ Actual 1845 Oct Solution

F1 But we have yet to come to the possible ultra secret of the Neptune affair. The original conspirators never published the 1845 Sept list of elements, given by Adams to Challis at that time (MemoC) — though all histories speak of this as the golden moment when Adams’ immortal prediction was lodged. (Indeed, Glaisher 1896 pp.xxvii repeats the typical version of the history in quoting Adams’ own rendition of what “bad luck” it was that, a few days after handing his solution to Challis, Adams missed dropping his 1845 Sept results off with Airy then, instead of a month later in Oct.)58 Upon noticing this, I wondered if I was getting near the solution to the peculiar Neptune case, that is: finally making some sense out of a story that has never made sense.

F2 Challis’ description mentions that this 1845/9 note (MemoC) included a geocentric place (unlike the published “1845/10” MemoR) for the end of Sept (Uranus’ opposition). So I knew what it was when I saw the 1845/9 list of planet elements, which I will call “MemoD” or (when referring to the elements) “Hyp D”. MemoD is printed innocently in Adams’ immortal prediction was lodged. (Indeed, Glaisher 1896 p.xxvii repeats the typical version of the history in quoting Adams’ own rendition of what “bad luck” it was that, a few days after handing his solution to Challis, Adams missed dropping his 1845 Sept results off with Airy then, instead of a month later in Oct.)58 Upon noticing this, I wondered if I was getting near the solution to the peculiar Neptune case, that is: finally making some sense out of a story that has never made sense.

57 Perihelion at 32.263 AU during 1826 (so near the JES time of passing Uranus that the coincidence might have suggested trouble for the solution). Around their later respective perihelions, Adams’ even more eccentric Hyp 1 & Hyp 2 also got comparably close, though for fewer decades: 32.22 AU & 32.78 AU, resp; so the important perihelion distance was increasing not decreasing, as Adams’ solutions progressed (& shrunk the most recent residuals).58 But if the solution was altered in the interim, which is the only defense insider R. Sampson can make, on the little known record we are about to explore, then one can hardly call it anything but good luck.
investigations.\textsuperscript{50} Though even freshman physicists are informed of Airy’s originality & intellectual gifts, Hist.sci chronicles of the Neptunian affair seem blissfully innocent of them. Numerous accounts — not the intelligent articles of Chapman & R. Smith, I am happy to say — routinely speak of Airy as a creativity-crushing dolt, whose dictatorial & bullying, hapless donkey”, “unusually conceited”, & “bitterly jealous of his assistants — or of any young astronomer.\textsuperscript{51} Fortunately, astronomer O. Eggen’s learned, near-simultaneous 1963/4 Sky & Telescope review provided a countering breath of sanity about Airy — plus a few gentle digs at the then-latest Hist.sci article’s inevitable technical-innocence slips, which have long provided such reliable entertainment for real science lovers.

E8 What eventually destroyed Leverrier’s personal lock on Neptune was that: [a] the real planet turned out to be only 30 AU from the Sun, not his final predicted mean distance of 36 AU (MNRAS 7.12:216, 1847/2/12; 7.15:270, 1847/5/14), while [b] Adams’ allegedly final, extrapolated solution, which we’ll call Hyp X (1846/9/2, M16:405-8), had predicted only 33.4 AU — twice as accurate (1846/12/17 letter of Challis Athenæum 1846/12/19 p.1300). Actually, it should be noted that 33.4 AU (Adams 1846/11/13; M16:456; Challis loc cit) is not what Adams said in his 1846/9/2 letter, which had Hyp X near-circular orbit radius 33.7 AU (M16:407); the 33.4 AU figure evolved from there via subsequent alteration of recent residuals. But the more important point no one has previously noticed is that, not only is the longitude of Hyp X way\textsuperscript{52} off (over 10° to the west of the real planet), but: when Adams announced this to the world for the first time (1846/11/13), the fact that Neptune’s distance was much less than 36 AU had already been known to him for about a month. See his 1845/10/27 (from Neptune’s 30 AU mean distance) & 1846/15/21 & 21 letters (Athenæum 1846/10/17 p.1069 & Astronach 25:106). It is also worth noting that since Hyp X had null eccentricity, its hypothetical planet was actually at 33.7 AU, whereas during the period of greatest Neptunian disturbance of Uranus (the decades near their 1821.74 heliocentric conjunction: see longitudes of Table 1) — the prime basis of both men’s math after all — Leverrier’s predicted planet was actually (due to high eccentricity) at distance less than 33 AU: crossing that boundary

\textsuperscript{50} Would Adams have accepted help from some other person deputed to help clean up his solution quickly before publication? Sampson 1904 (p.163) discovered that some of the work in preparing equations (M16:440-1) for the suspect Hyp 1 is not in Adams’ handwriting. Whoever it was (and fn 67 provides Challis’ assurance it was not he), the main question is: how could this assistive occur in 1845 if Adams was alone & smug?

\textsuperscript{51} See the long-standard college physics textbook Resnick & Haliday which, e.g., at p.735 of the 1970 edition illustrates and credits the Airy disk.

\textsuperscript{52} Newman 1963 p.175 speaks of Airy’s: “consumming despotism”, etc. Question: why is that the same folks, who have the most admirable passion for equity, are simultaneously (all too often) so woefully unable to separate real from dubious cases of injustice? — and so prone to waste precious energy concentrating upon the latter, e.g., the sagas of Frederick Cook, Sacco-Vanzetti, Wilhelm Reich, Joan Little, Greta Rodarte. Possibly there is a softheart-sotheaded correlation; also, the public likes its false-misdiagnosis-dramas to be simple, stark, cinematically; while the real ones usually aren’t, while the fabricated ones invariably are.

\textsuperscript{134} If the Shellshocked Mufa has lately begun uncertainly admitting that maybe some of its data from Hipparchos after all, Mufa still usually avoid telling their readers that Ptolemy insists he personally\textsuperscript{22} observed the whole Catalog outdoors with his alleged armillary astrolabe.

\textsuperscript{20} DIO 11.3 6 [C2] will contend that all 5 of those exceptional Catalog stars, whose longitudes end in 15°, were fudge-tailed by Ptolemy when he rigged longitudes of Venus and the eclipsed Moon, longitudes which he pseudo-real upon real astronomers’ observed conjunctions of the bodies with these stars. (The 5 stars’ longitudes therefore were not obtained merely by adding 2°40’ to Hipparchos’ longitudes.) The 5 stars (conjunctions): 43Gem (Venus 140/7/29-30), 15Y Vir (Venus – 271/10/11-12), 766 Vir (Moon – 133/3/20-21), 49 Vir (Moon – 719/3/9 & 62/3/14-15), 29 Psc (Moon 90/8/28-29). Three of these cases are attested (Almajest 7.19, 7.2 & 5.1). The 720 BC eclipse is a Ptolemy favorite (Almajest 6.9-6.9.6.9). See also DIO 11.3.6 (at the very time the Neptune case was brewing) had invented calendronic energy heights. (Unless we count Bill Stern’s legendary little Billy episode in the 1942 Lou Gehrig film bio, Cried in the Hankees.)

\textsuperscript{22} The Newman 1963 account is precious (thus its appearance in our limited bibliography here) — as an epitome of the unlikely popular myth of ogre Airy rebuffing & stomping wuvable Adams. Not since an earlier J.Newman’s circle was cooking up its astounding Lives of the English Saints (at the very time the Neptune case was brewing) had inveigled hagiography (e.g., fn 37) been elevated to such heights. (Unless we count Bill Stern’s legendary little Billy episode in the 1942 Lou Gehrig film bio, Cried in the Hankees.)

\textsuperscript{50} The import of this lay unnoticed until Rawlins 1969.\textsuperscript{51}

\textsuperscript{52} Two of them Challis’ then-precocious predisclosure ones of 1846/4/12 & 1846/4/15 — uniquely useful for a little while, because it required no more than the long-Angle study of light & Challis a greater time-span of data than anyone else had at hand. I.e., in 1846 Oct, Cambridge Observatory was the only one in the world that had made (& knew it had made — vs. others at j7 [B1] observations of the real Neptune over a period of months instead of weeks. So Challis’ search was not without utility (a point oft forgotten); indeed, the resulting data permitted the considerable mathematical talents of Adams & Challis to give Cambridge University a genuine and nontrivial priority in this affair: the first to publish the correct distance of Neptune.

\textsuperscript{20} DIO 11.3 6 [C2] will contend that all 5 of those exceptional Catalog stars, whose longitudes end in 15°, were fudge-tailed by Ptolemy when he rigged longitudes of Venus and the eclipsed Moon, longitudes which he pseudo-real upon real astronomers’ observed conjunctions of the bodies with these stars. (The 5 stars’ longitudes therefore were not obtained merely by adding 2°40’ to Hipparchos’ longitudes.) The 5 stars (conjunctions): 43Gem (Venus 140/7/29-30), 15Y Vir (Venus – 271/10/11-12), 766 Vir (Moon – 133/3/20-21), 49 Vir (Moon – 719/3/9 & 62/3/14-15), 29 Psc (Moon 90/8/28-29). Three of these cases are attested (Almajest 7.19, 7.2 & 5.1). The 720 BC eclipse is a Ptolemy favorite (Almajest 6.9-6.9.6.9). See also DIO 11.3.6 (at the very time the Neptune case was brewing) had invented calendronic energy heights. (Unless we count Bill Stern’s legendary little Billy episode in the 1942 Lou Gehrig film bio, Cried in the Hankees.)

\textsuperscript{22} The Newman 1963 account is precious (thus its appearance in our limited bibliography here) — as an epitome of the unlikely popular myth of ogre Airy rebuffing & stomping wuvable Adams. Not since an earlier J.Newman’s circle was cooking up its astounding Lives of the English Saints (at the very time the Neptune case was brewing) had inveigled hagiography (e.g., fn 37) been elevated to such heights. (Unless we count Bill Stern’s legendary little Billy episode in the 1942 Lou Gehrig film bio, Cried in the Hankees.)

\textsuperscript{50} The import of this lay unnoticed until Rawlins 1969.\textsuperscript{51}

\textsuperscript{52} Two of them Challis’ then-precocious predisclosure ones of 1846/4/12 & 1846/4/15 — uniquely useful for a little while, because it required no more than the long-Angle study of light & Challis a greater time-span of data than anyone else had at hand. I.e., in 1846 Oct, Cambridge Observatory was the only one in the world that had made (& knew it had made — vs. others at j7 [B1] observations of the real Neptune over a period of months instead of weeks. So Challis’ search was not without utility (a point oft forgotten); indeed, the resulting data permitted the considerable mathematical talents of Adams & Challis to give Cambridge University a genuine and nontrivial priority in this affair: the first to publish the correct distance of Neptune.
Now, at JHA 23.3:173-183 (1992/8), comes forth our favorite Muffia comedian-entertainer-satirist, Noel Coward\textsuperscript{23} Swerdlow (Univ Chicago Dep’t Astron, & Advisory Editor for the extremely handsome JHA), to pronounce his judicious\textsuperscript{24} quietus upon the Catalog controversy, in a pseudo-delayed paper called “The Enigma of Ptolemy’s Catalogue of Stars”. Indeed, NCS thinks his remarks are so smart and valuable that they should be the last word on the subject! (See NCS 1992 p.182.) And he calls skeptics demented?\textsuperscript{25}

C3 NCS still hides from debating skeptics. But Mr.Hide is also Dr.Jekyll. Evidently sobered by DIO 1.1, NCS has mated his previous abusive style (e.g., calling dissenters work “crazy”, “garbage”, etc: fn 23). Now, donning (publicly) the mask of Jekyll’s civility, he even poses as an arbiter of academic etiquette! (Humor-wise, that’s on the level of appointing NIXON to clean up dirty political can-do campaigns — or placing DIO in charge of good taste & style in academe.) Who else but the incomparable Lord Hoskin-0 Gingerich JHA Editorhip would select NCS (whose Hyde-side applied to the late RN the unretacted & Hist.sci-uncriticized libels “liar”, “crank”, & “con-man”: DIO 1.1 \& 3 \& 5D) as the ideal JHA AdVd to call RN & DR insufficiently polite? (See it happen, at NCS 1992 p.176.)

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

C7 Skeptics, starting with Tycho Brahe (who also faked a bit of his own justly famous 1598/1/2 star catalog),\textsuperscript{22} have always suspected that Ptolemy observed no stars but instead copied Hipparchos’ star catalog,\textsuperscript{26} thereby adding 2°40′′ of precession (mistaken by −1°.1) onto Hipparchos’ longitudes. In his Muffia-hated 1770s Johns Hopkins Univ book, The Crime of Claudius Ptolemy, physicist R.Newton revealed a startling fact: though the unaltert star latitudes exhibit the expected excess of 0′′ endings, the longitudes’ most common ending is 40′′ — exactly what one would expect if an indoor astrologer had added 4°23′.\textsuperscript{27}

It should be clearly understood that DIO’s nickname for Swerdlow is strictly based upon his Noel-Cowardesque talents as humorist — talents displayed, e.g., in Ist & JHA (sampled in DIO 1.1). His courage is unquestioned, since he and his Muffia friends have been brave enough to call Ptolemy-skeptics loony and incompetent for 20′′, while avoiding face-to-face public debate: DIO 1.1 \& 1 \& C & 7 \& 3D3.\textsuperscript{27}

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

DIO 2.1 14 shows that Tycho’s largely magnificent epoch-1601/03 catalog of 1004 stars (hitherto neither numbered nor even counted by Hist.sci)\textsuperscript{28} contains 10 faked stars: the first 6 stars of Oph, Tycho’s stars D675-680 (entirely invented); and all 4 stars in Cen, Tycho’s stars D1001-1004. (The Cen set was computed from fake longitudes \& real declinations — the latter probably observed at Wandsbeck, not Hven.) Tycho’s method of fabrication was essentially as follows: first, the Catalog’s preface accuses Ptolemy of “unimportant” for: just add a precession constant onto the longitudes of a predecessor’s star catalog, while not changing the latitudes. (Tycho: for Oph, add 24° to Hipparchos’ longitudes; for Cen, add 21° to Ptolemy’s longitudes.) Then, unlike heedless Ptolemy (whose data were invariably rounded to 5′ or 10′), Tycho tossed in a very few arcmin of scatter, so that the “observed” data would look real. (Tycho always rounded to arcmin or simple fractions thereof.) These 10 stars’ errors are gross by Tycho’s standards but they agree very closely with fabrication — and they are from the sole 2 subsets of his 227 final-rush 1596-7 stars for which no underlying data survive. Curiously, Evans’ 1878 JHA paper belies the fact that stars were real, because he neglected to properly weight these stars’ work. Instead, he invented his own laboriously-developed extinction formula: Evans 1878 pp.259-260, 267-271. Use of Evans’ formula produces magnitude 7.95 for 2g Cen at Tycho’s Danish observatory! As further shown at DIO 2.1 \& 4 fn 65, the massive Evans-JHA attack on DR includes also Evans (1878 p.168) claims which require naked eye observations of stars as dim as tenth magnitude, by Evans’ own formula. Well, 10th magnitude may be dim; but, if we were to assign a magnitude to the brilliance of refereeing at the extremely handsome JHA, could a mere 2 digits do it justice?\textsuperscript{49}

E4 The obvious implication here (especially for anyone familiar with such work): publication of Adams’ elements was being delayed out of fear that an error vitiating them would be found in the supporting calculations before the latter were published. But such a policy only makes sense if the possibility was being entertained by Adams’ mentors (\& maybe Adams) that the 1845 elements might perhaps be altered before publication in order that they fit polished-up final-version calculations. And this realization tells us just what the eventual official British rendition of events is worth on its face.

E5 So Adams waited until 1846/11/13 to release his hypothetical elements to the public. This may have been wise in one sense (the subtlety of the published Adams paper’s grasp of the relevant math quickly deflated French suspicions on that point). But the delay puts an ineradicable cloud over the version of events and the purported solutions subsequently produced.

E6 This cloud is only darkened by another peculiarity which no historian has remarked, probably because all either regard Airy as an enemy of Adams or are so loyal to Airy that they don’t like the obvious implications. Just after the discovery of Neptune, while Airy was returning to England, he stopped by Altoona (on about\textsuperscript{45} 10/5) to see Schumacher, the Editor of the eminent Astronomische Nachrichten. There is no record that he told Schumacher that he was about to support a British claim to co-discovery. Instead, what Airy did (as he admits in his 1846/10/14 letter to Leverrier, in a parit usually omitted in modern accounts but fortunately surviving at Glaisher 1896 p.xxiii) was to read carefully Leverrier’s extensive manuscript explaining the mathematics of his discovery — this over a month before Airy’s co-conspirator Adams got around to publishing a digit of his own math! (Leverrier’s ms was sent the AstrNachr 1846/9/8 for immediate publication, not for Airy’s private perusal\textsuperscript{46} — however much his read-through eventually moved Airy to creditable praise for Leverrier: see \S 3.)

E7 This ms later appeared at AstrNachr 25:53-80, 1846/10/12-22 (a final post-discovery Leverrier paper was also published at AstrNachr 25:91-92, 1846/11/5). The last date (10/22) shows that not only Leverrier’s orbital elements but the crucial details of his math had been published well before Adams had publicly committed himself in either department. Adams’\textsuperscript{49} release of his results was at the RAS meeting of 1846/11/13, so Airy’s peek at Leverrier’s math occurred over 5 weeks prior to the public debut of Adams’ elements. In order correctly to evaluate this point, it helps to know that Airy was a skilled mathematician (remembered for the Bessel-function-related Airy integral \& Airy disk),\textsuperscript{47} and he possessed expertise\textsuperscript{48} in some areas of celestial mechanics (having earlier discovered an important new Venus-related secular term in the solar tables) and thus was one of the very, very few persons in England who could understand anything of substance in Leverrier’s paper. The possibility of his noting, e.g., which perturbational terms were included & omitted — such information would (at the very least) have been useful for advice\textsuperscript{49} to Adams, even in spite of the various differences between the two...
[a] doubt that he yet had finalized them, and [b] denial of credit to him for Neptune.

D7 Though asserting on 1846/10/1 that Adams’ hypothetical elements had not been completed until (§B2) 1846 June, Challis had been “mortified” to find on 10/12 (see the pathetic uncensored version of M16:412-413 at DSB 3:186-7) that he had seen (and come within a whisker of capturing) Neptune on 1846/8/4 & 8/12, during the clandestine Cambr Obs search. History generally regards this near-miss as a tragedy for Adams&cco. I regard it as miraculous justice for Leverrier.

D8 Within days after finding Neptune in his 1846/8 records, Challis was transformed: he announced (Athenæum 1846/10/17 p.1069) on 1846/10/15 with a wave of the flag that an Englishman was responsible for the first observations of the planet and publicly proposed “Oceanaus” as its name — appearing to believe that his chance for immortality was yet retrievable.41 Challis’ seizure (one may almost call it that) at this juncture upset any possible Airy hope that his 1846/6/29 conspiracy would remain unknown. The public was now aroused to patriotic fervor for hero Adams, and soon sought a scapegoat to excuse his “bad luck” — did Challis know, when he unleashed the mad beast Nationalism, that he himself would become the prime sacrificial goat in the British rendition of the Neptune story?42

E Adams’ Waiting Game

E1 Meantime, the French, already frothing over Adams’ suspiciously late claim, were increasingly wondering aloud: where were Adams’ numbers? (See Athenæum 1846/10/31 p.1117.) But, just as the notorious Dr.F.Cook refused to release his 1908 North Pole “observations” until these alleged data had been carefully gone over,43 Adams waited. And waited.

E2 This delay seems to have been by plan, since the very first public announcement of Adams’ work knowledgeably anticipates it: “Mr. Adams . . . will, doubtless, in his own good time and manner, place his calculations before the public.” (The statement of co-conspirator J. Herschel 1846/10/1, Athenæum 1846/10/3 p.1019, Emph added.) When the skeptical French got openly impatient, the eminent British weekly Athenæum (1846/10/31 p.1117) assumed, as always, the rôle of sage & neutral arbiter: “Mr. Adams’s claim, whatever it might be [DR: this is over a month after the discovery!], should not be lost by an early [!] statement of the facts upon proof of which it is to rest — they [French skeptics] have hurt themselves, not us.” [DR: I like the “us.”] The facts of the discovery are not fleeting: . . . they rest on records on paper: . . . [Adams’ claim] is brought forward . . . in the shape of a statement to be substantiated as soon as the calculations and observations [!] can be published. Why, then, all this heat?

E3 Which of course evades the central point as neatly as Dr. Cook did: the 1845 Adams elements were simple bottom-line numbers which could have been produced — by succinct letter to the Paris Observatory and-or by publication in 1 cm of type in the Athenæum — at any time, without the full supporting calculations, exactly as the elements of the real Neptune instantly were produced.44 without supporting calculations, only about 3 days after discovery of some of the observations on which they were based. (Leverrier gave his final predicted elements to the Academy on 1846/8/31: Leverrier 1845-6 p.432. His detailed underlying perturbational calculations were sent to the Astronomische Nachrichten only 8 days later, even when no one was suspiciously pressing him; see his 1846/9/8 cover letter at AstrNacht 25:53-4.)

2°40’ onto Hipparchos longitude data originally having the expected excess of 0’ endings.

C8 NCS starts his attack on the famous fraction-endings proof with the astonishing claim that RN’s argument had assumed (NCS 1992 p.176) “an instrument graduated to half-degrees”. This statement is false. (Expect no DIO-citing retraction.) And it suggests why NCS has such passionate sympathy for the exposed plagiarist C.Ptolemy. The NCS mis-statement that RN assumed half-degree division is identical to the same error originated by G.D.Rahlhoff History of Ptolemy’s Star Catalogue Springer-Verlag 1990 pp.85, 88 (n.172!), C8 saying that RN’s argument had assumed (NCS 1992 p.176) “an instrument graduated to half-degrees”. 0 pretends to abhorrence of “abusive” writing — but does 0 seriously believe that merely using others to carry out his intents will successfully divert intelligent onlookers from recognizing the midlevel archon responsible for the low nature of the gang attack on RN’s findings? 0 is certainly not fooling DR, who knows from a private 1974/11/15 meeting with 0 (at Goddard) that, even that far back, 0 was personally acting as a Mufla agent — verbally dishing the Mufla slander that RN, an internationally respected physicist & a section chief at the Johns Hopkins University Applied Physics Lab, was simply a crank. (This rumor was spread so aggressively by Mufla&co, behind RN’s back, that it was accepted for awhile even at Scientific American, from whose people I heard it in 1978-9. Why shouldn’t Mufliss duck open debate, if academe lets them get away with fighting opponents this way instead?)
the same page on which he revealed his unfamiliarity with RN’s book — NCS has already demonstrated that he is touchingly deficient in the ability to trace the math of DR 1982C. The DR paper contained a hitherto-unsuspected crucial-experiment revelation: if Ptolemy observed the Catalog with his $-1^\circ.1$ longitudinal mis-set, this would also cause $29^\circ$ error-waves throughout the Catalog: $\Delta\lambda = 29^\circ \tan \sin\lambda$, $\Delta\phi = 29^\circ \cos\lambda$. But these (HUGE) waves are not found in the Catalog, thus (to an uncommitted critic) it is obvious that Ptolemy did not observe the Catalog. Predictably unwilling to accept that this simple test proves anything, NCS states (1992 p.176) that $\Delta\lambda$ would “be undetectable for stars within, say, $25^\circ$ of the ecliptic and [would] produce nonsense for stars within $25^\circ$ of the poles.” Hilariously false on both counts.31 (By its account, the JHA had a year to referee NCS 1992: fn 59.)

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

[a] Peters has provided the Catalog’s standard latitude error curve via 36 normals, spaced at $10^\circ$ longitude intervals around the zodiac. But Peters does not state the formula

which NCS permits the reader to believe was Peters’.

[b] As a sinusoidal fit to the Peters error curve, NCS’s formula is grossly mistaken.²⁰²²

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

²⁰²² See analysis at Rawlins 1982C p.361 & Fig.2 there.

Regrettably, when the same sinuosities are compared with the Peters’ $\Delta\phi$ curve, there are inconsistencies. (Of the Peters series, the whole is far more magnified than in NCS’ sinusoid.) In the absence of the RGO Neptune le, we have no way of knowing what Adams’ other hypothetical communications to Greenwich (besides that of 1846/9/2) may have speculated regarding Neptune’s distance.

D5 When on 1846/6/1 Leverrier published his first heliocentric longitude estimate ($325^\circ$ for 1847/1/1), he naturally used a Titius-Bode-based mean distance $38$ AU. (Real Neptune is at $30$ AU.) This also was Adams’ value at the time. On 1846/8/31, Leverrier announced on the floor of the Academy a reduced distance for his planet. The mails from France took a day or two to reach England (as is clear from Airy’s Neptune “Account”, M16); on 1846/9/2, Adams for the first time communicated to Airy that he had decreased his own planet’s mean distance. (The 1690 residual was the only residual Adams possessed in 1845 which he later used as a post-calculcation measure of whether his hypothetical planet’s distance was too large or too small. It was degraded not improved by decreased distance.) It is clear from Adams’ 1846/7 MemoW that only about 2 months earlier he had had no idea whatever of where Neptune’s distance was greater or smaller than the Titius-Bode value ($\approx 31$ item [c]). In the absence of the RGO Neptune file, we have no way of knowing what Adams’ other hypothetical communications to Greenwich (besides that of 1846/9/2) may have speculated regarding Neptune’s distance.

D6 It is odd enough that Adams published nothing on Neptune before its discovery. An extraordinary claim has not that been hitherto emphasized in any modern history is that Adams’ public silence³⁸ regarding his supposed elements continued for well over a month even after the real planet’s 1846/9/23 discovery. Not that Adams wasn’t heard from: on 1846/11/5 he joined Challis in publicly congratulating a journal for its helpful news coverage of the controversy (Athenæum 1846/11/7 p.1148-9), having already (1846/10/15 Athenæum p.1069) attempted publicly with Challis to propose a name for Neptune, “Oceanus!” (All with Airy’s private pre-approval: CON #18 1846/10/14, “I like your name Oceanus.”) The sheer nerve is admirable. (Hind scoffed 1846/11/12 to Sheepphants that “Oceanus” had about the same chance with foreigners as ’Wellington’.) In brief, while Adams was saying plenty (contra the modern legend: fn 40) — now that the planet was found — he was talking about everything except the elements upon which his claim rested. The ability of this is highlighted by the realization that Adams published (via Challis’ 1846/10/15 letter: Athenæum 1846/10/17 p.1069) elements for the real planet nearly a month before he published the elements he allegedly predicted for the planet in 1845! (When astrologers perform this way, we don’t take them entirely at their word, either.) Challis’ 10/15 letter also promised “Mr. Adams’s investigations will, in a short time, be published in detail.” But what were wanted swiftly at this juncture were not full details but simply: the predicted elements. Adams’ failure to produce these right away legitimizes
D  Neptuné's Discovery Brings Adams & Challis to Life

We note that Adams asked (through Challis, 1845/9/22) and was asked (by Airy 1845/9/29) to write his prediction to Airy (M16:394-5); but, instead of sending dated letters, Adams allegedly preferred to wander in personally and deposit undated scraps of paper! (By contrast, Airy wrote Sheepehanks 1846/11/17 relative to publishing Adams' paper: "It is important that you should note on Adams' paper the day when it was received." Compare to §E1 item c) & fn.36.) Adams had published orbital elements of a comet in 1844 (Grosser 1862 p.82) and had delivered a paper to the RoyAstrSoc on 1846/4/8 (MNRA 7.6:83). If he had a Neptune orbit he himself trusted (the issue which is at the heart of this controversy & the key to his loss of priority), then: why was this not the subject of Adams' 1846/4/8 RAS talk? Is it any wonder that the French were incensed that only after the discovery, Adams claimed he knew all about Neptune?

2D The French were robbed of priority by British maneuvering, 37 the most outrageous part of the process being that, as the theft proceeded, strong public French expressions of suspicion were used to show how irrational and undeserving the French were! The 1846/12/5 Athenæum p.1246 refers to the great physicist & top French astronomer F.Arago's "distorting mirror of national bias" and his "mania". Airy to Sheepehanks (1846/11/23): "I am sorry to see that the feeling of the French towards Challis amounts to hatred. (This has arisen entirely from Challis's imprudence in writing only on one side of a question at one time.)" (On 1846/11/5, Airy gently advised Challis to be careful about this: CON #21.) Co-conspirator J.Herschel tells his diary (1846/10/25) that he not Leverrier is the injured party: "Wrote to Guardian in reply to M Leverrier's savage letter [10/21] — These Frenchmen fly at one like wildcats —". (Herschel's diary contains nothing whatever on Neptune before the discovery, though [a] he was in on the search after 6/29, [b] co-conspirator Peacock visited Herschel 8/7-9, & [c] Herschel announced the prediction of Neptune at the 9/10 BAAS meeting at Southampton, but without mentioning Adams' name: Athenæum 1846/10/3 p.1019.)

D3 Let me cite some items which suggest that French suspicions were apt & proper — even commendable in a policy sense — and that Adams' actions exhibit some temporal relationships to Leverrier's publications which, curiously, have never previously been spotlighted. (Note also the near-simultaneous chronology in Challis' 1846/10/1 letter, quoted in fn.30.)

37 The traditional British version of the Neptune tale has little Adams being ignored by big Airy in 1845. The actual big-vs-little tale is rather different: little (in international astronomical politics) France was outdeceived & outpoliticized by big Britain in 1846. It is a measure of scholars' overwhelming sense of political suppression and unfairness in academy that the poor-neglected-Adams legend has gained such wide currency. (The sense of inequity is legitimate, but that does not ensure the truth of the instances often popularly held to illustrate it: fn.32.) The legend blames Airy's & Challis' paralyzing distrust of Adams' math. I agree, but with the key addition that this mistrust was primarily Adams' own. (See, e.g., §F1-§F3.) Assuming the record is real, there was no ignoring of Adams: Challis' 1845/9/22 letter of introduction (M16:394) of Adams to Airy said: "I should consider the deductions of his premises to be made in a trustworthy manner." Airy's reply to Adams' 1845/9 visit was a letter to Challis (1845/9/29 CON #42) asking him to tell Adams: "that I am very much interested with the subject of his investigations and that I should be delighted to hear of them by letter from him." After receiving Adams' 1845/10 note, Airy wrote Adams a friendly inquiring letter (1845/11/5, M16:396-7) and simply got no reply. (Though, the rendition at Newman 1963 p.175 invents a nonexistent Adams reply anyway.) The letter asked Adams whether the hypothetical planet which accounted for Uranus' longitudinal wanderings also explained Uranus' anomalies in the radius vector. (Chapman 1988 pp.126-8 prominently & repeatedly confuses Uranus' radius vector perturbations with Neptune's mean distance, which is irrelevantly found regularly in the extremely handsome Journal for the History of Astronomy where the process of meaningful refereeing is as mythic as anything in the Neptune affair.) Chapman's paper adds useful material to the Neptune controversy, but his ritual attack (p.136 n.6) upon the "heroes-&-villains" approach justifiably hammers home standard nonjudgemental Hist.scii. air of superiority to those who attempt an ethical review of history. And his supposition (Chapman pp.129 & 131) that Airy believed that publication establishment priority is based on Chapman's innocent reading of Airy's motives. This guess is as unchecked as it is cocksure. In fact, Airy explicitly countered this view in learned detail in a public letter of 1847/3/18, published in the 3/20 Athenæum p.309. (The effort is so obviously special pleading for Adams' priority that Airy tries to deny that. A key false claim in this letter is discussed at fn.19.)
out this effect (in order to find out what errors still need explaining)\textsuperscript{41} of course depresses the sine coefficient to virtual nullity. And it must be equally depressing to those attempting to pretend that large unexplained error waves may redeem Ptolemy. Once this known (not conjured-up) effect\textsuperscript{42} is removed from the actual (Peters) latitude errors, the best-fit sinusoid is merely $\Delta \beta = 9 \cos \lambda - 4 \sin \lambda = 10 \cos (\lambda + 25^\circ)$; this $10^\circ$ amplitude is catastrophically far below the $29^\circ$ amplitude that must exist if Ptolemy observed the Catalog.

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

C16 A final note on the Peters graph of the actual zodiacal latitude residuals: NCS refers (\S C11) to the “complex” shape of its curve. I.e., the curve’s 2\textsuperscript{nd} major peak (centered on $\lambda = c.300^\circ$) is too broadly flat for a pure sinusoid. In typically sterile selective-agnostic Muffia fashion, NCS sees this situation strictly in a weh-we-barely-slipped-out-of-that-clefstick light — instead of asking: how can we use this curve’s peculiarities to find out whose solar theory is indicated as having been adopted by the observer of the Catalog’s zodiacal latitudes? Inspired by NCS’s comment (and I am happy to acknowledge the debt), it took DR a few hours (1992/10/19-20) to derive and check out the solution. So it will be fair to give the Muffia a month (1992 Nov) to work on the same problem. The solution will be published in an upcoming number of DIO. Our offer: the Muffia capo (Toomer, Sward, Aaboe, or B.Goldstein) who, during 1992 November, is first to call us up (phone: 410-889-1414) with the correct solution, and who is able to describe a valid math derivation of it, receives a free one year DIO subscription. (Just what every Muffie dreams of finding under his Christmas Tree . . . .) Hopefully more attractive: a published note of admiration (acknowledging a Muffia share in this provocative discovery), to appear in the first 1993 issue of DIO. [Note added for 1995 reprint: Solution printed at DIO 1.2 (\S C1993) fn 152.]

C17 Delambre (1817) and DR (1892C) emphasized the total absence of stars from the c.5\textsuperscript{t} band of southern sky which is visible from Ptolemy’s Alexandria (latitude $L = 31^\circ 12^\prime$) but invisible from Hipparchos’ Rhodes ($L = 36^\circ$). So NCS uncritically follows the alibi of Evans (1987 p.166) and says (NCS 1992 p.176-177): “the object of Ptolemy’s catalogue was merely to list the stars in and near recognized constellations, and since these were formed around the Aegean there was no reason to include additional stars not then included within constellations even though visible near the southern horizon in Alexandria.” DR’s comments: [a] Ptolemy’s version of the Catalog (Almajest 7.5-8.1) contains dozens of stars explicitly labelled by him as “unformed” stars not belonging (though loosely attachable) to the traditional constellations. [b] While most of the constellations Ptolemy adopted were established by the time of Aratos (c.275 BC), Ptolemy is perfectly capable of breaking old tradition and states that he has done so “ . . . the descriptions which we

\textsuperscript{41} While the sine coefficient is primarily due to obliquity-setting error, the cosine coefficient’s non-nullity merely reflects the fact that the sky moves a little during the few moments between the astrolabe-observer’s setting of range ($r^\text{A}$) and the observer’s range ($r^\text{B}$).\textsuperscript{42} For Hipparchos, the real obliquity was $23^\circ 42^\prime 7^\prime$, so (assuming Almajest 1.12 is correct in saying that Hipparchos also used Eratosthenes’ obliquity, $23^\circ 51^\prime 20^\prime$), his obliquity-related error wave would be $–9^\prime\text{sin}L$. Instead of assuming that a given attested obliquity must have been accepted by the observer, DR 1982C instead used the actual error wave (Peters’ curve) to solve for the observer’s adopted obliquity — which came out as $23^\circ 56^\prime 3^\prime$ (DR 1982C eq.27).

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

\textsuperscript{C5} Among scholars today, the widely rumored belief is that the RGO Neptune file was borrowed (and never returned) by the astronomer Z, who used material from it in several publications. Missing from the “missing”-rumor is the fact that, around the time the file disappeared, Z was the Chief Assistant to the Astronomer Royal at RGO. The most likely gainers from this file’s disappearance are not Z but: [1] a British legend, and [2] the RGO’s reputation.\textsuperscript{34} If we ever want to see the RGO Neptune file, the plan of inaction is obvious: cease all mention of Adams as a “co-discoverer” of Neptune until the file resurfaces.\textsuperscript{35}

\textsuperscript{C6} The “missing” Greenwich Neptune file includes numerous key documents critical to reconstructing British activities, including the central document of the case, Britain’s holiest Neptune-chase relic: Adams’ three-page memorandum (allegedly 1845/10C:21) transcribed from F1 planet’s elements to Airy. I am calling this MemsR. (MemoR is now available only in photographic facsimile: SP pp.lvi-lviii and Jones pp.15-17. See Chapman 1988 p.125 n.21: “Original untraceable at RGO, presumed missing in ‘RGO Neptune file.’”). It is this “lost” 3 page document that is the physical basis for Britain’s claim of priority.

C7 And I will here announce that the date, “1845 October” on this document: [1] was added later, and [2] may be false. Why such a severe suspicion? Start by looking closely at the date on the photographic reproduction: [a] The date is distinctly darker (than the rest of the text): it was obviously added with a different writing instrument. (Pen vs. pencil?)\textsuperscript{b} The handwriting (for the date) is not Adams’ but Airy’s! — a fact not previously noted by any scholar. [c] A date lacking the day of the month obviously is written later since on the date itself the writer knows what day it is. (Is it credible that Airy — unequalled in record-keeping at RGO — would not date this memo immediately upon receipt?!)\textsuperscript{d} After noting this, I checked the first publication of the document (M16:395, as part of Airy’s 1846/11/13 presentation): the date (as a part of the document) is there lacking.

C8 The document given to Challis in 1845 September also lacked date (CON #32; Glaisher 1896 pp.xxx-xxxii). We will call this MemoC. Challis later wrote “September 1845” upon MemoC, since, again, Adams himself didn’t date it. (Adams tended to write dates on his important calculational results: \S H1.)

C9 The situation is therefore that both of the key 1845 documents (MemoC & MemoR), the entire basis of Adams’s claim to have predicted the planet’s place ahead of Leverrier: [a] cannot now even be assigned a precise date (and thus be checked against participants’ records of location & other activity), [b] were at best lying around in Airy’s (now missing) & Challis’ files for months without date, [c] were dated much later from memory by them, not Adams. If we take the case to its anti-conspiratorialist’s real sole refuge in this case (e.g., \S C1-[\S C2 & fn 67], one must ask: are scientific historians expected unquestioningly to accept such a record?

[a] Shackleton himself knew none of this, and [b] Marshall judged a bit (putting the party within 100 mi of the Pole) strictly in order to indicate his magnificently courageous chief to turn back, before the party plunged fatally over far into what the title of Shackleton’s popular book rightly called The Heart of the Antarctic (which he was the first ever to seriously penetrate). (Marshall’s later remorse may have been over whether his slight 1909 exaggeration contributed somewhat to Scott’s narrowly-fatal 1911-1912 overconfidence in Antarctic longitude-speed by manhauling.) There is no question that Shackleton’s 1909 exceeding of previous latitude records represented the greatest single latitude advance in the history of man: over 5 degrees at one excruciatingly risky leap (88° vs. the Scott-Wilson-Shackleton 1902 record of 82° 17’ 51”), which shaved survival-odds right up to the human limit.

\textsuperscript{34} It is unarguable that the RGO is plenty sensitive about this case. Z’s accounts defend Airy by portraying Challis as a crank & idiot. At the 1946 centenary, Astronomer Royal Jones actually attempted to suppress Smart’s defense of Adams (reported to DR by R.Smith 1989/7/28) — evidently because it reflected so badly upon Astron Royal Airy. The safest prediction DR ever made was that the “missing” RGO file is someday “found.” DR will not forward until another (politically Reliable) scholar has published a suitably-mild, soften-the-blows version of its contents.

E.Maunder records (as quoted at Smith 1989 n.27): Airy “devoted an entire afternoon to himself labelling a blotting-paper he would duly endorse the blotting-paper with the date and particulars of its use, and le it away

DIO 1.2 (\S C1993) fn 152].
Adams’ result reached him “in manuscript” before Leverrier’s 1846/6/1 paper; but the statement does not say anything about 1845 nor (since Airy saw Leverrier’s 1846/6/1 paper only on 6/23–24) does it preclude Adams’ completed Hyp 1 possibly reaching Airy very roughly at the time (June) specified by Challis’ initial public account (just cited above: text at fn 30). (If Adams’ work was handed to Airy in 1845/10, then shouldn’t Airy have said that it was completed way earlier than Leverrier’s, e.g., “last year” — rather than just: reached Airy first?)

C2 Now, the official documentary history has it that Challis wrote the 1845/9/22 intro for Adams’ visit to Airy, stating to Airy that Adams had “completed” his work on Neptune (M16:394). Since Challis was at the famous 1846/6/29 Greenwich meeting three quarters of a year after that “completion,” and when the secret search was not an act of chance or an invasion of Airy’s private space, then the whole Catalog must have been completed at the very recent 1846/6/1 publication, how could Challis possibly have gotten the impression that Adams’ work was completed at roughly the same time?22 Unless it was. (Or even later. But if Challis was right, then Hyp 1 — which will be often called MemoR in discussions below — was actually sent Airy not in 1845/10 but about early June of 1846.)

C3 Such a contradiction (as the Challis 10/1 letter presents us) is enough in itself to justify terminating our faith in the traditional British rendition. It may be true, but there must always be a question mark over it. No such question mark attaches to Leverrier, so he should always receive primacy in mention of Neptune’s discovery. The sudden 10/1 Challis & Herschel letters (mailed only hours after the news of discovery reached the writers) suggest possible pre-plan (Airy was abroad at the time) or collusion. But the more trenchant question: can a Cantab clique keep Adams’ name secret for months until just after the discovery and then claim a piece of it? Do we want to encourage this sort of claimjump? For a century, Airy’s Neptune correspondence file was sealed (à la Peary’s records). At the centenary (1946), two prominent British astronomers (Astronomer Royal H.Jones & W.Smart of Cambridge Univ) saw the file & shortly published accounts based on it, but without specifying its location. Some years later, it was found that the whole file had gone “missing”.

of an external planet, and upon this there are two remarkable calculations. One is by Adams of St.Johns [Cambr U] (which in manuscript reached me first). The other is by Le Verrier in the Comptes Rendus of 1 June 1846, which and a prior number of this issue (1845/10/1) strongly recommend you to consult. Both Adams & Le Verrier new at the same rate, viz. the present [celestial] longitude of the said disturber must be somewhere near 325°.” Smith 1899 also quotes (n.27) another portion of this important letter: if “I were a rich man or had an unemployed staff I would immediately take measures for the strict examination of that part of the heavens containing the position of the postulated planet.” And see §B1. But note his great caution at fn 15.

C22 An innocent interpretation is that Challis correctly realized that Adams’ work was not complete in 1845, which is my position and is the obvious reason for Adams’ nonpublication. But this destroys any British claim of priority, so it could not be admitted: this truth is perhaps the main secret of the Neptune affair, and I expect that it would be verified by the “lost” Airy papers. When such a key file is gone and meantime we are told nothing regarding the substance of communication between Airy & Adams between 1845/10c.21 & 1846/9/2 (not even when or so much as whether Adams ever saw Le Verrier’s Comptes Rendus papers before the discovery!) nobody should accept the British version of this history — especially if he is familiar with how another British leader’s underside was protected by institutional censorship, namely: what the Roy.Geog.Soc. & widow Kathleen Scott did to Robert Scott’s South Pole diary before publication. This notorious bowdlerization assisted in the curious historiographical process whereby most British children ended up believing (as a justifiably disgusted Amundsen reports in his 1927 autobiography, My Life the South Pole!) that Scott discovered the South Pole! — just to the tatter that Amundsen Pole! — 4 weeks ahead of Scott, who died while returning, partly from the disappointment of manhandling sledges hundreds of awful miles just to find that the South Pole looked like a Norwegian flag. As it may perhaps interest those reading of the Dogrib tradition that Amundsen’s account contains — one of Hollywood, Lourdes, and Las Vegas. The ultimate enshrinement of Amundsen may have hinged on a seemingly unrelated event: in 130 AD, the Emperor Hadrian was sailing on the Nile with his young Bithynian lover, Antinous, when the lad drowned . . . . Hadrian was emotionally shattered: he established a cult and named towns . . . . in the dead boy’s honor . . . . Immediately after the death, the re-mounting weight of evidence has reduced Mufasa to this feeble last ditch. Thus has an unsubtly transparent intellectual inertia gradually sucked defenders ever-deeper into a hodgepodge of ad-hoc exercises in special-pleading (for each separate suspicious Catalog circumstance: see §C22), a thicket of disconnected alibis — sorely in need of a mop-job by Occam’s Razor.

C23 E.g., to try answering RN’s fraction-endings argument and (inauditly) deflate DR’s error-wave-advance to “merely” 20’, NCS (1992 p.177) promotes Evans’ inepe-
posed Ptolemy-observing scheme, which: [a] was a Velikovsky-style victim of Collective Amnesia (since neither Ptolemy nor any other ancient astronomer ever mentioned it — NOR DID MUFFIOSI, until recent RN&DR-proposed crucial-testing cornered them), and [b] is more wildly comic than the surreal sobriety-test fantasy in the cinema-farce The Man With Two Brains. (Hollywood screenwriters have to use drugs to get this high. How does the Muffia do it?)

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

[a] An utterly GROSS — 1°.1, mean longitude error [Tycho].
[b] Absence of large 29’ amplitude error waves in northern longitudes [DR].
[c] Absence of large 29’ amplitude error waves in latitudes [DR].
[d] Longitudes with more 40’ endings than 0’ endings [RN].
[e] Longitudes with more 10’ endings than 30’ endings [RN].
[f] Absence of a near-quarter-degree constant error in celestial latitudes β [DR]. (Such an error is roughly entailed by Ptolemy’s false assumed geographical latitude46 for Alexandria, L = 30°58’ = atn[3/5], which he swiped from Vitruvius’ crude, 2-century-old equinoctial ratio, shadowlength:gnomonheight = 3.5. See DR at Vistas in Astronomy 1985 p.267 n.6 and at Amer J Physics 1987 p.236 & n.15. Alexandria’s actual L is 31°12’N.)

[g] No stars in the c.5° band of sky visible from Alexandria but not from Rhodes [Delambre].

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

C24 Even for the tiny but indicative sample of stars where Ptolemy copies errors of several degrees40 from Hipparchos, NCS still isn’t finally convinced. NCS 1992: “a few stars may have come from Hipparchos” ([C19], “but I think this part of the analysis should be carried further” (p.180, emph added). Note the remarkable coincidence that: the only evidence (bearing on whether the Catalog was stolen), which NCS desires Further-Research into, is that which (even he thinks) looks bad for Ptolemy as things now sit! This tactic is a failureous way of repeating the fraud found long ago was standard among the very Velikovskians.

46 Let’s see, we start by setting ring 5 NOT on the chosen fundamental star’s ACTUAL longitude at ring 3 but rather at the nearest whole-degree value LESS than the original value; then, after sighting the stellar quadary with ring 2, we read where ring 2 meets ring 3 AND THEN ADD, ONTO THIS READING, THE AMOUNT WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE… Got it? (Evans at JHA 1987 p.243 — including his couple of these.datum-misreport [303°03’ rendered as 303°05’], in the NCS tradition admired at DIO 1.1 ≡ ‘5 fn 7.’) Can RN-DR be accused of cruelty to dumb animals, given the tightness of the evidential Dixon vs. the poor Muffia? To watch prominent scholars thrashing about in such pathetic credibility-death agonies is akin to viewing Animal Rights films of stouts caught in spring traps. Trying to weasel out.

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

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

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

50 α Cen (Graloff’s discovery), 0 Gem, θ Eri. See Graloff 1990 pp.189, 291-2, 307-8, 313-4, 326, 331, 333.


B9 By poetically-just good fortune, the planet was discovered, within about 1° of Leverrier’s predicted spot, on 1846/9/23 (at the Berlin Observatory, by J.Galle & H.d’Arrest) on Leverrier’s written 1846/9/18 instructions (following his final published predicted Neptune place: 1846/8/31) — to all the British conspirators’ lifetime chagrin.

C Post-Discovery Secrecy & the Old Missing-File Routine

C1 A previously unremarked but critical point: Leverrier, by publishing his prediction (before his optical discovery), took all the chances of embarrassment29 if no planet turned up; published British attempts to take a share in the glory were wholly invented after discovery. (I do not utterly reject the idea either, given the lack of supporting documentation in continuous records such as minutes or diaries [IL].) But [a] I should not have to make that judgement (& would not, had Adams published even before the late Berlin Observatory discovery), and [b] it is undeniable that the British claim to Neptune is needlessly fishy: e.g.,

[1] No publication until more than 7 weeks after Neptune’s discovery at Berlin.


[3] The astounding fact (only mentioned in passing as a minor point in Chapman 1988 p.133 n.43; 1988/5) that the very first public claim for Adams (by co-conspirator Challis, 1846/10/1 letter to the Cambri Chronicle; retracted 10/16: both newsclips preserved, as CON #15&16) stated that Adams’ work was completed only in about June of 1846!30 Note also that co-plotters H.Sercial’s letter of the same date (1846/10/1; Athenæum 1846/10/3 p.1019) likewise makes no claim whatever that Adams’ work had any priority over Leverrier’s. (R.Smith’s important find, Airy’s 1846/6/25 letter31 to Whewell, does say that selection of objective was Airy’s.) This was the basis for the Airy 1846/7/13 plan’s estimate that the triple-sweep he recommended (zones of breadth 1°/4) would require only about 80 hrs per sweep. When Challis’ 7/18 letter resisted, Airy modestly & mercifully to his delusion that this overtedious approach was correct — even urging (M16:426) its adoption for all transit work, a suggestion which convinces one of [a] his personal dedication to hard work, & [b] his lack of Airy’s intuition regarding this eventuality later became real. In 1859, Leverrier discovered the non-gravitational precession of Mercury’s apoapse, which we now know is due to relativity. (This was one of the great discoveries in the history of astronomy, something that darkened his later years.

30 If one wishes to view this 1846/6 “completion” remark as merely claiming that Adams’ Hyp 1 was not a complete solution, then [a] it grossly exaggerates the earliness of the known dates of Hyp 2 (1846/8/18,9,2); and [b] it still destroys Adams’ priority. The Challis 10/1 text: “About four months ago, Mr. Adams, of St. John’s college, and M. Le Verrier, an eminent French mathematician, concluded independently from theoretical calculations, that anomalies which had been known to exist in the motion of the planet Uranus, could be accounted for by supposing a perturbing planet to move in an orbit at twice the distance of Uranus from the sun. These mathematicians agreed in fixing in 35° of heliocentric longitude at the most probable position of the supposed planet, which has proved to be very little different from the actual position. Le Verrier more recently inferred … that the mass of the disturbing planet was to that of Uranus in the proportion of 5 to 2 a result which Mr. Adams also arrived at (1846/9/2) by continuing his researches). For the last two months I have been engaged in mapping the stars in the neighborhood of the supposed planet. [illegible] A method eventually successful. The last investigations of Le Verrier came to my knowledge on Sept. 29. On the evening of that day I observed strictly according to his suggestions, and out of a vast number of stars which passed through the field of view (power 160 [vs. in 41. I selected one only, against which I directed my assistant to write ’seems to have a disc.’ This was the planet. (Cambrist Histoire Astronomique 1803/3 [emph added]).]
was never informed of Adams’ researches. Even more remarkable: while walking at Cambridge on 1846/7/2, Adams by chance actually bumped into Airy & the esteemed Hansen (Smart 1947 p.34); but nothing of Adams’ work was mentioned. (Apologists brush this aside by saying the meeting lasted but “a few minutes”. Comments: [a] Hansen’s visit with Airy lasted 24 days. [b] Had Adams at the 7/2 encounter simply mentioned to Hansen what he was up to, the meeting would have lasted a whole lot of minutes. [See fn 75.])

B7 Challis started his sky-sweep 1846/7/29, and from that date through 8/12 worked (at magnifying power 250) near the center of the search region: the point on the ecliptic at longitude 32°. (See Challis’ Neptune zone records, Cambr Obs.) At Airy’s 6/30, 7/9, & 8/6 urgings (CON #1, 2, & 6), Challis agreed on 8/7 to add RGO computer 52 J.Breen (Airy 6/30: “a rough genius… perfectly tractable”) to the search which also included Challis’ assistant Morgan. The apparent cover story was that Breen was just going to the Cambr Obs for a “month’s trial” toward acceptance to the post of Junior Assistant there (see Breen’s letter: CON #8 1846/8/8). From 8/14 through 9/18, Challis examined the western part of the region, “purposely” (AstrNachr 25:102), since that’s where the Sun would first encroach later in the year — but it’s also the region where Adams’ latest work seemed to be pointing (8/20 was the date on his Hyp 2’s first rough solution: Sampson 1904 p.167). Independent British astronomer J.Hind (of Geo.Bishop’s private observatory in Regent’s Park) wrote Challis on 1846/9/16 (CON #10) that he had recently heard of Challis’ search (possibly at the recent BAAS meeting). Hind’s letter mentions the fact that he and the French astronomer H.Faye were also preparing to search for “the new planet”. (In his letter, Hind strikes this expression and replaces it with “Le Verrier’s planet”. See also CON #13, where Hind thanks Challis for his recent “kind” letter; and compare to Hind’s letter further quoted at Rawlins 1984N & above at §B5 — when he learned that Challis had at this very time been keeping from him Adams’ confirmation of Le Verrier’s predicted position for Neptune.) Faye is quoted by Hind as expecting to spot the planetary quarry by searching for a disk of diameter about 2", following Le Verrier’s 8/31 advice.

B8 Hind’s letter would have been received by Challis on 9/17 or 18. After 9/18, Challis returns (starting 9/21) to the center of the search area, right where Le Verrier was pointing, presumably looking for a disk. Challis’ later accounts (1846/10/21 AstrNachr 25:102; 1846/12/16 SP lii) distort this history by [a] ignoring the possible rôle (in 18) of Adams’ latest results in pushing him west after 8/12, and [b] suppressing the fact that he had been following Le Verrier’s (not Adams’) instructions ever since 9/21. 53 Challis instead publicly claimed that he had switched back to following Le Verrier’s guidelines only on 9/29, the time the Concourse’s very last night, when Challis’ zone records state that he saw Neptune’s disk.

NCS compares RN to. Recall also the comment at DIO 1.112 in 7 (which might almost have inspired NCS 1992): “Parapsychologists, UFOlogists, & Ptolemites . . . prefer unending data-collection, thereby [ducking] the shame of having pursued & promoted a false path for decades.” NCS’ conclusion: “After more than a century of serious [read: Mufa] and not-so-serious [read: RN-DR] research into Ptolemy’s catalogue of stars, the fundamental question of its originality remains unanswered” (NCS 1992 p.182). Or, as another humorist is about to express it for us (C25): research on the Catalog is back to Square-One.

C25 The overwhelming array of evidence against Ptolemy ensures that skepticism on Ptolemy will continue, so the loyal Mufa will stand ever vigilant to defend its weirdo catalog. (Apologists brush data-collection, thereby [ducking] the shame of having pursued & promoted a false path for decades.” NCS’ conclusion: “After more than a century of serious [read: Mufa] and not-so-serious [read: RN-DR] research into Ptolemy’s catalogue of stars, the fundamental question of its originality remains unanswered” (NCS 1992 p.182). Or, as another humorist is about to express it for us (C25): research on the Catalog is back to Square-One.

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

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

SECOND SCIENTIST: Beats me, Bob!

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

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

THIRD SCIENTIST (entering the room): Hey, can you two guys lend me a hand?25 I need to screw in a light bulb.

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

We now step back to size up the general portrait of Ptolemy that has evolved from decades of Mufa apologia. NCS 1992 (p.175) adopts the excuse of Laplace 28 & Gingerich (Science 1976/8/6) for Ptolemy’s C26 – 1 mean Catalog error: maybe it’s just caused by the similar mean error in Ptolemy’s solar theory. DR’s comments:

[a] This argument directly inspired the DR 1982C absent-error-waves test, which definitively refuted the Laplace-Gingerich alibi. (Rawlins 1982C, eventually published by a real science journal, was originally submitted to JHA -- Editor-for-Life (EFL) Lord Hoskin in 1976 & 1977. His Lordship refused even to referee it. The JHA has now spent years — consuming scores of its extremely handsome pages — trying to justify its original 1976-7 mistake by vainly attacking this DR paper, using pseudoscience packaged as scholarship.)
b) NCS’ preferred vision of Ptolemy is of a scientist who spent years observing 1000+ stars outdoors with his astrolabe — yet never, during all this time, did NCS’ ancient precursor-in-geniusdom manage to notice that his observatory’s latitude L was off by -1° — an amount virtually equal to the solar semidiameter. Nor did Ptolemy ever realize (during at least 8 years of alleged solar observations, 132-140 AD: Almajest 3.1 &7) that the real Sun’s position differed from his tables by -1°.1! This error renders the easily-observable equinoctial solar declination off by c/2a.half a degree, an amount equal to roughly twice the solar semidiameter. (Such a fantastic error would instantly be revealed by transit circle or astrolabe, both of which Ptolemy claims to have regularly used. Heck, even an instrument as crude as an ancient astrologer’s asymmetric gnomon can do a lot better than this.)

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

C27 Since 1987, the JHA, utterly captured now by the Mufa, has published at least 7 pieces on the Ancient Star Catalog (running over a hundred pages in all). All seven have been from the pro-Ptolemy side of the controversy. So, now, the JHA publishes the capper to this 5 year demonstration of its idea of equity, by suggesting (NCS 1992 p.182) a “moratorium”. (And one notes that neither of the 1992 JHA papers cites DR’s 1991 analysis at DIO 1.1.6, which provides yet more novel evidence, positively attaching Hipparchos’ solar work to the Catalog’s zodiacal longitude error curve, with an ordnag 1°-precision match of amplitude.) I.e., now that the JHA has fired its last (for-as-long-as-We-feel-like-it) pro-Ptolemy shot on the Catalog, just in under its own welltimed moratorium wire,29 the JHA decrees it would be best to just end the Catalog controversy right here. Megalomania rarely achieves such heights of unreality.

C28 Unrealer yet: NCS unreels a proposal for more “research” (§C24) — even while calling for his moratorium. (It doesn’t take a linguist to translate: [a] NCS wants a moratorium on the chaos of conflicting Mufa claims — which he is now himself so brilliantly augmenting! — that has left the Mufa looking about as convincing as Ptolemy. But NCS wants no moratorium on Mufosi continuing to try to figure out new allibis for Ptolemy. [b] Given its tenuous hold on reality, the JHA perhaps even imagines that DR will submit a paper directly to the JHA in response; so, while it has left open the possibility of publishing some more of its own incomparable Mufa research on the Star Catalog, JHA’s “moratorium” is now in place, in print, as a pre-set official-excuse for rejection of a [believe me, PURELY] hypothetical direct30 DR submission. Isn’t the JHA a treasure?

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

C30 Seldom has a party of “experts” been so utterly defeated (and by scholars it exiled as fools) — so bare of substantial, coherent retort31 — that its ever-so-clever strategists 21 The MemoW ephemeris was entirely computed for a circular-orbit planet moving sidereally at exactly 15°/day, mean distance 9.5 AU. The actual (236°/6 period of the ms page of MemoW) a fit to 2 missing stars, including Wartmann’s. The MemoW scheme: for 1846/8/29 heliocentric longtud θ, the 8.92 geocentric longtud is found from the formula [Earth helioc longtung being set = 336°.4 on 8/29; θ = atan(θ/336°.4)(38.25 - 1)]; the central geoc motion (6/9 to 8/29) for the basic interval of 20 days was found using helioc motions 1°/12 day & 1°/9 (Earth); motion = (-1° - 38.25°/12)/27.25 = 0°. Const 2nd diff = -5° for θ = 315°; -3°, 320°; -2°, 325°; -1°, 330°; 0°, 335°.

22 In Adams’ defense, it should be noted that at the very bottom of MemoW, he mentions his perturbation-solution prior to the Wartmann-based result.

23 I believe Hind (among other initial British supporters of Leverrier’s claim) later became more orthodox-British regarding Adams; whether from pressure or from mistaken conviction, I cannot say.
examine not only the inevitably stark evidence of Airy's 1846/6/26 silence to Leverrier regarding Adams (this vs. Airy's key 6/25 letter to Whewell 24° before: §A & fn 31) — but also the eyeopening remarks concluding Airy's brief 1846/8/6 letter to Challis (regarding Breen's availability for the secret search; CON #6, hitherto unpublished except at DIO 1.1 §1 fn 10): as Airy left England to vacation on the continent, he told Challis to (while Airy was out of the country) write to his Main man, "write to Mr.[Rob't] Main [2nd-in-command at ROG] who is fully in my confidence and understands the position of the whole matter." In the Neptune context, can anyone doubt that this is one plotter writing another regarding who else may be trusted with the secret?  

B4 Search-designer- overseer Airy outlined & advised the celestial hunt's strategy in a series of letters starting the very day after the 6/29 plot was broached: 6/30, 7/9, 13, 21 (CON #1 to #5; see also M16:416). On 1846/7/18, Challis agreed to conduct the clandestine sky-sweep18 for the planet. The actual telescopic observations began on 7/29. Challis was from the outset privately guided by what we will here call "MemoW" which Adams computed & gave to Challis: ephemerides of geocentric places (for hypothetical planets at various heliocentric longitudes), for 20-day intervals starting 1846/7/20. The result was a monumental fiasco, now almost universally attributed to Challis' mental shortcomings.  

But, in extenuation of Challis' troubles: one ought to be apprised of a critical item which is unrecognized in any history of the Neptune scandal, namely: from 1846 July to Sept, Adams erratically provided Challis with hypothetic planets at heliocentric longitudes ranging from 336° (MemoW, CON #35) to 315° (Hyp X, M16:407) — a range of over 20°! (Tables 1 & 2, below, provide 1800-1850 ecliptical longitudes corresponding to predicted-geocentric longitudes.) Challis' long-lampooned indecision in his search was not due to a personality disorder (as is now commonly & abusively charged) but rather to Adams' conflicting directions for him. Another equally remarkable & heretofore-unknown point: Memo W's 20-day-interval ephemerides, the document guiding Challis' search, was not based on Adams' now-famous perturbation-computed Hyp 1 orbit-prediction (see fn 19) but rather upon a combination of: a) Flamsteed's lost19 start #1007 & Wattmann's weird 1831 alleged planet-sighting [note added 1997: see P.Baum & W.Sheehan In Search of Vulcan NYC 1997 pp.83-84 n.15], b) Leverrier's published longitude limits, & c) a circular-orbit distance (38 1/4 AU), not elliptical (Hyp 1). The last point is devastating to Adams' claim. And

18 The Challis 1846 search's observations (CambroObs archives, courtesy D.Dewhirst) suggest that, at the very time when Adams was arriving at his extrapolated solution Hyp X (up until 9/18-21: see §B8), Challis was looking in that solution's position, about 10° west of the planet's actual place. (In general, the correlation is not so sharp as to constitute proof of a connection.) When Challis was suggesting orbital inclination 12°1/2 & node about 300° thus putting the planet c.5° north of the ecliptic, Challis (for only the second time in the search) on 1846/9/15 actually looked outside the region Airy's plan had specified, virtually on the spot Adams was pointing at.

19 CambroObsNept file (CON #35) (1 page; the other 2 pages are post-discovery and thus not crucial), largely unpublished & hitherto uncheckd by any historian of the affair. These Adams computations are crude (remarkably so, for a mathematician being compared to Leverrier), based on a patently-invalid constant-second-difference arithmetic scheme (fn 21) exhibiting some impossible asymmetries about oppositions, and none of them genuine perturbationally-based orbits. Note: Challis' false public implication (M16:421), that MemoW's ephemerides is based on Hyp 1, is essential to Britain's crucial claim that Neptune was first seen on 1846/8/4 & 8/12 due to Adams' noncircular-orbit perturbational calculations. (Airy follows this sham in his 1847/3/18 letter defending Adams' priority: note the feebleness of Mufa attempts in this direction: "all five of these analyses are as one in swearing that Ptolemy was wise and honest. (You might think there is a wee difficulty here — like, maybe you suppose that there is something slightly dishonest about swiping, without acknowledgement, the labor of an astronomer who observed 1000+ stars. If you believe this, you will never make the grade in Hist.sci. Happily, Harvard Hist.sci Dep't head 0 Gingerich 1981 p.43 & Grasshoff 1990 p.215 will set your ethics straight for you.")

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

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

D1 An occasional nervous-neophyte Muffia may momentarily worry that the foregoing revelations could disturb granflow. Seasoned veterans of the game know better: happily, Hist.sci grants have not the slightest (positive) correlation with the grantee's accuracy or genuinely expert original scholarship. (As DIO readers know all too well.) So, we can relax. (Likewise, professional astrologers' amusing inability62 to compute horoscopes correctly has no effect at all upon their clients' generosity.) Further: by this time, so many Hist.sci. authors' reputations have been invested into the glorification of Muffia scholarship, that the cult cannot be permitted to be seen as having erred catastrophically in anathematizing RN-DR. Therefore, our favorite Unsinkable cult will positively insist on

by skeptics have been so pathetically thin that it is by now all too clear that the Muffia klan has simply been bluffing in this regard. (Note the feebleness of Muffia attempts in this direction: DIO 1.3 fn 252.) See DIO 1.1.5 fn 6 for Hist.sci. including Muffia) precedents for publishing lengthy error lists to attack authors.

62 Toomer was being convinced by Grasshoff at least as early as 1986.

63 Rawlins Skeptical Inquirer (Skino) 2.1:62 (1977) pp.73, 76-77; Rawlins 1984A pp.974-976.
keeping its course and will slide right past DIO’s iceberg. With barely a sound or a shudder. On its part, the Muffia must wonder why, despite years of archonal conspiring to ostracize RN-DR’s heresy, the hated 64 heterodoxy persists nonetheless. (Even Time-Life’s popular Hoaxes & Deceptions p.108 accepts that the Rawlins 1982C analysis, of the Ancient Star Catalog’s southern boundary, indicates that this “Ptolemy” Catalog was actually observed in Hipparchos’ Rhodes, not Ptolemy’s Alexandria. See §C17.)

D2 The DR-Muffia double-tarbaby-fracas will continue indefinitely, because: [a] DR positively won’t stop publicly admiring Muffia gyrations, so long as the Muffia insists on its snobbish & effectively censorial minimum-citation-practice, based upon its equally ludi-
cious WE’re-the-only-experts-around-here pose. (I.e., DR is asking that the Muffia acquire some fundamental ethics and integrity. But who’s going to fund the brain transplants?) [b] Muffia mout’pieces are irrevocably committed to forever clinging to their precious pretense that DR’s historical scholarship is utterly worthless. 65 This point is so sacred to Muffiosi that, in order to maintain the Muffia, they will pay any price (primarily: internal rot) — and, in order to cloak its ineptitude with the trappings of Reputability, will woo into its muck just as many major academic institutions as it is able to con into sharing that price. (Terseness borrowed-with-credit from etiquette-authority NCS’ IC5-sampled lexicon.)

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

D4 Question: why can’t you kill 68 a lawyer? Answer: what do you hammer the stake through?

119 1992 October  DIO-Journal for Hysterical Astronomy 2.3 ¶8 114 1992 October  DIO-Journal for Hysterical Astronomy 2.3 ¶9

64 Yes, hated. See DIO 1.1 ¶1 [ §7 & fn 20; 13 §D2-D3.]

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

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

67 DIO dedicates this joke to another joke: the Neugebauer Muffia — in honor of that cult’s highly original notions of 1846, that history and decency, not to mention its unquestioned talent in sucking tax monies out of the system, to fund its defense-lawyer fantasies.

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

68 The title of R. Newton’s 1977 Johns Hopkins Univ book (The Crime of Claudius Ptolemy) is still freaking out H史-science archons, e.g., HAA Editor-for-Life & renowned Britwit, Lord Hoskin. See DIO 1.3 [B2.]

18 Also, in Airy’s letter of 1846/7/21 (CON #5), he refers to “looking for the planet” and then takes the trouble to correct that last to: “possible planet”. See also fn 31.

69 It is sometimes supposed that Adams’ youth explains his peculiar behavior. Chapman 1988 n.53 rightly notes that Adams was 27, the same age at which Airy had assumed the Directorship of the Cambridge Observatory (1828-1835). Chapman adds (loc cit): “One suspects that [frequent contemporary] references [to Adams’ youth] may have more to do with Adams’s manner and the way he appeared to people, than with his age in years.” The foregoing strikes me as consistent with a person who was a scholar first and a politician last: all to the good.

19 Smith’s language in the previous sentence (just before that in which he rejects the secrecy-hypothesis) is curiously contrasting. After noting a case in which the “Cambridge network” helped to generate publicity, Smith adds (p.418): “But in the case of the Neptune discovery we see that the Cambridge network could be used to restrict information as well as to disseminate it.” How could a scholar who totally rejects the secrecy-hypothesis, compose the 3 words DR has italicized? If these words are struck (along with the “to” following), the sentence is then consistent with the nondeliberateness-theory Smith is loyal to, throughout the rest of his paper.
The Neptune Conspiracy

British Astronomy’s Post-Discovery Discovery

Summary

Britain’s J. Adams is generally believed to be the prior of the 2 pre-discovery locators of Neptune via math analysis of its gravitational disturbance upon Uranus’ orbital motion. However, for reasons still vigorously disputed, he published none of his alleged 1845 perturbational mathematics until 7 weeks after Frenchman U. Leverrier’s 1846 publications & 9/18 letter had caused the planet’s telescopic discovery at Berlin on 1846/9/23. Detailed evidence is presented\(^1\) indicating that, throughout 1846 Summer, Cambridge University astronomers conspired to capture Neptune by keeping Cantab Adams’ work unpublished while smoothing out his early secret that 2-mn’s math had independently put it to the same celestial position for Uranus’ unknown perturber. It is concluded that Leverrier ought to be recognized as the planet’s sole discoverer. In addition, a new hypothesis\(^2\) is proposed below, which accounts for a few of the worst of the Neptune affair’s hitherto intractable mysteries, and which might (partially) exonerate the legend’s prime popular villains.

A Misbehavior & British Gentlemen

A1 Basing his work upon misbehavior in the motion of Uranus, the brilliant & adven­
turous young Cambridge U mathematician John Couch Adams appears to have in 1845
arrived at a theoretical prediction of the elliptical position (near Cap-Agr border) of the
giant planet Neptune, then unknown. This is the same jovian planet that the wonderful US
spacecraft Voyager 2 visited 1989/8/24, thanks to NASA.

A2 Adams is widely held to be the true first predictor of Neptune’s position and is hon­
ored for this achievement by a memorial in Westminster Abbey near Isaac Newton’s tomb.
However, Adams’ rôle in the discovery was actually nil, and his behavior has always been
inexplicably murky — a point I will expand upon below, adding a novel, partly speculative
hypothesis which entails: [i] a solution-switch by Adams, & [ii] a high official’s possibly­
conscious back-dating of the controversy’s key document. This admittedly uncertain new

\(^{11}\) To Babbage’s credit, he later examined all the “accessible” documents and concluded that Leverrier deserved prime discovery franchise.

\(^{12}\) This is not a piece of popular science writing. Though much of the paper is accessible to anyone of intelligence, the analysis is essentially written for specialists. Those unfamiliar with the Neptune affair are urged, before proceeding here, to first read at least one of the various readily-available well written accounts of it, e.g., H.H. Turner 1904 Chap.2, M.Grosser 1962, or R.Smith 1989.

\(^{13}\) Text given more fully below: fn 67.
A5 Starting the same year as that alleged for Adams' Hyp 1 (1845), the at least equally famous mathematician Urbain J.L. Leverrier independently computed Neptune's position and announced it (Comptes Rendus 1846/6/1) virtually the same celestial location.

A6 Upon reading Leverrier's published paper (1846/6/23 or 6/24), Airy swiftly & secretly set in motion a huge Cambridge Observatory telescopic sky-search (§8 & §B1) — and, as part of the secrecy, deliberately suppressed news of Adams' confirmatory 1845 British researches (outside a small Cambridge U circle), in his 1846/6/26 letter to Leverrier, Airy never mentioned Adams' prior work — this despite the fact that just one day previously, in a 6/25 Airy letter (discovered by R.Smith's industry), he mentioned to a Cantab confidant (Wm.Whewell) both Adams' & Leverrier's agreeable planet predictions (on equal terms: see fn 31). Airy then did not respond to Leverrier's 6/28 detailed reply that this was anything other than deliberate secrecy (as modern apologists pretend) is directly contradicted by Adams' own common sense remarks: “I did think that the Astronomer Royal would have communicated my results among his correspondents. I took all that for granted and considered it [Adams' 1845/10 transmission to Airy of hypothetical orbit-elements] a publication”, (Letter of Adam Sedgwick to Airy 1846/12/6, written just minutes after conversing with Adams; Glaisher 1896 p.xxviii & Smart 1947 p.41. Chapman 1988 p.139 n.57 has found that both of two copies of the letter are now missing; below at fn 12, I suggest a possible reason for that particular oddity. This is part of a series of Neptune ms disappearances suggesting systematic suppression of documents, a situation encouraging some otherwise unthinkable speculations.)

A7 However, by his own just quoted criterion, Adams was, as we shall see, himself obsessively secretive, not publishing anything before Neptune’s 1846/9/23 optical discovery on Leverrier's instructions — and Airy's approval to do so. All of which casts doubt upon the sincerity of his 1846/12/6 remarks just quoted (§A6).

A8 These were made at a time when British public opinion was bitter against Airy, who saw (or came to see) that the need for a hero had made unacceptable his original post-discovery admission that Leverrier’s case for discovery was superior to Adams'. The “hero” Adams — and so he was called — was also useful as an inspiration to a revival of British mathematics from its early 19th century low state, so rightly and famously lamented by the revolutionary Cambr Univ trio of C.Babbage, J.Herschel, and G.Peacock, who had founded the Analytical Society at Cambridge a generation earlier (c.1813) to encourage British math to catch up to the Continent. (The pre-trio situation may be gauged from the fact that when Uranus was discovered at Bath on 1781/3/13 by Wm.Herschel, no one in Britain was able to find an orbit from his & the RGO’s observations of the new planet!) Note that all 3 of these Cambridge men were at the infamous little 1846/6/29 Royal Greenwich Observatory (RGO) Board of Visitors meeting at which the secret Cambridge search for Neptune was hatched

theory offers the prospect of clearing up some of the mysteries of the legendary Neptune tale (which I first investigated over a quarter century ago) — justly regarded as the prime predictive sensation in the history of astronomy. The “Neptune Controversy”, which has continued for over a century, centers on several contended questions, most particularly: [a] Should credit for Neptune’s discovery go to the Englishman Adams, to the Frenchman Leverrier, or to both? (The last position is fine by Britain, since Adams’ work is supposed to pre-date Leverrier’s.) [b] Which Brit was primarily responsible for the 1846 Summer secret-sky-search fiasco at the Cambridge Observatory? (The hitherto orthodox answer: Camb’r Obs director J.Challis. The present paper rather vindicates Challis.)

A3 In retrospect, we see that the Adams 1845 prediction’s accuracy was sufficient to effect that he was not making a false estimate of the perturbing force. But British astronomy’s pre-discovery ineptitude in it was not sufficient. And, though the Astronomer Royal & a few other leading Britons are routinely condemned for this, a case will be made below that the key person lacking the necessary confidence was Adams himself — partly due to his own astronomical inexperience, and partly due to his correct 1846 perception that he had been able to use the fact that his key mathematical theorem planed his theoretical planet at more than 1 distance from the Sun (arbitrarily presumed: below fn 5).

A4 The affair’s puzzles begin with Adams’ supposed private lodging of his preliminary computed orbit & position for Neptune, generally known today as “Hypothesis I”. The standard tale is that he deposited his Hyp 1 solution: [a] with Cambridge Observatory Director James Challis in 1845 late Sept, and then [b] with Britain’s greatest Astronomer Royal, George Airy (also Cambridge University), in 1845 late Oct. As will be seen below, Adams’ needlessly mysterious Hyp 1 is the key to the whole controversy. Though privately the Astronomer Royal regarded as the prime

5 Rawlins 1970G was the analysis that finally established that the discovery of Neptune was not a mathematical fluke, as had been charged by various astronomers for over a century, from B.Peirce (Harvard) to A.Newcomb (as the falling node of supposing that secular resonances cannot short-term resolve solutions. I also later found that the Lemonnier 1769 observations of Uranus — so useful to the work of Leverrier & Adams — were not bungled as is so commonly charged, e.g., by Hist. sci. biggie T.Kuhn (see Rawlins 1981L).

6 I have the impression, from Adams’ omission of precession in MemoC (§F4) and his roughness in MemoW (for ex. §J.2 where he uses 1846 a highly experienced and swift computer in these areas (see §7), Adams’ investigations have the flavor of a learning experience. I offer these judgements not in criticism (indeed, they suggest that Adams’ challenging the Uranus problem was even more creditable than otherwise) but because I believe they help explain Adams’ slowness to publish, which relates to the central mystery of the Neptune affair. From a draft of a letter (1847/2), some months after Adams was world-famous) from Challis to H.Schumacher (CON #30, emph added): “Mr. Adams. . . a young mathematician of excellent promise . . devotes his mathematical powers almost exclusively to astronomical science . . . a small observatory . . is under his care, and gives him the means of adding to his theoretical knowledge, an acquaintance with practical astronomy.” By the way, in an 1846/11/18 letter to Airy (Glaisher p.xxix), Adams says if others did not take up the search, he was preparing to look for the planet himself at this little St.John’s College (Cambr U) observatory. (Note that he could have done so in 1845 if he believed his math to that point warranted it.)

7 Even on the accepted record, Adams still went on beyond Hyp 1 to compute Hyp 2 and Hyp X. Again, this bears on the question of priority: a preliminary solution, as yet unchecked by variation of the mean distance is insufficient, as even Adams agreed (1846/9: M16:405) just after Hyp 2’s 1846/6/completion: “the investigation [Hyp 1] could scarcely be considered satisfactory while based on any thing arbitrary; and I therefore determined to repeat the calculation on a different hypothesis as to the mean distance of Neptune, after there being used the factor 1.03 for Hyp 2.” (That is, the correctness of the first Adams solution’s predicted longitude was very lucky — and he himself knew that, which is why he made the statement just quoted, and the inadequacy of this solution is an important cause of his nonpublication of it. I.e., Adams’ Hyp I-based priority-claim is self-confessedly feeble. [We regard Leverrier’s prediction as occurring not in just one paper but three:] So, why is Adams (who unquestionably lodged [1846/9/2] his distance-variation solution later than Leverrier’s comparable solution [1846/6/31]) regarded by anyone as the prior discoverer? A related question on another tangent: both Leverrier and Adams failed to get close to the actual mean distance of Neptune (30 AU), replacing the tedious process of repeated rigorous computation of perturbations (for various mean distances) by instead using shortcut schemes, both of which led to serious errors in their final orbits: see §E8. Suppose they had gone ahead with repeated distance-trials toward 30 AU, would their work have run aground on the huge 4000” Uranus-Neptune 2-1 resonance? A lovely evasion of this problem is that in S.Newcomb’s Orbit of Uranus 1873 pp.55, 178. Soon after, Adams’ 1878/9C 31.3 1.023 AU, replacing the tedious process of repeated rigorous computation of perturbations (for various mean distances) by instead using shortcut schemes, both of which led to serious errors in their final orbits: see §E8. Suppose they had gone ahead with repeated distance-trials toward 30 AU, would their work have run aground on the huge 4000” Uranus-Neptune 2-1 resonance? A lovely evasion of this problem is that in S.Newcomb’s Orbit of Uranus 1873 pp.55, 178. Soon after, Adams’ 1878/9C