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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. Hystericral Astron (Previews of Coming Detractions):

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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.

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The JHA 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.

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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.

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Table 1: Heliocentric Longitudes for Several Orbits & Dates (GM, Noon, E&Date of Date)

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<th>Leverr</th>
<th>MemC</th>
<th>HypG</th>
<th>HypW</th>
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Acknowledgements: This is to express my long-standing & 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, 12/2/16) xeroxas of a sizable part of the longitudinal Adams Neptune mss.
The Inequity Inequity: Rainbow MENu

The unspoken lesson of the 1991 Anita Hill-Clarence Thomas affair4 was that, in the US, the “race card” trumps the “gender card”. How many more decades must pass before TV’s newsyers discuss 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 undisussed. Items:

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, x 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.5 Central question (which, perhaps revealingly,6 has not been publicly asked): why bother with alliances when your own group already comprises over 50% of the electorate? Are women 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 §R; 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’ unsecrecy about everything contributed to spreading the British effort over a huge piece of sky, while intrepid (Columbus-like)80 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.

I11 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 communcated without loss of time to the scientific world . . . .

Or, . . . shen should at least have been taken to find the planet [by telescopic] 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 . . .

81 To upset an impending appointment, on the basis of an account of unwitnessed events (of 10 years’ antiquity) when not a scrap of written notes was kept by the accuser — and when it appears that she took the perps (that attached to tolerating alleged verbal abuse) as long as they lasted, and only went public after that well went dry.

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?

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

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.

Are most women still falling for the media-dangled short-term-easy escapist myth (implicated in ubiquitous ads for cosmetics, creams, & shampoo): 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 may 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 might generate more long-term progress by stimulating women’s own substantial intellect & ambition (and principled renunciation of using the superficial feminine-wiles-crutch for short-term gain and/or job-advancement) than by becoming eternal-victim-paranoid about men. (But: which pitch creates bestsellers & raises lobby-funds?)

Fact: admirably adless, issue-oriented MS magazine’s subscription numbers are many times smaller than beauty-myth-selling Cosmo’s. Until the melting of that scandalous ratio (which is hard to blame on men, since both mags sell largely to women), no legislative solution can substantially improve women’s status.

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 unevented events (of 10 years’ antiquity) when not a scrap of written notes was kept by the accuser — and when it appears that she took the perps (that attached to tolerating alleged verbal abuse) as long as they lasted, and only went public after that well went dry.

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Note similarity to §8 §A2.

1 This point is the most vital part of Biot’s speech (as it applies to the Neptune priority dispute), which is omitted from virtually every reprinting of it, e.g., Newman 1963 p.178.

2 Attacked at the time as incredible by Leverrier, Airy, and virtually all other astronomers except C.Delauney.

Partial Bibliography

J.Adams SP. Scientific Papers vol.1 (of 2), 1896, Camb U.
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; (Glaiser 1896 p.xxxii); [3] why he did not publish even after Leverrier’s 1846/6/1 paper.

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-erorrize 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.

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).

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 1889 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).

Thus, in my opinion, not only should the Adams claim be shelved until the “lost” RGO file is unshelved, but: given British astronomers’ demonstrated filtering of documentary material (e.g., §B2), there is reason to doubt whether that 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-female US presidency of the century (whose prime obsession has been packing the US Supreme Court with men he’s hoping will return women to pure baby-factorydom), and [b] Bush picking that cute little boy for his Vice-Presidential mate? (Granted, such a question is inexcusably prejudicial, since the undisputed truth is that Bush selected Quayle for his mental depth & celerity.)

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) fully deserves the subservient role her vote invites.

E Robert Newton & the Muffia

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.)

This while avoiding symposium-debate while he lived. His death now renders it impossible 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 withNibelungs. Now that he is gone, I realize all the more how rare are such people.
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. My detractors acknowledge that he was the subject of useful controversy. He will be missed.

DIO 1.1 §11 §C12 to consider the possibility 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-Muffia use of such terms is instead based upon interpretational disagreement. That is, difference from Muffia orthodoxy is instinctively equated with incompetence. (See, e.g., DIO 1.1 §5 §D14f & fn 20, or DictSciBiog 11:201.)

Noncitation: Robert Newton’s publications on Ptolemy’s fakes started in 1969. But the Neugebauer Muffia’s leading capos refused to cite them until 1977. Eight years. (See, e.g., even Muffia 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 §11 §C7.) Or does he instead regularly meet the allegedly­errant 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 Muffia preferred the former, censorial approach until 1977, when publicity (temporarily outside its immediate control) gutted this approach’s efficacy, and only then did the Muffia 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 power­operation.

F Power People

F1 To observe astronomer O Gingerich (now head of Harvard’s Hist.sci Dep’t — and an ideal choice for the post) calling Galileo a “scrambling social climber” is as entertaining as finding (1976/3/12) R.Kargon, sometime Isis boardperson, accurately describing a well­known 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 self­evident:

[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 biggie­editor & 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. 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.

19 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 highly free of the “company” that Leverrier (longtime head of the Paris Observatory) was extremely unpleasant to his colleagues is amply testied 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 (Canadian­born), extended even to his presenting Newcomb
H4 We are told by Adams (fn 59) that the 1845/9 & 1845/10 solutions were effectively identical (his “final values”); thus, his seriously improved Hyp 1 solution cannot be that of 1845/10. An integral part of the Neptune legend is that Adams tried to give his 1845/9 solution to Airy at that time but failed due to what has heretofore been regarded as (classically-mythological) bad luck (see \$F1). Yet this heart-of-the-myth 1845/9 orbit was actually the 14% Solution (\$F-3G), which was so flawed that Adams later suppressed it.

H5 Remember that Challis’ first announcement of Adams’ prediction placed it about 1846/6 (above \$C1 item [3]).

I A Cohering Hypothesis

I1 Therefore here propose the speculation that the long accepted “1845/10” MemoR from Adams to Airy was actually submitted in 1846 (perhaps as part of the 1846/11/12 material transmitted for the 1846/11/13 presentation to the RAS), and that the date 1845/10 was added later to the top of the first page of the document. How conscious Airy was of the truth, when he added the date (1845/10) to MemoR, I am not sure (since he presumably got 2 memos without date and might have confused the two). But Adams has to have known the difference.

And there is a hint that Airy did, too. The published version of MemoR makes one alteration, not previously noted: where Adams spoke of the elements of the “new planet” (SP liii), Airy edited to read just: “planet” (M16:396). Are we seeing here the caution of an experienced academic politician, one who figures that it is risky enough backdating a document, without including in it an expression which might be taken for confidence\(^6\) but also might look like a giveaway anachronistic slip?

I2 The question of confidence is central to Adams’ claim. His failure to publish is an obvious measure of truth. And his long-forgotten mid-1846 handwritten epemheris (MemoW, CON #35) places a (hitherto unpublished) lost-star-based circular orbit ahead of his published perturbation-based orbit! (Callad $\Phi$ of $\Phi$ a Perturbation-orbit?). This Restoration makes the possibility that Adams was even at this late moment unsure of what his now-immortal perturbational work was really worth: he was a knowledgeable theoretical mathematician, but would that provide him the same grasp, of the physical reality involved, as was possessed by a seasoned astronomer such as Leverrier? (See fn 4.) Airy’s praise of Leverrier in this connection has long been dammed as unfair to Adams (e.g., Smart 1847 p.35), but it may instead reflect the truth of the matter (well known to Airy at the time, but later swamped by a seasoned astronomer such as Leverrier?)

I7 Airy wrote Leverrier in 1846/10/14 (Smart 1847 p.33): “You are to be recognized beyond doubt as the real predictor of the planet’s place.” And Airy’s 1846/10/21 letter to Leverrier states (Smart 1947 p.35): “no person in England will dispute the completeness of your investigations, the sagacity of your remarks on the points it was important to observe, and the fairness of your moral convictions as to the accuracy and certainty of the results. With these things, the produce not only of a mathematical

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6 Accepting the 1845/9 MemoC (CON #32) as genuine, this confidence was in fact expressed by Adams (at least before finding his deflating sign-error), since his bold statement to Challis on this paper is: “The Elements of the New Planet I make to be as follows:...” and the published version of MemoR (Con #35) says: “The Elements of the New Planet I make to be as follows:...”

7 Airy’s politically-inspired behavior varied so much that it’s been hard to unravel. I see his evolution along these (rough) lines: [a] His first (1845) reaction to Adams is helpful and inquiring. [b] Hearing of Leverrier’s 1848/11/10 paper, he perhaps warns Adams of competition. [c] Seeing the 1846/11 Leverrier paper, he launches (6/29) a secret sky-search to win Neptune for Cambridge. [d] Upon hearing of Galle’s Leverrier-directed 1846/9/23 capture of the planet, he regards the English hopes as lost and makes ultimatum with the French, in vain hopes of heading off a fight that must lead to embarrassing revelations. [e] The 1846/6/16 Cambr.C.B.Obs. observations of Neptune (discovered 10/12) Challis to flagwars, but Airy credibly continues (1846 Oct & Nov, even while perhaps helping Adams get his solution into publishable shape) to regard Leverrier as the prime discoverer. [f] Instead: “We must be ascribed to the infinite mercy and goodness of God, and to the felicity of your Majesty’s times ... the wisest and peacefulness of your reign, in the largeness of your heart, in the noble variety of the books which you have composed your Majesty! Your Majesty’s Most bounden and devoted Servant, Francis Verulam, Chancellor.” And Jos. Addison similarly prefaced his justly famous Spectator (1711) with an even nearer-perfect extended browning-study — the sort that Airy publicly assigns to that person, enhances his stature — which 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.

18 Isaia Publications Committeeperson A.Van Helden’s 1990 review of the Journal for the History of Astronomy mentions prominently (Isai 81.2.298) that “some of the best articles in the JHA just-so-happen to be those of the esteemed JHA Editor-for-Life! (And neutral critic Van Helden just-so-happen to be a JHA Adv Editor.) Admire, too, 0. Gingersch’s Disariuscheide [Robt. Masssey Droushiunt 1991 p.21] tollow at work: fn 17. Actually, there was a time when these politicalician-scholar-scholars (especially British), climbing under archways, had to be alot more unsuitable than even today; though, convincing the vain patron not merely of his generosity, but additionally of his brilliance as an intellect & author, is a timelessness requirement. E.g., Sir Francis Bacon, Lord Chancellor, Baron Verulam (whose defense to a bribery scandal was the lawyer-oldie that he indeed took bribes — but without effect on his decisions!) dedicated Great Renewal (1620) to King James I rashly (emph added): “To our Most Gracious and Mighty Prince and Lord JAMYDES by the Grace of God of Great Britain, France and Ireland, Defender of the Faith, Etc. Your Gracious and Mighty [DR: like James’ personal 1611 translation of the Bible in his spare time!] ... May God Almighty long preserve your Majesty! Your Most bountous and devoted servant, Francis Verulam, Chancellor.” And Jos. Addison similarly prefaced his justly famous Spectator (1711) with an even nearer-perfect extended browning-study — the sort that Airy publicly assigns to that person, enhances his stature — which 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.\(^6\) However, [a] The more receptive The System is, the less important the issue. [b] The most important issue is: The System itself.
G Two Party Ping-Pong Pocket-Plumbing

G1 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.

G2 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’ minimistrations.

G3 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-imprisonment22 — even as TV ‘sneaks pundit-flunkies assure the plumbers of the sanctity and inherent wisdom of the “Two-Party System”.23

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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 Dem 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 — which swore it would never repeat that mistake . . . .)

22 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 ordnanz 10% — every single year.) Is the US turning into a vast company-store town?

23 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 divert 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 Littlelegy’s Jillionaire. 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 dried D.Duke vote away into oblivion.) Simple consideration: if TV ‘sneaks builds up a candidate (or, indeed, any Approved Leader of a worthwhile 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 civic courses.) Plunkitt: “I don’t care who does the electing, so long as I do the nominating.”

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H Dates

H1 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 clues 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 alone for Hyp 1, the supposed 1845/10 orbit (MemoR) and thus the key to the Neptune story is printed in [M16] and I never saw him but twice; once [1845/12/5] 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 last two minutes [§B6]. No other opportunity of seeing him.

— each of whom had been top math student in his respective Cumbr U class — were all so incompetent as not to know the difference); for it acknowledges that the ephemeris Adams was computing for Challis’ search was utterly based on a circular orbit — thus implicitly jettisoning as unworthy of Adams’ own trust (§B4) all the mathematical refinements of his 1845 elliptical orbital work, which intimately involved the unknown planet’s eccentricity, true longitude, & true distance. The only other viable interpretation here is worse yet: the sole orbit so far published by Leverrier (1846/6/1) gave just circular elements (38 AU & 325° longitude); thus, the “Adams” ephemeris (MemoW) done up for Challis is actually based precisely upon Leverrier’s published orbit — and precisely upon Leverrier’s published limits (+ 10°, as already noted at §B4 item [b], fn 19, & fn 27).

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 [1846/7/2] 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 last 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 longitude 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 his 1845 Oct letter, putting the planet at 325°05’ on 1847/1/1, only 1/2 degree different from Leverrier’s place.)

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

G9 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) 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). This suggests (but does not in itself prove)70 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 worked out only after 1846/8/29; see Sampson SP p.67.) Again: Hyp 1 is the orbit upon which Adams’ claim of priority rests. The only other 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

70 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 1846 solution (Hyp G) which gives 324°-23’ for 8/29, 324°-36’ 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 #42), gives 324°-22’ — also within 1° of Leverrier’s place.

71 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 a 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.

72 See also Airy’s 1846/7/12 instructions to Challis (CON #44): “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 RA 20:48 & declination -2° & RA 22:48 & decl -4°. He also comments (wrongly) that the known completed Berlin Sternkarten (Berlin Observatory) cover “a small portion” of the area, adding (7/21, CON #5): “There is only one [Berlin Hour 22] which applies partially to the inquiry.” In case it helps explain the chart’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 hr 22’s space until 1846/10/12; real Neptune, until 1846/8/2 — so, at very near the time of Airy’s comment, Neptune was slowly sidetracking 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.Angelender) covers more (c.35%) of the Airy-proposed 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 off 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 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 blased about Time’s removal of the guts of Gadberry’s letter if it weren’t so de­rigorously 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 ‘sneus. Thus, the only thing that progresses is the decay itself. (See DIO 1.1 §1 §D4)

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 morass (see & DIO 1.1 §1 §D4) from a DRIsp on 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 1981/1/13
From: DR

**B1** The December *Scientific American* states (p.74) that, after Galileo’s 1613 observation of Neptune (Kowal & Drake’s recent astonishing find), and before Galle’s 1846/9/23 Berlin Obs, optical discovery (directed by the mathematics 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>Mauvais (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>
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</table>

**B2** The Challis 1846 observations were part of his famous failed secret Cambridge U. sweep, aimed by J.Adams’ math, [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 year2 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

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1 This amusing item was revealed by J.Delambre.
2 But see f.19 in f.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 accent the 38th drought 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 1844-1781.

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**G4** The 1845/9 solution contains a small but fateful math bungle (touched upon at f.2) of which Adams was so ashamed that he never published it anywhere: the sign of a term was inverted (Sampson 1904 p.166). That is evidently66 a cause of the previously-mentioned large discrepancy in eccentricity (0.16 vs. 0.14: f.2). 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 1?) if it is effectively the same — *which Adams states that it was* (see above quotes from M16:429 at f.3 & 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 are shown, in *Adams’ 1846/11/13 paper, M16:454*). But the purportedly “1845 Oct” MemoR provides only residuals (M16:395-6), not perturbations.67 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 G? or Hyp 1?) written before 1846/9/2, M16:405-8.) Indeed, while we have (fn 68) Adams’ list (MemoD) of the numbers of his erroneous 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

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66 Sampson 1904 p.167 notes that a small alteration in procedure was made in the Hyp I 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 I work.)

67 By 1846/12/12 (after Hyp I’s publication in M16), Challis has straightened out the story and speaks of MemoR as displaying residuals, not perturbations (SP p.l). 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 I) 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 Glaisher 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 in want 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.lv; 1846/12/12): “the problem of determining, from perturbations, the unknown place of the disturbing body, was first solved here (Cambridge U.).” 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:

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<th>Recoverer(s) (Date)</th>
</tr>
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<tr>
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<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>Mauvais (1847)</td>
</tr>
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<td>1795/05/10</td>
<td>Paris</td>
<td>Walker (1847)</td>
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<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>
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</tr>
<tr>
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<td>Cambridge</td>
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<td>1846/09/07</td>
<td>Munich</td>
<td>Hind (1850)</td>
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<tr>
<td>8</td>
<td>J.Lamont</td>
<td>1846/09/11</td>
<td>Munich</td>
<td>Hind (1850)</td>
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</table>
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 calculations 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 ms; 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.14v): “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 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 tenths & hundredths 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’ ms 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 (932) & 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 MemoF 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 (or rather: incurred during the parallel calculations for Hyp 2: mid-1846) but rather (incidentally) during the parallel calculations for Hyp 2. Compare M16:407 & 455. Note also that the ratio Adams used to extrapolate-compile 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 1845/11/13 paper has 5:4 (apt to 0.575, near the value cited 1846/11/13). (The corrected 1843 residuals would lead to an untrusted distance ratio of 0.58.) 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 already altered, as noted above, 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 The 1690 residual disagrees by 0°:1’ 44°:4 vs. 44°:5. But Adams mss section E IV p.16 has 44°:4.5, so this difference is merely a matter of discretionary rounding. By contrast to this unmounted Adams-steadfastness, I note that just in the 10 weeks from 1846/9/2 to 1846/11/13, Adams altered by 0°:2’ the 1843 residuals for Hyp 1 and Hyp 2. Compare M16:407 & 455. Note also that the ratio Adams used to extrapolate-compile 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 1845/11/13 paper has 5:4 (apt to 0.575, near the value cited 1846/11/13). (The corrected 1843 residuals would lead to an untrusted distance ratio of 0.58.) 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 Greenwhich normal places of Uranus; however, the 1843 residuals are already altered, as noted above, 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.

8 Current Developments

A How Double-Sunsets Triggered the Discovery of America

A1 G.Corrface, 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.” 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 confident2 that: [a] the globe was much smaller than it actually was, and [b] the Eurasian landmass wrapped much more around the globe than is the reality. His Earth-size estimate was low by about 25%. It is well known that he got2 his faithful overestimate of Eurasia’s longitude-spread ultimately from Marinos of Tyre (c.100 AD), whose geographical data underlay C.Ptolemy’s famous Geographical Directory (GD), c.150 AD. Columbus adopted precisely Marinos’ value of 225° for the longitudinal breadth of Eurasia (adding 28° more for Marco Polo’s extension of the knowledge of China, plus 30° more for Japan).3

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

A4 In 1978, when watching a sunset at La Jolla beach, my wife & I noticed that, even after the Sun set on the beach, one could (for ordmag a minute after) see the Sun’s image reflected off windowpanes of houses on the heights. So we began regularly testing the effect (via stopwatch), computing the Earth’s size from the observer’s height $h$ and the time-difference $t$ between sunset at sealevel and sunset at height $h$. For the simple case where the observation is made at the Equator and at an equinox, the geometrically deduced Earth-radius in km will be $r = \frac{737000h^2}{t^2}$, for $h$ in meters & $t$ in times. DR wrote up the method for the American Journal of Physics 47:126 (1979/2); and, thanks to Jearl Walker, it then appeared in Scientific American (1979/5 p.172).

A5 But the method has a nontrivial flaw. The mathematical result is infected by atmospheric refraction (which the ancients, who lacked quantitative tables, couldn’t correct for): a horizontal ray of light is bent downhill (due to the vertical density-gradient of the Earth’s atmosphere), and the curvature of that bend averages about 1/6 of the curvature of the Earth itself. An extreme example will show that this effect will artificially reduce an observer’s double-sunset-based estimate (of $C$): for, if the atmosphere’s effect were 6 times stronger...
(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.8 Thus, \( t \) would be infinite, and the Earth-circumference \( C \) deduced (from the above formula) would be zero.

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

A7 There is another way of measuring the Earth’s size, namely, the dip-method. At a large height \( h \), measure how much the zenith-to-horizon angle exceeds 90°. (This method was modernly resuscitated by J.Gerver. See 1979/5 Scientific American.) Navigators call this small angle the “dip”. Geometrically, an estimate of Earth-size should be inversely proportional to the square of the observed dip. But this math, too, is affected by refraction.

Again (as in §A5): if the atmosphere’s density gradient were 6 times larger than it actually is, then horizontal light-rays would travel the Earth forever. So, rays coming to any observer would arrive without apparent dip, and computed \( r \) would involve division by the square of zero, yielding infinity. Thus, the Earth would look flat (sphere of infinite \( r \)) at any small height \( h \). In brief, refraction inflates a dip-method result. For the actual Earth atmosphere, the result will be high by the factor \( 6/5 \), yielding 25,290 nmi = 259,200 stades, agreeing (to c.3%) with Eratosthenes’ famous 200 BC estimate, \( C_E = 252,000 \) stades = 25,200 nmi.

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

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

B Heckathorn Scores Again

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

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) incompletely speaks of Venus sunrises.

9 Such persons as E.Lehmann-Haupt & C.Sagan depend upon fiddling with the length of the stake, 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 metronomical but physical.

1992 October DIO 2.3 §8

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. irregularities in the motion of Uranus may be accounted for by supposing the existence of an ext. planet the mass and orbit of wh. are as follows:

- Mean Dist. (presumed nearly in accordance with Bode’s law) 38.4
- Mean sid. mot. in 365.25 days 1°30.’9
- Mean Long. 1st Oct. 323°34’
- Long. Perih. 315°55’
- Eccentr. 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. 0.1428.
- Long. Perih. 320°30’.
- Mean Long. about the end of Sept. 1845 = 320°40’.
- Hence Geoc. Long. at the same time will be 320°30’ nearly, [dim.] 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 [h] (of the 3 Adams miscues listed in §F4). But perhaps all 3 errors were eventually cleaned up, leading to a (DR-speculated) 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’
- Eccentr. 0.1428
- Mass = 0.000173, that of Sun being unity
- Geoc. Long. at end of Sept.’ = 321°15’ nearly, dim. about 1’ per day

(In the documents we know are from 1845, his epoch of 1845 is called “the end of Sept” — not Oct 1, as in his 1846 descriptions of his alleged 1845 Hyp 1 work.62 Note that,

62 CON 932 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, 10/8 (M16:445, 453). Challis (SP p.) makes the earlier epoch explicit as 1845/9/30.)
as he plainly did.59 No popular account has ever mentioned the Fourteen Percent Solution of MemoC-MemoD.

F3 Adams may have been paralysed not only by his sign-error, but also by the fact that his various 1845-1846 solutions were (compared to Leverrier) all over the last octant of the zodiac. On the discovery-date, 1846/9/23, Adams’ various 1845-1846 solutions gave the following true heliocentric longitudes (with the date of arrival at the solution provided in parentheses): [a] Adams’ first 1845 hypothetical planet (1845/4/28-5/19) was at nearly 350. [b] Hyp G (1845/9/18) was at 324°31’. [c] Hyp W (1846/7), at 336°30’. [d] Hyp I (whenever), at 328°43’. [e] Hyp 2 (1846/8/20), at 329°26’. [f] Hyp X (1846/9/2), at 315°16’. (The MemoC planet was then at 323°48’, while Leverrier’s predicted planet was at 325°59’ — and the actual Neptune was at 326°58’.) The range of Adams’ swings was enormous (35° of longitude!) and must have given him & Challis plenty of (perfectly reasonable) doubts regarding where the planet really was. I note that the worst value (the 1845 Spring solution, item [a] in the above list) was not necessarily taken lightly by Adams, as modern historians assume. In the Adams mss (section E II p.10), we find his note that it well satisfies his Flamsteed equation of condition (“a very close agr.”), and, more important: we also have his written comparison (obviously inserted after Hyp 1’s completion) of this agreement with that of Hyp 1 — Hyp I being worse by this criterion (Sampson 1904 p.165). So, how could Adams be sure that Hyp 1 better was than this early (very erroneous 2-stage) 1845 Spring solution? Again: this is why post-discovery discoveries should be disallowed as a matter of policy.

F4 As already noted, I have long held that Adams’ supercaution was the key cause of his fatal nonpublication. I am here adding the speculations: [a] that the specific cause of Adams’ nonpublication before 1846 June was the 1845/9 solution’s “slight” perturbation term sign-slip discussed above (§F2), associated with a huge (suppressed) error in predicted eccentricity, and [b] that this error still infected the solution in 1845/10. (This also relates to the rockbottom broader point: Adams was terrifyingly unsure of Neptune’s actual longitude, this also for other reasons just noted in §F3.) If item [b] is true, then the actual solution handed Airy in 1845/10 was indeed a “slightly altered” version of the note handed Challis a month earlier — but the alterations were simply (some or all of) the following superficial corrections (no relation to perturbation theory): [a] 30° of precession error (Adams was so raw that his 1845/9 note, CON #32, neglected to include precession from his solution’s central epoch 1810.328 to his current epoch 1845.750), [b] 10’ of scribal error, and [c] 2° of rounding error. When these “slight alterations” are attended to (thus giving the elements correctly deduced from his 1845/9 perturbational solution)50 we have attained reasonably likely reconstructions of this speculative actual solution. So, I will suggest 2 possibilities (as to the actual solution handed Airy in 1845/10), labelling them “Hyp D” (a copy of which survives: §§6) and “Hyp G” (merely Hyp D shorn of superficial errors). I will provide these below, but for comparison, I first set out the famous Hyp X (MemoD). We will then try to discern which of these solutions (Hyp 1 or Hyp D — the latter being effectively identical to Hyp G) was actually given to Airy in 1845/10.

10 By Australian dating, which is used in the diaries and observations book.

11 The Report rightly rejects DR’s erroneous initial reading of Mrs. Peary’s sealed Betelgeux Document; though the NavFou’s own solution was also false. The correct interpretation, published for the first time in DIO 1.15 fn 14, was verbally assented to in private by the NavFou’s rep at the 1991/14/9 Naval Inst debate on Peary; but no NGs acknowledgment of this has been published, nor will it ever be, since National Geographic is constitutionally incapable of owning that its critic DR is ever right about anything. (See §A §F4.) This statement is itself a criticism; and the NGs’ consistent failure to publish anything on the Adamsian hypothesis is, I would suggest, a willful attempt to suppress all references to perturbation theory & integrity, one could confidently engrave it in granite. (NGs’ degree of opennessmindedness is as wellknown among serious scholars as is the personal nature of its positive & negative views on any issue. Nature 1990/94/26: “the National Geographic Society. . . will always believe [Peary] reached the Pole.”)

The NavFou also adds Cagni’s 1900 alleged Farthest (skeptically analysed at Fiction pp.65-68) without noting: [a] Cagni never even claimed to have approached the Pole, where steering for the polar point is critical. [b] Cagni did not report a transverse observation anyway at his turnaround-point (A.Luigi Hanssen); Sampson 1904 pp.50-51 obs. #149; cited at Fiction p.65 & 297). [c] Cagni’s reported compass variations (cited by NavFou Report p.62 as causing Cagni’s supposed steering by solar culminations) are mistaken by over 20°. (As noted for the first time at Fiction p.65).

Each DR-calculated longitude W (at latitude L) is good to ordmag a mile. (The chronometer rating supplied by H.Mohn 1915 is adopted. Mohn also provided all of the Amundsen expedition’s extensive compass variation V results, calculated from his own (nonexistent) transverse solar data.) For 1911/11/13 (L = 83°S); V = 132°.17’, W = 165°30’W (Hanssen); V = 134°.32’, W = 166°07’ (Amundsen). For 1911/11/9 (L = 84°S); V = 141°.14’, W = 165°12’ (Amundsen). (The two seemingly discrepant 11/9 longitudes actually differ by less than 5 mi, so near the Pole; V’s uncertainty is due to the roughness of solar compass bearing observations expressed largely in quarter-points of the compass.) DIO 2.2 also finds that Amundsen approached the Pole nearly parallel to the 164° W meridian. At the Pole, he found that the compass pointed along about 18°W.
with Amundsen’s own calculations shows that he used sph trig. This should interest the
regarding not only longitude but distance.

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

C The Jekyll&Hide Defense: I Say, What’s Astronomical HISTORY

Doing in the Journal for the History of Astronomy?

C1 In J.H.A 1.2 & DIO 1.3, DR’s “Muffia Orbitalitv” extensively admires the pioneering
work of 1991 lead papers in the Journal for the History of Astronomy and Isis, who are:
[i] lodging (& understandably promoting the originator of) Hist.sci’s unprecedented discovery of the WINTER EQUINOX 15 — as well as [ii] rewriting the canons of gradechool
arithmetic, 16 in order to promote certain precious Hist.sci tenets.

[a] The prominent JHA-Isis articles cited contend that none of three surviving Hipparchos trios of solar observations (Almajest 4.11, 4.5.85) can be satisfied by trig-based (Greek style) solar orbits. So, DIO 1.3 helpfully supplied all three of the allegedly-impossible orbits 18 — and noted that one of these Nonexistent orbits (satisfying the Hipparchos solar

15 In DIO 2.2 5%, compare eq.3 (Amundsen) to eq.10 (Scott). It is strange that the truth of Amundsen’s steering method should have become lost and (nowadays) so universally contradicted, since (from working with the original Amundsen) the Mohn 1915 gave (verbally) Amundsen’s computational procedure (DIO 2.2 §5 in 17).


17 JHA 22:101 (1991/5) p.119. Not every journal can boast of refereeing which comes up to the JHA’s rigorous standards. Isn’t 1991/9 paper is squarely based upon the prior JHA 1991/5 delight.

18 Readers who possess the advantage of an elementary school education may wish to check the arithmetic found in the go-funded Mufia paper selected as lead article for the 1991/5 issue of the extremely handsome Journal for the History of Astronomy, whose highest-ranking Polemiek 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 Society’s new journal: 17 of the JHA paper’s development, we learn: [a] 128/128 = 68/303 = 65°5′, and [b] the solar mean anomaly (increasing at Hipparchos’ 0° 085635/35/day changes by 67°25’ in 67°25’ days!) These adventures in Mufia New-Math are at the very core of the Muffia’s attempt to prove that the DR-solved Hipparchan solar trios cannot be solved by Greek trig methods. But this is the [JHA]: so, expect no DIO-citing or DIO- quoting retraction — despite the aim-to-please publisher’s statement appearing off at the end of DIO, issue.

19 E.g., the EH orbit (founded upon Hipparchos’ earliest adopted solar cardinal points), epoch Phil 1: Y = 365°1/14; e = 228°; A = 44°; e = 33°1/4. This satisfies eclipse-trio B of Almajest 4.11. The same chapter’s equally “impossible” eclipse-trio A is satisfied by a hybrid meld of the EH orbit with the famous PH orbit preserved in the Almajest. Also found in Almajest 4.11 are the Hipparchos lunar ratios, which have defied 2000 of attempts at explanation (from Ptolemy through Muffia capo G.Toomer): (327 2/3)/3144 and (247 1/2)/3122 1/2). In DIO 1.3, it is discovered 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 (MecmC) — though all histories speak of this as the golden moment when Adams’ immortal prediction was lodged. (Indeed, Glaisher 1896 p.xxivii 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.) 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 (MecmC) 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 1845/7 list of planet elements, which I will call “MemoD” or (when referring to the elements) “Hyp D”. MemoD is printed innocently in R.Sampson’s learned review of Adams’ mss (Sampson 1904 p.166). Challis’ written intro (for Adams to meet Airy) was 1845/9/22; the data leading to Adams’ MemoD includes a date: 1845/9/18. Sampson supposed that MemoD must be virtually equivalent to the famous but hitherto unpublished note given Challis at that time. Sampson’s conjecture (regarding the data at least: vs. fn 68) was verified when I then consulted the original Adams note to Challis, MecmC (CON #82: only a 10’ scribal error differentiates MecmC & MemoD). The shocking revelation here is this: though the planet’s mean longitude is not grossly discrepant, some of the orbital elements are severely different (§94) from those of the “1845/10” MemoR which has always previously been accepted as Adams’ first solution, “Hypothesis 1” (which is the crux of the whole Adams claim). In particular, a perturbation

term’s sign is wrong, which contributes to producing an orbital eccentricity (0.1428) which is about 1/8 lower than the MemoR value (0.16103) published. Moreover, we have Adams’ word (M16:429) that the 1845/9 elements are his “nal values” and that they were only published, two more nds have only increased our wonder at those

...
investigations. Though even freshman physicists are informed of Airy’s originality & intellectual gifts, Hist.sci chroniclers 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 dialectical stupidity ruined Adams’ chance for immortality. Extrapolating beyond Grosser’s unsympathetic portrait, J.Newman’s prominent review of Grosser’s book spoke of Airy as: “a school-bright, hapless donkey”, “unusually conceited”, & “bitterly jealous of his assistants — or of any young astronomer.” Fortunately, astronomer O.Eggen’s learned, near-simultaneous 1965/4 Sky & Telescope review provided a counterbalancing breath of sanity about Airy — plus a few gentle digs at the then-latest Hist.sci account’s inevitable technical-innocence slips, which have long provided such reliable entertainment for real science.

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, 1847/12/2; 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 noticed 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 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 distance-comparison of Neptune’s 30 AU mean distance (1846/6/24) observations given in Chalbis’ 1846/10/15 & 21 letters (Athenæum 1846/10/17 p.1069 & AstronNachr 25:106). It is also worth noting that since Hyp X had null eccentricity, its hypothetical planet was always 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 that: [a] these ratios were derived (to extremely high trig precision) from a simple trig development (showing that “Ptolemy’s Theorem” was known to Hipparchos) based upon eclipses A2&A3 and B1&B2 (ignoring A1 and B3), and mean-longitude-at-epoch = Ptolemy’s 178°.

That Ptolemy insisted he personally invented the whole Catalog from Hipparchos (128 BC). The Catalog issue became central when: [a] in the 1976/8/6 Science, loyalist G. Gingerich unconvincingly tried explaining-away Ptolemy’s solar, lunar, & planetary fugdings by calling them “pedagogic,” & [b] DR responded (p.362 of Publ Astr Soc Pacific 94:359 = DR 1982C) that this alibi was irrelevant to the Catalog, since over 90% of its stars aren’t used in any Almajest computation. (Gingerich 1976, following Neugebauer HAMA p.284, deemed Ptolemy’s star data: real, outdoor, and more accurate than Hipparchos’…)

The Catalog issue was also central when: [a] in the 1982/8/17 J.HA, no acknowledgements were made to the independent efforts of Almajest 7.5 & 8.1 by mathematician-astrologer C.Ptolemy (for epoch 137 AD), reported by him as if based on his own observations, though it has been knowledgeably suspected for centuries that Ptolemy stole virtually20 the whole Catalog from Hipparchos (128 BC). C2

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

C3

Disputation amongst the various editors of the unkillable popular myth of ogre Airy rebuffing & stomping wuvable Adams. Not since an earlier J.Newman’s episode in the 1942 Lou Gehrig lm bio, etc.

That Ptolemy’s “Theorem” was known to Hipparchos based upon eclipses A2&A3 and B1&B2 (ignoring A1 and B3), with the prescribed lunar periods: mean-anomaly-at-eclipse = 82° and mean-longitude-at-eclipse = Ptolemy’s 178°. (Both values probably from Aristarchos.) [b] The trio A Moon distance is based on Aristarchos’ famous 3° value for the half-Moon’s angular distance from quadrature, with both school’s use of an Astronomical Unit of 100 parts, so that \( R_M = 1000^{\text{an}} 3° = 52°24' = 3144° \); trio B’s distance is merely this same value, affected by one of the most common ancient scribal slips, \( R_B = 52°14' = 3122°12' \). Further DIO 1.3 analysis is based upon Swerdlow’s solid and original 1868/9 discovery of another Hipparchos value, \( R_B = 490\text{ Earth-radii} \). This DR article also begins DIO’s demonstration that all 3 of the astronomical distance estimates (surviving via pseudo-Aristarchos, Archimedes, & Poseidonios) — including the famous half-Moon experiment — of the school of Aristarchos (who wrote on light & vision) are based upon his correctly setting the limit of real human ocular discernment at 100000 radians.

A.H asteroids — and Neugebauer-Mufa’s two leading purported experts on the ongoing Catalog controversy: J.Evans & G.Griffith, who have by now wasted hundreds of humble Mufa-publication pages, fruitlessly defense-lawyering Ptolemy against a passel of persistent proofs of his theft. Note: since the shellshocked Mufa has lately begun uncertainly admitting that maybe some of the stars taken from Hipparchos after all, Mufa now usually avoid telling their readers that Ptolemy insists he personally22 observed the whole Catalog outdoors with his alleged armillary astrolabe.
Now, at JHA 23.3:173-183 (1992/8), comes forth our favorite Mufa comedian-entertainer-satirist, Noel Coward25 Swerdlow (Univ Chicago Dep’t Astron, & Advisory Editor for the extremely handsome JHA), to pronounce his judicious26 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? 

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

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

Skepticism, starting with Tycho Brahe (who also faked a bit of his own justly famous 1598/12 star catalog),27 has always suspected that Ptolemy observed no stars but instead copied Hipparchos’ star catalog, merely adding 2°40’ of precession (mistaken by −1°.1) unto Hipparchos’ longitudes. In his Mufia-hated 1775 Johns Hopkins Univ book, The Crime of Claudius Ptolemy, physicist R.Newton revealed a startling fact: though the unalterred 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 23

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 Isis & JHA (sampled in DIO 1.1). His courage is unquestioned, since he and his Mufia 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 [C7 & §3 [D3].

At p.180, NCS 1992 affects neutrality (a repeat of his equally honest 1981 pose, cited at DIO 1.1 §3 [D7]), by saying hyperbolically 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 together in hearing that Ptolemy-skeptics were nuts, fools, & crooks. He cannot now admit his error without losing face disastrously. In this connection, NCS & the Mufia 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 Mufa scholarship, and so enjoys a resultant noninterest in automatic denigration of the output of these self-created Enemies.)

DIO 2.1 §4 shows that Tycho’s largely magnificent epoch-1601.03 catalog of 1004 stars (hitherto neither numbered nor even counted by Hist-sci!) contains 10 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 & declinations — the latter probably observed at Wandsbeck, not Hven.) Tycho’s method of fabrication was essentially this: He copied the Catalog’s preface acreent (enriching with the Tycho Catalog’s ‘unique’ elements); for just adding 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 “observations” would look real. (Tycho always rounded to arcmin or simple fractions thereof.) These 10 stars’ errors are gross by Tycho’s standards but they agree very closely with fabrication — and they are from the sole 2 subsets of his 227 final-rush 1596-7 stars for which no underlying data survive. Curiously, Evans’ 1987 JHA paper belies this: “The stars were real, because the telescopes of this time …” But it is Tycho’s own laboriously-developed extinction formula: Evans 1987 pp.259-260, 267-271. Use of Evans’ formula produces magnitude 7.95 for 2g Cen at Tycho’s Danish observatory! As further shown at DIO 2.1 §4 fn 65, the massive Evans-JHA attack on DR includes also Evans 1987 p.168 claims which require naked eye observations of stars as dim as tenth magnitude, by Evans’ own formula. Well, 10th magnitude may be dim; but, if we were to assign a magnitude to the brilliance of refereeing at the extremely handsome JHA, could a mere 2 digits do it justice?

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.

So Adams waited until 1846/11/13 to release his hypothetical elements to the public. This may have been wise in one sense (the subtext 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.

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 Altona (on about 10/5) to see Schumacher, the Editor of the eminent Astronomsiche 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 part 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 perusal46 — however much his read-through eventually moved Airy to creditable praise for Leverrier: see §13.)

This ms later appeared at AstrNachr 25:53-80, 1846/10/12-22 (a final part-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’ 1st 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),47 and he possessed expertise48 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 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 advice49 to Adams, even in spite of the various differences between the two
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,26 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-consipirator 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?27

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.)

[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 censored version of M16:412-413 at DB 3:186-7) that he had seen (& come within a whisker of capturing) Neptune on 1846/8/4 & 8/12, during the clandestine Cambry Obs search. History generally regards this near-miss as a tragedy for Adams&co. 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 “Oceanus” 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 exude 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

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.Graßhoff History of Ptolemy’s Star Catalogue Springer-Verlag 1990 pp.85, 88 (n.1721), & 162 — a book which NCS (1992 p.180) deems “very acute” (a description NCS does not apply to any analysis by RN or DR). The truth is given at RN’s Crime p.247, which argues from evidence that “the circle was probably graduated only in degrees, rather than in intervals of 30°” (in NCS’s own words on pp.246 & 252, missing those on pp.247 & 255.) Throughout the long history of this controversy, no one else (besides Grasshoff & NCS) has ever made such an error. (Not even a scholar of 0 Gingerich’s intelligence & reliability.) For anyone who’s at home with the English language (& one must mercilessly note that Grasshoff is German), it is impossible to read RN’s discussion and misunderstand the point — central to RN’s entire analysis — that RN is proposing whole-degree graduation of the astrolabe’s longitude and latitude circles.

C9 Nonetheless, the section of NCS’s paper containing his amazing, unoriginal false allegation (that RN assumes half-degree graduation) is cited2 to RN’s book, not Grasshoff’s. This must be what 0 Gingerich has in mind when he speaks fawningly of NCS’ “meticulous scholarship” (JHA 23.2:149, 1992). In an equally “meticulous” 1979 book review — appearing in the journal of Phi Beta Kappa! — NCS falsely imagined he had detected RN’s use of an unquoted source. So he of course made the most dishonest move to RN. (See NCS at American Scholar 5 fn 6.) It is particularly ironic that this NCS attack on RN’s integrity was published as part of his review of RN’s 1979 book (Crime) — the very book we now find him unfamiliar with THE central novel argument of! This exquisite longterm serial-embarrassment not only enhances (in the Mufia’s special way) the reputation of Phi Beta Kappa, but even more so the Journal for the History of Astronomy, where NCS is a proudly-displayed & perfectly-apt Advisory Editor — and where his equally strange review of another of RN’s Johns Hopkins University books appeared, a review which showed (as an incredible RN demonstrated in DIO 1.1 §5) that NCS did not even understand the purpose or the very title of the book. . . . (I don’t believe that NCS has yet, in his standard gentile fashion, attacked some poor unoffending nonMufia scholar for fake book reviewing. But we NCS fans are all looking forward to the day when he does.)

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

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


28 Just as Gingerich was a top Editor at the JHA, when it published NCS’ insulting 1981 attacks on RN (sample at DIO 11 §C7); so also he was on the Editorial Board of American Scholar when it published NCS’ 1979 diatribe. 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 Mufia agent — verbally diffusing the Mufia 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 Mufia&clones, behind RN’s back, that it was accepted for awhile even at Scientific American, from whose people I heard it in 1978-9. Why shouldn’t Mufiasuck open debate, if academe lets them get away with fighting opponents this way instead?)

Having alibied the longitude problem to his satisfaction, NCS next tries explaining away the latitude-error $\Delta \beta$; the expression for which is so much smaller (than $\Delta \lambda$) that one might hope for a more capable outcome. Alas, even it defeats NCS (Archive Hist Exact Sci Ed Brd); he states (NCS 1992 p.176) that this error “appears to be absent from the catalogue although Peters had already found a more complex error for zodiacal stars, very roughly of $\Delta \beta = 20^{\circ}\cos(\lambda + 35^\circ)$.” (This equals $16^{\circ}{\cos\lambda} - 11^{\circ}\sin\lambda$) 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\(^\text{29}\) which NCS permits the reader to believe was Peters’.

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

NCS has simply eyeballed his $\Delta \lambda$ 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 asymptotic) peaks of the Peters’ $\Delta \lambda$ curve are precisely $40^\circ$ from the equinoxes, not $35^\circ$. (The difference is not large but it unambiguously reveals the effect of “neutral” NCS’ desire to wrench the phase nullward, in order to exaggerate the cosine-term’s coefficient, hoping to get it as near as possible to NCS’ desired $29^\circ$.) 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

\[^{29}\] See analysis at Rawlins 1982C p.361 & Fig.2 there.

\[^{30}\] Regarding the first NCS claim: basic amplitude $= 1^{\circ}3/4$; $\tan = 29^\circ$ (where $\epsilon = \text{obliquity}$, so for $\beta = 25^\circ$, the wave $\Delta \lambda = 29\tan 25^\circ\sin \lambda = 14^\circ \sin \lambda$, which (with $\cos$-acm $12^\circ$) is far from undetectable in this context, as the merest glance at the wellknown Peters error curves will make clear. (At least NCS does not go so far as Graßhoff, who, at p.167 of his 1990 Springer-Verlag book: [a] confuses Proxima’s $2^{\circ}/2^{\circ}$ precession since-Hipparchus with the real $3^\circ/4^\circ$ precession over this timespan, and [b] claims that the waves produced by such a longitude mis-set — of amplitude $3^\circ/4^\circ \tan = c.100^\circ$! — constitute “a small periodical error . . . so small for both coordinates $[\lambda \& \beta]$ that it cannot be significantly tested.” Amplitude isn’t Graßhoff’s only Waterloo: his work also includes a key error in phase of precisely $180^\circ$. It doesn’t get any better. What BrownU talent-scout did Springer-Verlag employ to locate such Muffia expertise? As for the latter NCS remark: it is based upon National Foundation Science-grantee Swerdlof’s untouched impression, that since $\tan/\beta$ of course approaches infinity as $\beta$ tends to $90^\circ$, then the above $\Delta \lambda$ wave expression will become impossible to deal with, as one nears an ecliptic pole; but anyone of experience in this work knows that the real error here is great-circle measure (see the weighting discussion at DR 1982C p.366) — and for $\Delta \lambda$ in $29^\circ\sin \lambda$, which merely goes to $29^\circ\sin \lambda$ near the S.ecliptic pole, (opposed at the S.ecliptic pole, which is not included in the Mediterranean-based Catalog.) To a scientist with even modest ability in spatial relations or possessing merely underlying familiarity with the peculiarities of a spherical coordinate system and its axis, this result would be self evident. If I may offer a slight connection to the shock sending suggestion to the JHA: could it try to find reviewers who are capable of performing the math they are purporting to analyse? Is that really possible at all? Well, in a Hist.sci community that takes Muffiosi seriously, maybe it is.

\[^{31}\] C.H.F.Peters Vierteljahrschrift der Astronomischen Gesellschaft 12:296-299 & 341 (1877), p.299. The translation at C.Peters & E Knobel Peters’s Catalogue of S stars (PK Carnegie Inst 1915 p.8: “[the — Q] curve of the J.A. in latitude has a maximum near $[\lambda \epsilon] = 140^\circ$”, and a minimum near $220^\circ$.” Peters’ 1877 graph of the errors $\Delta \delta$ and $\Delta \lambda$ is reproduced on PK p.6.) Peters does not attempt to fit sinusoids to his curves; if he had, he would not have arrived at a latitude phase anything like NCS — since, though the irregular curve happens to exhibit extrema about $40^\circ$ from the equinoxes, nonetheless, the bulk of each hill of the latitude curve is more nearly $60^\circ$ from an equinox.

\[^{32}\] Glaisher 1896 p.xxxvi (emph added): Airy “was a man of vigorous character, and it seems unreasonable that he should have taken no steps to secure the publication of Adams’s results, even after his correspondence with Le Verrier in December 1846. It is true, he may have written to Le Verrier on 1 December (12/1)…”, but “Le Verrier was not confirmed by the fortunate Frenchman the facts ought to have been out without more delay. Was Airy ever so much as told that Le Verrier was at his heels? Our astronomers ought to have got up a flare in an instant.” (See also [H3] Airy’s excuse is given at Smart 1947 p.40.

\[^{33}\] See also the authoritative context in which NCS has simply eyeballed his $\Delta \lambda$ 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 asymptotic) peaks of the Peters’ $\Delta \lambda$ curve are precisely $40^\circ$ from the equinoxes, not $35^\circ$. (The difference is not large but it unambiguously reveals the effect of “neutral” NCS’ desire to wrench the phase nullward, in order to exaggerate the cosine-term’s coefficient, hoping to get it as near as possible to NCS’ desired $29^\circ$.) 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

\[^{34}\] Glashier 1896 p.xxxvi (emph added): Airy “was a man of vigorous character, and it seems unreasonable that he should have taken no steps to secure the publication of Adams’s results, even after his correspondence with Le Verrier in December 1846. It is true, he may have written to Le Verrier on 1 December (12/1)…”, but “Le Verrier was not confirmed by the fortunate Frenchman the facts ought to have been out without more delay. Was Airy ever so much as told that Le Verrier was at his heels? Our astronomers ought to have got up a flare in an instant.” (See also [H3] Airy’s excuse is given at Smart 1947 p.40.

\[^{35}\] See also the authoritative context in which NCS has simply eyeballed his $\Delta \lambda$ 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 asymptotic) peaks of the Peters’ $\Delta \lambda$ curve are precisely $40^\circ$ from the equinoxes, not $35^\circ$. (The difference is not large but it unambiguously reveals the effect of “neutral” NCS’ desire to wrench the phase nullward, in order to exaggerate the cosine-term’s coefficient, hoping to get it as near as possible to NCS’ desired $29^\circ$.) 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
Neptune's Discovery Brings Adams & Challis to Life

We note that Adams asked (through Challis, 1845/9/22) 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 Sheeckphans 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 with §6 item [c] & fn 36.) Adams had published orbital elements of a comet in 1844 (Grosset 1862 p.82) and had delivered a paper to the Roy.Astr.Soc on 1846/4/8 (MNRAS 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?

D2 The French were robbed of priority by British maneuvering,37 the most outrageous part of the process being that, as the theft proceeded, strong French public expressions of suspicion were used to show how irrational and undeserving the French were! The 1846/12/5 Athenaeum p.1246 refers to the great physicist & top French astronomer F.Arago’s “distorting mirror of national bias” and his “mania”. Airy to Sheeckphans (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: Athenaeum 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 academia 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 & 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 from his premises to be made in a trustworthy manner.” Airy’s reply to Adams’ 1845/9/22 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 inviting 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 1998 pp.12:6 prominently & repeatedly confuses Uranus’ radius vector perturbations with Neptune’s mean distance.) Airy was extremely hard-nosed one time finding regularly in the extreme endly 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-and-villains” approach just repeats of 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 established 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 in 1847/3/18, published in the 3/20 Athenaeum 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.)

38 Isn’t this just the sort of bibliographical offense which NCS used to condemn as dishonest (see DIO 1.1 §6 fn 6), when he imagined RN had made such a revealing slip?

39 After two decades of privately calling RN&D&R nuts (which caused no public concern among the Hist.scii archons who honor NCS) and after refusing for over ten years to reply to the ironclad DR error-waves argument (snobbery which also caused no public concern among the Hist.scii archons who honor NCS), NCS has now been shown to lack an effort to respond to the toughest argument of those skeptics he has been slander- ing — a situation which continues to cause no public concern among the Hist.scii archons who continue to honor NCS.

37 Sent to the JHA in 1846.

38 If Ptolemy observed the Catalog, then [Δβ/Δ] should equal about 29° near the equinoxes. But the Peters curve’s mean curve was not absolutely less than 1° — in fact perfect agreement. Airy’s 29° is perhaps a superficial point, but it should at least have given pause to those attempting to pretend that the Peters curve suggests the presence of anything like 29°cosλ in the latitude residuals.


40 Quoted at the JHA in 1975.}

[The rest of the text continues with further analysis and discussion.]
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 $\Delta \beta = 9^\circ \cos \lambda - 4^\circ \sin \lambda = 10^\circ \cos (\lambda + 25^\circ)$; this $10^\circ$ amplitude is catastrophically far below the $29^\circ$ amplitude that must exist if Ptolemy observed the Catalog.

C15 Since Mufosis cannot answer DR’s error-waves argument, the frantic dears must ignore, miscompute, rearrange, and-or distort the actual errors inadvertently DR-confirming phase & cosine coefficient. (Even the pre-\textsuperscript{43}C14 amplitude needs Mufis-massaging, since $18^\circ \not = 29^\circ$.) 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 see these same charmingly programmed Mufia myopes upon the task of fiddling phase and amplitude of the effects observed by, say, Bradley & Bessel. When the sand-in-the-eyes settles, we’ll find that Bradley discovered stellar parallax & Bessel beat Chandler to his Wobble . . .

C16 A final note on the Peters graph of the actual zodiacal latitude residuals: NCS refers (\$C11) to the “complex” shape of its curve. I.e., the curve’s $2^{\text{nd}}$ major peak (centered on $\lambda = c.300^\circ$) is too broadly flat for a pure sinusoid. In typically sterile selective-agnostic Mufia fashion, NCS sees this situation strictly in a whew-we-barely-slipped-out-of-that-clefsit stick 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 Mufia a month (1992 Nov) to work on the same problem. . .

C17 Delambre (1817) and DR (1892C) emphasized the total absence of stars from the $5^\circ$ 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 surely to list the stars in and near recognized constellations, and since these were formed around the Aegean there was no reason to include additional stars not then included within constellations even though visible near the southern horizon in Alexandria.”

DR’s comments: [a] Ptolemy’s version of the Catalog (Almajest 7.5-8.1) contains dozens of stars explicitly labelled by him as “unformed” stars not belonging (though loosely attachable) to the traditional constellations. [b] While most of the constellations Ptolemy adopted were established by the time of Aratos (c.275 BC), Ptolemy is perfectly correct in asserting that he has done so “. . . the descriptions which we .

\textsuperscript{41}While the large sine coefficient is primarily due to obliquity-setting error, the cosine coefficient’s non-nulility merely reflects the fact that the sky moves a little during the few moments between the astrolabe-observer’s setting of rings 5&3. (\textsuperscript{50}JO3 1.1.7)

\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 Eraosthenes’ obliquity, $23^\circ 51^\prime 20^\prime$), his obliquity-related error wave would be $-9^\circ \sin \lambda$. Instead of assuming that a given attested obliquity must have been accepted by the observer, DR 1982C instead used the actual error wave (Peters’ curve) to solve for the observer’s adopted obliquity — which came out as $23^\circ 56^\prime \pm 1^\circ$ (DR 1982C eq.27).

\textsuperscript{43}I am not asking the Mufia to assent to specific interpretations. But I am challenging Mufiosis in this sense: Mufises 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{44}Since the widely rumored belief is that the RGO Neptune line was the sine coefficient to virtual nullity. And it must be equally depressing to those attempting to reconstruct British activities, including the central document of the case, Britain’s holiest Neptune-chase relic: Adams’ three-page memorandum (allegedly 1845/10:21) transmitted by 1 planet’s elements to Airy. I am calling this MemoR. (MemoR is now available only in photographic facsimile: SP pp.Ivi-ivii and Jones pp.15-17. See Chapman 1988 p.125 n.21: “Original untraceable at RGO, presumably 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?) [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 observer knows what day it is. (Is it credible that Airy — unequalled in notoriety among astronomers for his obsessively precise and compilatorial habits — would not date this memo immediately upon receipt?!) [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-xxxi). 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: \$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. Why is it that we supposed memory-unreliability is the anti-conspiratorialists’ sole refuge in this case (e.g., $JL$-[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 fudge (putting the party within 100 mi of the Pole) strictly in order to induce his magnificently courageous chief to turn back. before the party plunged fatally over into what the title of Shackleton’s popular book rightfully called The Heart of the Antarctic (which was he 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° S) v. v. the Scott-Wilson-Shackleton 1902 record of 82° 17’5”, which shaved survival-odds right up to the human limit.}

\textsuperscript{43}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 1899/7/28) — evidently because it reflected so badly upon Astron Royal Airy.

\textsuperscript{44}The safest prediction DR ever remorse has been the “missing” RGO file is someday “found”. DR will not overstate unless after another (politically Reliable) scholar has published a suitably-mild, soften-the-blows version of its contents.

\textsuperscript{45}E.Maunder records (as quoted at Smith 1898 n.27): Airy “devoted an entire afternoon to himself labelling a number of wooden cases ‘empty’ . . . His friend De Morgan jovially claimed that [if] Airy wiped his pen on a piece of blotting-paper he would duly endorse the blotting paper with the date and particulars of its use, and file it away amongst his papers.” See also below \$D1.
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 previous number [1845/11/10] I strongly recommend you to consult. Both [Adams & Le Verrier] have arrived at the same result, viz. that the present [ecclipsal] 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.

A3 An innocent interpretation 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 legend’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 curiously historical process whereby most British children ended up believing (as a justifiably disgusted Amundsen reports in his 1927 autobiography, My Life as an Explorer) that the British expedition chief scientist R.Priestley (subsequently Pres BAAS) told my wife and DR on 1970/8/11 that Marshall, main navigator of the polar dash, later “went crazy” and said he’d faked the southernmost data (DR Peary at the North Pole: Fact or Fiction p.82). The point is conceded in the new standard biography of Shackleton by R.Huntford (Shackleton 1985 p.311), which suggests, as I have always stated, that:

44 To quote from DR’s Queen’s Quarterly paper (Rawlins 1984a), p.973: “The reason that Polya’s stele [Canopic Inscription] was erected at an Egyptian miracle factory is: that’s where he worked — forty years at Canopus, an infamously licentious town which was an ancient combination—of Hollywood, Lourdes, and Las Vegas. The ultimate enshrinement of Polya’s stele 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 of those celestial bodies — the Canopus twin: the Canopic Inscr (Canopic stele is dedicated), and probably met Polya in person. A copy of the temple was soon erected in the ‘Canopic Vale’ of Hadrian’s Villa. A group of stars . . . AQUILA were named for Antinous . . . . Some twentieth century star-guies — e.g., Olcott’s — have carried Antinous as a minor constellation, an apt memorial for an Asia Minor minor.”

NSC 1992 p.182 concludes that the Catalog issue is a question that cannot ever be resolved; meanwhile, he has resolved that Polya was a genius. I.e., NSC has perfectly inverted the actual situation as to how much we can know.
posed Ptolemy-observing scheme, which: [a] was a Velikovsky-style victim of Collective Amnesia (since neither Ptolemy nor any other ancient astronomer ever mentioned it — NOR DID MUFFOSI, until recent RN&DR-proposed crucial-testing cornered them), and [b] is more wildly comic than the surreal sobriety-test fantasy in the cinema-farse The Man With Two Brains. (Hollywood screenwriters have to use drugs to get this high. How does the Muffa 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 – 1.5°, 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 C5 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 Muffa, innovently heeding the Vulturines’ quiet but inescapable call to faith in Ptolemy’s greattess [and originality (and his critics’ idiocy)]. As new test after new test came out against Ptolemy, Muffosi 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 and 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” ([JC]19), “but I think this part of the analysis should be carried further” (p.180, emph added). Note the remarkable coincidence that: the only evidence (bearing on whether the Catalog was stolen), which NCS desires Further-Research into, is that which (even he thinks) looks bad for Ptolemy as things now sit! This tactic is a faithful repeat of what DR found long ago was standard among the very Veloksiokivans.

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 jarry with ring 2, we read where ring 2 meets ring 3 AND THEN ADD BACK, onto THIS READING, the AMOUNT WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . .

I suggest that nobody ever seriously considered this, but the following statement is that which as the only actual evidence of Ptolemy’s own actual error I think is one of the most likely.”

C25  Note also that co-ploter J.Herschel’s letter of the same date (1846/10/1; Athenaeum 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 letter41 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 1/4) would require only about 80 hrs per sweep. When Challis’ 7/18 letter resisted, suggesting not simultaneous use of the telescope, Airy on 7/21 (CON #5) warned against equatorial motion, adding “I think you will find my plan sufficient even when stars come thick.” At M16-404, Airy was modest & merciful to Challis on this point. (Challis’ 10/1 text: “About four months ago, Mr. Adams, of St. Johns College, wrote me [1846/10/7] a letter, in which he said that he had worked on the problem of the planet’s movements for about three years, and that he was now convinced of its existence.”)

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 unrevealed but critical point: Leverrier, by publishing his prediction before optical discovery, took all the chances of embarrassment if no planet turned up; published British attempts to take a share in the glory were entirely post-discovery and if they are allowed will only encourage purely invented claims. Given, e.g., the mess Airy made for himself by involvement, I personally do not think it credible in this case that Adams’ work was 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 [§B1]). But [a] I should not have to make that judgement & would not, has Adams published even before the Demonstration Berlin Observatory discovery, and [b] it is unendurable 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 Cambre Chronicle; retracted 10/16: both newsclops preserved, as CON #15&16) stated that Adams’ work was completed only in about June of 184640. Note also that co-ploter J.Herschel’s letter of the same date (1846/10/1; Athenaeum 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 letter41 to Whewell, does say that

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42 This was the basis for the Airy 1846/7/13 plan’s estimate that the triple-sweep he recommended (zones of breadth 1 1/4) would require only about 80 hrs per sweep. When Challis’ 7/18 letter resisted, suggesting not simultaneous use of the telescope, Airy on 7/21 (CON #5) warned against equatorial motion, adding “I think you will find my plan sufficient even when stars come thick.” At M16-404, Airy was modest & merciful to Challis on this point. (Challis’ 10/1 text: “About four months ago, Mr. Adams, of St. Johns College, wrote me [1846/10/7] a letter, in which he said that he had worked on the problem of the planet’s movements for about three years, and that he was now convinced of its existence.”)
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 325°. (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**25 J.Breen (Airy 6/30: “a rough genius… beautifully tractable”) to the search region, 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” (AstroNachr 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 later fury — quoted at Rawlins 1984N & above at §B5 — when he learned that Challis had at this very time been keeping from him Adams’ confirmation of Leverrier’s predicted position for Neptune.) Faye is quoted by Hind as expecting to spot the planetary query by searching for a disk of diameter about 2", following Leverrier’s 8/31 advice.**26

**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 Leverrier was pointing, presumably looking for a disk. Challis’ later accounts (1846/10/21 AstroNachr 25:102; 1846/12/16 SP liii) 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 Leverrier’s (not Adams’) instructions ever since 9/21.**27 Challis instead publicly claimed that he had switched back to following Leverrier’s guidelines only on 9/29, the Con of the search’s very last night, when Challis’ zone records state that he saw Neptune’s disk.**28

**25** AstroNachr 25:103. Elsewhere, Challis estimates the power at 160 (fn 30).

**26** It has always been assumed that computer Breen was sent to Cambridge to help Challis. Was he actually sent also to help Leverrier?**29

**27** Faye’s letter confidently repeats his friends’ assurances that he is most likely to be elevated to fill the recently deceased Baron Damoiseau’s place in the Academy of Sciences. In fact, Leverrier got the position instead, because of the very discovery that was the subject of Faye’s letter.

**28** Leverrier’s final paper, 8/31, would have been available in England almost exactly at this time. (Regarding how long it customarily took the Comptes Rendus to reach England, see §J4.) Also: CON #31 is an undated slip of paper in Challis’ hand noting the “Verrier” 8/31 paper’s proposed longitude limits for his planet, taken from Leverrier 1845 p.436. 321° to 335°. So Challis almost certainly knew by 9/21 that Leverrier’s 8/31 paper was pointing farther to the east than had been suggested.

**29** It has not been previously pointed out that Challis’ failure to check this reported disk-observation immediately under higher magnification is strange: his use of an equatorial telescope (rather than the usual fixed transit instrument designed for such positional sweep work) allowed the advantage that whenever he wished to stop and examine a region finely, without the sky’s diurnal rotation quickly carrying it away, he could do so by engaging the telescope’s clock drive. (See his detailed 1846/12/12 description at SP 181.) It was this feature of Challis’ search-plan that fatefully helped slow it (and reduction of the data) because he desired (M16:405) to take all stars down to magnitude 10-11. Airy’s detailed 7/13 search-plan had set no magnitude limit and had instead proposed employing the Northumberland equatorial only for its greater light-gathering power, intending it to be used otherwise as a transit instrument. (The telescope’s objective had the misfortune to be French, so definition of images was not high quality. The 1835 NCS compares RN to. Recall also the comment at DIO 1.12 fn 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 apt to express it for us (§C25): research on the Catalog is back to Square-One.**52

**C25** The overwhelming array of evidence against Ptolemy ensures that skepticism on the Catalog will continue, so the loyal Mufa will stand ever vigilant to defend its weirdo question of its originality remains unanswered” (NCS 1992 p.182). Or, as another humorist is apt to express it for us (§C25): research on the Catalog is back to Square-One.**52

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 stumped, Bob! Back to square one!  

THIRD SCIENTIST (entering the room): Hey, can you two guys lend me a hand?**53 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

**C26** 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**55 & Gingerich (Science 1976/8/6) for Ptolemy’s – 1°.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 [ELI] 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.)

52 Mufa strategy at this desperate juncture resembles the tobacco lobby’s primarily in that there is finally no longer any real scientific innocence — no, the approach now is: “keep the money rolling in) enough to maintain the sham of a continuing “controversy”, in order to pretend that one hasn’t lost. National Geographic long used the same damage-control ploy to protect its Peary N.Pole lie. And TV networkdom’s tactic regarding the patently deleterious effect of TV violence on young viewers is similar: nothing­has­been­proved — so let’s just keep on profitably peddling الساد to kids.

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

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

55 Laplace was himself a notorious nonciting adopter of others’ work. See, e.g., G.Airy Report . . . [BAAS 1831:1 London 1833].

56 Noted at DR 1982C p.359.
b) NCS' preferred vision of Ptolemy is of a scientist who spent years observing 1000+ stars outdoors with his astrolabe — yet never, during all this time, did NCS' ancient precursor-in-geniusdom manage to notice that his observatory's latitude and the semi-diameter of the Sun never exactly matched. Nonetheless, NCS' ancient precursor-in-geniusdom manage to notice that his observatory's latitude

and the semi-diameter of the Sun never exactly matched. Thus, 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 simple as an ancient astrolabe'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 Mutfa, 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 DJO 1.1, which provides yet more novel evidence, positively attaching Hipparcos' 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, the JHA decrees it would be best to just end the Catalog controversy right here. Megalomania rarely achieves such heights of unreality.

C28 Unreal 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 Mutfa claims — which he is now himself so brilliantly augmenting! — that has left the Mutfa looking about as convincing as Ptolemy. But NCS wants no moratorium on Mufosi continuing to try to figure out new alibis for Ptolemy. [b] Given its tenuous hold on reality, the JHA perhaps even imagines that DR will submit a paper directly to the JHA in response; so, while it has left open the possibility of publishing some of more of its incomparable Mutfa research on the Star Catalog, JHA’s "moratorium" is now in place, in print, as a pre-set official-excuse for rejection of a [believe me] PURELY hypothetical direct DR submission. Isn't the JHA a treasure?

C29 After 5 years & dozens of pages of failed JHA attacks on 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 Mutfa'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:

C30 Seldom has a party of "experts" been so utterly defeated (and by scholars it excised as fools) — so bare of substantial, coherent reroot — that its ever-so-clever strategists

57 See above at §C22 item [f].

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

59 Following NCS 1992, we find: "Editorial Note: This article was received in June 1991, but was held over to permit publication of the Exarn Review already commissioned from James Evans, which appeared in our February issue." (Evans, too, defended Ptolemy — & failed to cite DOI 1.1, though the paper is known to Hist:sc. in f'r.) The timetable alleged (who asked?) pseudexcuses noncitation of the very DOI 1.1 that triggered NCS 1992.

60 However, see the DOI publisher's statement at the back of this issue.

61 I.e., since Mufosi have been damming skeptics for decades as incompetents, one would expect pages of examples of the purported incompetencies to be forthcoming. Mufosi's occasional efforts to expose alleged errors
examine not only the ineradicably 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: §6.6 & fn 31) — but also the eyepointing 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 RGO] 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 4 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 "Memo W" which Adams computed & gave to Challis: ephemerides of geocentric places (for hypothetical planets at various heliocentric longitudes), for 20-day intervals starting 1846/7/20.19 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 hypothetical planets at heliocentric longitudes ranging from 336° (Memo W, CON #35) to 315° (Hyp X, M16:407) — a range of over 20°! (Tables 1 & 2, below, provide 1800-1850 ecliptical longitudes corresponding to these spherical coordinates.) Challis' long-lampshaded indecision in his search was not due to a personality disorder (as is now commonly & abusively charged) but rather to Adams' conflicting directions for (as) himself. Another equally remarkable & heretofore-unknown point: Memo W’s 20-day-interval ephemeris, 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 lost20 star #1007 & Wattmann's weird 1831 alleged planet-sighting [note added 1997: see P.Baum & W.Sheenan 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.Dewhurst) suggest that, at the very time when Adams was arriving at his extrapolated solution Hyp X (up until 9/18-21: see §38), 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 CambroObsNepNeit 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-deviation arithmetic scheme (fn 21) exhibiting some impossible asymmetries about oppositions, and none of them perturbationally-based orbits. Note: Challis' false public implication (M16:421) that Memo W's ephemeris 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 1847/8/18 letter defending Adams' price for the discovery of the planet — fn 37.) To the contrary, the central epoch for Memo W's ephemerides, 1846/8/29, is simply the opposition date of the Wattmann-based hypothetical planet (fn 21): no relation to Adams' perturbation-computed planet! Moreover, the heliocentric longitude used (325°) was that which Leverrier had published on 1846/6/1 (and Adams only agreed with it by the accident of later corrected 1845/5 MemoW math error: [F2]); and MemoW's limits, 315° to 335° are exactly those already published the previous month at Leverrier 1845-6 p.917. (CON #34 is an undated slip, in Challis' handwriting, summarizing the Leverrier 1846/6/1 paper: "in assigning 325° for heliocentric longitude of the planet for Jan 1, 1847, on ne commet pas une erreur de 10°."

The French is copied verbatim from Leverrier (CON #34); the MemoW calculations written by Adams upon this document makes it clear that Adams was in on the secret CambroObs sweep from the start. (Given the 20-day interval & the 7/20 initial date, we may say that MemoW was probably written after 1846/6/30, certainly not after 7/20.)

20 Star #1007 of J.Flamsteed's British Catalog does not exist, and Adams and/or Challis supposed it might have been the planet.

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 grantflow. Seasoned veterans of the game know better: happily, Hist.sci grants have not the slightest (positive) correlation with the grantee's accuracy or genuinely expert original scholarship. (As DIO readers know all too well.) So, we can relax. (Likewise, professional astrologers' amusing inability21 to compute horoscopes correctly has no effect at all upon their clients' generosity.) Further: by this time, so many Hist.sci archons' reputations have been invested into the glorification of Muffia scholarship, that the cult cannot be permitted to be seen as having erred catastrophically in anathematizing RN-DR. Therefore, our favorite Unsinkable cult will positively insist on

by skeptics have been so pathetically thin that it is by now all too clear that the Muffia klan has simply been bluffing in this regard. (Note the feebleness of Muffia attempts in this direction: DIO 1.3 in 252.) See DIO 1.15 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 (Skq) 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\textsuperscript{64} heterodoxy persists nonetheless. (Even Time-Life’s popular
\textit{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 §C7.)

\textbf{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 & effectually 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 mou’t pieces are irrevocably committed to forever clinging to their precious
pretense that DR’s historical scholarship is utterly worthless.\textsuperscript{65} This point is so sacred to
Muffiosi that, in order to maintain it, the Muffia will pay any price (primarily: internal rot)
—and, in order to cloak its ineptitude with the trappings of Reputability, will woo into its
muck just as many major academic institutions as it is able to con into sharing that price.
(Terseness borrowed-with-credit from etiquette-authority NCS’\textsuperscript{1} IC5-sampled lexicon.)

\textbf{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?)\textsuperscript{66} Also, my mother’s father was
a prominent Maryland lawyer. And she married my friend, advisor, & stepfather, John
W. Avirett\textsuperscript{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: \textit{the lawyer-joke-to-crown-all-lawyer-jokes.}\textsuperscript{67} Ready?

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

\textsuperscript{64} Yes, hated. See \textit{DIO 1.1} \textit{\&} fn 20; \textit{13} \textit{\&} D2-D3.
\textsuperscript{65} Curiously, the Muffia’s null evaluation of DR’s scientific-history production is not shared by: the American
among others. Likewise, the prominent scientific historians: K. Moesaard (U.Aarhus, Denmark), S.Goldstein (UVA,
Charlottesville), B. van der Waerden (U.Zürich), Curtis Wilson (St.Johns, Annapolis). (Also the late R. Newton
of Johns Hopkins \& W.Hartner of U.Frankfurt, Germany.) Each has published or supported the publication of
DR science-history researches. Thus, Muffiosi’s 100\% rejection of these papers implicitly accuses each of these
institutions & scholars of incompetency.

\textsuperscript{66} Are top lawyers who revel in lawyer-jokes retching at the low end’s ethics? Or, is this stroke of humor
generally reproducible 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.

\textbf{DIO} dedicates this joke to another joke: the Neugebauer Muffia — in honor of that cult’s highly original
notions of 18C, that human-wealth-privacy, to not mention its unquestioned talent in sucking tax monies out of the
system, to fund its defense-lawyer fantasies.

\textsuperscript{67} Anti-lawyer line from [Marlowe] (\textit{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 skin of an innocent lamb
should be made parchment? That pamphlet, being scribbled o’er, should undo a man? . . . I did but seal once to a
thing, and I was never mine own man since.”

\textsuperscript{68} The title of R.Newton’s 1977 Johns Hopkins Univ book (\textit{The Crime of Claudius Ptolemy}) is still freaking out
Hist.sci archons, e.g., \textit{HHA Editor-for-Life} \& renowned Britwit, Lord Hoskin. See \textit{DIO 13}\textit{\&} B2.

\textsuperscript{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’ 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.

\textsuperscript{1} 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
nondeliberation-therapy Smith is loyal to, throughout the rest of his paper.
B The Stealth: Stealth & Disaster

B1 At the very moment Airy was keeping from Leverrier the Adams prediction’s agreement (with Leverrier). Airy & the above-mentioned tiny clique of fellow Cambridge scientists secretly plotted (1846/6/29: §A8) to launch, explicitly on the basis of the agreement of the 2 predictors’ solutions (§B5 & M16:400), a massive telescopic sweep for Neptune at the Cambridge Observatory, privately assisted by a loaned Greenwich Observatory employee of Airy (J.Breen. (The plan was sufficiently secret that no mention of it was entered into the private minutes of the 6/29 meeting. I thank the late P.Laurie of RGO for sending a copy of these.) The customarily lordly Airy so longed after the glory of the imminent discovery that he lowered himself to pleading & beggimg co-conspirator J.Challis, director of the Cambridge Observatory, to get moving on the search with Challis’ big telescope (which had been installed years earlier by fellow-plotters Airy & J.Herschel, who thus stood to bank on Airy’s fame). Without Challis’ help, Airy said the situation was “almost desperate” — adding (M16:403; see also fn 31) that he even intended if necessary to pay the cost himself!

B2 Among academic archons (who evidently attain power and suppress their critics, without ever planning anything at all), there is a boringly predictable tradition which

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11 Peacock’s “severe image is now preserved in a stained-glass window at Ely Cathedral!”

12 To Babbage’s credit, he later examined all the “accessible” documents and concluded that Leverrier deserved prime rˆole in the discovery. Airy “snubbed” Adams, etc. (See, e.g., Smart 1947 p.38. R.Smith’s crucial discovery of Airy’s 1846/6/25 letter to Whewell — §A6 & fn 31 — utterly and finally eliminates that durable popular legend. See Smith 1989 n.25.) I don’t doubt that Airy felt betrayed by some of Adams’ behavior: the curious footnote of Smart 1847 p.42 affirms perplexity at what seems obviously an Airy suggestion that Adams, whatever his math skills, was distorting the Neptune history to promote himself (§A6); a fragment of Airy’s letter reads: “I must have a very low opinion of those [the context makes it obvious that Airy is referring to Adams & his hearers] who have so taken it up that my old friend [Sedgwick] has felt himself obliged to question me as if I were a common criminal”. If Adams in 1846 June asked Airy not to mention his then-progressing work (& we note that Adams himself said nothing to expert Hansen at their July 2 encounter) but now after discovery was pretending he had taken it for granted that Airy had distributed his work (his pretense that MemoC was virtually identical to MemoR: fn 59). (Item [2] is essentially new here: Morton Grosser, author of the standard volume The Discovery of Neptune [Vol.2 1975 p.114, emph in orig], sent in response to a written complaint to Airy: “On the part of the [letter relating to Mr.Adams and the Glory of the Greatest Astronomical Discovery of Modern Times, etc., etc., etc., I have no remark to make.”

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‡9 The Neptune Conspiracy

British Astronomy’s Post-Discovery Discovery

Summary

Britain’s J.Adam’s is generally believed to be the prior of the 2 pre-discovery locators of Neptune via math analysis of its gravitational disturbance upon Uranus’ orbitional 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 telesopic discovery at Berlin on 1846/9/23. Detailed evidence is presented1 indicating that, throughout 1846 Summer, Cambridge University astronomers conspired to capture Neptune by keeping Cantab Adams’ work unpublished while they exploited the provocative secret that 2 men’s math had independently pointed 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 hypothesis2 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 ecliptical position (near Cap-Aqr border) of the giant planet Neptune, then unknown. This is the same jovian planet that the wonderful US spaceship Voyager 2 visited 1989/8/24, thanks to NASA.

A2 Adams is widely held to be the true first predictor of Neptune’s position & is hon­ ored for this achievement by a memorial in Westminster Abbey near Isaac Newton’s tomb. However, Adams’ rˆole 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 ofcial’s possibly­concious back-dating of the controversy’s key document. This admittedly uncertain new

1 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.

2 Unlike my 1973 conclusion on R.Peary’s hoax (which was, incidentally, explicitly anti-conspiratorial: Peary at the North Pole: Fact or Fiction? 1973 pp.4, 147, 158), the current paper’s tertiary hypothesis is not entirely certain. Undeniably: [1] Cambry U 1846 Summer secrecy (Rawlins 1984), & [2] some degree of Adamssian solution-switch (his pretense that MemoC was virtually identical to MemoR: fn 59). (Item [2] is essentially new here: Morton Grosser, author of the standard volume The Discovery of Neptune, Harvard U 1962, presumes at his pp.86-89 that the 1845/0 Adamss solution was the same as that of 1845/10. Likewise at Grosser’s 1970 bio of Adams in DSB 1 [1970] p.53. I do not blame Grosser: he was simply deceived.) However, I could be mistaken in the tentative suggestion here that [3] Adams’ “1845/10” Hyp 1 (MemoR, published 1846/11/13) was actually finalized around the middle of 1846. But I have decided to risk publishing this theory because [a] various circumstantial evidences support it, and [b] all my repeated efforts to disprove it have to date consistently met with failure. (Other scholars may prove more discerning. DIO’s Correspondence column invites their criticisms & disconfirming evidence.) I note that O.Eggan (familiar with the “lost” RGO Neptune file), in a 1970 bio of Airy (DSB 1), vaguely remarks that Adams “called unannounced the 2 men’s ‘early predictions’ (n.139, DIO 1973/3/26 note to the Admiralty) quoted by A.Meadows Greenwich Observatory vol.2 1975 p.114, emph in orig), sent in response to a written complaint to Airy: “On the part of the [letter relating to Mr.Adams and the Glory of the Greatest Astronomical Discovery of Modern Times, etc., etc., etc., I have no remark to make.”

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1 Text given more fully below: fn 67.
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: Cambr 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 more of an amateur than an astronomer. But British astronomy’s pre-dominance therein 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 not below in 1845 tested 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 had admitted of Adams’ sufficient, this solution was published only by the time it was sufficient by Adams himself since [a] he did not publish, and [b] in the months just before discovery, he went at least 2 major mathematical steps beyond it.5

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.Perce (Harvard) to A.Crawford (Cambridge). On the fable of supposing that secular resonances cause short-term resonation of 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).

I have the impression, from Adams’ omission of precession in MemoC (§44) and his roughness in MemoW (in S.Newcomb’s 1873 pp.55, 178. Soon after, Adams’ 1876 remarks (SP p.63), replacing “for several mean distances” by instead using shortcut schemes, both of which led to serious errors in perturbations (for various mean distances) by instead using shortcut schemes, both of which led to serious errors in the various required values (see Rawlins 1970G at §10.)