DIO
&
The Journal for Hysterical Astronomy
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Upcoming

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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. Hysterical Astron (Previews of Coming Detractions):
The Editors’ New Clothes.
Photographic proof: moonrise in the west.

Selected Short Subjects:

Hist.sci accepts, as genuine, famous ancient text putting Moon into retrograde!
Possible Greek Use of the 831 BC Feb 4 lunar eclipse.

16 Scrawlins

A Shorts

A1 In 1990, rich Japanese businessmen purchased1 a Van Gogh, a Renoir, and Peru. Peru was cheapest.
A2 One of the foundations of the search for wisdom is so simple that it can be crudely expressed in just a few lines: the most important -ism isn’t conservatism, leftism, Darwin­ism, theism, or whateverism. The central -ism is: Truthism. Place one’s prime loyalty there, and hold all else contingent on that.
A3 Stanislavski & Strasberg may have been great acting-instructors, but our 2 most effective dramatic-talent clinics are: Congress and prison. They differ2 in that: good acting gets you into one — and out of the other.

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

B1 Before ’88 election: [a] Bush decries Dems’ weekend release of allegedly-reformed criminal Willie Horton (whose idea of reform was to turn rapist), and [b] Bush pledges that no tax-increases would rape US pockets (by billions/month).
B2 Bush in ofce: raises taxes. Now, for the ’92 campaign, remorseful Bush swears: I­won’t-do-it-again! Social workers everywhere want to learn: how, in just 4 years, has Bush rehabbed Willie Horton from scorned Lib­symbol to fave speech­writer?

C Archimedean Santa

C2 Answer: just pull the little lever, and all your dreams will come true.

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1 The Van Gogh cost over $80,000,000; the Renoir, over $50,000,000. The bill for winning Perú’s Presidency was not reported, but the (over-the-table) cost of a Presidential campaign in the much larger US is only a few hundred million dollars. So a Peruvian election probably costs just a few million. Japanese may occasionally have paid more for another of their favorite Western land-acquisitions: US golf-courses. The Latin American nation of Peru has an area of over a million square kilometers. A few hours ago, its population was 22 million people.

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

3 Nightline 1992/1/15. And then there was USA Today’s 1992/9/10 headline (in which the President of the US comes off like a repentant 10-year-old), BUSCH: NO TAX HIKES AGAIN, “EVER, EVER”.

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D The Inequity Inequity: Rainbow MENu

The unspoken lesson of the 1991 Anita Hill-Clarence Thomas affair\(^4\) was that, in the US, the “race card” trumps the “gender card”.\(^5\) How many more decades must pass before TV news permits discussion of the lethally-revealing question: why do ethnic groups rate higher priority than women? Why are gross gender-inequities (in Congress, the Church, etc) of so much less urgent interest to the press than are ethnic inequities? The contrast is itself the worst prejudice-related inequity in the US. So, naturally, that very fact is publicly 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 will have, say, 9 male WASPS, 1 male black, 1 male Catholic, etc. — and 1 female. Hey, everybody’s represented, so everybody’s happy, right)\(^6\)

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.\(^7\) Central question (which, perhaps revealingly,\(^8\) has not been publicly asked): why bother with alliances when your own group already comprises over 50% of the electorate? Are women as innumerate as their detractors charge?

D5 Feminists also court political alliance with the male homosexual lobby (whose private attitude towards women is not abundantly respectful). Perhaps that’s why no feminist has yet gone public with an irresistible… query: is there any connection between [a] Bush running the most anti-female\(^9\) US presidency of the century (whose prime obsession\(^10\) has been packing the US Supreme Court with men he’s hoping will return women to pure babybrood),\(^11\) and [b] Bush picking that cute little boy for his Vice-Presidential mate? (Granted, such a question is inexcusably prejudicial, since the undoubted truth is that Bush selected Quayle for his mental depth & celerity.)

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

E Robert Newton & the Muffa

E1 Physicist Robert Russell Newton died in 1991. 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 Lord Hoskin (the scholar most responsible for unleashing the new journal DIO. (Happily, RN lived long enough to read issue#1 and see it widely distributed and well received.) His Ptolemy researches got vile treatment from the Otto Neugebauer-Muffa oldbypass-clone (for reasons which were, at bottom, careerist and thus largely fiscal — which make them particularly repugnant in light of the pseudointellectual veils used to hide this reality). Nonetheless, he remained admirably jocular about his various archen-smothing enemies. (I shall never forget his gentle, goodnatured attitude towards them, nor their ugly\(^13\) conniving to harm him as much as possible, for selfish professional advancement. This while avoiding any open debate while he lived. His disdain for goon provides the possibility ever to make substantial restitution to him.) A person of high mental abilities and wide culture, Newton became in his later years a rare combination of physicist and intellectual archaeologist, which resulted in a series of books (published by Johns Hopkins Univ Press or Univ Md) which used pretelescopie astronomical observations to determine the history of the Earth’s spin and revolution. If I were to specify the single quality which I most admired in him, and which ensured a firm friendship (despite numerous disagreements), it would be: when mobs of inferior minds yelped at him in unison,\(^14\) and scrambled to get in line to attack him (in order to cover the shame of the exposed archons they kiss up to), he had not the slightest interest either in muting the boldness of his theories or in politically compromising with Nibelungs. Now that he is gone, I realize all the more how rare are such

\(^4\) 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 impendning dispossess on the basis of an account of unawitnessed events (of 10° anticipated) when not a scrap of written notes was kept by the accuser — and when it appears that she took the percs (that attached to tolerating alleged verbal abuse) as long as they lasted, and only went public after that well went dry.

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

\(^6\) Lobbies’ insatiableness regarding proportions reminds one of the argument 23\(^\circ\) into the classic 1963 film b’s a Mad Mad . . . World. The US Senate’s bitty-state-placing disproportionate representation-math originated just so.

\(^7\) In the US a generation ago, labor unionism was as sacred a 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-a-vis rulers, the women’s movement has the same downside as labor: there are simply too many women. If they get riled, they’re trouble; so, the numerically-small lobbies are tolerated, but not the potentially massive ones.

\(^8\) Are most women still falling for the media-dangled short-term-easy escapist myth (implicit in ubiquitous ads for cosmetics, creams, & shampoos) of: seductress-wife as a career? If so, then feminism is faltering not because it isn’t using its numerical advantage, but because there is no such advantage — i.e., feminism unfortunately doesn’t represent the feminist movement! (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 longterm female 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.

\(^9\) One can ask women not to be too paranoid (as I do at fn 8), but when MS magazine finds it necessary to ban ads from its pages in order to acquire genuine editorial freedom, one has to guess that it has learned (from long experience with advertisers’ pressure against outspoken writing) that numerous corporate rulers of the US are as anti-choice as Bush is.

\(^10\) The lack of perspective is bizarre. There is a remark of Russell’s that applies: over a millennium ago, when civilization was collapsing, intellectually & economically, into the Dark Ages, what was Christianity’s prime public concern? — the preservation of virginity.

\(^11\) Will society’s eventual adoption of extra-uterine foetal-maturation techniques finally emancipate women? Or trigger their extinction?

\(^12\) Rôle reversal: I’m usually anti-quota, while Congress is usually pro-quota. But each of us makes an exception when he believes it believes it must for now — supposedly temporarily — ally itself with those above-cited lobbies which merely sap its potential force.)

\(^13\) See DIO 1.1: ¶ 126-C8, ¶ 13-D1.

\(^14\) Suggested profitable game-plan for Muffia-aspirants: [a] Publicly pretend to find (maybe) some slight merit in RN’s or DR’s work. [b] Watch horrified Mufflucy scurry to woo you away from your heretical error. (See DIO 1 § 9 fn 9.) [c] After sufficiently generous persuasion has been bestowed, recoveject back into the secure Muffa fold, to hosannahed thanks for your blessed enlightenment.
self-confidence and courage, in modern academe. Even some among his legion of craven detractors acknowledge that he was the subject of useful controversy. He will be missed.

E2 A critical distinction, commended here (as also that at DIO 1.1 §f. [C12]) to the consideration of those following the Ptolemy Controversy: scientists generally reserve the epithet “Incompetent” for those who are simply incapable of performing procedures necessary for the work at hand.15 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 §f. [D14f & fn 20, or DictSciBiog 11:201.)

E3 Nomenclature: 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 §f. [C7].) Or does he instead regularly meet the allegedly-errant scholar in polite public discussion at academic gatherings, where the evidential & logical merits of the matter can be rationally discussed? — and where, if the offender is indeed wrong or foolish, this can be demonstrated in an open-adversarial setting, on valid academic grounds. The 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 power-operation.

F Power People

F1 To observe astronomer 0 Gingerich (now head of Harvard’s Hist Sci Dep’t — and an ideal choice for the post) calling16 Galileo a “scrambling social climber” is as entertaining17 as finding (1976/3/12) R.Kargon, sometime Isis boardperson, accurately describing a well-known astronomer-historian-politician as “one of the biggest — kissers in the business.” Either of these eminent professors puts me in mind of Montaigne’s observation: he who gossips to you will gossip of you. (I can’t imagine why the next paragraph’s theme should follow so immediately upon admiration of the present paragraph’s magnates.)

F2 The cause of the dreary paucity of original thought in certain scholarly areas’ public discourse is self-evident: [a] One will not be listened to unless one possesses power. [b] One cannot attain power without laboring mightily towards its possession. [c] But this very effort so wipes out one’s time&energy, that there’s insufficient left over for original thought. [d] Upshot: the power operator labors for decades to get into a position where he can put over his new ideas — and by the time he’s got the power to do so, he has no substantial new ideas.

F3 When an academic biggie-editor & a productive scholar clash: the funniest item, in an age when abstract yardsticks are 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.

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


For archons long spoiled by routine assent, flattery,18 & bended knee: nongenuflection is Rebellion.) Common-sense-time: who gets his jollies, not from scholarly creativity, but through power games, fights, sadism, etc. — an editor or a scholar?

F4 Continuing a point raised in DIO 1.1 (§f. [C12]), the base reason that politically-motivated academic gangs systematically refuse to give ANY credit to an “enemy” is: every discovery, publicly assigned to that person, enhances his stature — 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

F5 When publicly assenting an unseemly truth, it is tempting to try working Within-The-System, since this is more pleasant and implicitly optimistic.20 However, [a] The more receptive The System is, the less important the issue. [b] The most important issue is: The System itself.
Two Party Ping-Pong Pocket-Plumbing

For your home’s plumbing needs, you call on plumber A. But he fails you, so you go to plumber B. When plumber B fails,21 you don’t try plumber C or D or whomever — but instead you go back to trying plumber A. Then, after plumber A lets you down again, you go right back to plumber B, etc.

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

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

Unwonted logical exercises: dedicated Dem voters regard the GOP as ghastly. (And I won’t say they’re wrong.) But none ask: what party’s mismanagement so grossed out voters that millions retched and elected Nixon. Twice. (That’s an indictment that would drive any self-respecting party to suicide. Well, maybe it did, at least in the sense that one can hardly tell Dems from GOPers anymore.) And, instead of moaning about Bush’s 1988 Willie Horton ad, why not ask: what party’s policies made that ad so effective? (What party ran US cities while they decayed into crime zones?) Answer: the Dems. (And what party’s insensitivity to poverty & simple justice so enraged 1932 voters that they turned for decades to the Dems? Answer: the GOP — which swore it would never repeat that mistake . . .)

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

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

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 23 2D) from a DRism on the former: the best cure for cancer is not getting it in the first place.

Unpublished Letters

A Mostly-Unpublished Warning

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-face-printed letter: “America is still looking for a gimmick to pull it out of an educational downturn.” Marion Gadberry, Orovile, WA.

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

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

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.

I would be more blasé about Time’s removal of the guts of Gadberry’s letter if it weren’t so de-rigorously typical. US media will not discuss “radical” (literally go-to-the-root) solutions to social decay. only band-aid “progressive” solutions are ever permitted in leading mags or TV ’snews. Thus, the only thing that progresses is the decay itself. (See DIO 1.1 2 2D.)

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 23 2D) from a DRism on the former: the best cure for cancer is not getting it in the first place.

21 Unwonted logical exercises: dedicated Dem voters regard the GOP as ghastly. (And I won’t say they’re wrong.) But none ask: what party’s mismanagement so grossed out voters that millions retched and elected Nixon. Twice. (That’s an indictment that would drive any self-respecting party to suicide. Well, maybe it did, at least in the sense that one can hardly tell Dems from GOPers anymore.) And, instead of moaning about Bush’s 1988 Willie Horton ad, why not ask: what party’s policies made that ad so effective? (What party ran US cities while they decayed into crime zones?) Answer: the Dems. (And what party’s insensitivity to poverty & simple justice so enraged 1932 voters that they turned for decades to the Dems? Answer: the GOP — 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 ording 10% — every single year.) Is the US turning into a vast company-store town?

Even allegedly reformist 3rd Parties have become increasingly suspect, starting with L.LaRouche (1976) & J.Anderson (1980) — for the simple reason that 3rd Party C may merely be regular Party A’s catspaw, injected to split the vote of (the other) regular party B. In the 1992 campaign, TV ’snews 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 dreaded D.Duke vote away into oblivion.) Simple consideration: if TV ’snews builds up a candidate (or, indeed, any Approved Leader of a warrisome 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, one learns more about US democracy than from ten years of civics courses.) Plunkitt: “I don’t care who does the electing, so long as I do the nominating.”
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>
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</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>
<td>Challis (1846)</td>
</tr>
<tr>
<td>6</td>
<td>J.Challis</td>
<td>1846/08/12</td>
<td>Cambridge</td>
<td>Challis (1846)</td>
</tr>
<tr>
<td>7</td>
<td>J.Lamont</td>
<td>1846/09/07</td>
<td>Munich</td>
<td>Hind (1850)</td>
</tr>
<tr>
<td>8</td>
<td>J.Lamont</td>
<td>1846/09/11</td>
<td>Munich</td>
<td>Hind (1850)</td>
</tr>
</tbody>
</table>

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

B3  Given Neptune’s rapid northward motion in the 1840s, it was sure to be captured by accident (as so many fainter asteroids were: not a single year’s 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

^1 This amusing item was revealed by J.Delambre.

^2 But see fn 19.

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

^4 My 1973 *Astronomy & Space* paper was perhaps the first to accent the 38° 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 1684-1781.

^5 See fn 14.

C  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 nonhibernating creature on Earth had learned that Columbus made his 1492 journey because he was confident that: [a] the globe was much smaller than it actually was, and [b] the Eurasian landmass wrapped much more around the globe than is the reality. His Earth-size estimate was low by about 25%. It is well known that he got his fateful overestimate of Eurasia’s longitude-spread ultimately from Marinus of Tyre (c.100 AD), whose geographical data underlay C.Ptolemy’s famous *Geographical Directory* (GD), c.150 AD. Columbus adopted precisely Marinus’ 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).^2

A3  The geographies of both Marinus & Ptolemy used the famous Earth-circumference of Poseidonios (c.50 BC), \(C_p = 180,000 \text{ stades or 18,000 nautical miles} \) (nmi); this is 5/6 of the correct value, which is 21,600 nmi. But: what was the origin of the huge error in \(C_p\)? — 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 and time-difference between sunset at sealevel and sunset at height \(h\). For the simple case 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).

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 downward (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 \(C_p\)): for, if the atmosphere’s effect was 6 times stronger...

^1 Cinemax promo interview; aired, e.g., 1992/10/13.

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


^4 GD 1.11.1 (Marinos 15° to 225°) vs. 1.14.10 (Ptolemy 12° to 180°).


^6 Now rigidly defined as 1,852 m, the naut mi was originally designed to be 1° (gt-circ) on the Earth’s globe; and, naturally, \(C = 360° \cdot \left(60°/°\right) = 21,600°\). For the stade’s length (185 m \pm 2%), see, e.g., E.Bumbary *HistAncGeogr* (1883) 1:209 & 624, *EncBr* 23:488H (1961), *Random House Dict* 1967; “stadium” \(\approx 607\text{ ft} \) (185 m).

^7 See S.Newcomb *Compendium of Spherical Astronomy* 1906 pp.198-203.
which genuinely first reached the South Pole on 1911/12/15.\textsuperscript{10} 4 weeks ahead of the martyred loser of the S.Pole race, Britain’s R.Scott. (Scott died on the return trip, late the following March — just short of a depot.)

B2 These Amundsen observations have long been prominently declared nonexistent by Cambridge University (1979), the President of the University of Alaska (1983), and the National Geographic Society’s hirpling “Navigation Foundation” (NavFou) — in its 1989 Report\textsuperscript{11} whitewash of National Geographic’s “North Pole” explorer R.Peary, aggressively promoted in the 1990 January National Geographic. The NavFou Report lost one leg when the scientific community creditably failed to accept NGS’ photogrammetry (e.g., 1990/38& Scientific American; also Nature 344-902, 1990/426). So, when Ted’s 1991 finding (of Amundsen’s longitude data) destroyed the other leg, NavFou-National Geographic’s deathly costly Peary-apology virus instantly reduced to pathetic pusleugle.

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

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

B5 Ted found Amundsen’s “nonexistent” transverse data right in the Norwegian edition of Amundsen’s widely read 1911 book! (Ted bought the book for $3. What he discerned in it now scuttles a National Geographic report costing ordmag 100,000 times as much.) Page 127 of the Norwegian observationsbook is reproduced photographically at vol.2 p.115 of Amundsen’s Sydpolen. In the light of widespread institutional insistence that Amundsen made no longitude observations, it is interesting to read the caption Amundsen prints below the photoreproduction of these data: “A page of azimuth and longitude observations.” Ted transmitted the data to DR, who computed\textsuperscript{13} the longitudes $W$ and compass variations $V$ from the spherical trig formulae which are standard in such work. The match

\begin{itemize}
  \item \textsuperscript{10}By Australian dating, which is used in the diaries and observationsbook.
  \item \textsuperscript{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 1.1/11 fn 14, was verbally assented to in private by the NavFou’s rep at the 1991/4/19 Naval Inst debate on Peary; but no NGS’ degree of openmindedness is as wellknown among serious scholars as is the personal nature of its positive & negative views on any issue. Nature 1990/426: “the National Geographic Society. . . will always believe [Peary] reached the Pole.”
  \item \textsuperscript{12}The NavFou also aduces Cagni’s 1900 alleged Farthest (skeptically analysed at DIO 2.2 & 6.1). 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 1.1/11 fn 14, was verbally assented to in private by the NavFou’s rep at the 1991/4/19 Naval Inst debate on Peary; but no NGS’ degree of openmindedness is as wellknown among serious scholars as is the personal nature of its positive & negative views on any issue. Nature 1990/426: “the National Geographic Society. . . will always believe [Peary] reached the Pole.”
  \item \textsuperscript{13}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 records. 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 1.1/11 fn 14, was verbally assented to in private by the NavFou’s rep at the 1991/4/19 Naval Inst debate on Peary; but no NGS’ degree of openmindedness is as wellknown among serious scholars as is the personal nature of its positive & negative views on any issue. Nature 1990/426: “the National Geographic Society. . . will always believe [Peary] reached the Pole.”)
\end{itemize}
with Amundsen’s own field calculations shows that he used sph trig. This should interest the Peary contingent, which has insisted for a decade that Amundsen did not “waste time” with sph trig and that the variously-inept R.Scott was a fool for having done so. (Scott’s critics seem to imply that he virtually deserved his ghastly death, for the crime of having used sph trig navigation! However, what killed Scott was not overprecise math but [a] censurable failure to anticipate adverse travel conditions, and [b] creditable determination genuinely to reach a Pole — instead of faking it, like some.) Indeed, Scott’s lately-much-lampedown navigation is utterly vindicated by the new findings, since (though both explorers would have been better served by cartesian navigation near the Pole), he and Amundsen used sph trig equally precisely in the same form and computational style.

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

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

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

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


C1 In J.H.A. 1.2 & DIO 1.3, DR’s “Muffla Orbuitary” 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 EQUINOX10 — as well as [ii] rewriting the canons of highschool arithmetic,11 in order to promote certain precious Hist.sci tenets.

[a] The prominent JHA-Isis articles cited contend that none of three surviving Hipparchos tris of solar observations (Almajest 4.11. 5.3&5) can be satisfied by trig-based (Greek style) solar orbits. So, DIO 1.3 helpfully supplied all three of the allegedly-impossible orbits12 — and noted that one of these Nonexistent orbits (satisfying the Hipparchos solar

14 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 observation notebooks) Mohn 1915 gave (verbally) Amundsen’s computational procedure (DIO 2.2 §5-17).


17 Readers who possess the advantage of an elementary school education may wish to check the arithmetic found in the gov-funded Muffla paper selected as lead article for the 1991/5 issue of the extremely handsome Journal for the History of Astronomy, whose highest-ranking Polymist defender is JHA co-editor and Harvard Hist.sci Dep’t head 0 Gingerich. (And the Muffla author’s followup paper led off the proud first Univ Chicago issue of the History of Science Society’s 1997 issue of the JHA.)

18 The trio revisiting the 30° = 65°05’, and the solar mean anomaly (increasing at Hipparchos’ 0° 058356/day) changes by 67 1/4 228°; 0 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 Polymist through Muffla capo G.Toomer): (327 2/3)/3144 and (247 1/2)/(3122 1/2). In DIO 1.3, it is discovered position trio of Almajest 5.3&5) had already been published in DIO 1.1 §6). This issue had been cited in Isis’ own sister publication;19 however, the fact that DR has solved The Unsolvables remains unmentioned in any of Hist.sci’s insecure captive journals. (By contrast, DIO has received probes from the highest Hist.sci levels, trying to find out if DR is continuing to unleash DIO-J.H.A — journals which regularly expose Hist.sci archons’ amusing attempts at pretending to scientific facility. Despite all this purported interest, not one of the inquirers has yet cited any result published by DIO, nor have they evidenced the slightest familiarity with its scholarly contents — meanwhile, they affect bewilderment at why DR isn’t taking them very seriously . . . )

20 The Ancient Star Catalog of 1025 celestial objects was compiled (Almajest 7.5-8.1) by mathematician-astrolger 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 virtually the entire Catalog from Hipparchos (128 BC). C2 This issue became central when: [a] in the 1976/8/6 Science, loyalist 0 Gingerich unconvincingly tried explaining-away Ptolemy’s solar, lunar, & planetary fugtings by calling them “pedagogic,” and [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 DIO 1.3 unsystematically savours some of the hilariously fooled-up analyses (still blissfully accepted by Muffla capos as perfectly valid . . . ) turned out by the Neugebauer-Muffla’s two leading purported experts on the ongoing Catalog controversy: J.Evans & G.Grafoiloff, who have by now wasted hundreds of handsome Muffla-publication pages, fruitlessly defense-lawyering Ptolemy against a passel of persistent proofs of his theft. Note: since the shellshocked Muffla has lately begun uncertainly admitting that maybe some of the Catalog was taken from Hipparchos after all, Mufflites now usually avoid telling their readers that Ptolemy insists he personally[7], p.181 observed the whole Catalog outdoors with his alleged armillary astrolabe.

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), with the presumed lunar elements: 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 = 228°; 0 1/2. Further 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 1/10000 radians.

19 Hist.sci Newsletter (1991/7) p.35.

An upcoming DIO analysis will show that all 5 of those exceptional Catalog stars, whose longitudes end in 15°, were trig-base-d in Polymist’s time when he rigged longitudes of Venus and the eclipsed Moon, longitudes which he pseudo-based upon real astronomers’ observed conjunctions of the bodies with these stars. (The 5 stars’ longitudes therefore were not obtained merely by adding 2° 30’ to Hipparchos’ longitudes.) The 5 stars (& conjunctions): 43° Gem (Venus 140/7/29–30), 15° Vir (Venus – 271/10/11/12), 76° Vir (Moon – 134/3/20/21), 49 Vir (Moon – 719/3/8/9), 29 Psc (Moon 908/8/26/29). Three of these cases are attested (Almajest 10.1&4, Neugebauer HAMA 1975 p.295 in 2.3), and the 720 BC eclipse is a Ptolemy favorite (Almajest 4.6-9, 6.9).

DIO’s paper presented, we learn at 236 of DR’s 1987 paper (Amer J Physics 55:235), a paper not cited by NCS’s 1992 JHA effort — even when elementary honesty requires it, e.g., at the p.181 discussion of the origins of the planetary theories’ longitudinal & latitudinal parameters. See DR paper at p.236 item 5 & p.237 and fns 27, 30, and the entirely original remark on latitudes at fn 38, which directly bears on the Swerdlow discussion of the Almajest latitude theory, p.181.

I commend NCS for not omitting this. He fairly describes Ptolemy’s account early on (at NCS 1992 p.174). However, he later fails to note the key consequence: if Ptolemy got the Catalog from Hipparchos, then calling Ptolemy a liar is not simply “impolite” — it communicates an important fact of astronomical history. I urge DIO readers to consult Swerdlow 1992 p.174 or Almajest 7.4 for the elaborate details of Ptolemy’s Catalog pretense. See also Almajest 7.2 & 5.1.
Swerdlow (Univ Chicago Dep’t Astron, & Advisory entertainer-satirist, Noel Coward & 25 entertainer-satirist, Noel Coward

23.3

JHA

23

Swerdlow’s nickname for Swerdlow is strictly based upon his Noel-Cowardesque talents as humorist — talents displayed, e.g., in *In & JHA* (sampled in *DIO 1.1*). His courage is unquestioned, since he and his Mufia friends have been brave enough to call Ptolemy-skeptic loony and incompetent for 20*, while avoiding face-to-face public debate: *DIO 1.1* [C7] & [C3].

27 At p.180, NCS 1992 affects neutrality (a repeat of his equally honest 1981 pose, cited at *DIO 1.1* [C3, D3], by saying half-degrees are visible. But he neglects to apply this to his own use of half-degrees in his book review of RN’s *Zodiac*!)

*The American Scholar* 1979 p.528. The elementary fatal error underlying NCS’ argument is exposed at *DIO 1.1* fn 66 in fn 6. It is particularly ironic that this NCS attack on RN’s integrity was published as part of his review of RN’s 1977 book (*Crime*) — the very book we now find him unfamiliar with THE central novel argument of! This auspicious longterm serial-embarrassment not only enhances (in the Mufia’s special way) the reputation of Phi Beta Kappa, but even more so for 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 incredibly credulous demonstration in *DIO 1.1* [C5]) 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 genteel fashion, attacked some poor unfounding nonMufia scholar for fake book reviewing. But we NCS fans are all looking forward to the day when he does.)

25 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
the same page on which he revealed his unfamiliarity with RN’s book — NCS has already demonstrated that he is touchingly deficient in the ability to trace the math of DR 1982C. The DR paper contained a hitherto-unsuspected crucial-experiment revelation: if Ptolemy observed the Catalog with his $1^\circ$.1 longitudinal mis-set, this would also cause $29^\circ$ error-waves throughout the Catalog: $\Delta \lambda = 29^\circ \tan \beta \sin \lambda, \Delta \beta = 29^\circ \cos \lambda$. But these (HUGE) waves are not found in the Catalog, thus (to an uncommitted critic) it is obvious that Ptolemy did not observe the Catalog. Predictably unwilling to accept that this simple test proves anything, NCS states (1992 p.176) that $\Delta \lambda$ “would be undetectable for stars within, say, $25^\circ$ of the ecliptic and [would] produce nonsense for stars within $25^\circ$ of the poles.” Hilariously false on both counts. 31 (By its account, the JIIA had a year to referee NCS 1992: fn 59.)

C11 Having abided the longitude problem to his satisfaction, NCS next tries explaining away the latitude-error $\Delta \beta$, the expression for which is so much simpler (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 32 which NCS permits the reader to believe was Peters’.

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

C12 NCS has simply eyeballed his $\Delta \beta$ 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 Peters $\Delta \beta$ 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 Mufa prejudice at work ought to check them or Peters’ original graph — in order to appreciate fully the NCS mental effort

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

31 Regarding the first NCS claim: basic amplitude = $1^\circ.1 - \tan 29^\circ$ (where $e$ = obliquity), so for $\beta = 25^\circ$, the wave $\Delta \lambda = 29^\circ \tan 25^\circ \sin 14^\circ \sin \lambda$, which (with g-cinc amplitude $12^\circ$) is far undetectable in this context, as the merest glint at the well-known 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 Ptolemy’s proposed 2$^\circ$3$^\prime$ precession since-Hipparchos with the real 3$^\prime$3$^\prime$4$^\prime$ precession over this timespan, and [b] claims that the waves produced by such a longitude mis-set — of amplitude 3$^\prime$3$^\prime$4$^\prime$-tan $e=100^\circ$! — constitute “a small periodical error . . . for so small for both coordinates [\& $\beta$] 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 Mufa expertise? As for the latter NCS remark: it is based upon National Science Foundation/grantee Swerdlow’s untutored 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 a great-circle measure (see the weighting discussion at DR 1982C p.366) — and for $\beta = 29^\circ$, the wave $\Delta \beta = 29^\circ \sin 29^\circ \sin \lambda$, which merely goes to $29^\circ \sin \lambda$ near the N-ecliptic pole. (Opposite at the S-ecliptic pole, which is of course not included in the Mediterranean-based Catalog.) To a scientist with even modest ability in spatial relations or possessing merely undergrad familiarity with the peculiarities of a spherical coordinate system near its axis, this result would be self-evident. If one may offer a slight cautionary suggestion to the JIIA: could it try to find reviewers who are capable of performing the math they are purported to analyse? Is that really too much to ask? Well, in a Hist.sci community that takes Mufiossi seriously, maybe it is.

C13 NCS has simply eyeballed his $\Delta \beta$ 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 Peters $\Delta \beta$ 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 Mufa prejudice at work ought to check them or Peters’ original graph — in order to appreciate fully the NCS mental effort
out this effect (in order to find out what errors still need explaining)\(^\text{41}\) 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\(^\text{42}\) is removed from the actual (Peters) latitude errors, the best-fit sinusoid is merely \(\Delta \beta = 9.0 \cos \lambda - 4.4 \sin \lambda = 10.0 \cos (\lambda + 25^\circ)\); this 10\(^\circ\) amplitude is catastrophically far below the 29\(^\circ\) amplitude that must exist if Ptolemy observed the Catalog.

\(\text{C15}\) Since Mufoshi cannot answer DR’s error-waves argument, the frantic dears must ignore, miscompute, rearrange, and or distort the actual latitude errors’ inconveniently DR-confirming phase & cosine coefficient. (Even the pre-\(\text{C14}\) amplitude needs Muffia-massaging, since \(18^\circ \neq 29^\circ\).) But, if we wish to unloose the alibi-power of preconception, there is no reason to limit the fun to Ptolemy. So I suggest that the JHA set these same charmingly programmed Muffia myopes upon the task of fudging 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 Challenger to his Wobble . . . .

\(\text{C16}\) A final note on the Peters graph of the actual zodiacal latitude residuals: NCS refers (\(\text{C11}\)) 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 Muffia fashion, NCS sees this situation strictly in a whew-we-barely-slipped-out-of-that-cleftstick 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’ comment (and I am happy to acknowledge the debt), it took DR a few hours (1992/10/19-20) to derive and check out the solution. So it will be fair to give the Muffia a month (1992 Nov) to work on the same problem. The solution will be published in an upcoming number of \(\text{DIO}\). Our offer: the Muffia capo (Toomer, Swerdlow, Aaboe, or B.Goldstein) who, during 1992 November, is first to call us up (phone: 410-889-1414) with the correct solution, and who is able to describe a valid math derivation of it, receives a free one year \(\text{DIO}\) subscription. (Just what every Muffie dreams of finding under his Christmas Tree . . . .) Hopefully more attractive: a published note of admiration (acknowledging a Muffia share in this provocative discovery), to appear in the first 1993 issue of \(\text{DIO}\). [Note added for 1995 reprint: Solution printed at \(\text{DIO}\) 1.2 (\(\text{C11}\)) in 152.]

\(\text{C17}\) Delambre (1817) and DR (1982C) emphasized the total absence of stars from the c.5\(^\circ\) band of southern sky which is visible from Ptolemy’s Alexandria (latitude \(L = 31^\circ 12\)) but invisible from Hipparchos’ Rhodes (\(L = 36^\circ\)). So NCS uncritically follows the alibi of Evans (1987/1.166) 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 nor then included within constellations even though visible near the southern horizon in Alexandria.” DR’s comments: [a] Ptolemy’s version of the Catalog (\(\text{Almajest}\) 7.5-8.1) contains dozens of stars explicitly labelled by him as “unformed” stars not belonging (though loosely attachable) to the traditional constellations. [b] While most of the constellations Ptolemy adopted were established by the time of Aratos (c.275 BC), Ptolemy is perfectly capable of breaking old tradition and states that he has done so “. . . the descriptions which we

\(\text{41}\) While the large sine coefficient is primarily due to obliquity-setting error, the cosine coefficient’s non-nullity merely reflects the fact that the sky moves a little during the few moments between the astrolabe-observer’s setting of range and the observer’s own.

\(\text{42}\) For Hipparchos, the real obliquity was 23\(^\circ\) 42’ 7”, so (assuming \(\text{Almajest}\) 1.12 is correct in saying that Hipparchos also used Eratosthenes’ obliquity, 23\(^\circ\) 51’ 20”), his obliquity-related error wave would be \(-9.5\sin\lambda\). 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’ \pm 1’ (DR 1982C eq.27).

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

have applied to the individual stars as parts of the constellations are not in every case the same as our predecessors’ (\(\text{Almajest}\) 7.4, G.Toomer 1984 \(\text{Almajest}\) p.340). [c] Ptolemy was unquestionably willing to adopt a brand-new asterism, since the \(\text{Almajest}\) is the first extant work to recognize the “Antinous” section of Aquila (\(\text{Almajest}\) 7.5, Toomer 1984 p.357), named in honor of Hadrian’s boyfriend, who had died\(^\text{43}\) only 7 years previous to Ptolemy’s 137 AD epoch for his rendition of the Catalog. [d] Having found it temporarily convenient to invoke Ptolemy’s UNORIGINALITY at pp.176-177, Univ Chicago Professor Swerdlow then turns right around and argues (feebly) in favor of the possibility that Ptolemy observed the stars by calling him unqualiedly “an astronomer and mathematician of the greatest Of the GREEK and the greatest depth” (NCS 1992 p.181, caps added). If DR readers happen to know by bright young chameleons seeking a useful alibi, do urge them to attend Swerdlow’s University of Chicago. He could teach them a few tricks.

\(\text{C18}\) Obvious question (ridiculously so): since we possess only a few extant scraps of astronomy between Hipparchos & Ptolemy, how can anyone confidently measure how original Ptolemy was?\(^\text{44}\) More curious yet: while NCS pronounces his purely faith-based judgement-certain (\(\text{C17}\)) on Ptolemy’s genius, NCS also says on the very same page (NCS 1992 p.181) that — despite symptom after symptom after symptom telling him that the Catalog is Hipparchos’ — NCS is still completely unsure of whether Ptolemy stole the Catalog or not. (Thus the “Enigma” of NCS’ paper’s title.) The contrast is provocative — and tells us that the “Enigma” here is (not the Catalog’s origin but) the impenetrability of a certain cult’s a priori mentality. (When an academic community utterly flouts & abandons all interest in falsifiability, it destroys the role of reason even in discourse — much less in that of the ever-mounting weight of evidence in its field. Such dementia is inevitable wherever cult-status has higher priority than evidence and truth.)

Despite Ptolemy’s vaunted originality, all his modern backs (including even NCS 1992 pp.180-181 & Evans at \(\text{JHA}\) 1992/2 p.66) admit that it begins-to-look—perhaps—a-little—like some stars “may have come from Hipparchos” (NCS 1992 p.181). (Admire the passive words for this. Like: “dependence”; never Impolite words like: “fraud”.

C20 I think we need some Impoliteness here. DR holds that Ptolemy stole — yes, stole — virtually the whole Catalog from Hipparchos. Besides a range of specific evidences of plagiarism, there is the simplicity of that hypothesis’ fit to the larger evidential situation: if we merely assume that Ptolemy swiped the Catalog, virtually all of the central purported “Enigmas” (\(\text{C22}\)) of the case immediately evaporate. The Muffia contrarily keeps insisting (at great length) that the theft is not yet absolutely, positively, completely, utterly proven. (And the ever-mounting weight of evidence has reduced Mufosi to this feeble last ditch). Thus has an unsoundly transparent intellectual inertia gradually sucked defenders ever-deeper into a hodgepodge of ad-hoc exercises in special-pleading (for each separate suspicious Catalog circumstance: \(\text{C22}\)), a thicket of disconnected alibis — sorely in need of a mow-job by Occam’s Razor.

C21 E.g., to try answering RN’s fraction-endings argument and (inadequately) deflate DR’s error-wave amplitude to merely ‘20’, NCS 1992 p.177) promotes Evans’ inane pro-

\(\text{43}\) To quote from DR’s \(\text{Quarterly}\) paper (Rawlins 1984A), p.973: “The reason that Ptolemy’s stele [Canopic Inscription] was erected at an Egyptian miracle factory is: that’s just where he worked . . . forty years at Canopus, near the ever-magnificent temple of Isis, which was an ancient combination-in-one of Hollywood, Louvres, and Las Vegas. The ultimate enslavement of Ptolemy 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, still in his grief, Hadrian visited the Canopus temple [of the god Serapis, to whom Ptolemy’s Canopic Insce is dedicated, and probably met Ptolemy 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-guides — e.g., Olcott’s — have carried Antinous as a minor constellation, an apt memorial for an Asia Minor minor.)”

\(\text{44}\) NCS 1992 p.182 concludes that the Catalog issue is a question that cannot ever be resolved; meanwhile, he has resolved that Ptolemy was a genius. I.e., NCS 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 MUFIOSI, until recent RN-DR-proposed crucial-testingcornered them), and [b] is more wildly comic46 than the surreal sobriety-test fantasy in the cinema-farce The Man With Two Brains. (Hollywood screenwriters have to use drugs to get this high. How does the Mufia do it?)

C22 Perhaps we can attain some perspective on the Catalog matter by simply listing the features we would expect to find in the Catalog if it “came from” Hipparchos. [Test’s first proponent: in brackets.]
[a] An utterly GROSS — 1°.1 mean longitude error [Tycho].
[b] Absence of large 29° amplitude error waves in northern longitudes [DR].
[c] Absence of large 29° amplitude error waves in latitudes [DR].
[d] Longitudes with more 40° endings than 0° endings [RN].
[e] Longitudes with more 10° endings than 30° endings [RN].47
[f] Absence of a near-quarter-degree constant error in celestial latitudes ± 3° [DR]. (Such an error is roughly entailed by Ptolemy’s false assumed geographical latitude48 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°12.9°)
[g] No stars in the c.5° band of sky visible from Alexandria but not from Rhodes [Delambre].49

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 Mufia, innocently heedless of all new test results, had publicly commitied itself to faith in Ptolemy’s greatness & originality (and his critics’ idiocy). As new test after new test came out against Ptolemy, Mufiosi stuck to their party line: we expert archons have learned nothing from RN&DR. And they’ll die stuck to that same unalterable proposition. C24 Even for the tiny but indicative sample of stars where Ptolemy copies errors of several degrees50 from Hipparchos, NCS still isn’t finally convinced. NCS 1992: “a few stars may have come from Hipparchos” ([c]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 Velikovskians51

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

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

48 Almajest 5.12-13. DR’s Amer J Physics 1987 paper (p.236) also notes that the same argument proves that the real-doesn’t-7.3-star declinations (4 stars mean error) are also stolen, though Ptolemy naturally presents them as results of his own outdoor observations.

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

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


52 Mufia strategy at this desperate juncture resembles the tobacco lobby’s primarily in that there is finally no longer any hope that Ptolemy’s 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? THIRD SCIENTIST: I’m totally stupefied, Bob! Back to square one! THIRD SCIENTIST (entering the room): Hey, can you two guys lend me a hand? 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 Mufia augmentation. NCS 1992 (p.175) adopts the excuse of Laplace55 & 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 inspired56 the DR 1982C absent-error-waves test, which definitively refuted the Laplace-Gingerich alibi. (Rawlins 1982C, eventually published by a real science journal, was originally submitted to JHA Editor-for-Life [EFL] Lord Horskin in 1976 & 1977. His Lordship refused even to refereee 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.)

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55 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.

56 Laplace was himself a notorious nonciting author of others’ work. See, e.g., GA Report. . . (BAAS 1983/1/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 observer’s latitude L was off by $-14^{\circ}$ — an amount virtually equal to the solar semidiameter. Nor did Ptolemy ever realize (during at least 8 years of allodated solar observations, 132-140 AD: *Almajest 3.1&7*) that the real Sun’s position differed from his tables by $-1^{\circ}$.1! This error renders the easily-observable equinoctial solar declination off by c. half a degree, an amount equal to roughly twice the solar semidiameter. (Such a fantastic error would instantly be revealed by transit circle or astrolabe, both of which Ptolemy claims to have regularly used. Heck, even an instrument as simple as an ancient astrologer’s symmetric gnomon can do alot better than this.)

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

C27 Since 1987, the JHA, utterly captured now by the Mufa, has published at least 7 pieces on the Ancient Star Catalog (running over a hundred pages in all). *All seven have been from the pro-Ptolemy side* of the controversy. So, now, the JHA publishes the capper to this 5 year demonstration of its idea of equity, by suggesting (NCS 1992 p.182) a “moratorium”. (And one notes that neither of the 1992 JHA papers cites DR’s 1991 analysis at *DIO* 1.1, §6, which provides yet more novel evidence, positively attaching Hipparchos’ solar work to the Catalog’s zodiacal longitude error curve, with an ordnag 1-precision match of amplitude.) I.e., now that the JHA has fired its last (for-as-long-as-We-feel-like-it) pro-Ptolemy shot on the Catalog, just in under its own welltimed moratorium wire,29 the JHA decrees it would be best to just end the Catalog controversy right here. Megalomania rarely achieves such heights of unreality.

C28 Unrevel 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 Mufia claims — which he is now himself so brilliantly augmenting! — that has left the Mufia looking about as convincing as Ptolemy. But NCS wants no moratorium on Mufiosi 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 more of its own incomparable Mufia research on the Star Catalog, JHA’s “moratorium” is now in place, in print, as a pre-set ofcial-excuse for rejection of a “believe or disbelieve”.)

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

C30 Seldom has a party of “experts” been so utterly defeated (and by scholars it exiled as fools) — so bare of substantial, coherent retort26 — 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 *DIO* mold.

59 Following NCS 1992, we find: “Editors Note: This article was received in June 1991, but was held over to permit publication of the Essay Review already commissioned from James Evans, which appeared in our February issue.” (Evans, too, defended Ptolemy — & failed to cite *DIO* 1.1, §6, though the paper is known to Hist.sci: fn 19.) The timetable alleged (who asked?) pseudoexcuses noncitation of the very *DIO* 1.1 that triggered NCS 1992.

60 However, see the *DIO* publisher’s statement at the back of this issue.

61 E.g., since Mufiosi have been dunning skeptics for decades as incompetents, one would expect pages of examples of the purported incompetencies to be forthcoming. Mufiosi’s occasional efforts to expose alleged errors got tangled up in such almost-artistically disjunct babbling. Who but our peerless Mufia jesters could even imagine proposing that a subject be ruled out of a historical journal on the ground that it is too historical?

C31 After the foregoing, it may be superfluous to attempt a brief review of Mufia sanity on the Catalog issue. But, anyway:

[a] In 1974, EFL-best-friend O.Pedersen disbelieved that the Catalog was stolen, because Ptolemy was too honest; Pedersen added that Ptolemy’s rep for “integrity would be damaged beyond repair” if the theft indeed occurred (Pedersen Survey of the Alm 1974 p.258 emph added; DR 1982C p.362). (Once the RN-DR proofs of Ptolemy’s thievery appeared, Pedersen’s self-evident conditional quietly slid down the Mufia Memory Hole.)

[b] In 1981, O.Gingerich admitted (*QJRAS* 22:42) that the RN-DR analyses show that Ptolemy probably did take the Catalog from Hipparchos.

[c] In the 1987 JHA, J.Evans attacked RN & DR to the extent of dozens of (frustratingly inconclusive) pages, swinging Gingerich’s JHA back to denial that the theft had been proven.

[d] Grafallo 1990 (“edited” by Mufia capo G.Toomer62 concluded that much of the Catalog was based on Hipparchos’ observations, after all. (RN-DR had long asserted this, but watch Grafallo grab off all the credit for proving it, while painting RN-DR as fools.)

[e] Now, NCS 1992 says: Maybe. Yes, possibly the Catalog was taken from Hipparchos; but . . . No, nobody has proved anything — and it doesn’t matter anyway.


C32 Is this a community of scholars honestly seeking a credible, consistent vision of the truth? — or are we instead enjoying: Jekyll&Hyde-govaudeville? (Perhaps the reason there seems to be no direction is: the Mufa has let its conscience be its guide.) But there

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

D1 An occasional nervous-neophyte Muffie 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 similarly executed originality. (As *DIO* readers know all too well.) So, we can relax. (Likewise, professional astrologers’ amusing inability63 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 glorication of Mufa 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 Mufia klan has simply been bluffing in this regard. (Note the feebleness of Mufia attempts in this direction: *DIO* 1.3 in 252.) See *DIO* 1.1, §5 in 6 for Hist.sci (including Mufia) precedents for publishing lengthy error lists to attack authors.

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

63 Rawlins *Skeptical Inquirer* (Skhp) 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 Muffa must wonder why, despite years of archonal conspiring to ostracize RN-DR’s heresy, the hated heterodoxy persists nonetheless. (Even Time-Life’s popular Hoaxes & Deceptions p.108 accepts that the Rawlins 1982C analysis, of the Ancient Star Catalog’s southern boundary, indicates that this “Ptolemy” Catalog was actually observed in Hipparchos’ Rhodes, not Ptolemy’s Alexandria. See §C17.)

D2 The DR-Muffa double-tarbaby-fracas will continue indefinitely, because: [a] DR positively won’t stop publicly admiring Muffia gyrations, so long as the Muffa insists on its snobbish & effectively censorial minimum-citation-practice, based upon its equally ludicrous WE’re-the-only-experts-around-here pose. (i.e., DR is asking that the Muffa acquire some fundamental ethics and integrity. But who’s going to fund the brain transplants?) [b] Muffia mout’pieces are irrevocably committed to forever clinging to their precious pretense that DR’s historical scholarship is utterly worthless. This point is so sacred to Mufiosition that, in order to maintain it, the Muffa will pay any price (primarily: internal rot) — and, in order to cloak its iniquity 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’ C5-sampled lexi-con.)

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 classicist — tell the goriest lawyer-jokes. It pays to advertise!) Also, my mother’s father was a prominent Maryland lawyer. And she married my friend, advisor, & stepfather, John W. Avirett 2nd — widely known as one of the very finest & most respected lawyers in the United States. So, as a member of a family of lawyers, DR is delighted to contribute here an original DIO creation: the lawyer-joke-to-crown-all-lawyer-jokes. Ready?

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

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64 Yes, hated. See DIO 1.1 §1 [C7 & fn 20; §3 D2-D3.
65 Curiously, the Muffia’s null evaluation of DR’s scientific-history production is not shared by: the American Astronomical Society, PASP, Amer J Physics, Arch Hist Exact Sci, the Royal Astronomical Society of London, among others. Likewise, the prominent scientific historians: K.Moesgaard (U.Aarhus, Denmark), S.Goldstein (UVe, 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.
66 Are top lawyers who revel in lawyer-jokes retching at the low end’s ethics? Or, is this strain of humor just a gruesome byproduct of the legalization of lawyer-advertising? (When a local lawyer was told that his TV ads were lowering the reputation of the legal profession, he pithily replied: that’s impossible.) There’s a famous regent (graduate of Bernie Cornfeld’s School of Asceticism) whose gentility & generosity are so universally respected that a mere sighting of her inspired colleagues to hum the Jaws theme in unison. Ashamed? Hell, she brags about it. DIO dedicates this joke to another joke: the Neugebauer Muffia — in honor of that cult’s highly original notions of ethics and human decency, not to mention its unquestioned talent in sucking tax monies out of the system, to fund its defense-lawyer fantasies.
67 An anti-lawyer line from [Marlowe] (Henry the Sixth Part 2 Act 4 Scene 2) has become popular of late, but the delicious mobocracy-fantasy context is rarely reproduced. Jack Cade [haranguing revolutionary]: “Be brave . . . [I vow] 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 parchment, being scribbled o’er, should undo a man? . . . I did be seal once to a thing, and I was never mine own man since.”

†9 The Neptune Conspiracy
British Astronomy’s Post-Discovery Scholars

Summary

Britain’s J.Adam’s generally believed to be the prior to the 2 pre-discovery locators of Neptune via math analysis of its gravitational disturbance upon Uranus’ orbital motion. However, for reasons still vigorously disputed, he published none of his alleged 1845 perturbational mathematics until 7 weeks after Frenchman U.Leverrier’s 1846 publications & 9/18 letter had caused the planet’s telescopic discovery at Berlin on 1846/9/23. Detailed evidence is presented 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 hypothesis 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 & adventurous young Cambridge U mathematician John Couch Adams appears to have in 1845 arrived at a theoretical prediction of the ecliptical position (near Cap-Agr border) of the giant planet Neptune, then unknown. This is the same jovian planet that the wonderful US spacecraft Voyager 2 visited 1989/8/24, thanks to NASA.

A2 Adams is widely held to be the true first predictor of Neptune’s position and is honored for this achievement by a memorial in Westminster Abbey near Isaac Newton’s tomb. However, Adams’ role in the discovery was actually nil, and his behavior has always been inexplicably murky — a point I will expand upon below, adding a novel, partly speculative hypothesis which entails: [i] a solution-switch by Adams, & [ii] a high official’s possibly-conscious back-dating of the controversy’s key document. This admittedly uncertain new
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 Le Verrier, or to both? (The last position is fine by Britain, since Adams’ work is supposed to pre-date Le Verrier’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 a mathematician than an astronomer. (See [2], where Leverrier praised, “a mathematician of high rank, an experienced and swift computer in these areas (see [37], Adams’ investigations have the flavor of a learning experience. I offer these judgements not in criticism (indeed, they suggest that Adams’ challenging the Uranus problem was even more creditable than otherwise) but because I help them believe explain Adams’ slowness to publish, which relates to the central mystery of the Neptune affair. From a draft of a letter (1847/2/1), some months after Adams was world-famous) from Challis to H. Schumacher (CON #30, emph added): “Mr. Adams. . . a young mathematician of excellent promise . . . devotes his mathematical powers almost exclusively to astronomical science. . . . a small observatory . . . is under his care, and gives him the means of adding to his theoretical knowledge, an acquaintance with practical astronomy.” By the way, in an 1846/11/18 letter to Airy (Glaisher p.xxix), Adams says if others did not take up the search, he was preparing to look for the planet himself at this little St. John’s College (Cambr) observatory. (Note that he could have done so in 1845 if he believed his math to that point warranted it.)

[b] When comparing places (computed therefrom) to existing theory (the variously corrected Bouvard 1821 tables). That Adams never satisfactorily computed orbit elements for Neptune, generally known today as “Hypothesis 1”. 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 & 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 better not 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 1”. The central mystery of the Neptune affair. From a draft of a letter (1847/2/1, some months after Adams was world-famous) from Challis to H. Schumacher (CON #30, emph added): “Mr. Adams. . . a young mathematician of excellent promise . . . devotes his mathematical powers almost exclusively to astronomical science. . . . a small observatory . . . is under his care, and gives him the means of adding to his theoretical knowledge, an acquaintance with practical astronomy.” By the way, in an 1846/11/18 letter to Airy (Glaisher p.xxix), Adams says if others did not take up the search, he was preparing to look for the planet himself at this little St. John’s College (Cambr) observatory. (Note that he could have done so in 1845 if he believed his math to that point warranted it.)

5 Even on the accepted record, Adams still went on beyond Hyp 1 to compute Hyp 2 and Hyp X. Again, this bears on the question of priority: a preliminary solution, as yet unchecked by variation of the mean distance is insufficient, as even Adams agreed (1846/9/2, M16:405) just after Hyp 2’s 1846/8/27 completion: “the investigation [Hyp 1] could scarcely be considered satisfactory while based on any thing arbitrary; I and therefore determined to repeat the calculation on a different hypothesis as to the mean distance, if possible, as we are there by the factor 1.103 for Hyp 2.” (That is, the correctness of the first Adams solution’s predicted longitude was very lucky — and he himself knew that, which is why he made the statement just quoted, and the inadequacy of this solution is an important cause of his publication of it. I.e., Adams’ Hyp 1-based priority claim is self-confessedly feeble. [We report Le Verrier’s prediction as occurring in not just one paper but three.): So, why is Adams (who unquestionably lodged [1846/9/2] his distance-variation solution later than Le Verrier’s comparable solution [1846/8/31]) regarded by anyone as the prior discoverer? A related question on another tangent: both Le Verrier and Adams failed to get close to the actual mean distance of Neptune (30 AU, replacing the tediously precise longitudes of Neptune (30 AU, replacing the tedious procedure of computing the Neptune perturbations (for various mean distances) by instead using shortcut schemes, both of which led to serious errors in their final orbits: see §E8. Suppose they had gone ahead with repeated distance-trials toward 30 AU, would their work have run aground on the huge 4000” Uranus–Neptune 2-1 resonance? A lovely evasion of this problem is that in his Newcomb’s Orbit of Uranus 1873 pp.55, 178. Soon after, Adams’ 1878 paper (DIO 2.3 §15 [A10],) replacing to Peirce’s attacks, show he understood the problem then. See Rawlins 1970G. [Also DIO 7.1 §5 [A10] 116 1992 October DIO 2.3 §9

A5 Starting the same year as that alleged for Adams’ Hyp 1 (1845), the at least equally able French mathematician Urbain J. J. Le Verrier independently computed and prominently announced (Comptes Rendus 1846/6/1) virtually the same celestial location.

A6 Upon reading Le Verrier’s published paper (1846/6/23 or 6/24), Airy swiftly & secretly set in motion a huge Cambridge Observatory telescopic sky-search (§A8 & §B1) — and, as part of the secrecy, deliberately suppressed news of Adams’ confirmatory 1845 British researches (outside a small Cambridge U circle). In his 1846/6/26 letter to Le Verrier, Airy never mentioned Adams’ prior work — this despite the fact that just one day previously, in a 6/25 Airy letter (discovered by R. Smith’s industry), he mentioned to a Cantab confidant (Wm. Wheewell) both Adams’ & Le Verrier’s agreeable planet predictions (on equal terms: see fn 31). Airy then did not respond to Le Verrier’s 6/28 detailed reply: “That this was anything other than deliberate secrecy (as modern apologists pretend) is directly contradicted by Adams’ own common sense remarks: “I did think that the Astronomer Royal would have communicated my results among his correspondents. I took all that for granted and considered it [Adams’ 1845/10 transmission to Airy of hypothetical orbit elements] a publication”. (Letter of Adams to Sedgwick to Airy 1846/12/6, written just minutes after conversing with Adams; Glaisher 1896 p.xxvii & Smart 1947 p.41. Chapman 1988 p.139 n.57 has found that both of two copies of the letter are now missing; below at fn 12, I suggest a possible reason for that particular oddity. This is part of a series of Neptune ms disappearances suggesting systematic suppression of documents, a situation encouraging some otherwise unthinkable speculations.)

A7 However, by his own just quoted criterion, Adams was, as we shall see, himself obsessively secretive, not publishing anything before Neptune’s 1846/9/23 optical discovery on the basis of his 1845/10 transmission to Airy & Adams’ needlessly mysterious Hyp 1 is the key to the whole controversy. Though privately 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 better not tested his theoretical planet at more than 1 distance from the Sun (arbitrarily presumed: below fn 5).
(M16:399, §A6) — and note further that Airy’s crucial 1846/7/9 plea (asking Challis to perform the planet search) was written from Peacock’s deanship (Ely) while Airy visited his old friend there. (As shown by Airy’s 1846/9/14 letter to RAS Sec’y R.Sheepshanks, Airy also met Peacock at the continental baths some weeks later, where Peacock got a cough that led to his being bled twice! Which imparts an idea of the comparative sophistication of astronomy & medicine in that era.) Recognition of the Analytical Society’s lust for a British mathematical god to inspire its long-envisioned revival allows us to see the Neptune affair in the larger context of the sociology of British mathematics.12 (Note Challis’ revealing remark in his 1846/12/2 Report to the Observatory Syndicate, SP p.liv: Adams "has at once done me honour to the University, and maintained the scientific reputation of the country."13 Indeed, it is a viable hypothesis that Airy and-or Challis are not the villains but the (flawed) saints of this affair, in the sense that in order to semi-create and defend that needed great-Brit-mathematician rôle-model, they kept silence about or even actively obscured Adams’ limitations — at terrible cost to their own reputations in history.

B The Search: 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 6/9/29 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 & begging 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 in the case of a new planet’s discovery). 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

13 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 credit because of prior publication. I think Babbage smelled the same fish the French & I have always intuited. Airy’s part in this matter has never been appreciated by those who get diverted by the ludicrous propo-myth that Airy “snubbed” Adams, etc. (See, e.g., Smith 1989 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 affects 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 henchmen] that 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 (section B1), then who could blame Airy for having a low regard for Adams’ character? He may well have spoken of Adams in extremely blunt terms. (Perhaps this is why both copies of this letter have disappeared.) Those who promote the idea that Airy did dirt to Adams conveniently forget that it was Airy who in early 1847 led the small clique that, against a large majority, successfully prevented the awarding of the RAS medal to Leverrier because that act would admit Adams’ work was inferior to Leverrier’s. But nothing satisfied Airy’s critics. That Airy thus felt ill-treated, but was determined to remain dutifully constrained to silence about the shortcomings of Adams’ case, is suggested by his 1847/3/26 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 (dated to Mr.Adam’s and the Glory of the Greatest Astronomical Discovery of Modern Times, etc., etc., etc., I have no remark to make).
examine not only the inextricably stark evidence of Airy’s 1846/6/26 silence to Leverrier regarding Adams (this vs. Airy’s key 6/25 letter to Howell 24 before: §6 & fn 31) — but also the eyeopening remarks concluding Airy’s brief 1846/8/6 letter to Challis (regarding Breen’s availability for the secret search; CON #6, hitherto unpublished except at DIO 1.1 ¶ 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-oversee Airy outlined & advised the celestial hunt’s strategy in a series of letters starting the very day after the 6/29 plot was broached: 6/30, 7/9, 13, 21 (CON #1 to #5; see also M16:416). On 1847/6/18, Challis agreed to conduct the clandestine sky-sweep 18 for the planet. The actual telescopic observations began on 7/29. Challis was from the outset privately guided by what we will here call “MemoW” which Adams computed & gave to Challis: ephemerides of geocentric places (for hypothetical planets at various heliocentric longitudes), for 20-day intervals starting 1846/7/20. 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° (MemoW, CON #35) to 315° (Hyp X, M16:407) — a range of over 20°! (Tables 1 & 2, below, provide 1800-1850 ephemerides corresponding to projected & real orbits.) Challis’ long-lampooned indecision in his search was not due to a personality disorder (as is now commonly & abusively charged) but rather to Adams’ conflicting directions for him. Another equally remarkable & heretofore-unknown point: Memo W’s 20-day-interval ephemerides, the document guiding Challis’ search, was not based on Adams’ now-famous perturbation-computed Hyp 1 orbit-prediction (see fn 19) but rather upon a combination of: [a] Flamsteed’s lost 20 star #1007 & Wartmann’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 (CambObs 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 §88), Challis was looking in that solution’s position, about 10° west of the planet’s actual position. (In general, the correlation is not so sharp as to constitute proof of a connection.) When Adams 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 CambObsNept file (CON item #35: 1 page; the other 2 pages are post-discovery and thus not crucial), largely unpublished & hitherto unchecked by any historian of the affair. These Adams computations are crude (remarkably so, for a mathematician being compared to Leverrier), based on a patently-invalid constant-second-difference arithmetic scheme (fn 21) exhibiting some impossible asymmetries about oppositions, and

20 MemoW’s ephemers (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’ nonircular orbit perturbational calculations. (Airy follows this shem in his 1847/5/18 letter defending Adams’ priority: idem, 336/7/18 p.309.) To the contrary, the central epoch for MemoW’s ephemeries, 1846/8/29, is simply the opposition date of the Wartmann-based hypothetical planet (fn 21): no relation to Adams’ perturbation-computed planet! Moreover, the heliocentric longitude used (325°) was that which Leverrier had computed at the accident of date 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: “On leverrier found the cit. The date from Leverrier for cit. The date (1846/7/20) on the page MemoW, was added by Adams upon this document makes it clear that Adams was in on the secret CambObs sweep from the start. (Given the 20-day interval & the 7/20 initial date, we may say that MemoW was probably written after 1846/30, certainly not after 7/20).
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 power24) 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 computer25 J.Breen (Airy’s “a rough genius. . . perfectly tractable”) to the search-team (with Astronomer Royal26) to help Challis’ assistant Morgan. The apparent cover story was that Breen was just going to the Cambri Obs for a “month’s trial” toward acceptance to the post of Junior Assistant there (see Breen’s letter: CON #8 1846/8/8). From 8/14 through 9/18, Challis examined the western part of the region, “purposely” (AstrNachr 25:102), since that’s where the Sun would first encroach later in the year — but it’s also the region where Adams’ latest work seemed to be pointing (8/20 was the date on his Hyp 2’s first rough solution: Sampson 1904 p.167). Independent British astronomer J.Hind (of Geo.Bishop’s private observatory in Regent’s Park) wrote Challis on 1846/9/16 (CON #10) that he had recently heard of Challis’ search (possibly at the recent BAA meeting). Hind’s letter mentions the fact that he and the French astronomer H.Faye were also preparing to search for “the new planet”. (In his letter, Hind strikes this expression and replaces it with “Le Verrier’s planet”. See also CON #13, where Hind thanks Challis for his recent “kind” letter; and compare to Hind’s letter — quoted at Rawlins 1848N & above at §§5 — when he learned that Challis had at this very time been keeping from him Adams’ confirmation of Le Verrier’s predicted position for Neptune.) Faye is quoted by Hind as expecting to spot the planetary quarry by searching for a disk of diameter about 2°, following Le Verrier’s 8/31 advice.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 Le Verrier was pointing, presumably looking for a disk. Challis’ later accounts (1846/10/21 AstrNachr 25:102; 1846/12/12 SP liii) distort this history by [a] ignoring the possible rôle (fn 18) of Adams’ latest results in pushing him west after 8/12, and [b] suppressing the fact that he had been following Le Verrier’s (not Adams’) instructions ever since 9/21.27 Challis instead publicly claimed that he had switched back to following Le Verrier’s guidelines only on 9/29, the Cambri Obs search’s very last night, when Challis’ zone records state that he saw Neptune’s disk.28

24 AstrNachr 25:103. Elsewhere, Challis estimates the power at 160 (fn 30).
25 It has always been assumed that computer Breen was sent to Cambridge to help Challis. Was he actually sent also to help Adams? 26 Faye’s letter confidently repeats his friends’ assurances that he is most likely to be elevated to the very discovery that was the subject of Faye’s letter. 27 Le Verrier’s final paper, 8/31, would have been available in England almost exactly at this time. (Regarding how long Le Verrier’s Comptes Rendus reached England, see SJ#4.) Also: CON #31 is an unsigned slip of paper in Challis’ hand noting the “Verrier” 8/31 paper’s proposed longitude limits for his planet, taken from Le Verrier 1845 p.436: 321° to 335°. So Challis almost certainly knew by 9/21 that Le Verrier’s 8/31 paper was pointing fairly definitely toward the supposed planet. 28 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 lii.) 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

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

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

C1 A previously unremarked but critical point: Le Verrier, by publishing his prediction (before optical discovery), took all the chances of embarrassment29 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: §§8.) But: [a] I should not have to make that judgement (& would not, had Adams published before the planet’s actual Berlin Observatory discovery), and [b] it is undeniable that the British claim to Neptune is needlessly fishy; e.g.,

[1] No publication until more than 7 weeks after Neptune’s discovery at Berlin.
[3] The astounding fact (only mentioned in passing as a minor point in Chapman 1988 p.133 & n.43; 1988/5) that the very first public claim for Adams (by co-conspirator Challis, 1846/10/1 letter to the Cambri Chronicle; retracted 10/16: both newsclips preserved, as CON #15&16) stated that Adams’ work was completed only in about June of 1846.30
[4] Also note that co-clotter 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 Le Verrier’s. (R. Smith’s important find, Airy’s 1846/6/25 letter to Whewell, does say that selection of objective was Airy’s.) This was the basis for the Airy (1846/7/13 plan’s estimate that the triple-sweep he recommended (zones of breadth 1°/4) would require only about 80 hrs per sweep. When Challis’ 7/18 letter resisted, suggesting nonutil use of the telescope, Airy on 7/21 (CON #5) warned against equatorial adoption, 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°-width-zone triple-sweep would’ve taken 30000 stars!) Challis defended to the end his delusion that this overtedious approach was correct — even urging (M16:426) its adoption for all transit work, a suggestion which convinces one of [a] his personal dedication to hard work, & [b] his lack of Airy’s intuition regarding the need for procedural simplicity’s minimization of all error sources in positional astronomy observations. Challis is sometimes regarded as a crank (e.g., DSB entry on him), and Airy privately wrote Sheepshanks (1846/11/23) of watching helplessly as the “Oceanus”-enraged French scientists wreaked vengeance by exposing “Challis’ absurdities in Hydrodynamics”, noting that on such matters, “as I told you some years ago, Challis is perfectly dry.”

This discovery priority later became real. In 1859, Leverrier discovered the nongravitational precession of Mercury’s apoapsis, which we now know is due to relativistic gravity. (This was one of the great discoveries in the history of astronomy, hinting at the need for a new physics — long before Michelson-Morley.) But Le Verrier naturally interpreted it gravitationally, and so predicted the nonexistent planet “Vulcan” within Mercury’s orbit. A series of failed searches for it somewhat darkened that his later years.

30 If one wishes to view this 1846/6 “completion” remark as merely as claiming that Adams’ Hyp 1 was not a complete solution, then [a] it grossly exaggerates the earliness of the known dates of Hyp 2 (1846/8/16/9-2); and [b] it still destroys Adams’ priority. The Challis 10/1 text: “About four months ago, Mr. Adams, of St. Johns college, and M. Le Verrier, an eminent French mathematician, concluded independently from theoretical calculations, that anomalies which had been long known to exist in the motion of the planet Uranus, could be accounted for by supposing a perturbing planet to move in an orbit at twice the distance of Uranus from the Sun. These mathematicians agreed in fixing on 325° of heliocentric longitude as the most probable position of the supposed planet, which has proved to be very little different from the actual position. M. Le Verrier more recently inferred . . . that the mass of the disturbing planet was to that of Uranus in the proportion of 3 to 2 (a result which Mr. Adams also arrived at 1846/9/2 by continuing his researches) . . . For the last two months I have been engaged in mapping the stars in the neighborhood of that supposed planet, and have, as far as I have carried my work, met with no success. The last investigations of Le Verrier came to my knowledge on Sept. 29. On the evening of that day I observed strictly according to his suggestions, and out of a vast number of stars which passed through the field of view (power 160 [vs. fn 24].) I selected one only, against which I directed my assistant to write ‘seems to have a disc.’ This was the planet.” (AstrNachr 25:103: retracted 10/16: both newsclips preserved, as CON #15&16)
Adams’ result reached him “in manuscript” before Leverrier’s 1846/6/1 paper; but the statement does not say anything about 1845 nor (since Airy saw Leverrier’s 1846/6/1 paper only on 6/23-24) does it preclude Adams’ completed Hyp 1 possibly reaching Airy very roughly at the time (June) specified by Challis’ initial public account (just cited above: text at fn 30). If Adams’ work was handed to Airy in 1845/10, then shouldn’t Airy have said that it was completed way earlier than Leverrier’s, e.g., “last year” — rather than just: reached Airy first?)

Now, the official documentary history has it that Challis wrote the 1845/9/22 intro for Adams’ visit to Airy, stating to Airy that Adams had “completed” his work on Neptune (M16:394). Since Challis was at the famous 1846/6/29 Greenwich meeting three quarters of a century after “this completion” — when the secret search was for a new planet’s elements to Airy. I am calling this MemoR. (MemoR is now available only in photographic facsimile: SP pp.Ivi-iviii 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.

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 writer knows what day it is. (Is it credible that Airy — unequalled in numismatics — would not have checked the date itself the writer knows what day it is. (Is it credible that Airy — unequalled in numismatics — would not have checked the date?) [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.

The situation is therefore that both of the key 1845 documents (MemoC & MemoR), the entire basis of Adams’ 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) Airy’s papers until late, [c] were dated much later from memory by them, [d] add nothing to the expected unquestioning acceptance of the documents. What is the “lost” 3 page document that is the physical basis for Britain’s claim of priority?

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

C6 The “missing” Greenwich Neptune file includes numerous key documents critical to reconstructing British activities, including the central document of the case, Britain’s holiest Neptune-chase relic: Adams’ three-page memorandum (allegedly 1845/10/c.21) transmitting the predicted Hyp 1 planet’s elements to Airy. I am calling this MemoR. (MemoR is now available only in photographic facsimile: SP pp.Ivi-iviii 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 writer knows what day it is. (Is it credible that Airy — unequalled in numismatics — would not have checked the date itself the writer knows what day it is. (Is it credible that Airy — unequalled in numismatics — would not have checked the date?) [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; Glaiser 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’ 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) Airy’s papers until late, [c] were dated much later from memory by them, [d] add nothing to the expected unquestioning acceptance of the documents. What is the “lost” 3 page document that is the physical basis for Britain’s claim of priority?

33 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 Complex Rendus paper of 1 June 1846, which is a priorus version of this calculation. (The later point is concerned in the new standard biological sketch of Shackleton by R.Huntford (Shackleton 1985 p.311), which suggests, as I have always stated, that:

32 An innocent interpretation is that Challis correctly realized that Adams’ work was not complete in 1845, which is my position and is the obvious reason for Adams’ nonpublication. But this destroys any British claim of priority, so it could not be admitted: this truth is perhaps the main secret of the Neptune affair, and I expect that it would be verified by the “lost” Airy papers. When such a key file is gone and meantime we are told nothing regarding the substance of communication between Airy & Adams between 1845/10/c.21 & 1846/9/2 (not even when or so much as whether Adams ever saw Leverrier’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.Georg Soc & widow Kathleen Scott did to Robert Scott’s South Pole diary before publication. This notorious bowdlerization assisted in the curious hagiographical process whereby most British children ended up believing (as a justifiably disgusted Amundsen reports in his 1927 autobiography, My Life as an Explorer op cit p.71 or ”op cit” Shackleton 1962 op cit c.175)” that the man who discovered the South Pole — first 4 weeks ahead of Scott, who died while returning, partly from the disappointment of manuhaling sledges hundreds of awful miles just to find that the South Pole looked like a Norwegian flag. As it may perhaps interest those reading of the man who discovered the South Pole, Amundsen’s succinct conclusion (op cit of 1962 p.2.2.4.5) was: “If we accept the British a race of very bad losers.” (The geographical establishment proved just how wrong-headed Amundsen was by: continuing to denigrate his incomparable achievements — adding only that Amundsen’s autobiography shows evidence of advancing insanity . . . .) This Chapman 1988 p.136 n.6. This also happened in the case of the Roy.Georg Soc’s original file of the 1907-1909 Antarctic expedition of the great British explorer Ernest Shackleton, the precise value of whose genuinely-record-setting 1909/119 southern latitude is suspect: 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 Priesty 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:

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D Neptune’s Discovery Brings Adams & Challis to Life

D1 We note that Adams asked (through Challis, 1845/9/22) and was asked (by Airy 1845/9/29) to write his prediction to Airy (M16:394-5); but, instead of sending dated letters, Adams allegedly preferred to wander in personally and deposit undated scraps of paper! (By contrast, Airy wrote Sheeplanks 1846/11/17 relative to publishing Adams’ paper: “It is important that you should note on Adams’ paper the day when it was received.”) Concerning the 7 October item [c] & fn 36, Adams had published orbital elements of a comet in 1844 (Grosser 1962 p.82) and had delivered a paper to the Roy.Astr.Soc. on 1846/4/8 (MNras 7:683). 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’ 1846/8/4 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,35 the most outrageous part of the process being that, as the theft proceeded, strong public French expressions of suspicion were used to show how irrational and undeserving the French were! The 1846/12/5 Athenaeum p.1246 refers to the great physicist & top French astronomer F.Arago’s “distorting mirror of national bias” and his “mania”. Airy to Sheeplanks (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.) 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 before been spotlighted. (Note also the near-simultaneous chronology in Challis’ 1846/10/1 letter, quoted in fn 30.)

35 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 outmaneuvered & outpoliticked by big Britain in 1846. It is a measure of scholars’ overwhelming sense of political suppression and unfairness in academe that the poor-neglected-Adams legend has gained such wide currency. (The sense of inequity is legitimate, but that does not ensure the truth of the instances often popularly held to illustrate it: fn 32.) The legend blames Airy’s & Challis’ paralyzing distrust of Adams’ math. I agree, but with the key addition that this mistrust was primarily Adams’ own. (See, e.g., §3.1-3.3.) 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 visit was a letter to Challis (1845/9/29 CON #42) asking him to tell Adams: “I am very much interested with the subject of his investigations and that I should be delighted to hear of them by letter from him.” After receiving Adams’ 1845/10 note, Airy wrote Adams a friendly inquiring letter (1845/11/5, M16:396-7) and simply got no reply. (Though, the rendition at Newman 1963 p.175 invents a nonexistent Adams reply anyway.) The letter asked Adams whether the hypothetical planet which accounted for Uranus’ longitudinal wanderings also explained Uranus’ anomalies in the radius vector. (Chapman 1898 pp.126-8 prominently & repeatedly confuses Uranus’ radius vector perturbations with Neptune’s mean distance to the kind of thing one finds regularly in extremely handwaving “The true history of the Earth-Moon system” where the process of meaningful refereeing is as mythic as anything in the Neptune affair.) Chapman’s paper adds useful material to the Neptune controversy, but his ritual attack (p.136 n.6) upon the “heroes—villains” approach just repeats the standard mythologemen Hist.sci. 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 of 1847/5/18, published in the 3/20 Athenæum p.309. (The effort is so obviously special pleading for Adams’ priority that Airy tries to deny that. A key false claim in this letter is discussed at fn 19.)

D4 Leverrier’s 1845/11/10 Comptes Rendus paper on Uranus’ misbehavior is called a “First Memoir” and ends with the promise that his next memoir will supply an explanation of the discrepancies. This paper would have reached England at the beginning of December. (The delay factor here is gaugeable as 3 weeks, since Airy saw the 1846/6/1 paper on 6/23-4; M16:398.) Having seen Adams’ 1845/10/c2 memo (supposedly MemoR), would not Airy communicate36 to Adams that one of most able living astronomers was now on the trail? We note that 1845/12/4-6 is the very time (Smart 1947 p.34) when allegedly occurred the only prediscovery occasion where Airy & Adams talked,39 from MemoR’s purported 1845/10 deposit at Greenwich — all the way until a chance outdoor meeting on 1846/7/2! D5 When on 1846/6/1 Leverrier published his first heliocentric longitude estimate (325° for 1847/1/1), he naturally used a Titius-Bode-based mean distance 38 AU. (Real Neptune is at 30 AU.) This also was AdA1, Leverrier the time at the floor of the Academy a reduced distance for his planet. The mails from France took a day or two to reach England (as is clear from Airy’s Neptune “Account”, M16); on 1846/9/2, Adams for the first time communicated to Airy that he had decreased his own planet’s mean distance. (The 1690 residual was the only real Adams possessed in 1845 which he later used as a post-calculation measure of whether his hypothetical planet’s distance was too large or too small. It was degraded not improved by decreased distance.) It is clear from Adams’ 1846/7 MemoW that only about 2 months earlier he had had no idea whatever of whether Neptune’s distance was greater or smaller than the Titius-Bode value (§B4 item [c]). In the absence of the RGO Neptune file, we have no way of knowing what Adams other hypothetical communications to Greenwich (besides that of 1846/9/2) may have said regarding Neptune’s distance.

D6 It is odd enough that Adams published nothing on Neptune before its discovery. An extraordinary additional point that has not been hitherto emphasized in any modern history is that Adams’ public silence40 regarding his supposed elements continued for well over a month even after the real planet’s 1846/9/23 discovery. Not that Adams wasn’t helpful news coverage of the controversy (Athenæum 1846/11/7 p.1148-9), having already (1846/10/15 Athenæum p.1069) attempted publicly with Challis to propose a name for Neptune, “Oceanus”! (All with Airy’s private pre-approval: CON #18 1846/10/14, “I like your name Oceanus.”) The sheer nerve is admirable. (Hind scoffed 1846/11/12 to Sheepshanks that “Oceanus” had about the same chance with foreigners as “Wellington”.) In brief, while Adams was saying plenty (contra the modern legend: fn 40) — now that the planet was found — he was talking about everything except the elements upon which the actual big-vs.-little tale is rather different: little (in international astronomical politics) France was outdeceived & outmaneuvered. (The standard explanation is implicit: Adams was just inherently quiet. J.Newman (1956) countered this view in learned detail in a public letter of 1847/3/18, published in the 3/20 Athenæum p.821): “shy, gentle, unaffected . . . . refused to be drawn into the bitter controversy over the question of who was the first. The only trouble with this genial myth is that it does not fit the post-discovery facts — most especially Adams’ overreaching “Oceanus”-shot at becoming the new planet’s names: §§D6.)
[a] doubt that he yet had finalized them, and [b] denial of credit to him for Neptune.

D7 Though asserting on 1846/10/1 that Adams’ hypothetical elements had not been completed until (§B2) 1846 June, Challis had been “mortified” to find on 10/12 (see the pathetic uncensored version of M16:412-413 at DSBB 3:186-7) that he had seen (& came within a whisker of capturing) Neptune on 1846/8/4 & 8/12, during the clandestine CambR Obs search. History generally regards this near-miss as a tragedy for Adams&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 (Athenaeum 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 excuse his “bad luck”; did Challis know, when he unleashed the mad beast Nationalism, that he himself would become the prime sacrificial goat in the British rendition of the Neptune story?42

E Adams’ Waiting Game

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

E2 This delay seems to have been by plan, since the very first public announcement of Adams’ work knowledgeably anticipates it: “Mr. Adams . . . will, doubtless, in his own good time and manner, place his calculations before the public.” (The statement of co-conspirator J. Herschel 1846/10/1, Athenaeum 1846/10/3 p.1019, Emph added.) When the skeptical French got openly impatient, the eminent British weekly Athenaeum (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?44

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 Athenaeum — 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.)

41 This estimate (good to about a day) is induced from Airy’s 1846/10/13 letter to Sheepphanks.

42 This sort of outrage is common today, but clearly there is nothing novel about it. Anyone out there still asking why DR is starting his own journal?


44 Which suggests a simple & novel hypothesis: were Adams’ 1845 Autumn trip to Greenwich (to see Airy) undertaken to launch a sky-search (which Challis had better equipment for, right next door to Adams at Cambridge) — or were they in truth just a neophyte’s plea for a specialist’s assistance with an extremely tough mathematical problem? The ossification of this history has so constrained all of us, that this remarkably simple thought has never been previously suggested.

45 In letters of 1846/10/14 (same as those quoted at §B2) Airy denies having had any part in the theoretical work. But in the same letters, he also denies having had any part in the observations — of which he was the instigator, designer, & advisor (§B4).
investigations.\textsuperscript{50} Though even freshman\textsuperscript{51} physicists are informed of Airy’s originality & intellectual gifts, Hist.sci chroniclers of the Neptune affair seem blissfully innocent of them. Numerous accounts — not the intelligent articles of Chapman & R.Smith, I am happy to say — routinely speak of Airy as a creativity-crushing dolt, whose dictatorial

ruined Adams’ chance for immortality. Extrapolating beyond Groser’s unsympathetic portrait, J.Newman’s prominent review\textsuperscript{53} of Groser’s book spoke of Airy as a: “school-bright, hapless donkey”, “unusually conceited”, & “bitterly jealous of his assistants — or of any young astronomer.”\textsuperscript{54} Fortunately, astronomer O.Eggen’s learned, near-simultaneous 1963/4 Sky & Telescope review provided a countering breath of sanity about Airy — plus a few gentle digs at the then-latest Hist.sci account’s inevitable technical-innocence slips, which have long provided such reliable entertainment for real scientists.\textsuperscript{55}

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, 1847/212; 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 Athenaeum 1846/12/19 p.1300). Actually, it should be noted that 33.4 AU (Adams 1846/11/13; M16:456; Challis loc cit) is not what Adams said in his 1846/9/2 letter, which had Hyp X near-circular orbit radius 33.7 AU (M16:407); the 33.4 AU figure evolved from there via subsequent alteration of recent residuals. But the more important point no one has previously noticed is that, not only is the longitude of Hyp X way\textsuperscript{56} 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 action of computing Neptune’s 30 AU mean distance (from 1846 observations)\textsuperscript{57} given in Challis’ 1846/10/15 & 21 letters (Athenaeum 1846/10/17 p.1069 & Astr Nachr 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 during 1806 & 1846.\textsuperscript{58} Thus, Leverrier’s final prediction was superior to Adams’ (Hyp X) regarding not only longitude but distance.

F Speculative Reconstruction of Adams’ Actual 1845 Oct Solution

F1 But we have yet to come to the possible ultra secret of the Neptune affair. The original conspirators never published the 1845 Sept list of elements, given by Adams to Challis at that time (MemoC) — though all histories speak of this as the golden moment when Adams’ immortal prediction was lodged. (Indeed, Glaisher 1896 p.xxvii repeats the typical version of the history in quoting Adams’ own rendition of what “bad luck” it was that, a few days after handing his solution to Challis, Adams missed dropping his 1845 Sept results off with Airy then, instead of a month later in Oct.\textsuperscript{59} Upon noticing this, I wondered if I was getting near the solution to the peculiar Neptune case, that is: finally making some sense out of a story that has never made sense.

F2 Challis’ description mentions that this 1845/9 note (MemoC) included a geocentric place (unlike the published “1845/10” MemoR) for the end of Sept (Uranus’ opposition). So I knew what it was when I saw the 1845/9 list of planet elements, which I will call “MemoD” or (when referring to the elements) “Hyp D”. MemoD is printed innocently in 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, MemoC (CON #32: only a 10° scribal error differentiates MemoC & 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 (\textsuperscript{64}4 from those of the “1845/10” MemoR which has always previously been accepted as Adams’ final 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 “final values” and that they were only “slightly corrected” (idem) to become the 1845/10 version. (MNRAS 7.150 has slightly altered; Challis 1846/12/12: “slightly different”, SP p.) Sampson (loc cit) suggests that this refers to correcting the 1845/9 perturbation term sign error (and then recomputing the whole problem from that point on, in the larger calculation of elements) — all this before the transmission of his proposed Hyp 1 (MemoR) elements to Airy (allegedly in 1845/10). But this is no slight shift (or slight recomputation). Indeed, I speculate that his sign-slip was the fateful error that numbed Adams’ original 1845 fervor into long inaction regarding planet prediction. (How the oscillation could destroy such a solution, see fn 71.) The troublesome term being tiny, the error’s discovery must instantly have terrified Adams with the vision of his lodging a solution with a major perturbation term miscomputed. (Just the time Adams would want to go see Airy!) The non-“slight” nature of the difference between MemoC & Hyp 1 (MemoR) is easily illustrated: Adams’ two famous predicted Neptune orbits had eccentricities of 0.16 (Hyp 1) & 0.12 (Hyp 2), so the original 1845/9 orbit’s eccentricity (0.14 or 14% — MemoC & MemoD) is nearly 1/2 way between the two! It is inexcusable that Adams called a shift from 0.14 to 0.16 merely “slight” — and falsely referred to the 0.14 solution as his “final” solution (of a series of such, “differing little from each other”!),

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

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

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

\textsuperscript{53} ScAtm 1963/3 pp.169-178.

\textsuperscript{54} The Newman 1963 account is precious (thus its appearance in our limited bibliography here) — as an epitome of the unlikable popular myth of ogre Airy buffeting & stomping wuvable Adams. Not since an earlier J.Newman’s circle was cooking up its astounding Lives of the English Saints (at the very time the Neptune case was brewing) had invented hagiography (e.g., fn 31) been elevated to such heights. (Unless we count Bill Stern’s legendary little Billy & the 1942 Lou Gehrig film bio, Cried in the Hanke.)

\textsuperscript{55} The import of this law unnoted until Rawlins 1969.

\textsuperscript{56} Two of them Challis’ then-precious prediscoveries of 1846/8/4 & 12 — uniquely useful for a little while, because they provided Adams & Challis a greater time-space of data than anyone else had at hand. I.e., in 1846 Oct, Cambridge Observatory was the only one in the world that had made (& knew it had made — vs. other’s �(B1) observations of the real Neptune over a period of months instead of weeks. So Challis’ search was not without utility (a point oft forgotten); indeed, the resulting data permitted the considerable mathematical talents of Adams & Challis to give Cambridge University a genuine and nontrivial priority in this affair: the first to publish the correct distance of Neptune.

\textsuperscript{57} Perihelion at 32.263 AU during 1826 (so near the JES time of passing Uranus that the coincidence might have suggested trouble for the solution). Around their later respective perihelions, Adams’ even more eccentric Hyp 1 & Hyp 2 also got comparably close, though for fewer decades: 32.42 & 32.78 AU; resp; so the important perihelion distance was increasing not decreasing, as Adams’ solutions progressed (which shrunk the most recent residuals).

\textsuperscript{58} But if the solution was altered in the interim, which is the only defense insider R. Sampson can make, on the little known record we are about to explore, then one can hardly call it anything but good luck.
as he plainly did. 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 1 (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 1 being worse by this criterion (Sampson 1904 p.165). So, how could Adams be sure that Hyp 1 was better 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 all relates to the rockbottom broader point: Adams was terribly 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: [F6]) 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 1 (Memor). 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.

50 Adams (M16:429, 1846/11/13, emph added): “After obtaining several solutions differing little from each other, by gradually taking into account more and more terms of the series expressing the perturbations, I communicated to Professor Challis, in September 1845, the final values which I had obtained for the mass, heliocentric longitude, and elements of the orbit of the assumed planet. The same results, slightly corrected, I communicated in the following month to the Astronomer Royal . . . .” Sedgwick’s 1846/12/6 account of an interview with Adams relays a similar story (Glasher 1896 p.xxviii): “I wanted [in 1845] to send my papers in good order to the Astronomer Royal. I went over all my calculations three times. I added a few [perturbation] terms, without changing my results. I was much interrupted, so it was my vacation [between Sept’s Memoc-Memod and Oct’s Hyp 1 on MemoR] before I could finish my last revision”. Nowhere does Adams [a] admit the serious difference between the Sept & Oct solutions, or [b] discuss his Neptune-related activities between 1845/10 and 1846/6.

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

**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 = 321°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 [b] (of the 3 Adams miscues listed in [F4]). But perhaps all 3 errors were eventually cleaned up, leading to a (DR-specified) document bearing what I am calling “Hyp G”:

- Mean Dis. 38.4 AU (assumed nearly in accordance with Bode’s law).
- Mean Long. at end of Sept’ 322°08’
- Long. Per’ = 321°00’
- Eccentr’ = 0.1428
- Mass = 0.000173, that of Sun being unity.
- Geo. 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 302 or Sampson 1904 p.166 vs. M16:395 or SP 1. Note also the slip at M16:406 (or SP 3), accidentally substituting 10/1 for the correct 1846 date of Uranus’ opposition, 10/6 (M16:445, 453). Challis (SP p.) makes the earlier epoch explicit as 1845/9/30.)
whether or not Hyp G was ever written out by Adams, it is the correct rendition of the elements he deduced in 1845/9 — which include e = 14%. It thus represents what a capable astronomer, e.g., Airy or Breen, would have been using on that basis.) Having broached this novel speculation (which may help explain why the RGO Neptune file has never been made public), let me now turn to the various evidences that lend credence to it.63

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

G1 Adams worked slowly & cautiously (which is why he lost Neptune); it is on the face of it unlikely that, in less than a month (the 4 weeks prior to 1845/10/21), he performed all the necessary calculations64 that would turn up his term-sign miscue — and then recomputed and re-checked this lengthy solution, to his notoriously-perfectionist satisfaction-certain that no more such errors lurked (and then with blithe what-me-worry?-confidence instantly frisked off to bother the Astron Royal). Of the Adams ms solutions, that in question here (Hyp 1: sections E IV-V of the 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: §II2.) From Challis’ 1846/12/12 Report to the Observatory Syndicate (SP p.114: “It is to be regretted that Mr Adams was more intent upon bringing his calculations to perfection, than on establishing his claims to priority by early publication.” (DR italics for irony.)

G2 If we are to believe that the “1845 Oct” MemoR (Hyp 1) really existed at the purported time, we must also believe that even though Adams subsequently rechecked lots of the work (Sampson 1904 p.166-7), nonetheless: all 30 of the residuals finally presented to the world on 1846/11/13 (M16:406-7) were identical65 (down to the arcsec tens & hundreds displayed) with those Adams allegedly handed Airy 13 months previously (M16:396). G3 Comparing Adams’ published version of (his Neptune work) to his ms, 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 shows that he was setting up the figures for perturbations on 1845/11/28, 12/16, & 12/24 (Sampson 1904 pp.158, 168). The 1845/12/16 work rightly should have occurred at the beginning of the calculation of Hyp 1 (which indeed is where Adams places it in his published 1846/11/13 RAS presentation, M16:435) — not after its alleged 1845/10 submission to Airy.

63 Keep in mind: Hyp G is not a very farout speculation, since MemoC & MemoD physically exist, in CON (32) & the Adams Neptune papers, respectively; and Hyp G is merely: their numbers corrected for the 3 small errors noted at §F4. (Since the positions for the MemoC planet given in the table at Sampson 1904 p.152 agree closely with those I get for Hyp G’s elements — given above at §F3 — it is clear that Sampson also essentially corrected the same errors in MemoC that I have noted: i.e., the small differences between MemoC & Hyp G.)

64 A priori, it is more reasonable to suppose that the sign-error involved was found not during the doublechecking of Hyp 1 but rather (incidentally) during the parallel calculation of Hyp G. But that is a point we need not take here (see below). The language of MemoR. One notes that the 10

65 The 1690 residual disagrees by 0°1: 44°4. vs 44°5.5. But Adams must somehow have confused the two. The small differences between MemoC & Hyp G are tiny: it is not clear that this was the case.

66 From Challis’ 1846/12/19, quoted by Glaisher 1896 p.xxx: “It will hardly be believed that before I began my observations (1846/9/29) I had seen nothing of his [Adams’] work...”
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 1874/1/1 heliocentric longitude), while the Hyp G orbit (which I suggest was the orbit actually given by Airy by Adams in 1845 Oct) puts the planet at 325° 05’ on 1847/1/1, only 1/12 degree different from Leverrier’s place.669

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

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 ephemeres, 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’-39’ 18’ (1846/8/29-1847/1/1).71 This suggests (but does not in itself prove)72 that the famous Hyp 1 orbit (alleged in the totality of 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 we know it was not released until after 1846/8/20; see Sampson 1904 p.67) — thus: 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

669 For 1846/8/29 or the epoch Adams used in 1846 (his perturbation-based solutions always used Uranus’ opposition as epoch: 1846/10/6 for Hyp 2), 325° is consistent with his real 1845 solution (Hyp G) which gives 324°-23’ for 8/29; 324° 36’ for 1846/10/6 — while the real purported “1845 Oct” MemoR solution (Adams’ wellknown “Hypothesis 1”) gives values that cannot possibly be confused with 325°, no matter the rounding: 328° 20’ for 8/29, 328° 47’ for 10/6. For 1847/1/1, the 1845 Sept solution actually handed Challis, including the errors noted here, MemoC (CON #43), gives 324° 22’ — almost within 1° of Leverrier’s place.

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

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

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° 4’ and RA 22:48 & declination -2° 4’. He also comments (wrongly) that the known completed Berlin Sternkarten (Berlin Observatory) cover “a small portion” of the area, adding (9/21, CON #5): “There is only one [Berlin Hour 22] which applies partially to the inquiry.” In case it helps explain the search’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 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 the very moment Airy was downplaying the Hyp 2 chart’s utility, Neptune was slowly sidestepping 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.Argelander) covers more (c.35%) of the Airy-projected search area than the famous HR 21 (1844, by C.Bremiker, also renowned for his 1856 log-tables) that made possible the swift Berlin DBs 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.

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) his 2 (§F4), 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 affair: there are no dates given anywhere on the manuscript pages of the work.

H2 Which suggests the possibility that the calculator of this work did not want its date to be known. Adams’ various memos (the famous solutions for the elements of the predicted planet), handed to Airy & Challis, are also all oddly undated by Adams, as remarked. This is not a record to be accepted at face value.

H3 We have never been told what the devil Adams was doing for the first half of 1846. Not a scrap of dated Adams perturbational calculations has ever been located, between 1845 Xmas Eve73 (Sampson 1904 p.168) and 1846/8/20 (ibid p.167), less than 2 weeks before his final 1846/9/2 report to Greenwich. This is as credible a record as supposing that Adams went off with Santa Claus for 8 months. A related & clearly inexplicable item (see also §D4), one which suggests culling of files (e.g., CON): after seeing Leverrier’s published position of Neptune on 1846/6/23-4, Airy wrote Leverrier on 6/26 and wrote Whewell on 6/25 of Adams & Leverrier’s “remarkable calculations”. But Airy denies he wrote any letter75 at this time to Adams — and no Airy letter to Challis exists (in CON) for this time. Considering that Leverrier’s paper had just lit a fire under Airy, and considering that he regarded the Adams work (which Adams’ advisor Challis had steered to him) as “remarkable”, why should he immediately write to Whewell & Leverrier but not to Adams & Challis, the two principals of the British prediction & upcoming claim? Recall Airy’s promises to Adams & Challis (§B2) that, in choosing “extracts” from their correspondence with him: “I will not compromise any one.”

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

75 Airy to Sedgewick 1846/12/4 (Smart 1847 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 might last two minutes [§B6]. No other opportunity of seeing him.”
I. A Cohering Hypothesis

1. 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 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 16 but also might look like a giveaway anachronistic slip?

2. The question of confidence is central to Adams’ claim. His failure to publish is an obvious measure of the truth. And his long-forgotten mid-1846 handwritten ephemeris (MemoW, CON #35) places a (hitherto unpublished) lost-star-based circular orbit ahead of his so-revised perturbation-based orbit! (Recall $\delta$-Wartmann, etc.) This raises the possibility that Adams was at this very 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 damned as unfair to Adams (e.g., Smart 1947 p.35), but it may instead reflect the truth of the matter (well known to Airy at the time, but later swamped by British national fervor).17 Airy wrote Leverrier on 1846/10/14 (Smart 1947 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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16 Accepting the 1845/9 MemoC (CON #32) as genuine, this confidence was in fact expressed by Adams (at least before finding his defacing sign-error), since his bold statement to Challis on this paper is: “The Elements of the New Planet I make to be as follows . . . .”

17 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 1845/11/10 paper, he perhaps warns Adams of competition. [c] Seeing the 1846/6/1 Leverrier paper, he launches (6/29) a secret sky-search to win Neptune for Cambridge. [d] Upon hearing of Galle’s Leverrier-directed 1846/6/23 capture of the planet, he regards British hopes as lost and makes ultranice with the French, in vain hopes of heading off a fight that must lead to embarrassing revelations. [e] The 1846/8 Camb.Obs. observations of Neptune (discovered 10/12) inspire Challis to flagwave, but Airy creditably continues (1846 Oct & Nov, even while perhaps helping Adams get his solution into publishable shape) to regard Leverrier as the prime discoverer. (Contra this: a privately stated Airy purpose for the 11/13 presentation was to “do justice to England”: CON#18 p.2; Smart 1947 p.34.) [f] However, by 1846 Dec, Airy has become the public villain who ignored public hero Adams in 1845, and, so (fearfully or opportunistically, and aided by the unfolding differences between the real & Leverrier-predicted planet), he slides back to his Cantab cabal’s original 6/29 intent (item [e], above) to use Neptune for the glorification of Cambridge mathematics.

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18 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 have preferred the former’s company. That Leverrier (bigtime head of the Paris Observatory) was extremely unpleasant to his colleagues is amply testified to. (In a devilish play on Neptune’s symbol, Humboldt called Leverrier “the man of the trident.”) But it is also fair to ask: was being cheated (of his proper due regarding Neptune) a partial explanation of why Leverrier became nasty? Against this theory: Leverrier’s bad temper appears to have been mostly the product of a paranoid personality, and aided by the unfolding differences between the real & Leverrier-predicted planet. (Contra this: a privately stated Airy purpose for the 11/13 presentation was to “do justice to England”: CON#18 p.2; Smart 1947 p.34.) [f] However, by 1846 Dec, Airy has become the public villain who ignored public hero Adams in 1845, and, so (fearfully or opportunistically, and aided by the unfolding differences between the real & Leverrier-predicted planet), he slides back to his Cantab cabal’s original 6/29 intent (item [e], above) to use Neptune for the glorification of Cambridge mathematics.
I believe now are in a position at last to explain the Neptune scandal’s inexcusable: [1] why Adams didn’t reply to Airy’s 1845/11/5 letter (Sampson 1904 p.168 has some private Adams math work, also dated, from 1845/11/28 & 12/24 on the letter’s question); [2] why his 1845/12/5/±1 meeting with Airy (Smart 1947 p.34 fn) produced nothing (which [like their mutual attendance at RAS’ 1846/2/13 meeting] shoots down Adams’ excuse that he didn’t reply to Airy’s letter because he preferred verbal intercourse to writing letters; Glashier 1896 p.xxix); [3] why he did not publish even after Leverrier’s 1846/6/1 paper.

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-errorize 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’d presumably informed Adams of Leverrier’s paper), then Airy’s predisclosure 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 1988 p.123), thus 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 forthcoming 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 non-inner-circle-British astronomers) Adams’ precise initial agreement with Leverrier’s longitudinally due to his own paralaxis, 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 [J8]; however, this is but one among various possible non-mutually-exclusive alternatives, e.g., §C2 §9, fn 67, fn 70.) The most extreme irony of the Neptune affair is the fashion in which the Cambridge conspiracy backfired: Airy thought that his privately knowing of Adams’ work gave him an advantage, but Adams’ uncertainty about everything contributed to spreading the British effort over a huge piece of sky, while intrepid (Columbus-like)81 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.

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 Athenaeum 1847/4/3 p.371; minor DR alterations in that translation):

The laurel which you have been the first to deserve has been merited also by . . . of being communcated without loss of time to the scientific world . . . . I see . . . a young man of talent . . . . I shall say to him . . . “The laurel which you have been the first to deserve has been merited also by another, who has carried it off before you had the boldness to seize it. The discovery belongs to him who proclaimed and published it to all, while you reserved the secret to yourself. This is the common, unwritten law, without which no scientific title could be assured.81 But, in your own mind, you are conscious that the new planet was known theoretically to yourself before any one else knew of it. This inward success ought to give you the consciousness of your power, and excite you to direct it to the many other great questions yet remaining to be resolved in the system of the world; and if my years give me the privilege of offering advice, I shall express it in one word — PERSEVERE.”

II2 The felicitous ending to our story is that Adams creditably followed Biot’s just & fatherly charge to him: his later hotly-disputed (now unquestioned)82 discovery of the correct gravitational lunar-acceleration (overturning Laplace) by itself places Adams in the front rank of history’s mathematical astronomers.

Partial Bibliography

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81 Note similarity to §8 [A2].

82 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).
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Acknowledgements: This is to express my longstanding (& long overdue!) gratitude to David Dewhirst (Cambridge Obs) for sending (1967/3/2) photocopies of [a] Challis’ 1846 Neptune-sweep zone records [b] most of CON (which I believe David first organized), & [c] the latitudinal Adams Neptune mss. (All this despite David’s wide & overmodest disagreement with DR’s view of the Neptune affair.) Also: my thanks go to J.Bennett, former RAS Archivist (now, like David, an Adv Editor of the J.HA), for sending (1975/12/1) 70 pp. of material from the R.Sheepshanks correspondence, and to Malcolm Pratt of the St.Johns College Library, Camb University, for airmailing (on very short notice: 1988/11/30, 12/16) xeroxes of a sizable part of the longitudinal Adams Neptune mss.
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