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DIO

&

The Journal for Hysterical Astronomy

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‡6 Scrawlins

A Shorts

A1 In 1990, rich Japanese businessmen purchased[1] 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 differ[2] in that: good acting gets you into one — and out of the other.

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

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

B2 Bush in office: raises taxes. Now, for the '92 campaign, remorseful Bush swears:[3] I-won't-do-it-again! Social workers everywhere want to learn: how, in just 4 years, has Bush rehabbed Willie Horton from scorned Lib-symbol to fave speech-writer?

C Archimedean Santa

C1 What theme do these items have in common: [1] a wishbone, [2] a B.F.Skinner superstitious pigeon, [3] a US election?

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

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

[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 (& 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), BUSH: NO TAX HIKES AGAIN, "EVER, EVER".

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 'snews 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 undiscussed. 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, x 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 baby-factorydom),[11] and [b] Bush picking that cute little boy for his Vice-Presidential mate? (Granted, such a question is inexcusably prejudicial, since the undoubted truth is that Bush selected Quayle for his mental depth & celerity.)

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

E Robert Newton & the Muffia

E1 Physicist Robert Russell Newton died in 1991 June. He was the retired Supervisor of the Space Sciences Division of the Johns Hopkins U Applied Physics Lab. I am proud to have known him, in good times and rough ones. He is survived by his 2nd wife Gene Newton and several children by his late 1st wife, Doris. RN was (along with 0 Gingerich & Lord Hoskin) the scholar most responsible for unleashing the new journal *DIO*. (Happily, RN lived long enough to read issue#1 and see it widely distributed and well received.) His Ptolemy researches got vile treatment from the Otto Neugebauer-Muffia oldboyperson-clique (for reasons which were, at bottom, careerist and thus largely fiscal — which make them particularly repugnant in light of the pseudointellectual veils used to hide this reality). Nonetheless, he remained admirably jocular about his various archon-smooching enemies. (I shall never forget his gentle, goodnatured attitude towards them, nor their ugly[13] conniving to harm him as much as possible, for selfish professional advancement. This while avoiding symposium-debate while he lived. His death now renders it impossible ever to make substantial restitution to him.) A person of high mental abilities and wide culture, Newton became in his later years a rare combination of physicist and intellectual archaeologist, which resulted in a series of books (published by Johns Hopkins Univ Press or Univ Md) which used pretelescopic astronomical observations to determine the history of the Earth's spin and revolution. If I were to specify the single quality which I most admired in him, and which ensured a firm friendship (despite numerous disagreements), it would be: when mobs of inferior minds yelped at him in unison,[14] and scrambled to get in line to attack him (in order to cover the shame of the exposed archons they kiss up to), he had not the slightest interest either in muting the boldness of his theories or in politically compromising with 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 impending appointment, on the basis of an account of unwitnessed events (of 10^{y} antiquity) when not a scrap of written notes was kept by the accuser — and when it appears that she took the percs (that attached to tolerating alleged verbal abuse) as long as they lasted, and only went public after that well went dry.

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

[6] Lobbies' insatiability regarding proportions reminds one of the argument 23^{m} into the classic 1963 film *It's a Mad Mad . . . World*. The US Senate's bitty-state-placating disproportionate representation-math originated just so.

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

[8] Are most women still falling for the media-dangled short-term-easy escapist myth (implicit in ubiquitous ads for cosmetics, creams, & shampoos) of: seductress-wife as a career? If so, then feminism is faltering not because it isn't using its numerical advantage, but because there *is* no such advantage — i.e., feminism unfortunately doesn't represent most women. (And that 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 for Congress itself. Congress' reasoning is self-evident. So is mine: for most jobs, just let talent & drive tell; but Congress is different, since it is supposed to *represent* groups, in roughly proportional fashion. Regardless, I don't expect we'll hear anyone argue that girlpersons don't belong in Congress because they lack the intellectual necessities.

[13] See *DIO 1.1*: ‡1 §C6-C8, ‡3 §D3.

[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 Muffiosi scurry to woo you away from your heretical error. (See *DIO 1* ‡9 fn 9.) [c] After sufficiently generous persuasion has been bestowed, reconvert back into the secure Muffia fold, to hosannahed thanks for your blesséd enlightenment.

selfconfidence and courage, in modern academe. Even some among his legion of craven detractors acknowledge that he was the subject of useful controversy. He will be missed.

E2 A critical distinction, commended here (as also that at *DIO 1.1* ‡1 §C12) to the consideration of those following the Ptolemy Controversy: scientists generally reserve the epithet "Incompetent" for those who are simply incapable of performing procedures necessary for the work at hand.[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* ‡5 §D14f & fn 20, or *DictSciBiog 11*:201.)

E3 Noncitation: Robert Newton's publications on Ptolemy's fakes started in 1969. But the Neugebauer Muffia's leading capos refused to cite them until 1977. Eight years. (See, e.g., even Muffia princess Janice Henderson's evasive piece in *Sky & Telescope* 1976/2.) Why? Simple: only in 1977 did the general public become aware (through articles in *Time* and *Science*) of RN's findings in this area. Now, if an honest academic critic sees what he regards as erroneous work by a respected scholar, does he handle the problem (until forced to do otherwise) just by sealing off mention of the work & by privately slandering the author? (See *DIO 1.1* ‡1 §C7.) Or does he instead regularly meet the allegedly-errant scholar in polite public discussion at academic gatherings, where the evidential & logical merits of the matter can be rationally discussed? — and where, if the offender is indeed wrong or foolish, this can be demonstrated in an open-adversarial setting, on valid academic grounds. The 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) calling[16] Galileo a "scrambling social climber" is as entertaining[17] as finding (1976/3/12) R.Kargon, sometime *Isis* boardperson, accurately describing a well-known astronomer-historian-politician as "one of the biggest —-kissers in the business." Either of these eminent professors puts me in mind of Montaigne's observation: he who gossips to you will gossip of you. (I can't imagine why the next paragraph's theme should follow so immediately upon admiration of the present paragraph's magnates.)

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

[a] One will not be listened to unless one possesses power.

[b] One cannot attain power without laboring mightily towards its possession.

[c] But this very effort so wipes out one's time&energy, that there's insufficient left over for original thought.

[d] Upshot: the power operator labors for decades to get into a position where he can put over his new ideas — and by the time he's got the power to do so, he has no substantial new ideas.

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

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

[16] *Scientific American 247.2*:132 (1982/8), p.136.

[17] See ‡8 §C9 or *JHA 22.2*:187 (1991/5).

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* (‡1 fn 12, §C7, §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 asserting an unwelcome truth, it is tempting to try working Within-The-System, since this is more pleasant and implicitly optimistic.[20] However, [a] The more receptive The System is, the less important the issue. [b] The most important issue is: The System itself.

[18] *Isis* Publications Committeeperson A.Van Helden's 1990 review of the *Journal for the History of Astronomy* mentions prominently (*Isis 81.2*:298) that "some of the best articles" in the *JHA* just-so-happen to be those of the esteemed *JHA* Editor-for-Life! (And neutral critic Van Helden just-so-happens to be a *JHA* Adv Editor.) Admire, too, 0 Gingerich's Disraeliesque [Robt.Massey *Dreadnought* 1991 p.21] trowel at work: fn 17. Actually, there was a time when those politician-businessman-scholars (especially British), climbing under archons, had to be alot more unsubtle than even today; though, convincing the vain patron not merely of his generosity, but additionally of his *brilliance* as an intellect & author, is a timeless requirement. E.g. Sir Francis Bacon, Lord Chancellor, Baron Verulam (whose defense to a bribery scandal was the lawyer-oldie that he indeed took bribes — but without effect on his decisions!) dedicated *Great Renewal* (1620) to King James I thusly (emph added): "To our Most Gracious and Mighty Prince and Lord JAMES by the Grace of God of Great Britain, France and Ireland, Defender of the Faith, Etc. Your Gracious and Mighty [DR: aah, didn't we cover that already?] King, . . . if there be any good in what I have to offer, it may be ascribed to the infinite mercy and goodness of God, and to the felicity of your Majesty's times . . . the wisest and most learned of kings . . . . you who resemble Solomon in so many things — in the gravity of your judgements, in the peacefulness of your reign, in the largeness of your heart, in the noble variety of the books *which you have composed* [DR: like James' personal 1611 translation of the Bible in his spare time?] . . . . May God Almighty long preserve your Majesty! Your Majesty's Most bounden and devoted Servant, Francis Verulam, Chancellor." And Jos.Addison similarly prefaced his justly famous *Spectator* (1711) with an even nearer-perfect extended browning-study — the sort of thing which implicitly tells one more about what is required for the attainment of true archon-seraph-hood than anything ever written explicitly. (I quote from an edition, published in 1753, which has been in my family's library ever since.) The peaks, from *six pages* of slathered ecstasy: "To the Right Honourable John Lord Sommers, Baron of Evesham. My Lord, I should not act the Part of an impartial Spectator, if I Dedicated the following Papers to one who is not of the most consummate and most acknowledged Merit. None but a Person of a finished Character, can be the proper Patron of a Work . . . . I know that the Homage I now pay You, is offering a kind of Violence to one who is as solicitous to shun Applause, as he is assiduous to deserve it. . . . While Justice, Candor, Equanimity, a Zeal for the Good of your Country . . . are valuable Distinctions, You are not to expect that the Public will so far comply with your Inclinations, as to forbear celebrating such extraordinary Qualities. It is in vain that you have endeavored to conceal your Share of Merit . . . . Do what you will, the present Age will be talking of your Virtues, tho' Posterity alone will do them Justice. . . . your Lordship, who could bring into the Service of your Sovereign the Arts and Policies of Ancient Greece and Rome; as well as the most exact Knowledge of our own Constitution in particular, and of the interests of Europe in general . . . . to enumerate the great Advantages which the Public has received from your Administration, would be a more proper Work for an History than for an Address . . . . I would . . . rather . . . speak of the Pleasure You afford all who are admitted into your Conversation, of Your Elegant Taste in all the Polite Parts of Learning, of Your great Humanity and Complacency of Manners, and of the surprising Influence which is peculiar to You in making every one who converses with your Lordship prefer You to himself, without thinking the less meanly of his own Talents. But if I should take notice of all that might be observed in your Lordship, I should have nothing new to say upon any other Character of Distinction. I am, My Lord, Your Lordship's most Obedient, most Devoted, Humble Servant, The Spectator." (Did Addison aim here at unbestability? Poor 0. One is reminded of Rupe's dream of Jerry's how-do-you-do-it adulation in the delightful film, "King of Comedy". It should perhaps be observed that Addison could tell satire from sincerity. And probably knew who couldn't.) Numerous sects' religious invocations still retain similar language (though lighter on the shoveling & heavier on the groveling). Which suggests: either established religion is anthropomorphic, or the deity needs shrinking. Why would patrons, kings, or gods be so insecure as to wish or even tolerate such flattery? To his credit, extremely-literate James I demoted Bacon & extremely-literally axed superbutterer & tobacco-pusher W.Raleigh. A king who [a] loathed tobacco & [b] executed (with Bacon's happy assistance) the very person responsible for introducing that lethal addiction-plague-stench into European civilization, is my kind of royalty.

[19] One recalls Woody Allen's comment on the famous photo of Will Rogers with Warren Harding: a leader of show business shown with a leader of politics, back in the old days — before the 2 fields merged.

[20] Nonviolent revolutions generally produce higher quality results than violent ones. However, the former usually do not occur without fear of the latter.

G Two Party Ping-Pong Pocket-Plumbing

G1 For your home's plumbing needs, you call on plumber A. But he fails you, so you go to plumber B. When plumber B fails,[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.
G2 If you actually did turn your everyday searches for talent into such boring & infantile table-tennis exercises, you'd create, with respect to plumbers A&B:
[a] understandably low regard for your intelligence,
[b] your rapid impoverishment to fund plumbers' mansions, limousines, yachts, & tourist junkets,
[c] behind-the-scenes cartel-collusion-merging of A & B,
[d] a home perpetually agurgle with new demands for plumbers' ministrations.
G3 Yet, sheeplike US voters follow exactly this pattern in their recourse to the two political parties that are taxing them (and their progeny) into economic debtor-imprisonment[22] — even as TV 'snews pundit-flunkies assure the plumbees of the sanctity and inherent wisdom of the "Two-Party System".[23]

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

[23] Even allegedly reformist 3rd Parties have become increasingly suspect, starting with L.LaRouche (1976) & J.Anderson (1980) — for the simple reason that 3rd Party C may merely be regular Party A's catspaw, injected to 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 Littleguy'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 worrisome 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."

‡7 Unpublished Letters

A A Mostly-Unpublished Warning

A1 The subject of *Time*'s 1991/9/16 cover story was Lamar Alexander, the US Cabinet's Education Sec'y, who was going on about how he hopes-to-reverse-the-degeneration of US education. The response leading off those printed in the 1991/10/7 *Time* "Letters" was the bold-face-printed letter: " '**America is still looking for a gimmick to pull it out of an educational downturn.**' Marion Gadberry, Oroville, WA."
A2 A glance at the letter suggested that Gadberry had written more of value than what was published. So, I instantly reached him by phone and learned that the published statement was indeed just a snippet and that its writer understandably felt his message had been virtually eviscerated. So *DIO* presents here the original letter, in full, with thanks to the writer for his trust in transmitting it to us:

> To: Letters to the Editor, *Time*, Rockefeller Ctr, NYC 10017
> From: Mr. Marion Gadberry, a teacher, P.O.B. 1429, Oroville, WA 98844; phone: 509-476-2306
> **A3** The politicians and the American people are still looking for the gimmick which will pull us out of our educational downturn. They are blind to the sociological facts that no gimmick can ever overcome, namely — a 50% divorce rate that traumatizes students, 40% Latch Key children that come home to unsupervised homes, 40% of American students being raised by single-parent families that have neither enough money nor enough energy to properly supervise their children, children participating in a plethora of extra curricular activities that are "more important" than coming after school for extra academic help, taking days out of school for family reunions, hunting trips, vacations to Hawaii, orthodontist appointments, etc., 5% to 10% of the students involved in drug usage, an educational system that pushes every student through the same educational curriculum regardless of their intellectual and emotional characteristics, and all the above compounded by the insidious [side] of America's affluence.
> **A4** A computer in every student's lap and every educational reform will never erase the negative effects of the above-mentioned sociological facts. Let's face it, Americans are getting back exactly what they put into their families and schools — very little.

A5 I would be more 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 §D.)
A6 There is a creepy resemblance between the search after cures for cancer and for the US' educational collapse: both searches are expensive, lobby-ridden, seemingly endless & fruitless. Perhaps we can learn something about the latter morass (see & *DIO 1.1* ‡1 §D4) from a DRism on the former: the best cure for cancer is not getting it in the first place.

B Prediscovery Observations of Neptune

To: Letters, *Scientific American*, 415 Mad Ave, NYC 10017 1981/1/13
From: DR

B1 The December *Scientific American* states (p.74) that, after Galileo's 1613 observation of Neptune (Kowal & Drake's recent astonishing find), and before Galle's 1846/9/23 Berlin Obs. optical discovery (directed by the mathematics of Leverrier), only "One observation of Neptune . . . was already known [1795, by] Joseph Lalande, a French astronomer who catalogued some 50,000 stars. . . ." As author of the article [*AJ 75*:856 (1970)] cited in support of this, may I mention two items? [a] The 1795 observer was actually Joseph Jerome Lalande's nephew, Michel Lalande. Of the 50,000 stars in J.J.Lalande's 1801 *Histoire Celeste*, not one was observed by the titular author.[1] [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:

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

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[3] observations, also [due to] the planet's southerly position).

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

[1] This amusing item was revealed by J.Delambre.

[2] But see ‡9 fn 19.

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

[4] My 1973 *Astronomy & Space* paper was perhaps the first to accent the 38^y 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 ‡9 §I4.

‡8 Current Developments

A How Double-Sunsets Triggered the Discovery of America

A1 G.Corraface, star of the 1992 film *Christopher Columbus, the Discovery*: While making the film, "We were on those long boats for *ten hours*, before we finally set foot on the newfound land. And it was quite a relief, let me tell you, because everyone was getting sea-sick."[1] *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[2] 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[3] his fateful overestimate of Eurasia's longitude-spread ultimately from Marinos of Tyre (c.100 AD), whose geographical data underlay C.Ptolemy's famous *Geographical Directory* (*GD*), c.150 AD. Columbus adopted precisely Marinos' value[4] 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).[5]

A3 The geographies of both Marinos & Ptolemy used the famous Earth-circumference of Poseidonios (c.50 BC), C_P = 180,000 stades or 18,000 nautical miles (nmi); this is 5/6 of the correct value, which is 21,600 nmi.[6] 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 h and the time-difference t between sunset at sealevel and sunset at height h. For the simple case where the observation is made at the Equator and at an equinox, the geometrically deduced Earth-radius in km will be $r = 378000h/t^2$, for h in meters & t in timeseconds. DR wrote up the method for the *American Journal of Physics 47*:126 (1979/2); and, thanks to Jearl Walker, it then appeared in *Scientific American* (1979/5 p.172).

A5 But the method has a nontrivial flaw. The mathematical result is infected by atmospheric refraction (which the ancients, who lacked quantitative tables, couldn't correct for): a horizontal ray of light is bent 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.[7] 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 were 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 §I10.

[3] See D.Boorstin *Discoverers* NYC 1983 p.230. (Also: J.Thomson *Hist. Ancient Geogr* Cambr Univ 1948 p.3.) For a photograph of Columbus' handwritten comments on Ptolemy, see p.20 of *Log of Christopher Columbus* transl. R.Fuson, Camden ME 1987.

[4] *GD* 1.11.1 (Marinos 15^h = 225°) vs. 1.14.10 (Ptolemy 12^h = 180°).

[5] S.Morison *European Discovery of America: the Southern Voyages* Oxford Univ 1974 p.30.

[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, $C = 360° \cdot (60'/1°) = 21{,}600'$. For the stade's length (185 m ± c.2%), see, e.g., E.Bunbury *HistAncGeogr* (1883) 1:209f & 624, *EncBrit 23*:488H (1961), *Random House Dict* 1967: "stadium" = c.607 ft (185 m).

[7] See S.Newcomb *Compendium of Spherical Astronomy* 1906 pp.198-203.

(i.e., if a horizontal light-ray's curvature were equal the Earth's), then a horizontal light-ray would eternally skim the Earth, and the Sun would never set.[8] Thus, t would be infinite, and the Earth-circumference C deduced (from the above formula) would be zero.

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

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

A8 $C_{\rm E}$ = 252,000 stades and $C_{\rm P}$ = 180,000 stades were the only values widely adopted in antiquity. Their average is exactly correct: 216,000 stades (= 21,600 nmi); but C = 216,000 stades is not attested in antiquity — despite the fact that the famous & laborious "Eratosthenes method" (supposedly entailing direct traverse of arid desert along the straight line between Alexandria & Aswan-Syene), ascribed to him in every modern astronomy textbook, would give this correct value. Obviously, academic-socialite Eratosthenes simply used or adopted the dip-method result (which could have involved merely ordmag an hour's work atop the famous lighthouse, as against weeks of wearing & dangerous travel from Alexandria to Aswan) — not the method for which he has been unjustly immortalized.[9]

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

B Heckathorn Scores Again

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

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

[9] Such persons as E.Lehmann-Haupt & C.Sagan depend upon fiddling with the length of the stade, in order to make 252,000 stades seemingly equal the correct figure. (To their great credit, D.Dicks & O.Neugebauer wisely avoided and explicitly rejected following that evidence-twisting precedent.) However, it now turns out (as shown above) that the answer to the ancient mystery of these disparate C estimates is not metrological but physical.

which genuinely first reached the South Pole on 1911/12/15,[10] 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 hireling "Navigation Foundation" (NavFou) — in its 1989 Report's[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., 1990/3&6 *Scientific American*; also *Nature 344*:902, 1990/4/26). So, when Ted's 1991 finding (of Amundsen's longitude data) destroyed the other leg, NavFou-National Geographic's doubly-costly Peary-apology was instantly reduced to pathetic paraplegia.

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

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

B5 Ted found Amundsen's "nonexistent" transverse data right in the Norwegian edition of Amundsen's widely read 1912 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 current institutional insistence that Amundsen made no longitude observations, it is interesting to read the caption Amundsen prints below the photoreproduction of these data: "A page of azimuth and longitudeobservations." Ted transmitted the data to DR, who computed[13] the longitudes W and compass variations V from the spherical trig formulas which are standard in such work. The match

[10] By Australian dating, which is used in the diaries and observationsbook.

[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* ‡1 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 acknowledgement of this has been published, nor will it ever be, since *National Geographic* is constitutionally incapable of owning that its critic DR is ever right about anything. (See ‡6 §F4.) This statement is itself a criticism; and, given NGS' consistent level of neutrality & integrity, one could confidently engrave it in granite. (NGS' degree of openmindedness is as wellknown among serious scholars as is the personal nature of its positive & negative views on any issue. *Nature* 1990/4/26: "the National Geographic Society . . . will always believe [Peary] reached the Pole.")

[12] The NavFou also adduces Cagni's 1900 alleged Farthest (skeptically analysed at *Fiction* pp.65-68) without noting: [a] Cagni never even claimed to have approached the Pole, where steering for the polar point is critical. [b] Cagni did report a transverse observation anyway at his turnaround-point (A.Luigi *Osserv Scientif* Milan 1903 pp.50-51 obs.#149; cited at *Fiction* pp.65 & 297). [c] Cagni's reported compass variations (cited by NavFou Report p.62 as confirming Cagni's supposed steering by solar culminations) are mistaken by over 20° (as noted for the first time at *Fiction* p.65).

[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 V results, calculated in the field from its Nonexistent transverse solar data.) For 1911/11/9 (L = 83°S): V = 132°37′, W = 165°30′W (Hanssen); V = 134°32′, W 166°07′ (Amundsen). For 1911/11/13 (L = 84°S): V = 141°14′, W = 165°12′ (Amundsen). (The two seemingly discrepant 11/9 longitudes actually differ by less than 5 mi, so near the Pole; V's uncertainty is due to the roughness of solar compass bearing observations expressed largely in quarter-points of the compass.) *DIO 2.2* also finds that Amundsen approached the Pole nearly parallel to the 164°W meridian. At the Pole, he found that the compass pointed along about 18°W.

with Amundsen's own 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-lampooned navigation is utterly vindicated by the new findings, since (though both explorers would have been better served by cartesian navigation near the Pole), he and Amundsen used sph trig equations of precisely the same form and computational difficulty.[14]

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

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

[b] DR finds that Royal Geogr Soc Pres A.Hinks (whose views are uncomprehendingly promoted by the originators of the idea that Amundsen ignored transverse shots), in his learned 1944 article[15] on the Amundsen-Scott 1911-1912 S.Pole data, states at p.169 that the Norwegian edition displays "facsimile reproductions of observation books".

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

C The Jekyll&Hide Defense: I Say, What's Astronomical HISTORY Doing in the Journal for the History of Astronomy?

C1 In *J.HA 1.2* & *DIO 1.3*, DR's "Muffia Orbituary" extensively admires the pioneering work of 1991 lead papers in the *Journal for the History of Astronomy* and *Isis*, who are: [i] lodging (& understandably promoting the originator of) Hist.sci's unprecedented discovery of the WINTER EQUINOX[16] — as well as [ii] rewriting the canons of gradeschool arithmetic,[17] in order to promote certain precious Hist.sci tenets.

[a] The prominent *JHA-Isis* articles cited contend that none of three surviving Hipparchos trios of 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 orbits[18] — and noted that one of these Nonexistent orbits (satisfying the Hipparchos solar 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 Unsolvable 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.HA* — 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 . . . .)

[b] The Ancient Star Catalog of 1025 celestial objects was compiled (*Almajest* 7.5-8.1) by mathematician-astrologer C.Ptolemy (for epoch 137 AD), reported by him as if based on his own observations, though it has been knowledgeably suspected for centuries that Ptolemy stole virtually[20] the whole Catalog from Hipparchos (128 BC).

C2 The Catalog issue became central when: [a] in the 1976/8/6 *Science*, loyalist 0 Gingerich unconvincingly tried explaining-away Ptolemy's solar, lunar, & planetary fudgings 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 fouled-up analyses (still blissfully accepted by Muffia capos as perfectly valid . . .) turned out by the Neugebauer-Muffia's two leading purported experts on the ongoing Catalog controversy: J.Evans & G.Graßhoff, who have by now wasted hundreds of handsome Muffia-publication pages, fruitlessly defense-lawyering Ptolemy against a passel of persistent proofs[21] of his theft. Note: since the shellshocked Muffia has lately begun uncertainly admitting that maybe some of the Catalog was taken from Hipparchos after all, Muffiosi now usually avoid telling their readers that Ptolemy insists he personally[22] observed the whole Catalog outdoors with his alleged armillary astrolabe.

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[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 observationsbook), Mohn 1915 gave (verbally) Amundsen's computational procedure (*DIO 2.2* ‡5 fn 17).

[15] *Geographical Journal 103*:160 (1944/4).

[16] *JHA 22*:101 (1991/5) p.119. Not every journal can boast of refereeing which comes up to the *JHA*'s rigorous standards. *Isis*' 1991/9 paper is squarely based upon the prior *JHA* 1991/5 delight.

[17] Readers who possess the advantage of an elementary school education may wish to check the arithmetic found in the gov't-funded Muffia paper selected as lead article for the 1991/5 issue of the extremely handsome *Journal for the History of Astronomy*, whose highest-ranking Ptolemy defender is *JHA* co-Editor and Harvard Hist.sci Dep't head 0 Gingerich. (And the Muffia author's followup paper led off the proud first Univ Chicago issue of the History of Science Society's *Isis*.) At p.117 of the *JHA* paper's development, we learn: [a] $128°35' - 65°30' = 65°05'$, and [b] the solar mean anomaly (increasing at Hipparchos' $0°.985635$/day) changes by $67°2/3$ in 67 2/3 days! These adventures in Muffia New-Math are at the very core of the Muffia's attempt to prove that the DR-solved Hipparchan solar trios cannot be solved by Greek trig methods. But this is the *JHA*; so, expect no *DIO*-citing or *DIO*-quoting retraction — despite the aim-to-please publisher's-statement offer appearing at the end of each *DIO* issue.

[18] E.g., the EH orbit (founded upon Hipparchos' earliest adopted solar cardinal points), epoch Phil 1: $Y = 365^{d}1/4$; $\epsilon = 228°$; $A = 44°$; $e = 3^{p}1/4$. 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^{y} of attempts at explanation (from Ptolemy through Muffia capo G.Toomer): (327 2/3)/3144 and (247 1/2)/(3122 1/2). In *DIO 1.3*, it is discovered 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 *pre*sumed lunar elements: mean-anomaly-at-epoch = 82° and mean-longitude-at-epoch = Ptolemy's 178°. (Both values probably from Aristarchos.) [b] The trio A Moon distance is based on Aristarchos' famous 3° value for the half-Moon's angular distance from quadrature, combined with his school's use of an Astronomical Unit of 1000 parts, so that $r_M = 1000^{a}\tan 3° = 52^{a}24' = 3144'$; trio B's distance is merely this same value, affected by one of the most common ancient scribal slips, $r_M = 52^{a}1/24 = 3122'1/2$. Further *DIO 1.3* analysis is based upon Swerdlow's solid and original 1968-9 discovery of another Hipparchos value, $r_S = 490$ Earth-radii. This DR article also begins *DIO*'s demonstration that all 3 of the astronomical distance estimates (surviving via pseudo-Aristarchos, Archimedes, & Poseidonios) — including the famous half-Moon experiment — of the school of Aristarchos (who wrote on light & vision) are based upon his correctly setting the limit of raw human ocular discernment at 1/10000 radians.

[19] *Hist Sci Soc Newsletter* (1991/7) p.35.

[20] An upcoming *DIO* analysis will show that all 5 of those exceptional Catalog stars, whose longitudes end in 15′, were fudge-tailored by Ptolemy 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°40′ to Hipparchos' longitudes.) The 5 stars (& conjunctions): 43ζ Gem (Venus 140/7/29-30), 15η Vir (Venus −271/10/11-12), 76h Vir (Moon −134/3/20-21), 49 Vir (Moon −719/3/8-9), 29 Psc (Moon 90/8/28-29). Three of these cases are attested (*Almajest* 10.1&4, Neugebauer *HAMA* 1975 p.295 n.23), and the 720 BC eclipse is a Ptolemy favorite (*Almajest* 4.6-9, 6.9).

[21] A list of proofs against Ptolemy's authorship of the Catalog is provided at p.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 at his p.181.

[22] 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.

C4 Now, at *JHA 23.3*:173-183 (1992/8), comes forth our favorite Muffia comedian-entertainer-satirist, Noel Coward[23] Swerdlow (Univ Chicago Dep't Astron, & Advisory Editor for the extremely handsome *JHA*), to pronounce his judicious[24] quietus upon the Catalog controversy, in a pseudo-delayed paper called "The Enigma of Ptolemy's Catalogue of Stars". Indeed, NCS thinks his remarks are so smart and valuable that they should be the last word on the subject! (See NCS 1992 p.182.) And he calls *skeptics* demented?

C5 NCS still hides from debating skeptics. But Mr.Hide is also Dr.Jekyll. Evidently sobered by *DIO 1.1*, NCS has muted his previous abusive style (e.g., calling dissenters' work "crazy", "garbage", etc: fn 23). Now, donning (publicly) the mask of Jekyllian civility, he even poses as an arbiter of academic etiquette! (Humor-wise, that's on the level of appointing Nixon to clean up dirty political campaigning — or placing *DIO* in charge of good taste & style in academe.) Who else but the incomparable Lord Hoskin-0 Gingerich *JHA* Editorship would select NCS (whose Hyde-side applied to the late RN the unretracted & *Hist.sci-uncriticized* libels "liar", "crank", & "con-man": *DIO 1.1* ‡3 §D) as the ideal *JHA* AdvEd to call RN & DR insufficiently polite?! (See it happen, at NCS 1992 p.176.)

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

C7 Skeptics, starting with Tycho Brahe (who also faked a bit of his own justly famous 1598/1/2 star catalog),[25] have always suspected that Ptolemy observed no stars but instead copied Hipparchos' star catalog, merely adding $2°40'$ of precession (mistaken by $-1°.1$) onto Hipparchos' longitudes. In his Muffia-hated 1977 Johns Hopkins Univ book, *The Crime of Claudius Ptolemy*, physicist R.Newton revealed a startling fact: though the unaltered star latitudes exhibit the expected excess of $0'$ endings, the longitudes' most common ending is $40'$ — exactly what one would expect if an indoor astrologer had added $2°40'$ onto Hipparchos longitude data originally having the expected excess of $0'$ endings.

C8 NCS starts his attack on the famous fraction-endings proof with the astonishing claim that RN's argument had assumed (NCS 1992 p.176) "an instrument graduated to half-degrees". This statement is false. (Expect no *DIO*-citing retraction.) And it suggests why NCS has such passionate sympathy for the exposed plagiarist C.Ptolemy. The NCS misstatement that RN assumed half-degree division is identical to the same error originated by G.Graßhoff *History of Ptolemy's Star Catalogue* Springer-Verlag 1990 pp.85, 88 (n.172!), & 162 — a book which NCS (1992 p.180) deems "very acute" (a description NCS does not apply to any analysis by RN or DR). The truth is given at RN's *Crime* p.247, which argues from evidence that "the circle was probably graduated only in degrees, rather than in intervals of $30'$." (Graßhoff read only the RN statements[26] on pp.246 & 252, missing those on pp.247 & 255.) Throughout the long history of this controversy, no one else (besides Graßhoff & NCS) has ever made such an error. (Not even a scholar of 0 Gingerich's intelligence & reliability.) For anyone who's at home with the English language (& one must mercifully note that Graßhoff is German), it is impossible to read RN's discussion and misunderstand the point — *central to RN's entire analysis* — that RN is proposing whole-degree graduation of the astrolabe's longitude and latitude circles.

C9 Nonetheless, the section of NCS's paper containing his amazing, unoriginal false allegation (that RN assumes half-degree graduation) *is cited*[27] *to RN's book, not Graßhoff's.* This must be what 0 Gingerich has in mind when he speaks fawningly of NCS' "meticulous scholarship" (*JHA 23.2*:149, 1992). In an equally "meticulous" 1979 book review — appearing in the journal of Phi Beta Kappa! — NCS falsely imagined he had detected RN's use of an uncited source. So he of course sneeringly imputed dishonest motives to RN. (See NCS at *American Scholar*[28] 1979 p.528. The elementary fatal error underlying NCS' assault is exposed at *DIO 1.1* ‡6 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 exquisite longterm serial-embarrassment not only enhances (in the Muffia's special way) the reputation of Phi Beta Kappa, but even more so the *Journal for the History of Astronomy*, where NCS is a proudly-displayed & perfectly-apt Advisory Editor — and where his equally strange review of another of RN's Johns Hopkins University books appeared, a review which showed (as an incredulous RN demonstrated in *DIO 1.1* ‡5) that NCS did not even understand the purpose or the very *title* of the book. . . . (I don't believe that NCS has yet, in his standard genteel fashion, attacked some poor unoffending nonMuffia scholar for fake book reviewing. But we NCS fans are all looking forward to the day when he does.)

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

[23] It should be clearly understood that *DIO*'s nickname for Swerdlow is strictly based upon his Noel-Cowardesque talents as humorist — talents displayed, e.g., in *Isis* & *JHA* (sampled in *DIO 1.1*). His courage is unquestioned, since he and his Muffia friends have been brave enough to call Ptolemy-skeptics loony and incompetent for 20^y, while avoiding face-to-face public debate: *DIO 1.1* ‡1 §C7 & ‡3 §D3.

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

[25] *DIO 2.1* ‡4 shows that Tycho's largely magnificent epoch-1601.03 catalog of 1004 stars (hitherto neither numbered nor even counted by Hist.sci!) contains 10 faked stars: the first 6 stars of Oph, Tycho's stars D675-680 (entirely invented); and all 4 stars in Cen, Tycho's stars D1001-1004. (The Cen set was computed from fake longitudes & real declinations — the latter probably observed at Wandsbeck, not Hven.) Tycho's method of fabrication was essentially the same one, which the Tycho catalog's preface accuses Ptolemy of "usurpation" for using: just add a precession constant onto the longitudes of a predecessor's star catalog, while not changing the latitudes. (Tycho: for Oph, add $24°$ to Hipparchos' longitudes; for Cen, add $21°$ to Ptolemy's longitudes.) Then, unlike heedless Ptolemy (whose data were invariably rounded to $5'$ or $10'$), Tycho tossed in a very few arcmin of scatter, so that the "observations" would look real. (Tycho always rounded to arcmin or simple fractions thereof.) These 10 stars' errors are gross by Tycho's standards but they agree very closely with fabrication — and they are from the sole 2 subsets of his 227 final-rush 1596-7 stars for which no underlying data survive. Curiously, Evans' 1987 *JHA* paper believed (p.168) that the 4 Cen stars were real, because he neglected to apply to them this same weighty *JHA* paper's own laboriously-developed extinction formula: Evans 1987 pp.259-260, 267-271. Use of Evans' formula produces magnitude 7.95 for 2g Cen at Tycho's Danish observatory! As further shown at *DIO 2.1* ‡4 fn 65, the massive Evans-*JHA* attack on DR also includes (Evans 1987 p.168) claims which require naked eye observations of stars as dim as *tenth magnitude*, by Evans' own formula. Well, 10th magnitude may be dim; but, if we were to assign a magnitude to the brilliance of refereeing at the extremely handsome *JHA*, could a mere 2 digits do it justice?

[26] RN 1977 starts his argument by looking about for an explanation of the fractional distribution of the latitudes; in this spirit, merely for the sake of testing, he momentarily hypothesizes (p.246, emph added): "*Suppose* that a circle is graduated at intervals of $30'$ . . . ." However, even before the next page is finished, RN has discarded the half-degree hypothesis (due to $0'$ endings outnumbering $30'$ endings in the latitudes) and repeatedly states subsequently that his analysis assumes whole-degree division.

[27] NCS 1992 n.14.

[28] Just as 0 Gingerich was a top Editor at the *JHA*, when it published NCS' insulting 1981 attacks on RN (sampled at *DIO 1.1* ‡1 §C7); so also he was on the Editorial Board of *American Scholar* when it published NCS' 1979 diatribe. 0 pretends to abhorrence of "abusive" writing — but does 0 seriously believe that merely using others to carry out his intents will successfully divert intelligent onlookers from recognizing the midlevel archon responsible for the low nature of the gang attack on RN's findings? 0 is certainly not fooling DR, who knows from a private 1974/11/15 meeting with 0 (at Goddard) that, even that far back, 0 was personally acting as a Muffia agent — verbally diffusing the Muffia slander that RN, an internationally respected physicist & a section chief at the Johns Hopkins University Applied Physics Lab, was simply a crank. (This rumor was spread so aggressively by Muffia&clonies, behind RN's back, that it was accepted for awhile even at *Scientific American*, from whose people I heard it in 1978-9. Why shouldn't Muffiosi duck open debate, if academe lets them get away with fighting opponents this way instead?)

[29] *Publ Astr Soc Pacific 94*:359; 1982/4.

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[30] error-waves throughout the Catalog: $\Delta\lambda = 29'\tan\beta\sin\lambda$, $\Delta\beta = 29'\cos\lambda$. 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° of the ecliptic and [would] produce nonsense for stars within 25° of the poles." Hilariously false on both counts.[31] (By its account, the *JHA* had *a year* to referee NCS 1992: fn 59.)

C11 Having alibied 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 $\Delta\beta = 20'\cos(\lambda + 35^\circ)$." (This equals $16'\cos\lambda - 11'\sin\lambda$.) DR's comments:

[a] Peters has provided the Catalog's standard latitude error curve via 36 normals, spaced at 10° 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 obvious that the (misleadingly asymmetric) peaks of the Peters $\Delta\beta$ curve are precisely 40° from the equinoxes, not 35°. (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′.) No one consulting the original Peters curve could possibly get this wrong. (Indeed, I don't see how anyone could get 35° even from the Peters-curves published later by DR 1982C p.366 & Evans 1987 p.252. Readers who wish to measure raw Muffia prejudice at work ought to check them or Peters' original graph — in order to appreciate fully the NCS mental effort

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

[31] Regarding the first NCS claim: basic amplitude = $1^\circ.1\cdot\tan\epsilon = 29'$ (where ϵ = obliquity), so for $\beta = 25^\circ$, the wave $\Delta\lambda = 29'\tan 25^\circ\sin\lambda = 14'\sin\lambda$, which (with gt-circ amplitude 12′) is far from undetectable in this context, as the merest glance at the wellknown Peters error curves will make clear. (At least NCS does not go as far as Graßhoff, who, at p.167 of his 1990 Springer-Verlag book: [a] confuses Ptolemy's proposed 2°2/3 precession-since-Hipparchos with the real 3°3/4 precession over this timespan, and [b] claims that the waves produced by such a longitude mis-set — of amplitude $3^\circ3/4\cdot\tan\epsilon$ = c.100′ ! — constitute "a small periodical error . . . . so small for both coordinates [λ & β] 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°. It doesn't get any better. What BrownU talent-scout did Springer-Verlag employ to locate such Muffia expertise?) As for the latter NCS remark: it is based upon National Science Foundation-grantee Swerdlow's untutored impression that, since $\tan\beta$ of course approaches infinity as β tends to 90°, then the above $\Delta\lambda$ wave expression will become impossible to deal with, as one nears an ecliptic pole; but anyone of experience in this work knows that the real error here is great-circle measure (see the weighting discussion at DR 1982C p.366) — and for this we have $[\cos\beta]\Delta\lambda = 29'\sin\beta\sin\lambda$, which merely goes to $29'\sin\lambda$ 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 I may offer a slightly shocking suggestion to the *JHA*: could it try to find reviewers who are capable of performing the math they are purporting to analyse? Is that really too much to ask? Well, in a Hist.sci community that takes Muffiosi seriously, maybe it is.

[32] C.H.F.Peters *Vierteljahrsschrift der Astronomischen Gesellschaft 12*:296-299 & 341 (1877), p.299. The translation at C.Peters & E.Knobel *Ptolemy's Catalogue of Stars* (PK) Carnegie Inst 1915 p.8: "the [C−O] curve of errors in latitude has a maximum near to [λ =] 140°, and a minimum near 320°. (Peters' 1877 graph of the errors $\Delta\lambda$ and $\Delta\beta$ is reproduced on PK p.6.) Peters does not attempt to fit sinusoids to his curves; if he had, he would not have arrived at a latitude curve phase anything like NCS' — since, though the irregular curve happens to exhibit extrema about 40° from the equinoxes, nonetheless, the bulk of each billow of the latitude curve is more nearly 60° from an equinox.

that went into his purely invented 35° phase.) Yet, strangely enough, NCS 1992 n.7 cites[33] Peters-Knobel 1915 (where the original Peters graph, with grid, is reproduced at p.6) as a source for the NCS 1992 paper. But the most uncomfortable question here is: after *over a decade* of silence,[34] while now finally attempting to reply to *THE* most airtight argument[35] ever raised against Ptolemy's authorship of the Catalog, why is NCS — a member of Univ Chicago's Astronomy Dep't (and recipient of NSF & MacArthur-genius grants) — *eyeballing* a sinusoidal fit? Badly. One might expect such an eminent scholar to be capable of calculating his own least-squares fit to the Peters data. Had NCS done so, he would instead have confirmed DR's rigorously computed sinusoid: $\Delta\beta = (18'-)\cos(\lambda + 58^\circ) = 9'\cos\lambda - 15'\sin\lambda$. We recall (§C10) that the latitude effect which Muffiosi need (to salvage Ptolemy) is $29'\cos\lambda$ — so the real cosine coefficient (9′ instead of 29′) falls terribly short of the mark. Note that NCS' erroneous sinusoid replaces the correct[36] cosine-coefficient 9′ by NCS' false 16′, which makes the amplitude appear much nearer the 29′ value needed to save Ptolemy's rep. (Though, even this inflated 16′ value is far short of what "neutral" NCS seeks.) The bottom line: NCS' sinusoid is a falsehood (expect no *DIO*-citing retraction) — born not of mathematical anaysis but of NCS' Ptolemaic eyeballs.[37]

C13 "No one else" has ever gotten anything like the $\Delta\beta$ sinusoid which "meticulous" NCS claims is a valid rough fit to the Peters curve. (Different-context NCS asnide at *JHA* 1981 p.61: RN "knows something about calculating syzygies that no one else knows." Like NCS on van der Waerden at *Isis 64*:239.) R.Newton[38] makes it $15'\cos(\lambda + 60^\circ) = 8'\cos\lambda - 13'\sin\lambda$. And J.Evans, whom NCS rates[39] as "acute" fellow, finds (*JHA* 1987 p.251) $\Delta\beta = (0^\circ.31)\cos(\lambda + 63^\circ) = 8'\cos\lambda - 17'\sin\lambda$, close to DR's result — and even lower in the crucial cosine coefficient, where the Muffia is looking for the desired 29′, not a piddling 8′ or 9′. Evans, not willing to misreport the latitude error curve, instead just pretends the correct one isn't much different from $29'\cos\lambda$, admitting only (*idem*) "the phase is not exactly right" — i.e., 63° of phase difference is merely "not exactly right"! But, one cannot play with the phase. The cosine & sine terms are independent. And 8′ or 9′ is a far cry from the required 29′. Thus, Ptolemy is convicted, and the Muffia has lost the Catalog controversy. (Expect no retraction — *DIO*-citing or otherwise.)

C14 But, perhaps the funniest part of this travesty has not yet been discussed. Even while F. Lee Evans & NCS conjure up amazing *speculations* in order to depress the required 29′ amplitude (e.g., NCS 1992 p.177) and try to [a] inflate the actual error-curve's 18′ amplitude & [b] shift its Muffia-offending phase, they have inexplicably ignored a huge effect: an 11′ wave which is (by their hypothesis[40] that Ptolemy observed the Catalog) entirely *nonspeculative*, namely, the wave due to the Cataloger's error in his adopted obliquity. Ptolemy's obliquity was unquestionably the Eratosthenes value, 23°51′20″ (e.g., *Almajest* 1.12&15), but the correct obliquity in his day was 23°40′.7. This +11′ error would introduce into the stars a latitude error wave $\Delta\beta = -11'\sin\lambda$, which — at a stroke — accounts for roughly *half of the amplitude found by all parties*. Subtracting out this effect (in

[33] Isn't this just the sort of bibliographical offense which NCS used to condemn as dishonest (see *DIO 1.1* ‡6 fn 6), when he imagined RN had made such a revealing slip?

[34] After two decades of privately calling RN&DR nuts (which caused no public concern among the Hist.sci archons who honor NCS) and after refusing for over ten years to reply to the ironclad DR error-waves argument (snobbery which also caused no public concern among the Hist.sci archons who honor NCS), NCS has now been shown to lack an honest rejoinder to the toughest argument of those skeptics he has been slandering — a situation which continues to cause no public concern among the Hist.sci archons who continue to honor NCS.

[35] Sent to the *JHA* in 1976.

[36] If Ptolemy observed the Catalog, then $|\Delta\beta|$ should equal about 29′ near the equinoxes. But the Peters curve's mean equinoctial absolute magnitude is slightly less than 10′ — in almost perfect agreement with DR's 9′. This is perhaps a superficial point, but it should at least have given pause to those attempting to pretend that the Peters curve suggests the presence of anything like $29'\cos\lambda$ in the latitude residuals.

[37] On the eyeballs of NCS' ancient mentor: see DR at *Vistas in Astronomy 28*:255 (1985) p.266.

[38] *QJRAS 20*:383, 1979, p.390.

[39] Also "judicious": NCS 1992 p.177.

[40] Yes, it's deliberate.

order to find out what errors still need explaining)[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[42] is removed from the actual (Peters) latitude errors, the best-fit sinusoid is merely $\Delta\beta = 9'\cos\lambda - 4'\sin\lambda = 10'\cos(\lambda + 25°)$; this $10'$ amplitude is catastrophically far below the $29'$ amplitude that must exist if Ptolemy observed the Catalog.

C15 Since Muffiosi 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-§C14 amplitude needs Muffia-massaging, since $18' \neq 29'$.) But, if we wish to unloose the alibi-power of preconception, there is no reason to limit the fun to Ptolemy. So I suggest that the *JHA* set these same charmingly programmed Muffia myopes upon the task of fiddling phase and amplitude of the effects observed by, say, Bradley & Bessel. When the sand-in-the-eyes settles, we'll find that Bradley discovered stellar parallax & Bessel beat Chandler to his Wobble . . . .

C16 A final note on the Peters graph of the actual zodiacal latitude residuals: NCS refers (§C11) to the "complex" shape of its curve. I.e., the curve's 2nd major peak (centered on λ = c.300°) 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.[43] The solution will be published in an upcoming number of *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 *DIO* subscription. (Just what every Muffie dreams of finding under his Christmas Tree . . . .) Hopefully more attractive: a published note of admiration (acknowledging a Muffia share in this provocative discovery), to appear in the first 1993 issue of *DIO*. [Note added for 1995 reprint: Solution printed at *DIO 1.2* (©1993) fn 152.]

C17 Delambre (1817) and DR (1982C) emphasized the total absence of stars from the c.5° band of southern sky which is visible from Ptolemy's Alexandria (latitude L = 31°12′) but invisible from Hipparchos' Rhodos (L = 36°). So NCS uncritically follows the alibi of Evans (1987 p.166) and says (NCS 1992 p.176-177): "the object of Ptolemy's catalogue was surely to list the stars in and near recognized constellations, and since these were formed around the Aegean there was no reason to include additional stars not then included within constellations even though visible near the southern horizon in Alexandria." DR's comments: [a] Ptolemy's version of the Catalog (*Almajest* 7.5-8.1) contains dozens of stars explicitly labelled by him as "unformed" stars not belonging (though loosely attachable) to the traditional constellations. [b] While most of the constellations Ptolemy adopts 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

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 rings 5&3. (See *DIO 1.1* ‡6 §G.)

42 For Hipparchos, the real obliquity was 23°42′.7, so (assuming *Almajest* 1.12 is correct in saying that Hipparchos also used Eratosthenes' obliquity, 23°51′20″), his obliquity-related error wave would be $-9'\sin\lambda$. 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 $23°56' \pm 1'$ (DR 1982C eq.27).

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

have applied to the individual stars as parts of the constellations are not in every case the same as our predecessors" (*Almajest* 7.4, G.Toomer *1984 Almajest* p.340). [c] Ptolemy was unquestionably willing to adopt a brand-new asterism, since the *Almajest* is the first extant work to recognize the "Antinous" section of Aquila (*Almajest* 7.5, Toomer 1984 p.357), named in honor of Hadrian's boyfriend, who had died[44] 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 *unqualifiedly* "an astronomer and mathematician of the greatest ORIGINALITY and the greatest depth" (NCS 1992 p.181, caps added). If *DIO* readers happen to know any bright young chameleons seeking a useful education, do urge them to attend Swerdlow's University of Chicago. He could teach them a few tricks.

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?[45] More curious yet: while NCS pronounces his purely faith-based judgement-*certain* (§C17) on Ptolemy's genius, NCS also says on the very same page (NCS 1992 p.181) that — despite 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 rôle of reason even in discourse — much less in the evolution of accepted ideas in its field. Such dementia is inevitable wherever cult-status has higher priority than evidence and truth.)

C19 Despite Ptolemy's vaunted originality, all his modern flacks (including even NCS 1992 pp.180-181 & Evans at *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 language — as if the stars did the walking.) It's fun watching defenders think up Nice 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" (§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 Muffiosi to this feeble last ditch). Thus has an unsubtly transparent intellectual inertia gradually sucked defenders ever-deeper into a hodgepodge of ad-hoc exercises in special-pleading (for each separate suspicious Catalog circumstance: §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-

44 To quote from DR's *Queen's 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, an infamously licentious town which was an ancient combination-in-one of Hollywood, Lourdes, and Las Vegas. The ultimate enshrinement 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 Inscr* 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 in . . . 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.)"

45 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 MUFFIOSI, until recent RN-DR-proposed crucial-testing cornered them), and [b] is more wildly comic[46] than the surreal sobriety-test fantasy in the cinema-farce *The Man With Two Brains*. (Hollywood screenwriters have to use drugs to get this high. How does the Muffia do it?)

C22 Perhaps we can attain some perspective on the Catalog matter by simply listing the features we would expect to find in the Catalog if it "came from" Hipparchos. [Test's first proposer: 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 β [DR]. (Such an error is roughly entailed by Ptolemy's false assumed geographical latitude[48] for Alexandria, $L = 30°58' = \text{atn}[3/5]$, which he swiped from Vitruvius' crude, 2-century-old equinoctial ratio, shadowlength:gnomonheight = 3:5. See DR at *Vistas in Astronomy* 1985 p.267 n.6 and at *Amer J Physics* 1987 p.236 & n.15. Alexandria's actual L is $31°12'$N.)

[g] No stars in the c.5° band of sky visible from Alexandria but not from Rhodos [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 Muffia, innocently heedless of all 5 of these tests, had publicly & totally committed its reputation for expertise to faith in Ptolemy's greatness & originality (and his critics' idiocy). As new test after new test came out against Ptolemy, Muffiosi stuck to their party line: we expert archons have learned nothing from RN&DR. And they'll die stuck to that same unalterable principle.

C24 Even for the tiny but indicative sample of stars where Ptolemy copies errors of *several degrees*[50] from Hipparchos, NCS still isn't finally convinced. NCS 1992: "a few stars may have come from Hipparchos" (§C19), "but I think *this part of the analysis* should be carried further" (p.180, emph added). Note the remarkable coincidence that: the only evidence (bearing on whether the Catalog was stolen), which NCS desires Further-Research into, is that which (even he thinks) looks bad for Ptolemy as things now sit! This tactic is a faithful repeat of what DR found long ago was standard among the very Velikovskians[51]

[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 quarry with ring 2, we read where ring 2 meets ring 3 AND THEN ADD BACK, ONTO THIS READING, THE AMOUNT WE JUST AS NEEDLESSLY SUBTRACTED OFF IN THE FIRST PLACE. . . . Got it? (Evans at *JHA* 1987 p.243 — including his conveniently thesis-aiding datum-misreportage [$303°03'$ rendered as $303°05'$], in the NCS tradition admired at *DIO 1.1* ‡5 fn 7.) Can RN-DR be accused of cruelty to dumb animals, given the tightness of the evidential vise they've closed on the poor Muffia? To watch prominent scholars thrashing about in such pathetic credibility-death agonies is akin to viewing Animal-Rights films of stoats caught in spring traps. Trying to weasel out.

[47] It is seldom noted that this contrast (182 stars with $10'$ endings vs. merely 88 stars with $30'$ endings) is even more overwhelming than item [d] (246 with $40'$ endings vs. 226 with $0'$ endings). The totals for all endings are given at R.Newton *Crime* 1977 p.245, followed by his brilliant & pioneering 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-dozen *Almajest* 7.3 star declinations (null mean error) are also stolen, though Ptolemy naturally presents them as results of his own outdoor observations.

[49] First broached by the eminent astronomer J.Delambre in his 1817 *Hist Astron 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) Muffiosi cannot tear down the math, they must try alibiing in other fashions: see above at §C17.

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

[51] See *DIO 1.1* ‡3 §D2; also §B2 of DR's 1972-4 paper, "Freudian Astronomy, or: Do Planetary Orbits, Bristlecone Pines, & Velikovsky's Believers Suffer from Collective Amnesia", published in 1990 in the anti-kook newsletter of Leroy Ellenberger, 3929 Utah Street, St.Louis, MO 63116, phone 314-773-0329.

NCS compares RN to. Recall also the comment at *DIO 1.1* ‡2 fn 7 (which might almost have inspired NCS 1992): "Parapsychologists, UFOlogists, & Ptolemists . . . prefer unending data-collection, thereby [ducking] the shame of having pursued & promoted a false path for decades." NCS' conclusion: "After more than a century of serious [read: Muffia] and not-so-serious [read: RN-DR] research into Ptolemy's catalogue of stars, the fundamental question of its originality remains unanswered" (NCS 1992 p.182). Or, as another humorist is about to express it for us (§C25): research on the Catalog is back to Square-One.[52]

C25 The overwhelming array of evidence against Ptolemy ensures that skepticism on the Catalog will continue, so the loyal Muffia will stand ever vigilant to defend its weirdo hero, and — as part of that effort — Muffiosi will keep right on pretending (fn 24) to impartial scientific curiosity on the Catalog issue. The spectacle of the Ptolemy lobby struggling with the Ancient Star Catalog pseudo-enigma reminds me of another farce, Dave Barry[53] on the tobacco lobby:

> It's time somebody spoke up for the troubled US cigarette industry . . . . [and] the fine research being done at the famous Tobacco Institute, which is staffed by leading tobacco-industry scientists using sophisticated equipment and wearing state-of-the-art leashes. These scientists have been researching for years, but they are darned if they can find any solid evidence that smoking is bad for you. Although naturally they are continuing to look just as hard as they can:
>
> FIRST SCIENTIST: Well, Ted, for the 13,758th consecutive experiment, all of the cigarette-smoking rats developed cancer! What do you make of it?
>
> SECOND SCIENTIST: Beats me, Bob!
>
> FIRST SCIENTIST: It's a puzzle, all right. Hey, look at this: These rats have arranged their food pellets to form the words "CIGARETTES CAUSE CANCER, YOU ZITBRAINS." What could this possibly mean?
>
> SECOND SCIENTIST: I'm totally stumped, Bob! Back to square one!
>
> THIRD SCIENTIST (entering the room): Hey, can you two guys lend me a hand? 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 Muffia apologia. NCS 1992 (p.175) adopts the excuse of Laplace[55] & Gingerich (*Science* 1976/8/6) for Ptolemy's $-1°.1$ mean Catalog error: maybe it's just caused by the similar mean error in Ptolemy's solar theory. DR's comments:

[a] This argument directly inspired[56] the DR 1982C absent-error-waves test, which definitively refuted the Laplace-Gingerich alibi. (Rawlins 1982C, eventually published by a real science journal, was originally submitted to *JHA* Editor-for-Life [EfL] Lord Hoskin in 1976 & 1977. His Lordship refused even to referee it. The *JHA* has now spent years — consuming scores of its extremely handsome pages — trying to justify its original 1976-7 mistake by vainly attacking this DR paper, using pseudoscience packaged as scholarship.)

[52] Muffia strategy at this desperate juncture resembles the tobacco lobby's primarily in that there is finally no longer any hope of proving Ptolemy or cigarettes innocent — no, the approach now is simply: obfuscate (to keep the money rolling in) enough to maintain the sham of a continuing "controversy", in order to pretend that one hasn't lost. National Geographic long used the same damage-control ploy to protect its Peary N.Pole lie. And TV networkdom's tactic regarding the patently deleterious effect of TV violence on young viewers is similar: nothing-has-been-proved — so let's just keep on profitably peddling sadism to kids.

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

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

[55] Laplace was himself a notorious nonciting adopter of others' work. See, e.g., G.Airy *Report. . .* [BAAS 1831-1832] London 1833.

[56] Noted at DR 1982C p.359.

[b] NCS' preferred vision of Ptolemy is of a scientist who spent years observing 1000+ stars outdoors with his astrolabe — yet never, during all this time, did NCS' ancient precursor-in-geniusdom manage to notice that his observatory's latitude L was off[57] by $-14'$ — an amount virtually equal to the solar semidiameter. Nor did Ptolemy ever realize (during at least 8 years of alleged solar observations, 132-140 AD: *Almajest* 3.1&7) that the real Sun's position differed from his tables by $-1^\circ.1$! 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 awful as an ancient astrologer's asymmetric 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 Muffia, 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*[58] *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 ordmag $1'$-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,[59] the *JHA* decrees it would be best to just end the Catalog controversy right here. Megalomania rarely achieves such heights of unreality.

C28 Unrealer yet: NCS unreels a proposal for more "research" (§C24) — even *while* calling for his moratorium. (It doesn't take a linguist to translate: [a] NCS wants a moratorium on the chaos of conflicting Muffia claims — which he is now himself so brilliantly augmenting! — that has left the Muffia looking about as convincing as Ptolemy. But NCS wants no moratorium on Muffiosi 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 Muffia research on the Star Catalog, *JHA*'s "moratorium" is now in place, in print, as a pre-set official-excuse for rejection of a [believe me, PURELY] hypothetical direct[60] DR submission.) Isn't the *JHA* a treasure?

C29 After 5 years & dozens of pages of failed *JHA* attacks on RN-DR's Star Catalog analyses, the *JHA* is now suddenly struck — like St.Paul on the Damascus road — with a New Awareness of The-Meaning-of-It-All. NCS 1992 (p.182): "life is too short to waste on questions that cannot be answered." Especially a silly nothing like: did the Muffia'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 retort[61] — that its ever-so-clever strategists got tangled up in such almost-artistically disjunct babbling. Who but our peerless Muffia 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 Muffia 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 Muffia Memory Hole.)

[b] In 1981, 0 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] Graßhoff 1990 ("edited" by Muffia capo G.Toomer)[62] concluded that much of the Catalog was based on Hipparchos' observations, after all. (RN-DR had long asserted this, but watch Graßhoff 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.

So, the Muffia line on whether Ptolemy stole lots of the Catalog: [a] 1974 no, [b] 1981 yes, [c] 1987 no, [d] 1990 yes, [e] 1992 no-yes-but-either-way-we're-still-right.

C32 Is this a community of scholars honestly seeking a credible, consistent vision of the truth? — or are we instead enjoying: Jekyll&Hyde-go-vaudeville? (Perhaps the reason there seems to be no direction is: the Muffia has let its conscience be its guide.) But there *is* coherence. Indeed, the best part of the show is that the foregoing seemingly inconsistent positions [a]-[e] have one glorious factor in common: *all five of these analyses are as one in swearing that Ptolemy was wise and honest*. (You might think there is a wee difficulty here — like, maybe you suppose that there is something slightly dishonest about swiping, without acknowledgement, the labor of an astronomer who observed 1000+ stars. If you believe this, you will never make the grade in Hist.sci. Happily, Harvard Hist.sci Dep't head 0 Gingerich 1981 p.43 & Graßhoff 1990 p.215 will set your ethics straight for you.)

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

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

D1 An occasional nervous-neophyte 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 genuinely expert original scholarship. (As *DIO* readers know all too well.) So, we can relax. (Likewise, professional astrologers' amusing inability[63] to compute horoscopes correctly has no effect at all upon their clients' generosity.) Further: by this time, so many Hist.sci archons' reputations have been invested into the glorification of Muffia scholarship, that the cult cannot be *permitted* to be seen as having erred catastrophically in anathematizing RN-DR. Therefore, our favorite Unsinkable cult will positively insist on

[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: "*Editorial 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 Muffiosi have been damning skeptics for decades as incompetents, one would expect pages of examples of the purported incompetencies to be forthcoming. Muffiosi's occasional efforts to expose alleged errors by skeptics have been so pathetically thin that it is by now all too clear that the Muffia klan has simply been bluffing in this regard. (Note the feebleness of Muffia attempts in this direction: *DIO 1.3* fn 252.) See *DIO 1.1* ‡5 fn 6 for Hist.sci (including Muffia) precedents for publishing lengthy error-lists to attack authors.

[62] Toomer was being convinced by Graßhoff at least as early as 1986.

[63] Rawlins *Skeptical Inquirer* (*SkInq*) *2.1*:62 (1977) pp.73, 76-77; Rawlins 1984A pp.974-976.

keeping its course and will slide right past *DIO*'s iceberg. With barely a sound or a shudder. On its part, the Muffia must wonder why, despite years of archonal conspiring to ostracize RN-DR's heresy, the hated[64] heterodoxy persists nonetheless. (Even Time-Life's popular *Hoaxes & Deceptions* p.108 accepts that the Rawlins 1982C analysis, of the Ancient Star Catalog's southern boundary, indicates that this "Ptolemy" Catalog was actually observed in Hipparchos' Rhodos, not Ptolemy's Alexandria. See §C17.)

D2 The DR-Muffia double-tarbaby-fracas will continue indefinitely, because: [a] DR positively won't stop publicly admiring Muffia gyrations, so long as the Muffia insists on its snobbish & effectively censorial minimum-citation-practice, based upon its equally ludicrous WE're-the-only-experts-around-here pose. (I.e., DR is asking that the Muffia acquire some fundamental ethics and integrity. But who's going to fund the brain transplants?) [b] Muffia mout'pieces are irrevocably committed to forever clinging to their precious pretense that DR's historical scholarship is utterly worthless.[65] This point is so sacred to Muffiosi that, in order to maintain it, the Muffia will pay any price (primarily: internal rot) — and, in order to cloak its ineptitude with the trappings of Reputability, will woo into its muck just as many major academic institutions as it is able to con into sharing that price. (Terseness borrowed-with-credit from etiquette-authority NCS' §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 classiest — tell the goriest lawyer-jokes. It pays to advertise?)[66] Also, my mother's father was a prominent Maryland lawyer. And she married my friend, advisor, & stepfather, John W. Avirett 2nd — widely known as one of the very finest & most respected lawyers in the United States. So, as a member of a family of lawyers, DR is delighted to contribute here an original *DIO* creation: *the* lawyer-joke-to-crown-all-lawyer-jokes.[67] Ready?

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

[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 (UVa, Charlottesville), B.van der Waerden (U.Zürich), Curtis Wilson (St.Johns, Annapolis). (Also the late R.Newton of Johns Hopkins & W.Hartner of U.Frankfurt, Germany.) Each has published or supported the publication of DR science-history researches. Thus, Muffiosi's 100% rejection of these papers implicitly accuses each of these institutions & scholars of incompetency.

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

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

[68] 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 but seal once to a thing, and I was never mine own man since."

‡9 The Neptune Conspiracy

British Astronomy's Post-Discovery Discovery

Summary

Britain's J.Adams is generally believed to be the prior of the 2 pre-discovery locators of Neptune via math analysis of its gravitational disturbance upon Uranus' orbital motion. However, for reasons still vigorously disputed, he published none of his alleged 1845 perturbational mathematics until 7 weeks after Frenchman U.Leverrier's 1846 publications & 9/18 letter had caused the planet's telescopic discovery at Berlin on 1846/9/23. Detailed evidence is presented[1] indicating that, throughout 1846 Summer, Cambridge University astronomers conspired to capture Neptune by keeping Cantab Adams' work unpublished while 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[2] is proposed below, which accounts for a few of the worst of the Neptune affair's hitherto intractable mysteries, and which might (partially) exonerate the legend's prime popular villains.

A Misbehavior & British Gentlemen

A1 Basing his work upon misbehavior in the motion of Uranus, the brilliant & adventurous young Cambridge U mathematician John Couch Adams appears to have in 1845 arrived at a theoretical prediction of the ecliptical position (near Cap-Aqr border) of the giant planet Neptune, then unknown. This is the same jovian planet that the wonderful US 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' rôle in the discovery was actually nil, and his behavior has always been inexplicably murky — a point I will expand upon below, adding a novel, partly speculative hypothesis which entails: [i] a solution-switch by Adams, & [ii] a high official's possibly-conscious back-dating of the controversy's key document. This admittedly uncertain new

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

[2] Unlike my 1973 conclusion on R.Peary's hoax (which was, incidentally, explicitly anti-conspiratorial: *Peary at the North Pole: Fact or Fiction?* 1973 pp.4, 147, 158), the current paper's tertiary hypothesis is not entirely certain. Undeniable: [1] Cambr U 1846 Summer secrecy (Rawlins 1984), & [2] some degree of Adamsian solution-switch (his pretense that MemoC was virtually identical to MemoR: fn 59). (Item [2] is essentially new here: Morton Grosser, author of the standard volume *The Discovery of Neptune*, Harvard U 1962, presumes at his pp.86-89 that the 1845/9 Adams solution was the same as that of 1845/10. Likewise at Grosser's 1970 bio of Adams in *DSB 1* [1970] p.53. I do not blame Grosser: he was simply deceived.) However, I could be mistaken in the tentative suggestion here that [3] Adams' "1845/10" Hyp 1 (MemoR, published 1846/11/13) was actually finalized around the middle of 1846. But I have decided to risk publishing this theory because [a] various circumstantial evidences support it, and [b] all my repeated efforts to disprove it have to date consistently met with failure. (Other scholars may prove more discerning. *DIO*'s Correspondence column invites their criticisms & disconfirming evidence.) I note that O.Eggen (familiar with the "lost" RGO Neptune file), in a 1970 bio of Airy (*DSB 1*), vaguely remarks that Adams "called unannounced to present one of his early predictions" (p.86); and, in a 1971 bio of Challis (*DSB 3*), Eggen just says (p.187): "Adams presented Challis in September 1845 with some predictions as to where [Neptune] might be found."

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[3] regarded as the prime predictive sensation in the history of astronomy. The "Neptune Controversy", which has continued for over a century, centers on several contended questions, most particularly: [a] Should credit for Neptune's discovery go to the Englishman Adams, to the Frenchman Leverrier, or to both? (The last position is fine by Britain, since Adams' work is supposed to pre-date Leverrier's.) [b] Which Brit was primarily responsible for the 1846 Summer secret-sky-search fiasco at the Cambridge Observatory? (The hitherto orthodox answer: Cambr Obs director J.Challis. The present paper rather vindicates Challis.)

A3 *In retrospect*, we see that the Adams 1845 prediction's accuracy was sufficient to effect the planet's discovery. But British astronomy's pre-discovery confidence in it was not sufficient. And, though the Astronomer Royal & a few other leading Britons are routinely condemned for this, a case will be made below that the key person lacking the necessary confidence was Adams himself — partly due to his own astronomical inexperience,[4] and partly due to his correct 1846 perception that he had not in 1845 tested his theoretical planet at more than 1 distance from the Sun (arbitrarily presumed: below fn 5).

A4 The affair's puzzles begin with Adams' supposed private lodging of his *preliminary* computed orbit & position for Neptune, generally known today as "Hypothesis 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 then (temporarily) spoken of by Adams as sufficient, this solution clearly was *not* considered sufficient by Adams himself since [a] he did not publish, and [b] in the months just before discovery, he went at least 2 major mathematical steps beyond it.[5]

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

[4] I have the impression, from Adams' omission of precession in MemoC (§F4) and his roughness in MemoW (fn 19), that he was more of a mathematician than an astronomer. (See §I2.) Whereas Leverrier was by 1846 a highly experienced and swift computer in these areas (see §I7), Adams' investigations have the flavor of a learning experience. I offer these judgements not in criticism (indeed, they suggest that Adams' challenging the Uranus problem was even more creditable than otherwise) but because I believe they help explain Adams' slowness to publish, which relates to the central mystery of the Neptune affair. From a draft of a letter (1847/2/1, 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 U) 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 completion: "the investigation [Hyp 1] could scarcely be considered satisfactory while based on any thing arbitrary; and I therefore determined to repeat the calculation [Hyp 2], making a different hypothesis as to the mean distance [reducing Hyp 1's mean distance by the factor 1/1.03 for Hyp 2]." (That is, the correctness of the first Adams solution's predicted longitude was very lucky — and he himself knew that, which is why he made the statement just quoted, and the inadequacy of this solution is an important cause of his nonpublication of it. I.e., Adams' Hyp 1-based priority-claim is self-confessedly feeble. [We regard Leverrier's prediction as occurring not in just one paper but three.]) So, why is Adams (who unquestionably lodged [1846/9/2] his distance-variation solution later than Leverrier's comparable solution [1846/8/31]) regarded by anyone as the prior discoverer? A related question on another tangent: both Leverrier and Adams failed to get close to the actual mean distance of Neptune (30 AU), replacing the tedious process of repeated rigorous computation of perturbations (for various mean distances) by instead using shortcut schemes, both of which led to serious errors in their final orbits: see §E8. Suppose they had gone ahead with repeated distance-trials toward 30 AU, would their work have run aground on the huge 4000^y Uranus-Neptune 2-1 resonance? A lovely evasion of this problem is that in S.Newcomb's *Orbit of Uranus* 1873 pp.55, 178. Soon after, Adams' 1876 remarks (SP p.63), replying to Peirce's attacks, show he understood the problem then. See Rawlins 1970G. [Also *DIO 7.1* ‡5 §A10.]

A5 Starting the same year[6] as that alleged for Adams' Hyp 1 (1845), the at least equally able French mathematician[7] Urbain J. J. Leverrier independently computed and prominently announced (*Comptes Rendus* 1846/6/1) virtually *the same celestial location*.

A6 Upon reading Leverrier's published paper (1846/6/23 or 6/24), Airy swiftly & secretly set in motion a huge Cambridge Observatory telescopic sky-search (§A8 & §B1) — and, as part of the secrecy, deliberately suppressed news of Adams' confirmatory 1845 British researches (outside a small Cambridge U circle):[8] in his 1846/6/26 letter to Leverrier, Airy *never mentioned Adams' prior work* — this despite the fact that just *one day* previously, in a 6/25 Airy letter (discovered by R.Smith's industry), he mentioned to a Cantab confidant (Wm.Whewell) both Adams' & Leverrier's agreeable planet predictions (on equal terms: see fn 31). Airy then did not respond to Leverrier's 6/28 detailed reply.[9] 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".[10] (Letter of Adam Sedgwick to Airy 1846/12/6, written just minutes after conversing with Adams; Glaisher 1896 p.xxviii & Smart 1947 p.41. Chapman 1988 p.139 n.57 has found that both of *two* copies of the letter are now missing; below at fn 12, I suggest a possible reason for that particular oddity. This is part of a series of Neptune ms disappearances suggesting systematic suppression of documents, a situation encouraging some otherwise unthinkable speculations.)

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

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

[6] When checking priority claims here, one must commend Leverrier for taking time at the outset of his Uranus investigations to check thoroughly the available observations, by consulting original mss (e.g., Lemonnier's) & comparing places (computed therefrom) to existing theory (the variously corrected Bouvard 1821 tables). That Adams apparently finished his 1845 Autumn orbit ahead of Leverrier is partly due to his not tending to this groundwork. Opinions will vary as to whether Adams was initially wise in so proceeding. (He was probably not familiar with reductions of observations, anyway: fn 4.) In the event, Adams' conduct on this point proved completely justified — in that Neptune's perturbations on Uranus were far larger than the errors in previous reductions of the Uranus data.

[7] Leverrier is remembered not only for his astronomy but for his matrix inversion procedure, which I have long used a variant of (for researches astronomical, statistical, historical, even photogrammetric).

[8] DR 1984N (1980) remarks "the Cambridge . . . clandestine summer-long 1846 fine-tooth sky-search, which attempted to exploit a situation [where] the provocative agreement of [Adams' &] Leverrier's results was known only to a handful of [Cambr.U.] astronomers." See the much more complete paper (and its various hard-earned new evidential finds) by R.Smith "The Cambridge Network in Action: The Discovery of Neptune" (*Isis 80*:395; 1989).

[9] And he states to RoyAstrSoc Sec'y R.Sheepshanks 1846/12/18 that "Adams was not acquainted with Le Verrier's [6/28] letter to me."

[10] A few days later, in an 1846/12/17 letter to Sheepshanks, Adams altered his position (I suspect in response to a sharp private rebuke from Airy): "I fully allow that I have to blame myself severely in this matter . . . for having trusted to anyone but myself to make known the results at which I had arrived".

(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 deanery (Ely)[11] 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 1846/12/12 Report to the Observatory Syndicate, SP p.liv: Adams "has at once done 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 6/29 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 bask in the new planet's glow). 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

[11] Peacock's severe image is now preserved in a stained-glass window at Ely Cathedral!

[12] To Babbage's credit, he later examined all the "accessible" documents and concluded that Leverrier deserved prime 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 prole-myth that Airy "snubbed" Adams, etc. (See, e.g., Smart 1947 p.38. R.Smith's crucial discovery of Airy's 1846/6/25 letter to Whewell — §A6 & fn 31 — utterly and finally eliminates that durable popular legend. See Smith 1989 n.25.) I don't doubt that Airy felt betrayed by some of Adams' behavior: the curious footnote of Smart 1947 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 hearers] who have so taken it up that my old friend [Sedgwick] has felt himself obliged to question me as if I were a common criminal". If Adams in 1846 June asked Airy not to mention his then-progressing work (& we note that Adams himself said nothing to expert Hansen at their July 2 encounter) but now after discovery was pretending he had taken it for granted that Airy had distributed his work (see quote at §A6) — 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 illtreated, 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 about Airy: "On the part of [the] letter relating to Mr.Adams and the *Glory of the Greatest Astronomical Discovery of Modern Times*, etc., etc., etc., I have no remark to make."

[13] Text given more fully below: fn 67.

kneejerk-rejects anything smacking of Conspiracy. The rule is simple: there *are* no real conspiracies — every apparent one is merely the product of paranoid imagination(s). (The tobacco lobby has a forbidden C-word: Cancer. But Hist.sci outdoes this by condemning *two* never-never C-words: Crime[14] and Conspiracy.) It is commonly protested that the whole Neptune scandal has been misunderstood — that the episode merely involved a bunch of honorable British gentlemen, sort-of bumping along unconsciously, with no deliberate plotting. (I would agree only to the limited extent indicated below, e.g., at fn 77: alot of unplanned consequences flowed, when Britain's Neptune scheme went awry.) Rigid anti-conspriracy fanatics also tend to ignore Airy's revealingly fretting expressions (§B1). And they pass too lightly over a small but crucial indicator: we can positively prove deliberate deception by Airy to distort the history his way and cover-up British searchers' actual July uncertainties. This tendency is unambiguously exhibited by his artful public doctoring of his own 1846/7/9 letter (to Challis) broaching the sky-sweep, which said (ambivalently, to protect himself and show he'd been dutiful, whether or no a planet turned up): "You know that I attach importance to the examination of that part of the heavens in which there is *a possible shadow of* reason for suspecting the existence of a planet exterior to Uranus." I have italicized "a possible shadow of" because (as first revealed on 1966/12/21 by honest Cambridge U astronomer David Dewhirst), Airy replaced those revealing words by an ellipsis when he published the letter (M16:402) — *while on the same page* (M16:402) Airy states that *at the very moment of this letter*: "I had now no longer any doubt upon the reality and general exactness of the prediction of the planet's place."[15] (Comparing Adams' parallel *post-discovery* sureness to his pre-discovery daze[16] is intimately related to the main issue of this paper. Was the deleted "shadow"-phrase reflecting the uncertainty of Airy *or of Adams*? Or both?) One may also compare the Airy-published version (M16:412) of Challis' 1846/10/12 letter, to the awful reality (see §D7). Regarding the general point of how well we may trust the version of events Airy left us: one need only read his private 1846/10/14 promise to Challis, with respect to how he intends to tell the Neptune story at the upcoming day of reckoning (1846/11/13 RAS meeting): "*The matter being one of delicacy*, I will not compromise any one . . . . All I ask is — Will you allow me to publish your correspondence with me on this subject, or extracts from it taken at my discretion?" (CON#18 pp.2-3, emph added; & Airy wrote similarly to Adams the same day, Smart 1947 p.34: "it would be wrong for me to compromise anyone . . . .") The tone of such remarks is certainly censorial — and can be viewed as somewhat conspiratorial (§H3, §I3).

B3 In defending Airy as erring but dutiful & honorable, Chapman 1988 p.126 even goes so far as to point out that Airy was distracted by an 1845-6 failed court charge against one of his Greenwich assistants (RAS medalist W.Richardson) for allegedly murdering his incest offspring. Given the mafia-like purity of Cambridge U blood in all the plotters whose 1846 scheming so brilliantly tattered British astronomy's reputation for honesty, I think Chapman (Oxford) is concentrating upon the wrong incest. I might add that anyone (such as Smith 1989 p.418 n.77), who claims[17] to doubt conscious, deliberate secrecy, should

[14] The title of R.Newton's 1977 Johns Hopkins Univ book (*The Crime of Claudius Ptolemy*) is still freaking out Hist.sci archons, e.g., *JHA* Editor-for-Life & renowned Britwit, Lord Hoskin. See *DIO 2.1* ‡3 §B2.

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

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

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

examine not only the inevadably stark evidence of Airy's 1846/6/26 silence to Leverrier regarding Adams (this vs. Airy's key 6/25 letter to Whewell 24^h before: §A6 & fn 31) — but also the eyeopening remarks concluding Airy's brief 1846/8/6 letter to Challis (regarding Breen's availability for the secret search; CON #6, hitherto unpublished except at *DIO 1.1* ‡1 fn 10): as Airy left England to vacation on the continent, he told Challis to (while Airy was out of the country) write to his Main man, "write to Mr.[Rob't] Main [2nd-in-command at RGO] who is fully in my confidence and understands the position of the whole matter." In the Neptune context, can anyone doubt that this is one plotter writing another regarding who else may be trusted with the secret?

B4 Search-designer-overseer Airy outlined & advised the celestial hunt's strategy in a series of letters starting the very day after the 6/29 plot was broached: 6/30, 7/9, 13, 21 (CON #1 to #5; see also M16:416). On 1846/7/18, 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 ecliptical longitudes corresponding to predicted & 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 ephemeris, the document guiding Challis' search, was not based on Adams' now-famous perturbation-computed Hyp 1 orbit-prediction (see fn 19) but rather upon a combination of: [a] Flamsteed's lost[20] star #1007 & Wartmann's weird 1831 alleged planet-sighting [note added 1997: see P.Baum & W.Sheehan *In Search of Vulcan* NYC 1997 pp.83-84 n.15], [b] Leverrier's published longitude limits, & [c] a *circular*-orbit distance (38 1/4 AU), not elliptical (Hyp 1). The last point is devastating to Adams' claim. And

[18] The Challis 1846 search's observations (CambrObs archives, courtesy D.Dewhirst) suggest that, at the very time when Adams was arriving at his extrapolated solution Hyp X (up until 9/18-21: see §B8), Challis was looking in that solution's position, about 10° west of the planet's actual place. (In general, the correlation is not so sharp as to constitute proof of a connection.) When 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] CambrObsNept 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 *none of the work used Adams' famous perturbationally-based orbits*. Note: Challis' false public implication (M16:421), that MemoW's ephemeris is based on Hyp 1, is essential to Britain's crucial claim that Neptune was first seen on 1846/8/4 & 8/12 due to Adams' *noncircular orbit* perturbational calculations. (Airy follows this sham in his 1847/3/18 letter defending Adams' priority: *Athenæum* 1847/3/20 p.309. See fn 37.) To the contrary, the central epoch for MemoW's ephemerides, 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 published on 1846/6/1 (and Adams only agreed with it by the accident of his later-corrected 1845/9 MemoC 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 of 10°." The French is copied verbatim from Leverrier *loc cit*.) The first date (1846/7/20) of the MemoW calculations written by Adams upon this document makes it clear that Adams was in on the secret CambrObs sweep from the start. (Given the 20-day interval & the 7/20 initial date, we may say that MemoW was probably written after 1846/6/30, certainly not after 7/20.)

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

the use of Wartmann's data as a primary[21] prop (given at the *top*[22] of MemoW, above the 20-day-interval ephemeris Challis later brought forth at M16:421 to support Adams' claim) also raises questions (§G9) regarding how strongly Adams believed in his own perturbation-work's accuracy. (The Wartmann connection was entirely suppressed by Challis at M16:421 and is unmentioned in any history of the Neptune affair. See §I2.)

B5 As DR has been emphasizing since 1966: no other observatory besides Cambridge moved so forcefully precisely because: *no one else was allowed to know of the sensational Leverrier-Adams double confirmation of Neptune's place*, a mathematical agreement that was the private secret of this inner Cambridge Univ circle. That the value & purpose of the plotters' secret was fully known to them is plain from Challis' own remarks at M16:416 where he explains general world inaction on Leverrier's published results as precisely due to their unsupported solitude: "In no other way I think, is it to be accounted for, that for nearly four months after the publication, at the beginning of last June, of M. Le Verrier's first determination of the probable longitude of the planet, not a single step appears to have been taken in any continental observatory in search of it." The inner circle's secret (double confirmation) was the key. Challis goes on (*idem*): "I do not know whether the close agreement of M. Le Verrier's determination of the longitude with that which Mr.Adams had previously obtained, induced the Astronomer Royal to suggest to me. . . a systematic attempt to discover the planet. [DR: Airy says plainly, ibid p.400, that he told Challis & the other Cambridge circle conspirators that this mathematical agreement was indeed his inducement to search; Airy stated this at the very 1846/6/29 Greenwich meeting where the Neptune plot was hatched by Airy, Challis, & J.Herschel.] I can say, however, that this concurrent evidence of the reality of the disturbing body from two independent investigations, weighed strongly with me in coming to the determination of undertaking the observations in the face of the great amount of labor they might be expected to entail." I lodged this newly-revived secrecy interpretation of the Neptune affair at Johns Hopkins 1966/5/11-20. Eight years later, I found that British astronomer J.Hind had privately recognized[23] this secrecy in a 1846/11/12 letter (discovered by DR in 1974, quoted in Rawlins 1984N) written to Sheepshanks of RAS: "the Cambridge people . . . do their best for their own . . . . the inexcusable secrecy observed by . . . those acquainted with Mr.Adams' results . . . [is a] secrecy which [deprives him] of all share [of Neptune and] I am not the only one who thinks so." (Edw.Cooper, Markree Observatory, 1846/12/30 to Challis, CON #28, remarks "the lamentable oversight . . . on the part of Mr.Adams in keeping private his important calculations. . . . I feel M.Leverrier's claim to supercede that of Mr.Adams. I sincerely wish that it were otherwise.")

B6 As to how secretive the Cambridge Univ plotters were (evidence outlined in Rawlins 1984N): from 1846/6/10 to 7/4 (the very period when the plot was hatched), Airy's house-guest was the immortal P. Hansen, the leading figure of the world in celestial mechanics and the author of some equations Adams was using to deduce Neptune's place! (For some of Hansen's influences on Adams' private computations, see Sampson 1904 pp.156f. Hansen's name appears explicitly in Adams' manuscript work on his planet, e.g., *ibid* reverse of plates 3&6.) Nonetheless, during all these weeks, while the sky-search secret plan was frantically & desperately (§B1) developed, Hansen — living right in the central conspirator's home

[21] The MemoW ephemeris was entirely computed for a circular-orbit planet moving sidereally at exactly 15″/day: mean dist 38.25 AU, the $236^y.6$ period being from (see top of the ms page of MemoW) a fit to 2 missing stars, including Wartmann's. The MemoW scheme: for 1846/8/29 heliocentric longtd θ, the 8/29 geocentric longtd is found from the formula (Earth helioc longtd being set = 336°.4 on 8/29): $\theta - \mathrm{atn}[\tan(\theta - 336°.4)/(38.25-1)]$. The central geoc motion (8/9 to 8/29) for the basic interval of 20 days was found using helioc motions 1°/12 (planet) & 19° (Earth): motion = $(19° - 38.25 \cdot 1°/12)/37.25 = 25'$. Const 2nd diff = $-5'$ for θ = 315°; $-3'$, 320°; $-2'$, 325°; $-1'$, 330°; $0'$, 335°.

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

[23] I believe Hind (among other initial British supporters of Leverrier's claim) later became more orthodox-British regarding Adams; whether from pressure or from mistaken conviction, I cannot say.

— was never informed of Adams' researches. Even more remarkable: while walking at Cambridge on 1846/7/2, Adams by chance actually bumped into Airy & the esteemed Hansen (Smart 1947 p.34); but *nothing of Adams' work was mentioned*. (Apologists brush this aside by saying the meeting lasted but "a few minutes". Comments: [a] Hansen's visit with Airy lasted 24 days. [b] Had Adams at the 7/2 encounter simply mentioned to Hansen what he was up to, the meeting would have lasted a whole lot of minutes. [See fn 75.])

B7 Challis started his sky-sweep 1846/7/29, and from that date through 8/12 worked (at magnifying power[24] 170) near the center of the search region: the point on the ecliptic at longitude 325°). (See Challis' Neptune zone records, Cambr Obs.) At Airy's 6/30, 7/9, & 8/6 urgings (CON #1, 2, & 6), Challis agreed on 8/7 to add RGO computer[25] J.Breen (Airy 6/30: "a rough genius . . . . perfectly tractable") to the search-team, which also included Challis' assistant Morgan. The apparent cover story was that Breen was just going to the Cambr Obs for a "month's trial" toward acceptance to the post of Junior Assistant there (see Breen's letter: CON #8 1846/8/8). From 8/14 through 9/18, Challis examined the western part of the region, "purposely" (*AstrNachr 25*:102), since that's where the Sun would first encroach later in the year — but it's also the region where Adams' latest work seemed to be pointing (8/20 was the date on his Hyp 2's first rough solution: Sampson 1904 p.167). Independent British astronomer J.Hind (of Geo.Bishop's private observatory in Regent's Park) wrote Challis on 1846/9/16 (CON #10) that he had recently heard of Challis' search (possibly at the recent BAAS meeting). Hind's letter mentions the fact that he and the French astronomer H.Faye were also preparing to search for "the new planet". (In his letter, Hind strikes this expression and replaces it with "Le Verrier's planet". See also CON #13, where Hind thanks Challis for his recent "kind" letter; and compare to Hind's later fury — quoted at Rawlins 1984N & above at §B5 — when he learned that Challis had at this very time been keeping from him Adams' confirmation of Leverrier's predicted position for Neptune.) Faye is quoted by Hind as expecting to spot the planetary quarry by searching for a disk of diameter about 2″, following Leverrier's 8/31 advice.[26]

B8 Hind's letter would have been received by Challis on 9/17 or 18. After 9/18, Challis returns (starting 9/21) to the center of the search area, right where Leverrier was pointing, presumably looking for a disk. Challis' later accounts (1846/10/21 *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 Leverrier's (not Adams') instructions ever since 9/21.[27] Challis instead publicly claimed that he had switched back to following Leverrier's guidelines only on 9/29, the Cambr 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 fill the recently deceased Baron Damoiseau's place in the Academy of Sciences. In fact, Leverrier got the position instead, because of the very discovery that was the subject of Faye's letter.

[27] Leverrier's final paper, 8/31, would have been available in England almost exactly at this time. (Regarding how long it customarily took the *Comptes Rendus* to reach England, see §D4.) Also: CON #31 is an undated slip of paper in Challis' hand noting the "Verrier" 8/31 paper's proposed longitude limits for his planet, taken from Leverrier 1845-6 p.436: 321° to 335°. So Challis almost certainly knew by 9/21 that Leverrier's 8/31 paper was pointing farther to the east than Adams was suggesting.

[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 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 nonfixed use of the telescope, Airy on 7/21 (CON #5) warned against allowing equatorial motion, adding "I think you will find my plan sufficient even when stars come thick." At M16:404, Airy was modest & merciful to Challis on this point. (Challis' 10′-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 dreamy".

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

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

C1 A previously unremarked but critical point: Leverrier, by publishing his prediction (before optical discovery), took all the chances of embarrassment[29] 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: §I9.) 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.

[2] Deliberate pre-discovery secrecy regarding Adams' work.

[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 *Cambr Chronicle*; retracted 10/16: both newsclips preserved, as CON #15&16) stated that Adams' work was completed only in about June of 1846![30] Note also that co-plotter J.Herschel's letter of the same date (1846/10/1; *Athenæum* 1846/10/3 p.1019) likewise makes no claim whatever that Adams' work had any priority over Leverrier's. (R.Smith's important find, Airy's 1846/6/25 letter[31] to Whewell, does say that

[29] This eventuality later became real. In 1859, Leverrier discovered the nongravitational precession of Mercury's apse, which we now know is due to relativity. (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 Leverrier naturally interpreted it gravitationally, and so predicted the nonexistent planet "Vulcan" within Mercury's orbit. A series of failed searches for it somewhat darkened his later years.

[30] If one wishes to view this 1846/6 "completion" remark as merely claiming that Adams' Hyp 1 was not a complete solution, then [a] it grossly exaggerates the earliness of the known dates of Hyp 2 (1846/8/18-9/2), and [b] it still destroys Adams' priority. The Challis 10/1 text: "About four months ago, Mr. Adams, of St. 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. Le Verrier more recently inferred . . . that the mass of the disturbing planet was to that of Uranus in the proportion of 5 to 2 (a result which Mr. Adams also arrived at [1846/9/2] by continuing his researches) . . . For the last two months I have been engaged in mapping the stars in the neighbourhood of the probable place, a method which, though slow, must eventually have been successful. The last investigations of Le Verrier came to my knowledge on Sept. 29. On the evening of that day I observed strictly according to his suggestions, and out of a vast number of stars which passed through the field of view (power 160 [vs. fn 24]), I selected one only, against which I directed my assistant to write 'seems to have a disc.' This was the planet." (*Cambr Chronicle* 1846/10/3 [emph added].)

[31] Airy to Whewell 1846/6/25 (cited by R.Smith 1989 n.25): "Peoples' notions have long been turned to the effects

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

C2 Now, the official documentary history has it that Challis wrote the 1845/9/22 intro for Adams' visit to Airy, stating to Airy that Adams had "completed" his work on Neptune (M16:394). Since Challis was at the famous 1846/6/29 Greenwich meeting *three quarters of a year after this "completion"*, when the secret search was born (triggered by Leverrier's very recent 1846/6/1 publication), how could Challis possibly have gotten the impression that Adams' work was completed at roughly the same time?[32] Unless it was. (Or even later. But if Challis was right, then Hyp 1 — which will be often called MemoR in discussions below — was actually sent Airy not in 1845/10 but about early June of 1846.)

C3 Such a contradiction (as the Challis 10/1 letter presents us) is enough in itself to justify terminating our faith in the traditional British rendition. It may be true, but there must always be a question mark over it. No such question mark attaches to Leverrier, so he should always receive primacy in mention of Neptune's discovery. The sudden 10/1 Challis & Herschel letters (mailed only hours after the news of discovery reached the writers) suggest possible pre-plan (Airy was abroad at the time) or collusion. But the more trenchant question: can a Cantab clique keep Adams' name secret for months until *just* after the discovery and then claim a piece of it? Do we want to encourage this sort of claimjump?

C4 For a century, Airy's Neptune correspondence file was sealed (à la Peary's records). At the centenary (1946), two prominent British astronomers (Astronomer Royal H.Jones & W.Smart of Cambridge Univ) saw the file & shortly published accounts based on it, but *without specifying its location*. Some years later, it was found that the whole file had gone "missing".[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 *Comptes Rendus* of 1 June 1846, which and a previous number [1845/11/10] I strongly recommend you to consult. Both [Adams & Leverrier] have arrived at the same result, viz. that the present [ecliptical] longitude of the said disturber must be somewhere near 325°." Smith 1989 also quotes (n.27) another portion of this important letter: if "I were a rich man or had an unemployed staff I would immediately take measures for the strict examination of that part of the heavens containing the position of the postulated planet." And see §B1. But note his great caution at fn 15.

[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 RoyGeogrSoc & widow Kathleen Scott did to Robert Scott's South Pole diary before publication. This notorious bowdlerization assisted in the curious hagiological process whereby most British children ended up believing (as a justifiably disgusted Amundsen reports in his 1927 autobiography, *My Life as an Explorer* p.72) that Scott discovered the South Pole! — no matter the trifle that Amundsen got there 4 weeks ahead of Scott, who died while returning, partly from the disappointment of manhauling 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 Neptune case, I will cite Amundsen's succinct conclusion (*op cit* p.71 or *DIO 2.2* ‡5 fn 14) "by and large the British are 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 . . . .)

[33] Chapman 1988 p.136 n.6. This also happened in the case of the Roy.Geogr.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 1909/1/9 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 *Peary at the North Pole: Fact or Fiction* p.82). The point is conceded in the new standard biography of Shackleton by R.Huntford (*Shackleton* 1985 p.311), which suggests, as I have always stated, that:

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 gainers from this file's disappearance are not Z but: [1] a British legend, and [2] the RGO's reputation.[34] If we ever want to see the RGO Neptune file, the plan of inaction is obvious: cease all mention of Adams as a "co-discoverer" of Neptune until the file resurfaces.[35]

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.lvi-lviii and Jones pp.15-17. See Chapman 1988 p.125 n.21: "Original untraceable at RGO, presumed missing in 'RGO Neptune file'.") It is this "lost" 3 page document that is *the physical basis for Britain's claim of priority*.

C7 And I will here announce that the date, "1845 October" on this document : [1] was added later, and [2] may be false. Why such a severe suspicion? Start by looking closely at the date on the photographic reproduction: [a] The date is distinctly darker (than the rest of the text): it was obviously added with a different writing instrument. (Pen vs. pencil?) [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 notoriety among astronomers for his obsessively precise & complete[36] records — would not date this memo immediately upon receipt?!) [d] After noting this, I checked the first publication of the document (M16:395, as part of Airy's 1846/11/13 presentation): the date (as a part of the document) is there lacking.

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

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

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

[34] It is unarguable that the RGO is plenty sensitive about this case. Z's accounts defend Airy by portraying Challis as a crank & idiot. At the 1946 centenary, Astronomer Royal Jones actually attempted to suppress Smart's defense of Adams (reported to DR by R.Smith 1989/7/28) — evidently because it reflected so badly upon Astron Royal Airy.

[35] The safest prediction DR ever lodged: if the "missing" RGO file is someday "found", DR will not be informed until after another (politically Reliable) scholar has published a suitably-mild, soften-the-blow version of its contents.

[36] E.Maunder records (as quoted at Smith 1989 n.27): Airy "devoted an entire afternoon to himself labelling a number of wooden cases 'empty' . . . . His friend De Morgan jocularly claimed that [if] Airy wiped his pen on a piece of blotting-paper he would duly endorse the blotting-paper with the date and particulars of its use, and file it away amongst his papers." See also below §D1.

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 Sheepshanks 1846/11/17 relative to publishing Adams' paper: "It is important that you should note on Adams' paper the day when it was received." Compare to §C7 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 RoyAstrSoc on 1846/4/8 (*MNRAS 7.6*:83). If he had a Neptune orbit *he himself trusted* (the issue which is at the heart of this controversy & the key to his loss of priority), then: why was this not the subject of Adams' 1846/4/8 RAS talk? Is it any wonder that the French were incensed that only *after* the discovery, Adams claimed he knew all about Neptune?

D2 The French were robbed of priority by British maneuvering,[37] the most outrageous part of the process being that, as the theft proceeded, strong public French expressions of suspicion were used to show how irrational and undeserving the French were! The 1846/12/5 *Athenæum* p.1246 refers to the great physicist & top French astronomer F.Arago's "distorting mirror of national bias" and his "mania". Airy to Sheepshanks (1846/11/23): "I am sorry to see that the feeling of the French towards Challis amounts to hatred. (This has arisen entirely from Challis's imprudence in writing only on one side of a question at one time.)" (On 1846/11/5, Airy gently advised Challis to be careful about this: CON #21.) Co-conspirator J.Herschel tells his diary (1846/10/25) that he not Leverrier is the injured party: "Wrote to *Guardian* in reply to M Leverrier's savage letter [10/21] — These Frenchmen fly at one like wildcats —". (Herschel's diary contains nothing whatever on Neptune before the discovery, though [a] he was in on the search after 6/29, [b] co-conspirator Peacock visited Herschel 8/7-9, & [c] Herschel announced the prediction of Neptune at the 9/10 BAAS meeting at Southampton, but without mentioning Adams' name: *Athenæum* 1846/10/3 p.1019.)

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

[37] The traditional British version of the Neptune tale has little Adams being ignored by big Airy in 1845. The actual big-vs.-little tale is rather different: little (in international astronomical politics) France was outdeceived & 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 52.) The legend blames Airy's & Challis' paralysing distrust of Adams' math. I agree, but with the key addition that this mistrust was primarily Adams' own. (See, e.g., §F1-§F3.) Assuming the record is real, there was no ignoring of Adams: Challis' 1845/9/22 letter of introduction (M16:394) of Adams to Airy said: "I should consider the deductions from his premises to be made in a trustworthy manner." Airy's reply to Adams' 1845/9 visit was a letter to Challis (1845/9/29 CON #42) asking him to tell Adams: "that I am very much interested with the subject of his investigations and that I should be delighted to hear of them by letter from him." After receiving Adams' 1845/10 note, Airy wrote Adams a friendly inquiring letter (1845/11/5, M16:396-7) and simply got no reply. (Though, the rendition at Newman 1963 p.175 invents a nonexistent Adams reply anyway.) The letter asked Adams whether the hypothetical planet which accounted for Uranus' longitudinal wanderings also explained Uranus' anomalies in the radius vector. (Chapman 1988 pp.126-8 prominently & repeatedly confuses Uranus' radius vector perturbations with Neptune's mean distance, the kind of thing one finds regularly in the extremely handsome *Journal for the History of Astronomy*, where the process of meaningful refereeing is as mythic as anything in the Neptune affair.) Chapman's paper adds useful material to the Neptune controversy, but his ritual attack (p.136 n.6) upon the "heroes-&-villains" approach just reflects the standard nonjudgemental 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 uncheckable as it is cocksure. In fact, Airy explicitly countered this view in learned detail in a public letter of 1847/3/18, published in the 3/20 *Athenæum* p.309. (The effort is so obviously special pleading for Adams' priority that Airy tries to deny that. A key false claim in this letter is discussed at fn 19.)

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/c.21 memo (supposedly MemoR), would not Airy communicate[38] 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 Adams' value at the time. On 1846/8/31, Leverrier announced on the floor of the Academy a reduced distance for his planet. The mails from France took a day or two to reach England (as is clear from Airy's Neptune "Account", M16); on 1846/9/2, Adams for the first time communicated to Airy that he had decreased his own planet's mean distance. (The 1690 residual was the only residual Adams possessed in 1845 which he later used as a post-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 speculated 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 silence[40] regarding his supposed elements continued for well over a month even *after* the real planet's 1846/9/23 discovery. Not that Adams wasn't heard from: on 1846/11/5 he joined Challis in publicly congratulating a journal for its helpful news coverage of the controversy (*Athenæum* 1846/11/7 p.1148-9), having already (1846/10/15 *Athenæum* p.1069) attempted publicly with Challis to propose a name for Neptune, "Oceanus"! (All with Airy's private pre-approval: CON #18 1846/10/14, "I like your name Oceanus.") The sheer nerve is admirable. (Hind scoffed 1846/11/12 to 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 his claim rested. The oddity of this is highlighted by the realization that Adams published (via Challis' 1846/10/15 letter: *Athenæum* 1846/10/17 p.1069) elements for the real planet nearly a month before he published the elements he allegedly predicted for the planet in 1845! (When astrologers perform this way, we don't take them entirely at their word, either.) Challis' 10/15 letter also promised "Mr.Adams's investigations will, in a short time, be published in detail." But what were wanted swiftly at this juncture were not full details but simply: the predicted elements. Adams' failure to produce these right away legitimizes

[38] Glaisher 1896 p.xxvi (emph added): Airy "was a man of vigorous character, and it seems unaccountable that he should have taken no steps to secure the publication of Adams's results, *even after* his correspondence with Leverrier in June 1846. Sedgwick's letter [1846/12/6] . . . contains the following passage: 'When it was found that Adams was confirmed by the fortunate Frenchman the facts ought to have been out without more delay. Was Adams ever so much as told that Le Verrier was at his heels? Our astronomers ought to have got up a flare in an instant.' " (See also §H3.) Airy's excuse is given at Smart 1947 p.40.

[39] See fn 75. Note the slightly suggestive circumstance that Challis was also present. [M16:397 explicitly says Leverrier's paper reached RGO in December.]

[40] The standard explanation is implicit: Adams was just inherently quiet. J.Newman (*World of Mathematics* 1956 p.821): "shy, gentle, unaffected . . . . refused to be drawn into the bitter controversy over the question of who was 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 namer: §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 *DSB 3*:186-7) that he had seen (& come within a whisker of capturing) Neptune on 1846/8/4 & 8/12, during the clandestine Cambr Obs search. History generally regards this near-miss as a tragedy for Adams&co. I regard it as miraculous justice for Leverrier.

D8 Within days after finding Neptune in his 1846/8 records, Challis was transformed: he announced (*Athenæum* 1846/10/17 p.1069) on 1846/10/15 with a wave of the flag that an Englishman was responsible for the first observations of the planet and publicly proposed "Oceanus" as its name — appearing to believe that his chance for immortality was yet retrievable.[41] Challis' seizure (one may almost call it that) at this juncture upset any possible Airy hope that his 1846/6/29 conspiracy would remain unknown. The public was now aroused to patriotic fervor for hero Adams, and soon sought a scapegoat to 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, *Athenæum* 1846/10/3 p.1019. Emph added.) When the skeptical French got openly impatient, the eminent British weekly *Athenæum* (1846/10/31 p.1117) assumed, as always, the rôle of sage & neutral arbiter: "Mr. Adams's claim, whatever it might be [DR: this is over a month after the discovery!], should not be lost by an early [!] statement of the facts upon proof of which it is to rest — they [French skeptics] have hurt themselves, not us." [DR: I like the "us".] The facts of the discovery are not fleeting. . . . They rest on records on paper. . . . [Adams' claim] *is* brought forward . . . in the shape of a statement to be substantiated as soon as the calculations and observations [!] can be published. Why, then, all this heat?"

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

[41] He was partly right (fn 56), but that was eventually of little consolation to him or to Cambridge.

[42] Perhaps he did, but too late. Once unleashed, patriotic passion has a life of its own. Airy's 11/5 letter to Challis, on the controversy's heat, says he thinks a judicious recent Challis letter to the Manchester *Guardian* "goes further in the withdrawal of claims on Adams' part than *I* should."

[43] DR *Fiction* p.172.

[44] On 1846/10/15 for the 10/17 *Athenæum*; see above at §D6.

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

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

E6 This cloud is only darkened by another peculiarity which no historian has remarked, probably because all either regard Airy as an enemy of Adams or are so loyal to Airy that they don't like the obvious implications. Just after the discovery of Neptune, while Airy was returning to England, he stopped by Altona (on about[45] 10/5) to see Schumacher, the Editor of the eminent *Astronomische Nachrichten*. There is no record that he told Schumacher that he was about to support a British claim to co-discovery. Instead, what Airy did (as he admits in his 1846/10/14 letter to Leverrier, in a part usually omitted in modern accounts but fortunately surviving at Glaisher 1896 p.xxiii) was to *read carefully Leverrier's extensive manuscript explaining the mathematics of his discovery* — this over a month before Airy's co-conspirator Adams got around to publishing a digit of his own math! (Leverrier's ms was sent the *AstrNachr* 1846/9/8 for immediate publication, not for Airy's private perusal[46] — however much his read-through eventually moved Airy to creditable praise for Leverrier: see §I3.)

E7 This ms later appeared at *AstrNachr 25*:53-80, 1846/10/12-22 (a final post-discovery Leverrier paper was also published at *AstrNachr 25*:91-92, 1846/11/5). The last date (10/22) shows that not only Leverrier's orbital elements but the crucial details of his math had been published well before Adams had publicly committed himself in either department. Adams' 1$^{\text{st}}$ release of his results was at the RAS meeting of 1846/11/13, so Airy's peek at Leverrier's math occurred over 5 weeks prior to the public debut of Adams' elements. In order correctly to evaluate this point, it helps to know that Airy was a skilled mathematician (remembered for the Bessel-function-related Airy integral & Airy disk);[47] and he possessed expertise[48] in some areas of celestial mechanics (having earlier discovered an important new Venus-related secular term in the solar tables) and thus was one of the very, very few persons in England who could understand anything of substance in Leverrier's paper. The possibility of his noting, e.g., which perturbational terms were included & omitted — such information would (at the very least) have been useful for advice[49] to Adams, even in spite of the various differences between the two

[45] This estimate (good to about a day) is induced from Airy's 1846/10/13 letter to Sheepshanks.

[46] 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?

[47] See, e.g., J.Jackson *Classical Electrodynamics* 1962 p.484, J.Meyer-Arendt *Introduction to Classical & Modern Optics* 1989 p.253 fn.

[48] Which suggests a simple & novel hypothesis: were Adams' 1845 Autumn trips 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.

[49] 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.[50] (Though even freshman[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[52] stupidity ruined Adams' chance for immortality. Extrapolating beyond Grosser's unsympathetic portrait, J.Newman's prominent review[53] of Grosser's book spoke of Airy as: a "school-bright, hapless donkey", "unusually conceited", & "bitterly jealous of his assistants — or of any young astronomer."[54] Fortunately, astronomer O.Eggen's learned, near-simultaneous 1963/4 *Sky & Telescope* review provided a counterbalancing breath of sanity about Airy — plus a few gentle digs at the then-latest Hist.sci account's inevitable technical-innocence slips, which have long provided such reliable entertainment for real scientists.)

E8 What eventually destroyed Leverrier's personal lock on Neptune was that: [a] the real planet turned out to be only 30 AU from the Sun, not his final predicted mean distance of 36 AU (*MNRAS 7.12*:216, 1847/2/12; *7.15*:270, 1847/5/14), while [b] Adams' allegedly final, extrapolated solution, which we'll call Hyp X (1846/9/2, M16:405-8), had predicted only 33.4 AU — twice as accurate (1846/12/17 letter of Challis *Athenæum* 1846/12/19 p.1300). Actually, it should be noted that 33.4 AU (Adams 1846/11/13; M16:456; Challis *loc cit*) is not what Adams said in his 1846/9/2 letter, which had Hyp X near-circular orbit radius 33.7 AU (M16:407); the 33.4 AU figure evolved from there via subsequent alteration of recent residuals. But the more important point no one has previously noticed is that, not only is the longitude of Hyp X way[55] 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 accurate computation of Neptune's 30 AU mean distance (from 1846 observations)[56] given in Challis' 1846/10/15 & 21 letters (*Athenæum* 1846/10/17 p.1069 & *AstrNachr 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

[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?

[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.

[52] Newman 1963 p.175 speaks of Airy's: "consuming 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 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-softhead 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.

[53] *SciAm* 1963/3 pp.169-178.

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

[55] The import of this lay unnoted until Rawlins 1969.

[56] Two of them Challis' then-precious prediscovery ones of 1846/8/4&12 — uniquely useful for a little while, because they provided Adams & Challis a greater time-span of data than anyone else had at hand. I.e., in 1846 Oct, Cambridge Observatory was the only one in the world that had made (& knew it had made — vs. others at ‡7 §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.

during 1806 & 1846.[57] Thus, Leverrier's final prediction was superior to Adams' (Hyp X) regarding not only longitude but distance.

F Speculative Reconstruction of Adams' Actual 1845 Oct Solution

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

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 (§G4) from those of the "1845/10" MemoR which has always previously been accepted as Adams' first solution, "Hypothesis 1" (which is the crux of the whole Adams claim). In particular, a perturbation term's sign is wrong, which contributes to producing an orbital eccentricity (0.1428) which is about 1/8 lower than the MemoR value (0.16103) published. Moreover, we have Adams' word (M16:429) that the 1845/9 elements are his "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.l.) Sampson (*loc cit*) supposes 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 publication, *since a single such fouled-up term could destroy his solution*. 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"!),

[57] Perihelion at 32.263 AU during 1826 (so near the §E8 time of passing Uranus that the coincidence might have suggested trouble for the solution). Around their later respective perihelions, Adams' even more eccentric Hyp 1 & Hyp 2 also got comparably close, though for fewer decades: 32.22 AU & 32.78 AU, resp; so the important perihelion distance was *increasing* not decreasing, as Adams' solutions progressed (& shrunk the most recent residuals).

[58] But if the solution was altered in the interim, which is the only defense insider R. Sampson can make, on the little known record we are about to explore, then one can hardly call it anything but good luck.

as he plainly did.[59] No popular account has ever mentioned the Fourteen Percent Solution of MemoC-MemoD.

F3 Adams may have been paralysed not only by his sign-error, but also by the fact that his various 1845-1846 solutions were (compared to Leverrier) all over the last octant of the zodiac. On the discovery-date, 1846/9/23, Adams' various 1845-1846 solutions gave the following true heliocentric longitudes (with the date of arrival at the solution provided in parentheses): [a] Adams' first 1845 hypothetical planet (1845/4/28-5/19) was[60] 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^{t}"), 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)[61] 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.

[59] Adams (M16:429, 1846/11/13, emph added): "After obtaining several solutions differing little from each other, by gradually taking into account more and more terms of the series expressing the perturbations, I communicated to 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 (Glaisher 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.

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

[61] Section E III of the Adams Neptune mss at St.Johns College Library (Cambridge Univ); Sampson 1904 pp.165-6.

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^{d}. irregularities in the motion of Uranus may be accounted for by supposing the existence of an ext^{r}. planet the mass and orbit of wh. are as follows
>
> Mean Dist. (presumed nearly in accordance with Bode's law)
> 38.4
> Mean sid^{l} mot^{n} in 365.25 days
> 1°30′.9
> Mean Long. 1st Oct^{r}.
> 323°34′
> Long. Perih^{n}.
> 315°55′
> Eccentr^{y}.
> 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^{y} = 0.1428.
> Long. Perih^{n} 320°30′.
> Mean Long. about the end of Sept^{r}, 1845 = 321°40′.
> Hence Geoc. Long. at the same time will be 320°30′ nearly, [dim.^{g}] 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-speculated) document bearing what I am calling "Hyp G":

> Mean Dist. 38.4 AU (assumed nearly in accordance with Bode's law)
> Mean Long. at end of Sept^{r} = 322°08′
> Long. Per^{n} = 321°00′
> Eccentr^{y} = 0.1428
> Mass = 0.000173, that of Sun being unity
> Geo. Long. at end of Sept^{r} = 321°15′ nearly, dim^{g} 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 #32 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.l) 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 calculations*[64] *that would turn up his term-sign miscue* — and then recomputed *and re-checked* this lengthy solution, *to his notoriously-perfectionist satisfaction-certain that no more such errors lurked* (and then with blithe what-me-worry?-confidence instantly frisked off to bother the Astron Royal). Of the Adams ms solutions, that in question here (Hyp 1: sections E IV-V of the mss; Sampson 1904 pp.166-7) is one of the longest (22 ms pages). A characteristic of Adams was his admirably scrupulous reworking of results: fn 59. (Which was perhaps a weakness in the Neptune race — but proved a strength that won him glorious & legitimate victory in his later lunar controversy: §I12.) From Challis' 1846/12/12 Report to the Observatory Syndicate (SP p.liv): "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 identical[65] (down to the arcsec tenths & hundredths displayed) with those Adams allegedly handed Airy 13 months previously (M16:396).

G3 Comparing Adams' published version (of his Neptune work) to his mss, reveals occasional anachronistic or temporally-uncheckable dovetailing (e.g., Sampson 1904 p.162) of material from loose ms pages that are only rarely dated. Adams' mss show that he was setting up the formulae for perturbations on 1845/11/28, 12/16, & 12/24 (Sampson 1904 pp.158, 168). The 1845/12/16 work rightly should have occurred at the beginning of the calculation of Hyp 1 (which indeed is where Adams places it in his published 1846/11/13 RAS presentation, M16:433) — not after its alleged 1845/10 submission to Airy.

[63] Keep in mind: Hyp G is not a very farout speculation, since MemoC & MemoD physically exist, in CON (#32) & the Adams Neptune papers, respectively; and Hyp G is merely: their numbers corrected for the 3 small errors noted at §F4. (Since the positions for the MemoC planet given in the table at Sampson 1904 p.152 agree closely with those I get for Hyp G's elements — given above at §F7 — it is clear that Sampson also essentially corrected the same errors in MemoC that I have noted: i.e., the small differences between MemoC & Hyp G.) The genuinely speculative part of the new theory here is: whether in 1845/10 Adams presented Airy with MemoR as he later asserted, or whether the solution given Airy in 1845/10 was actually Hyp G — i.e., effectively MemoC or its Hyp D.

[64] *A priori*, it is more reasonable to suppose that the sign-error involved was found not during the doublechecking of Hyp 1 but rather (incidentally) during the parallel calculations for Hyp 2: mid-1846.

[65] The 1690 residual disagrees by $0''.1$: $44''.4$ vs. $44''.5$; but Adams mss section E IV p.16 has $44''.45$, so this difference is merely a matter of discretionary rounding. By contrast to this unwonted Adams-steadfastness, I note that just in the 10 weeks from 1846/9/2 to 1846/11/13, Adams altered by $0''.27$ the 1843 residuals for Hyp 1 and Hyp 2. Compare M16:407 & 455. Note also that the ratio Adams used to extrapolate-compute his final (Hyp X) mean & true longitude of $315^\circ 20'$ (M16:407) is 14:11 (apt to U-N mean distance ratio 0.57), while his 1846/11/13 paper has 5:4 (apt to 0.575, near the value cited 1846/11/13). (The corrected 1843 residuals would lead to an unstated distance ratio of 0.58.) Adams' 1846/11/13 RAS paper states that this 5:4 ratio was sent to Airy on 1846/9/2, based upon the 1843, 1844, & 1845 Greenwich normal places of Uranus; however, the 1843 residuals are altered, as already noted, and the 1844 & 1845 residuals can hardly have influenced Adams' 9/2 letter since that very letter states (M16:407) that he does not possess these normals and asks Airy to send them! I suspect that a good deal of selection of material (if not worse) went on before Airy's 1846/11/13 publication of his version of the history. With the RGO Neptune file "missing", we are conveniently protected from knowing all the details of this process.

G4 The 1845/9 solution contains a small but fateful math bungle (touched upon at §F2) of which Adams was so ashamed that he never published it anywhere: the sign of a term was inverted (Sampson 1904 p.166). That is evidently[66] a cause of the previously-mentioned large discrepancy in eccentricity (0.16 vs. 0.14: §F2). Understandably believing that if this eccentricity-discrepancy were known, his claim to co-discovery would fall (& his 1846/9/2 extrapolated solution Hyp X would of course utterly collapse), Adams published only the corrected solution, alleging that it was transmitted to Airy in 1845/10. (One begins to see why post-discovery renditions are not to be quite trusted.)

G5 We now have the fact of Adams' suppression of his 1845/9 note (MemoC), and a motive for Adams' possible suppression of the 1845/10 document (Hyp G? or Hyp 1?) if it was effectively the same — *which Adams states that it was* (see above quotes from M16:429 at §F2 & fn 59). The MemoC which Adams handed Challis in 1845/9 specified geocentric longitude (see either CON #32 or Sampson 1904 p.166) — i.e., here is the spot (in the outdoor sky) to search at. But, MemoR (allegedly the 1845/10 document) does not so specify. (Why bother, after the discovery?)

G6 Airy's 1845/11/5 reply to Adams' 1845/10 submission says that it displayed perturbations (as are shown, e.g., in Adams' 1846/11/13 paper, M16:454). But the purportedly "1845 Oct" MemoR provides only residuals (M16:395-6), not perturbations.[67] A 1845/10/23 Adams letter to his parents (noting he had just failed to see Airy at RGO) is quoted at Smart 1947 p.19: "I left a note for him, however, containing a short statement of the results at which I had arrived." This tells us nothing of the Neptune celestial place imparted. But "note . . . short . . . results" sounds like brief MemoC or MemoD (Hyp D: §F6) — rather than the detailed MemoR (3 pp., hundreds of digits). Moreover, it brings up yet another troubling question: where are the covering notes to Adams' MemoR? Though signed, it is not a letter, and neither it nor MemoC contains the name of the recipient. The same is true of yet another undated Adams note, MemoD,[68] containing the MemoC data. (MemoC starts out "My dear Sir", but Adams supplies neither date nor addressee. Besides this dubious case: no Adams letter to any astronomer has been put into the Neptune record, written before 1846/9/2, M16:405-8.) Indeed, while we have (fn 68) Adams' list (MemoD) of the numbers of his erroneous MemoC, his copy of the crucial MemoR (Hyp 1) has not been found in the Adams mss (Sampson 1904 p.167). In that connection: noting that Adams' perturbational solution is for epoch 1810.328 and so must be precessed forward

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

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

[68] See above at §F2. MemoD is in the Adams papers (text at Sampson 1904 p.166; based on math which is dated by Adams at 1845/9/18); it contains virtually the same data as MemoC but has (almost verbatim: §F6) the textual language of MemoR. One notes that the $10'$ mean longitude error in MemoC is corrected on MemoD, which indicates (though it does not prove) that MemoD is later than MemoC. All of this suggests the hypothesis that MemoD is Adams' draft of his actual 1845/10 note to Airy. (If so, then Hyp G was not corrected for $30'$ of precession, as assumed elsewhere here.)

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 $329^\circ 18'$ (over 4° ahead of Leverrier's position and about 2° ahead of the real planet's 1847/1/1 heliocentric longitude), while the Hyp G orbit (which I suggest was the orbit actually given Airy by Adams in 1845 Oct) puts the planet at $325^\circ 05'$ on 1847/1/1, only 1/12 degree different from Leverrier's place![69]

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 ephemeris, where true not mean longitude is all that is relevant)[71] that the heliocentric longitude of his perturbationally-predicted planet was "325° very nearly", rather than Hyp 1's $328^\circ 34'$-$329^\circ 18'$ (1846/8/29-1847/1/1).[72] This suggests (but does not in itself prove)[73] that the famous Hyp 1 orbit (allegedly left at Airy's home on 1845/10/c.21) did not exist until after 1846/6/30 (since that is the earliest reasonable date for the existence of the MemoW just quoted). (Hyp 2 is irrelevant here since it was not completed until after 1846/8/20; see Sampson 1904 p.167.) Again: Hyp 1 is the orbit upon which Adams' claim of priority rests. The only other possible interpretation is that Adams — and Airy & Challis — were referring to *mean* longitude 325° (not true longitude), which is consistent with Hyp 1 (1846/10/6 mean longitude 325°: fn 71). But that is just as devastating to Adams' claim (not to mention implying that he, Airy, & Challis

[69] For 1846/8/29 or the epoch Adams used in 1846 (his perturbation-based solutions always used Uranus' opposition as epoch: 1846/10/6 for Hyp 2), 325° is consistent with his real 1845 solution (Hyp G) which gives $324^\circ 23'$ for 8/29, $324^\circ 36'$ for 1846/10/6 — while the crucial purported "1845 Oct" MemoR solution (Adams' wellknown "Hypothesis 1") gives values that cannot possibly be confused with 325°, no matter the rounding: $328^\circ 20'$ for 8/29, $328^\circ 47'$ for 10/6. For 1847/1/1, the 1845 Sept solution actually handed Challis, including the errors noted here, MemoC (CON #32), gives $324^\circ 22'$ — also within 1° of Leverrier's place.

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

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

[72] See also Airy's 1846/7/12 instructions to Challis (CON #4): "The investigations of Mr. Adams and M. Le Verrier having made it probable that the place of the supposed planet is not far from 325° longitude, I would propose to examine a zodiacal zone of which that point on the ecliptic is the center, with an extent of 15° in each direction from that point in longitude, and to 5° of latitude north and south." Airy then blocks out a jagged parallelogram of this description, whose acute angles are at RA 20:48 & declination -24° and RA 22:48 & decl -4°. He also comments (wrongly) that the known completed Berlin Sternkarten (Berlin Observatory) cover "a small portion" of the area, adding (7/21, CON #5): "There is only one [Berlin Hour 22] which applies partially to the inquiry." In case it helps explain the 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 Hr 22 chart's utility, Neptune was slowly sidewinding 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-proposed search area than the famous Hr 21 (1844, by C.Bremiker, also renowned for his 1856 log-tables) that made possible the swift Berlin Obs 1846/9/23 discovery. (Hr 21 added less than another 20% to what Hr 22 already covered.)

[73] See alternate possibility here following; also fns 71 & 72.

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

H Dates

H1 The question remains: when was the famous Hyp 1 list of elements (MemoR, allegedly 1845/10) actually transmitted to Airy? An examination of Sampson's description of Adams' solutions clues us. Sampson (1904 pp.165-8) finds that there were 4 distinct 1845-6 solutions:

[1] an early inferior one, then

[2] Hyp D (§F6), which (if the superficial corrections [a]&[c] of §F4 are applied) is fundamentally identical to Hyp G (§F7),

[3] Hyp 1 (§F5), & finally

[4] Hyp 2 (merely Hyp 1, repeated for slightly reduced mean distance: fn 5).

Dates appear on the Adams mss during the work for the early solution: 1845/4/28 & 5/19 (Sampson 1904 p.165); for Hyp D: 1845/9/18 (p.166); for Hyp 2: 1846/8/20 (p.167). (Adams then found Hyp X by linear extrapolation from Hyp 1 & Hyp 2, 1846/9/2.) But alone for Hyp 1, the supposed 1845/10 orbit (MemoR) and thus the key to the Neptune 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 above. 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 Eve[74] (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 letter[75] 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."

[74] 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 Sedgwick 1846/12/4 (Smart 1947 p.40 emph added): "My whole *epistolary* communication with Adams is printed in [M16] and I never saw him but twice; once [1845/12/5±1], somewhere with Challis (I totally forget where) and once [1846/7/2] when Hansen and I came for half a day to Cambridge and we were walking over St.John's Bridge. The interview on each occasion might last two minutes [§B6]. No other opportunity of seeing him."

H4 We are told by Adams (fn 59) that the 1845/9 & 1845/10 solutions were effectively identical (his "final values"); thus, his seriously improved Hyp 1 solution cannot be that of 1845/10. An integral part of the Neptune legend is that Adams tried to give his 1845/9 solution to Airy at that time but failed due to what has heretofore been regarded as (classically-mythological) bad luck (see §F1). Yet this heart-of-the-myth 1845/9 orbit was actually the 14% Solution (§F-§G), which was so flawed that Adams later suppressed it.
H5 Remember that Challis' first announcement of Adams' prediction placed it at about 1846/6 (above §C1 item [3]).

I A Cohering Hypothesis

I1 I therefore here propose the speculation that the long accepted "1845/10" MemoR from Adams to Airy was actually submitted in 1846 (perhaps as part of the 1846/11/12 material transmitted for the 1846/11/13 presentation to the RAS), and that the date 1845/10 was added later to the top of the first page of the document. How conscious Airy was of the truth, when he added the date (1845/10) to MemoR, I am not sure (since he presumably got 2 memos without date and might have confused the two). But Adams has to have known the difference. And there is a hint that Airy did, too. The published version of MemoR makes one alteration, not previously noted: where Adams spoke of the elements of the "new planet" (SP lviii), Airy edited to read just: "planet" (M16:396). Are we seeing here the caution of an experienced academic politician, one who figures that it is risky enough backdating a document, without including in it an expression which might be taken for confidence[76] — but also might look like a giveaway anachronistic slip?
I2 The question of confidence is central to Adams' claim. His failure to publish is an obvious measure of 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 own precious perturbation-based orbit! (Recall §B4: Wartmann, etc.) This raises the possibility that Adams was even at this late moment unsure of what his now-immortal perturbational work was really worth: he was a knowledgeable theoretical mathematician, but would that provide him the same grasp, of the physical reality involved, as was possessed by a seasoned astronomer such as Leverrier? (See fn 4.) Airy's praise of Leverrier in this connection has long been 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 nationalist fervor).[77] 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

[76] Accepting the 1845/9 MemoC (CON #32) as genuine, this confidence was in fact expressed by Adams (at least before finding his deflating sign-error), since his bold statement to Challis on this paper is: "The Elements of the New Planet I make to be as follows . . . ."

[77] 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 1846/9/23 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 Cambr.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 [c], above) to use Neptune for the glorification of Cambridge mathematics.

but also of a *philosophical* mind, *we have nothing which we can put in competition*. My acknowledgment of this will never be wanting; nor, I am confident, will that of any other Englishman WHO REALLY KNOWS THE HISTORY OF THE MATTER." (Caps by DR.)
I3 Such statements constitute, it seems to me, agreement (and by one in a position to know) that the key to British failure to capture Neptune was simply Adams' own inability to correctly overview-gauge the reliability of his mathematical accomplishment — and that in itself eclipses his claim of priority. Airy granted similar public concessions to Leverrier even during the famous 1846/11/13 RAS meeting at which his filtered (§B2) version of events made its then-inauspicious (but ultimately triumphant) public debut (M16:411, emph added):

> I cannot attempt to convey to you the impression which was made on me by the author's [Leverrier's] undoubting confidence in the general[78] truth of his theory, by the calmness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, "Look in the place which I have indicated, and you will see the planet well." . . . . [For centuries], nothing so bold, and so justifiably bold, had been uttered in astronomical prediction. It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician [DR: the reference to Adams is painfully self-evident — and overdone, as Airy himself later realized]; it is here that we see the philosopher. The mathematical investigations will doubtless be published in detail; and they will, as mathematical studies, be highly instructive; but no details published after the planet's discovery can ever have for me the charm which I have found in this abstract [Leverrier's 1846/9/8-submitted *AstrNachr* paper] *which preceded the discovery*.

I4 The degree of sanctity attained by Adams (vs. Leverrier) may be measured by the fact that these eloquent expressions of Airy have been almost entirely neglected by historians (unless used to damn Airy), though they are printed right in the prime published source on the affair (M16). Not yet turned by still-mounting nationalist vitriol, Airy is here expressing an honest wonder (which we who read the story after the event cannot ever quite share) at the most dazzling public-prediction miracle in the history of astronomy: Leverrier's math-deduction declaration of the place of a giant planet "which no one has yet seen" (to quote the incredulous press before 1846/9/23) — and the swift vindication of that courageous gamble by the man "who discovered a planet with the point of his pen", to quote from Paris Observatory chief Arago's unforgettably poignant announcement of his colleague's success, an event that will always remain unique in the history of the oldest science.
I5 It is a matter not merely of appreciation but of the most elementary fairness that: Adams, who published after the fact, cannot justly divide the credit due Leverrier for his daring. Moreover, the actual discovery was made due to Leverrier's 1846/9/18 letter to Galle, which Adams had no part in whatsoever. Even less can Adams be given *higher* credit than Leverrier — though this has often been the case. (Neptune's 1846 predictive location has become commonly referred to as "the Adams-Leverrier discovery of Neptune".)[79]

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

[79] Since the facts of this case have long since led me to come down on Leverrier's side in this controversy, I should say: [1] I am of U.K. extraction. [2] Everything I have seen regarding Adams' & Leverrier's demeanor tells me I would have preferred the former's company. That Leverrier (longtime head of the Paris Observatory) was extremely unpleasant to his colleagues is amply testified to. (In a devilish play on Neptune's symbol, Humboldt called Leverrier "the man of the trident".) But it is also fair to ask: was being cheated (of his proper due regarding Neptune) a partial explanation of why Leverrier *became* nasty? Against this theory: Leverrier's bad temper appears to have been reserved almost exclusively for his countrymen, not foreigners. E.g., his kindness to his equally brilliant US Naval Observatory counterpart, USNO chief Simon Newcomb (Canadian-born), extended even to his presenting Newcomb

I6 I believe we now are in a position at last to explain the Neptune scandal's inexplicables: [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; Glaisher 1896 p.xxix); [3] why he did not publish even after Leverrier's 1846/6/1 paper.

I7 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 prediscovery secrecy about Adams' work is rendered less blameworthy (though hardly blameless). Had this secret ever been revealed (before or after discovery), Adams' claim would be virtually defunct.

I8 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).

I9 Note also the number of odd lacunae in the history (especially in *continuous records*, harder to fudge): [a] No dates on Adams' mss during the key period 1845/12/24-1846/8/20, and we know nothing specific of his communication with Airy on Neptune during this time. (See §G3.) [b] Adams' 1837-1844 diaries have been used by his chief modern biographer (Smart 1947 pp.12-18), but nothing from the Adams diaries has been quoted from 1845-6. [c] No mention of Neptune in the minutes of the very RGO meeting at which the desperate search for it was launched (1846/6/29, §B1). [d] Likewise, no mention of Neptune in J.Herschel's diary 1846/6/29-10/1. [e] Adams' name does not appear in Airy's diary from 1845 Summer until 1846 Xmas (Chapman 1988 p.123), this despite Airy's (§B1) "almost desperate" drive to find Neptune due partly to Adams' calculations. [f] We are left with a grossly unacceptable (nearly total) lack of knowledge regarding Adams' activities during the most important and peculiar period (1845/10-1846/6) relative to establishing the reasons for his nonpublication, which are crucial to the credibility of his belated claim of priority (§C2). [g] Add to all this the astonishing fact that when on 1846/10/1 Challis and Herschel first brought Adams' name before the public (instantly after the discovery, and while Airy was abroad), both failed to claim Adams' work had priority (§C1).

I10 Thus, in my opinion, not only should the Adams claim be shelved until the "lost" RGO file is unshelved, but: given British astronomers' demonstrated filtering of documentary material (e.g., §B2), there is reason to doubt whether that claim can ever be fully restored to health. Our foregoing review permits us to rewrite the infamous Neptune history in 3 crucial *and related* ways: [1] There was undeniably a conspiracy to keep secret (from non-inner-circle-British astronomers) Adams' precise initial agreement with Leverrier's longitude; [2] Adams' failure to push for publication (which lost him the discovery) was primarily due to his own paralysis, caused at least in part by his sign-error in a term of the math producing the elements set forth in MemoC. [3] Adams switched solutions on the public by pretending (e.g., §F2) that MemoC and MemoR (whenever the latter was composed) were only slightly different, whereas he knew that they were so seriously discrepant that he had feared publishing anything until making further tedious checks. (If the switch extended to having belatedly substituted MemoR for Hyp G as the 1845/10 document given to Airy — the hypothesis tentatively suggested in §F6-§F7 — then that could additionally help explain much of the secrecy surveyed in §I8; however, this is but one among various possible non-mutually-exclusive alternatives, e.g., §C2 §G9, fn 67, fn 70.) The most extreme irony of the Neptune affair is the fashion in which the Cambridge conspiracy backfired: Airy thought that his privately knowing of Adams' work gave him an advantage, but Adams' unsurety about everything contributed to spreading the British effort over a huge piece of sky, while intrepid (Columbus-like)[80] Leverrier's not entirely justified confidence in his prediction's precision inspired the Berlin Observatory's Galle (not Hamletized by Adamsian vagueness) to find the planet at one poke.

I11 We conclude with Biot's sage and too-kind comments on the British Neptune disaster, reflecting a diplomatically, procedurally, & providentially correct position which should have been adopted for good as the official view, just as soon as Adams' late claim was lodged, since it would have instantly ended the necessity for investigations of the sad truths behind the Neptune Scandal (the following quotation is taken from *The Athenæum* 1847/4/3 p.371; minor DR alterations in that translation):

> in . . . 1845, . . . eight months before M. Leverrier's first announcement, the new planet was predicted by the figures of Mr. Adams, and he alone was in the secret of its celestial position. These calculations . . . were well worthy . . . of being communcated without loss of time to the scientific world . . . . Or . . . [steps] should at least have been taken to find the planet [by telescope in 1845] . . . I see . . . a young man of talent . . . . I shall say to him . . . "The laurel which you have been the first to deserve has been merited also by another, who has carried it off before you had the boldness to seize it. The discovery belongs to him who proclaimed and published it to all, while you reserved the secret to yourself. This is the common, unwritten law, without which no scientific title could be assured.[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."

I12 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.

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with an invaluable relic: the page proofs — corrected in Leverrier's own hand — of the *Annals Paris Obs* math development that underlay his famous, long-standard *AnnParObs* tables of the planets. This treasure now resides at the Johns Hopkins Univ Library. (It was somewhat damaged during decades of nonrecognition after JHU prof Newcomb's 1909 death — until DR identified it, causing transfer off the shelf & into the safety of JHUL Special Collections.)

[80] Note similarity to ‡8 §A2.

[81] 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.

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

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

Date	Uranus	Neptun	Leverr	MemC	HypG	HypW	Hyp1	Hyp2	HypX
1800/01/01	174°08′	225°07′	230°54′	235°29′	236°08′	264°46′	235°58′	236°40′	228°27′
1810/01/01	220°45′	246°47′	250°36′	252°19′	252°59′	280°07′	253°35′	255°26′	247°02′
1820/01/01	264°53′	268°28′	271°04′	270°28′	271°09′	295°28′	272°42′	275°10′	265°36′
1830/01/01	306°09′	290°14′	291°51′	289°48′	290°31′	310°49′	293°08′	295°30′	284°12′
1840/01/01	345°40′	312°07′	312°27′	310°00′	310°43′	326°10′	314°21′	315°55′	303°46′
1845/09/22	008°09′	324°45′	323°59′	321°45′	322°28′	334°58′	326°35′	327°26′	313°24′
1846/06/01	010°52′	326°16′	325°22′	323°10′	323°53′	336°01′	328°03′	328°49′	314°41′
1846/06/25	011°08′	326°25′	325°29′	323°18′	324°01′	336°08′	328°11′	328°56′	314°49′
1846/07/29	011°30′	326°37′	325°41′	323°29′	324°12′	336°16′	328°23′	329°07′	314°59′
1846/08/31	011°51′	326°49′	325°51′	323°40′	324°24′	336°25′	328°35′	329°18′	315°09′
1846/09/23	012°06′	326°58′	325°59′	323°48′	324°31′	336°30′	328°43′	329°26′	315°16′
1847/01/01	013°11′	327°34′	326°31′	324°22′	325°05′	336°56′	329°18′	329°58′	315°47′
1850/01/01	025°07′	334°11′	332°25′	330°31′	331°14′	341°32′	335°38′	335°54′	321°21′

Table 2: Geocentric Longitudes Corresponding to Key Historical Dates

Date	Neptun	Leverr	MemC	HypG	HypW	Hyp1	Hyp2	HypX
1845/09/22	323°38′	322°57′	320°39′	321°24′	334°20′	325°36′	326°30′	312°09′
1846/06/01	328°10′	327°04′	324°52′	325°35′	337°33′	329°49′	330°32′	316°16′
1846/06/25	328°00′	326°54′	324°41′	325°25′	337°30′	329°41′	330°24′	315°59′
1846/07/29	327°20′	326°17′	324°02′	324°47′	337°03′	329°06′	329°50′	315°16′
1846/08/31	326°27′	325°29′	323°14′	323°59′	336°23′	328°17′	329°03′	314°29′
1846/09/23	325°53′	324°59′	322°45′	323°29′	335°53′	327°46′	328°31′	314°02′

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