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DIO

DIO 9.1 Established French Planet Priority: *Scientific American* Dec. 2004

Recovery of the RGO Neptune Papers: Safe & Sounded

Ecliptical Traces Beneath Hipparchos' Commentary

Aristarchos' Precession: 150 Years Before Hipparchos

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News Notes

Alex Jones' Unique Discovery:

As we go to press with this issue, we learn that Alex Jones[1] (the most honest and self-assured of all professional specialists in ancient astronomy) is publishing through the American Philosophical Society (*APS Memoirs* vol. 233; 1999) a sensational find: the first extant raw Greek astronomical observation[2] from the 2nd century AD not coming through Ptolemy. Alex discovered the papyrus containing this wholly unexpected treasure in the Oxyrhynchus collection. This papyrus is not just the *first* of its kind — it may remain ***forever*** *the sole surviving example* of its kind.

Moreover, the document in which it is embedded (very possibly by Menelaos!) contains serious astronomy — relevant to checking (by observation) planetary period-relations of the sort discussed[3] in *DIO* as preferable to Ptolemy's approach. Jones recognizes the difference and politely[4] cites the views of R. Newton, G. Toomer, and DR on this issue.

The document appears to be a rare window into the long-"lost" period between Hipparchos and Ptolemy, hopefully a glimpse of real astronomers at work deriving their often impressively accurate[5] orbital parameters, not by neat geometric proofs from a minimal number of data (as indoor-mathematician Ptolemy presented things, after appropriating these astronomers' parameters) but by repeated laborious and meticulous celestial observations, which (long before Gauss) scrupulously ensured their orbits' reliability by generous observational overdetermination (fn 3). Who would have expected such a shocker from original Greek papyri? (Even from the Oxyrhynchus collection.) This discovery alone (and there will be others) ensures Jones' recognition as one of the great figures in our field.

The Ptolemy Debate:

O.Gingerich & J.Evans finally agreed to a debate on Ptolemy (U.Notre Dame, 99/7/3), once assured ogre DR would not be on the skeptical team. (Which was: *DIO*'s K.Pickering & H.Thurston.) *DIO* will print all debate papers submitted here. Evans had long refused to answer Thurston's polite written questions on the amusing Evans star-magnitude problems noted at *DIO 4.1* ‡4 (& *DIO 8* News Notes, sent to Evans, who trashcanned the whole issue without reading it). So DR asked these questions from the floor; but we *still* have no numbers from Evans. Urging live conversation & mutual understanding, DR afterwards asked Evans (former massive DR-attacker: 64pp in 1987 *JHA*!) for his phone number, home or office. He refused both requests.

The debate was so one-sided that it wasn't even necessary to bring in DR's surprise "Magnitude-Split" test [at www.dioi.org/cot.htm#msmr], which uses the major deep-south stars of any historical catalog to determine whether it was incomplete or the observer's southern horizon was blocked. If neither, the test easily finds his latitude and epoch.

[1] [Note added 2015. Jones now at NYU. On his integrity since joining *JHA*, see *DIO 16* ‡3.]

[2] Jupiter near opposition in Cnc, 104/12/31 — observed to verify an accurate period relation of 344 sidereal years, comparing the observation to a −240 observation of Jupiter by the Dionysians (from whom this journal partly draws its name, since it appears that this was the earliest known group of heliocentrist astronomers: *DIO 1.1* ‡1 §D2). The 104 datum is presumably part of a series of observations of Jupiter's entire retrograde-loop replay of its 241 BC behavior.

[3] *DIO 1.2* fn 78 & *DIO 6* fn 51.

[4] One of the many delightful ironies of the Ptolemy affair is watching the youngest able scholar in the field — teaching good manners to all his elders.

[5] *Queen's Qrtly 91*:969, *Vistas in Astron 28*:255, *AmJPhys 55*:239 n.30 (vs *DIO 11.2* cover) & n.38.

‡1 British Neptune-Disaster File Recovered

NOAO-DIO Preservation Project Succeeds
Long-Hidden RGO File on 1846 Planet-Chase: Safe At Last
Adams' Final Prediction Missed by Over Ten Degrees
Britain's "Discoverer": Perfect Simp or Conniving Babe?

French mathematical astronomer Urbain Leverrier's amazing 1846/9/23 discovery of the giant planet Neptune "with the point of his pen"[1] is the grandest legend in the long history of astronomy. From tiny, nearly invisible deviations in the orbit of Uranus, he induced by refined gravitational mathematics the position of Uranus' perturber — the then-unknown planet Neptune — and published its celestial longitude in the journal of the French Academy on 1846/8/31; 23 days later, Neptune was found telescopically at the Berlin Observatory, upon Leverrier's written request, within ***one degree*** of the very spot predicted.

Leverrier's success culminated a year of the most delicate and intensive mathematical labor (starting no later than 1845 Sept); but his 1846 glory was immediately sullied by a peculiar, entirely post-discovery-published claim of prior prediction by University of Cambridge mathematician John Couch Adams, a claim publicly backed by the ultimo Astronomer Royal, Geo. Airy. Over a century later, the Adams claim became even ichthier: soon after DR asked (1967-1969) Airy's modern successor as Astronomer Royal for permission to examine the British file on the case, the *whole file* was "stolen". We will call this long-gone key resource the "RGON" (Royal Greenwich Observatory's Neptune) file.

DIO was the first (and is still the only) journal to reveal (1992 & 1994) that the "thief"[2] was in truth the then-recent Chief Ass't to the Astronomer Royal, Australian Richard Woolley, and was Woolley's closest professional confidante (who had by this time become Director of Australia's Mt. Stromlo Observatory). All of which raised the obvious question: was the RGON file's disappearance related to possibly-embarrassing contents? (Obvious point: no one person can protect a file from unrestricted access for a century and a half — yet the ultra-sensitive RGON file has been so sheltered.)

In late 1998, the former Chief Ass't died. And, lo, the RGON file was found among his effects by Nick Suntzeff (NOAO), as *DIO* first learned from Owen Gingerich. (Gingerich, while recognizing the British establishment to be obsessively secretive, is going along with that establishment's position that the file's longtime unavailability to historians was just an accident.) British scholars were very shy of talking about the matter, but several *DIO* probes by phone gradually pieced together the situation sufficiently that we were able to approach NOAO (National Optical Astronomy Observatories) with reliable knowledge that the longlost file now resided (in diplomatic limbo) at NOAO's Chilean observatory.

Upon *DIO*'s detailed-fax 1999/4/29 appeal, NOAO extraordinarily xeroxed the file there (Cerro Tololo) and sent three copies via diplomatic bag to NOAO headquarters in

[1] The just-right appreciation declared (*Comptes Rendus 23*:660; 1846/10/5), in the afterglow of the moment of discovery, by topflight physicist & Paris Observatory head F. Arago.

[2] RGO defenders are saying he didn't really steal. (If taking a ***unique***, 32^y-sought file *for the rest of your long life* isn't theft, what is?) *DIO* holds (§L) that the Chief Ass't (formerly OSS-CIA) didn't take the file on his own. This is disputed by establishmentarians largely on the ground that he took other RGO material as well. But the timing (DB §C2) of & varying cover stories (DB §I1) for the removal of *the ORIGINALS of the Neptune file AND (§L6) the RGO list of its contents* (in a xerox era: compare DB §D10), smells like the old RGO's wish to hide the messy truth underlying its Neptune-fumbling farce. Whatever ingenious alibis & diversionary tactics are generated, the fact is: there's been no other decade-long public call for any other *entire* missing RGO file. (A call begun not by DR but by fresh RGO archivist Adam Perkins in 1987 and by Ian Ridpath, who wrote the 1988/1 *Popular Astronomy* notice quoted at DB §B.) In fairness to England, it should be noted that the former Chief Ass't (though long a high official in British and British Commonwealth astronomy) was born in the US, not the UK. [Note added 2003. He stole parts of Airy *DSB* entry verbatim from Airy autobio. More "accident"?]

Tucson, one to be kept at the NOAO library (Mary Guerrieri), two for transmission directly from Tucson to *DIO* (Baltimore and Cal Tech branches: DR and Myles Standish, resp).

The NOAO effort was not in vain. The file's contents throw crucial new light upon British maneuvering (before and after discovery) as well as the Univ Cambridge principals' fingerpointing after losing the planet to foreigners. Among the most remarkable new finds: [a] From the outset, the Royal Greenwich Observatory (RGO) had inside knowledge of Leverrier's Uranus project (fn 69), [b] Airy privately hinted that he regarded Adams' generating post-discovery alibis (for not publishing his results) as "conniving" in falsehoods (§J8). [c] Adams is reported (§H11) to have been reluctant to publish his results even many weeks *after* the discovery. [d] Adams' very-little-known large error (about 12°) in his final pre-discovery prediction of Neptune's place (a prior *DIO* revelation: §G1, Rawlins 1992W fn 55) is specifically proven (and is the subject of a regretful explanation by Adams: §G3).

In the article that follows, unashamedly-ubiquitous references to our three previous *DIO* discussions of the Neptune affair (all published before the RGON file's recent recovery) are merely intended to permit our readers' efficiently-precise consultation of these detailed analyses & data. For convenience, these publications will be abbreviated below as DA (Rawlins 1992W), DB (Rawlins 1994N), and DC (*DIO 7.1* ‡5 §A).

Notes of Thanks:

DIO's fateful 1999/4/29 faxed request to NOAO, asking for the creation of a safety-backup copy of this invaluable file — so long elusive and so often bowdlerized — was supported by prominent interested parties, e.g., Cal Tech's JPL (Myles Standish), Univ Alberta (Robert W. Smith), AAAS' *Science* (Eliot Marshall), and the *New York Times* Science Dep't (Nicholas Wade). (Standish and Marshall followed up with valuable faxes of their own. They deserve much of the credit for success in this project — which came true largely because we simply appealed to scientific institutions' best ideals and intentions.)

To our mutual delight, the safety copy was indeed made. Thus, thanks to the responsiveness and archival concern of NOAO, the RGON file has been firmly preserved for posterity. Astronomers' gratitude must go in particular to Elaine MacAuliffe of NOAO's Cerro Tololo Observatory (Chile), who swiftly accomplished a task[3] not only arduous (501 leaves sent!) but highly delicate, given the fragile state of these ancient documents.

Finally, we thank Owen Gingerich for setting aside past difficulties and sending *DIO* news of our confirmation on the file's location. Without this admirable act, the remarkable chain of recent events described here might not have occurred at all.

[Note added 1999 Oct. Thanks to current RGO Archivist Adam Perkins' openness, Nicholas Kollerstrom was able to locate & transmit to *DIO* (1999/7/24) the long-hidden central text of Astronomer Royal Geo. Airy's refreshingly frank & sardonic key 1846/12/8 letter. (Text below at §H8.) We are grateful both to Nick and to Perkins. Though proof-certain is impossible here, *DIO* is obliged to point out: [1] This letter implies (& see fn 51), that (contra *DIO* at DA §B2) Airy never knew the actual cause (précis here at §K2) of Adams' publication-delay, which cost Britain the planet. (But see fn 91. And Nick's other major find indicates Airy&Adams may've been closer in early 1846 than most historians have previously suspected: §H6.) [2] Nothing new in the RGON file supports *DIO*'s 1992 speculation that the prediction Adams evidently handed Airy in 1845 Oct was the same (miscomputed) one handed to Challis in 1845 Sept.[4] (But the DA §G items in favor of this theory still stand.) Regardless, see the conclusive point emphasized below at §E3.]

[3] This work has not only an historical benefit: Adam Perkins, the able RGO archivist, was to pick up the records to return them to England, so Elaine's work protected Perkins (providentially, it turned out) from suspicion of being connected to the RGON file's gaps.
[Happily, subsequent events (e.g., §H8) indicate that Perkins is as properly uncensorial as Elaine.]

[4] The 1845 Sept solution (CON #32 & Sampson 1904 p.166) bears a mark of reality, time, & utility lacking in the "1846 Oct" document, by appending current *geocentric* longitude. See fn 42.

A Celestial Mechanics' Most Miraculous Day

A1 The 1846/9/23 discovery of the planet Neptune is the most magical predictive-math event in the history of the oldest science. On the morning of that date, Johann Galle of the Berlin Observatory received a 9/18 letter from the great Paris Observatory celestial mechanist, Urbain J. J. Leverrier, telling Galle that if he aimed his observatory's telescope on the ecliptic in the region of geocentric longitude $325^\circ.0$, he would discover the eighth planet of the Solar System ("which nobody has yet seen", as the press was quizzically noting at the time: *Athenæum* 1846/6/13 p.612) — a jovian-gaseous giant planet whose existence and very position Leverrier had predicted by applying (partly in reverse) Laplacian perturbation-gravitation math, to the slight hitherto-untamably non-elliptical motion of the planet Uranus.

A2 That very evening, Galle and H. d'Arrest searched with the Berlin Observatory's excellent 25 cm Merz & Mahler refractor (using Carl Bremiker's beautiful unpublished Berlin Starchart of the region, which had been completed in 1844 but not yet mailed out,[5] and thus existed only at Berlin). Within a fraction of an hour, they made the discovery — at geocentric longitude $325^\circ.9$, less than a degree from the predicted position (§A1). On 1846/9/25 (after confirming the planet's daily motion and nonpunctal appearance), an understandably dazzled Galle was able to write Leverrier (Galle's emphasis): "the planet whose place you have [computed] *really exists*."

B British Seizures

B1 Instantly upon receipt of the news in England, a bold British claim to seize the new planet was promoted by John Herschel, Cambridge Observatory Director James Challis, and Astronomer Royal Geo. Airy (all three University of Cambridge men) — a trio which ultimately went so overboard that they actually had the brass to push their Brit name "Oceanus" publicly for Neptune (a name which — as independent British astronomer J. Hind snickered — had the same chance abroad as "Wellington": DA §D6). Airy was so seized by this bizarre idea that he actually wrote a (previously unknown) private letter to discoverer Leverrier asking[6] him to agree to Challis-Adams-proposed "Oceanus" — i.e., to let the British name a French-discovered planet![7]

B2 The Oceanus letter to Leverrier has got to be THE nuttiest notion of Airy's long and illustrious career; and he naturally got back an aggrieved (though polite) Leverrier letter (10/16: fn 85) attacking John Herschel, the son of Wm. Herschel, Leverrier's only companion in giant-planet discovery. This was followed by an enraged 10/19 Leverrier letter on hunkering[8] Challis' ineptly contradictory supernationalism at home, simultaneously with diplomacy abroad; Leverrier: "*blanc* en France . . . *noir* en Angleterre". (DC showed that, almost a half-century after the discovery, Astronomer Royal Wm. Christie was still urging censorship of Leverrier's letters, which are only now finally available unfiltered.)

[5] The Berlin Observatory (whose telescope was a twin to that of Dorpat) was saving money by mailing out the high-quality Berlin Sternkarten (a project initiated in 1825 by the immortal Bessel: fn 80) only in pairs, and no other charts had come to completion in the 2 year interim (1844-1846).

[6] Letter in RGON file, Airy to Leverrier 1846/10/14 p.3: "If you would consent to adopt the name Oceanus instead [of Neptune], it would, I think, be better received" being more similar to Uranus. But did Airy ever tell Leverrier that Adams-Challis (whose claims and very names Leverrier was by then understandably incensed over) had thought up this sea-faring-nation name for his planet?

[7] Hey, it worked in 1930 for US-discovered Pluto — which was named through H. H. Turner, another Chief Ass't to the Astronomer Royal. Of course, this was just a diplomatic ploy — highly agreeable to the Lowell Observatory — to merge Percival Lowell's initials (instead of rival claimant Wm. Pickering's) into the acronym of the planet.

[8] See fn 82 and DA §D2.

B3 The French reacted to "Oceanus" with Paris Observatory chief Arago's 10/5 announcement of his determination henceforth to call the planet "Leverrier" instead! Having made this reactive choice only after "Neptune" (the name Leverrier himself had originally given to his planet) had already been entered into the official records of the French Bureau of Longitudes, Arago went to far as to personally (in his distinctive hand) alter that record so that it no longer seemed to pre-empt the later decision.[9] This is an instance of an honest person believing that only by counter-dishonesty can he fend off theft.[10]

B4 Between the planet-deprived Brit public and the apoplectic French, Airy was under such intense siege that he and his family obviously feared for his job.[11] His brother Wm. Airy even nightmared (§J5) over the spat. (See the 'til-now secret Airy-Adams war: §J.)

B5 The all-Cantabridgian circle of §B1 alleged that the extremely talented young Cantab mathematician John Couch Adams had in 1845 made the same prediction as Leverrier, but — *oops* — the elements had *somehow* never gotten published during the **year** since. (This *somehow* has turned out to be ***some*** how-to-explain.) It soon came out that the Cambridge Observatory had (*in secret*)[12] been massively searching after the planet for months (working towards an ultimate triple-sweep of 300 square degrees of sky!),[13] partly on Adams' private — again, unpublished — instructions (§E), with Director Challis working nightly (heading a three-man team)[14] at the eyepiece of the Observatory's 30 cm telescope (largest refractor in England, established in 1835 through the influence of John Herschel and Airy — see §I1), according to a plan allegedly discussed (again, entirely *in private*) at an 1846/6/29 RGO board meeting in which Airy, Challis, & Herschel participated. (Nothing of this was entered into the official minutes of the meeting.)

B6 *DIO* has taken the hotly Brit-resented position that Adams should not be recognized as co-discoverer because of the secrecy he (and his Cambridge circle) deliberately maintained throughout. Among other considerations: acceptance of a nationalistically[15] inspired post-discovery claim-jump would set an intolerable precedent,[16] converting the issue of credit for research-advances into merely a question of who has the most political power. (Too often the case anyway.)

B7 Adams was said to have deposited with Challis (1845 Sept) & Airy (1845 Oct) the orbital elements for an unknown planet, the contended Adams result known as "Hypothesis 1" (which we will abbreviate as just "Hyp 1") — a planet of mean distance 38 Astronomical Units from the Sun (over 1/4 too large) and in the eastern part of the constellation Capricorn (about where Neptune really was). The whole Brit case for priority is falsely (fn 20) hung on this single document. Problems with the Hyp 1 legend: [a] Neither 1845 document was dated by Adams (hardly the behavior of someone who believes he's lodging an immortal prediction!) — and no cover-letter addressed to either party has ever been produced; thus, both documents were dated (year & month, without precise day) only later, and in the *recipient*'s hand (DA §§C7-C8). [b] All continuous records of these late-1845 to mid-1846 events — Adams' diary,[17] Herschel's diary (photocopies sent DR by J.Moll, C.Henderson, C.Cordova, U.Texas Austin), Airy's diary, Challis' diary, RGO minutes (§D4 item [b]), RGO visitor's book — are either blank (on Neptune) or missing (DA §I9). [c] The solution given Challis in 1845 Sept was not Hyp 1 (fnn 48&62 and DA §§F1-F2; and see fn 4). [d] At the time of discovery (1846/9/23), Adams was in fact pointing Challis over 10° to the west of Neptune's actual position. (See below at §G; details at DA §E8.)

B8 British excuses for Adams' nonpublication & non-reply to Airy's nice 1845/9/29 & 1845/11/5 notes of reception of Adams have been as varied as ridiculous-cover alibis always are: Adams didn't like to write letters[18] (but he and Airy were both at the 1846/2/13 Royal Astronomical Society meeting, so Adams could have spoken to Airy then, or 1845 Dec or 1846/7/2: see fn 68 & §B11 [also §H6]); he was shy (though not too shy to help publicly promote the Cantab name for the planet, "Oceanus", *after* the discovery: fn 82 & DA §D6); he procrastinated (Challis to Airy 1846/12/19); he was a naïve simpleton (§H12); he (by bad-luck) accidentally missed seeing Airy at the Royal Greenwich Observatory in 1845 Sept (DA §F1); he was (fn 89) disappointed at not talking with Airy — a wholly mythical "snubbing" — when he returned in 1845 Oct (see Airy's irate reaction to this "rank fib": §J8); Adams just-missed (yet another *oops*) an 1846 Sept BAAS meeting[19] at which (he later stated) he was planning to announce his results (yet, in fact, even weeks ***after*** the discovery Adams was reluctant to produce his numbers: see §H11 below).

B9 Every one of these alibis was easily exploded in DA (e.g., §D1). But such amusing exercises in apologia should not divert scholars from the true, crucial, and exceedingly elementary cause of Adams' nonpublication (which he himself wrote out, in a little-noted passage: DA fn 5) — he had simply *not completed his math*:[20] **the** central reality of

[9] Bureau des Longitudes, Compte-rendu 1846/9/30 p.3. The alteration made it seem as if "Neptune" was merely discussed that day as a possibility. A photograph of this document was sent to DR by the Bureau des Longitudes on 1967/9/26.

[10] Deceit (e.g., §I1) frequently produces a reflection. It's rather as if dishonesty meets one of the tests of life: reproduction. For a case similar to Arago's, see *DIO 4.3* ‡12 fn 4. (See also below: ‡3 §F7.)

[11] At age 13, Airy had seen his father lose his job. Rob Smith has made the sort of observation which epitomizes historians' superiority to scientists in some areas: alot of commentators have found it hard to believe that lordly Geo. Airy would be as frightened as he was over the Neptune fracas. But Rob just made a deft perspective-shift-observation and commented: Airy had not *yet* achieved his unique status (by which we see him today) as the greatest of all Astronomers Royal.

[12] Hind knew of Challis' search by mid-Sept (CON #10), but not its double-prediction cause.

[13] Myles Standish asks one of those common-sense questions that can get overlooked by scholars who get too bogged down in details: the Brits were alone in the secret that 2 math analyses were pointing to the same part of the sky, so why was the Brit search so tediously wide while the Berlin search was so efficiently narrow? (The answer is key: Adams was pointing to various parts of the sky.)

[14] Challis, a Mr. Morgan, & RGO's off-the-books loaner James Breen. The official pre-*DIO* record had Challis refusing RGO help. See Smart 1947 p.27 and M16:403 (Airy) & 404 (Challis) vs DA §B1. It is a famous part of the British Neptune tragedy that Challis in 1846 Aug accidentally stopped comparing stars (in two lists of the same slice of sky) after 39 stars when the 49th "star" was Neptune. What has not up to now been asked is: why was Breen not doing this work, promptly, for all stars? Challis' previously unpublished 1846/8/7 letter to RGO begs for the help of Breen (an able computer) in the search, so: why didn't Breen (upon his mid-August arrival at Cambridge) compare every star taken so far? Partial answer: one of the double-lists containing Neptune was a small thing, with just a few relatively bright reference stars — and Challis wasn't expecting an object nearly as bright as Neptune proved to be. (A deep planet's dimness is about proportional to the distance's fourth power.) But the other star-list with Neptune on it was full, so Breen should've been rapidly comparing it & all other data right away. Question (suggested by Challis' hogging the eyepiece, too): was Challis lowering the efficiency of the search in order to make sure that he made this discovery himself, both optically and cartographically? (But note the implicit integrity, too: alot of project-chiefs would let others do the work and then grab the credit anyway. See, e.g., *DIO 4.1* ‡2 fn 39.)

[15] See DA §§A8&D8. Brit fervor for Adams was particularly unseemly in a toppe nation. (Similar to, e.g., current US madness to be first in every sport at every Olympics. When one is the richest nation on Earth, some modesty ought to be in order; however, the process of getting to the top in the first place evidently leaves a residue of pushiness that continues long after its need has passed.) Had Adams been, say, Bulgarian (i.e., without the power of Britain behind him), one doubts that anyone in Britain or elsewhere would have paid much mind to his peculiarly late claims.

[16] See Biot's common sense at DA §I11.

[17] [Kollerstrom, with St.Johns archivist Jonathan Harrison's help, has just found a 1846 March piece of Adams' diaries (JCASJ 20/22/3); but it has nothing on the search for Neptune. See §H6 & fn 73. Adams left dated memos (e.g., fn 81) which could be selected excerpts from now-missing diaries.]

[18] See Adams to Airy 1846/11/18 p.4 (RGON).

[19] BAAS Pres. J. Herschel recollected (DA §D2) he'd mentioned (9/10) the planet prediction at the BAAS meeting; but (*oops* cubed) he didn't cite Adams. Adams thought (M16:408) of giving a paper but instead "stayed up late" calculating 9/10 (JCASJ 20/21/4). *Oops*^{4}. Challis skipped his own scheduled paper (*Athenæum* 1846 p.963). *Oops*^{5}.

[20] See §H5 and DA §I7. (Also Airy's too-late lament at M16:414.) Adams-defenders point out the resemblance of Adams' alleged 1845 Autumn predicted longitude to Neptune's — without recognizing:

the Cambridge failure to net Neptune, which so unambiguously shoots down the whole claim (of British priority) that no Brit mythmaker is willing to face it. (In answer to the natural question of why Cambridge didn't discover the planet in 1845, Challis [SP p.li, his emph] doubted not "the evidence . . . of the *existence* of the planet, . . . [however] its position was determined but roughly, [thus] . . . a search for it must necessarily be long and laborious. . . . consequently I had no thought of commencing the search in 1845, the planet being considerably past opposition at the time[21] Mr Adams completed his calculations.")

B10 Most historians have bought at least partly into the mythology of non-publishing Adams as prior co-discoverer, a legend that can only be maintained by blaming Challis and Airy for discouraging hero Adams through lack of understanding and initiative. (None of which explains why Adams himself didn't publish or announce; after all, he delivered a paper at the Royal Astronomical Society in 1846 April, but on an entirely different subject than Uranus & Neptune.) This has left utterly illogical gaps in the history. (Gaps we now learn Challis explicitly stated 1846/12/19 to be a good idea: see below at §H.)

B11 Nor has the secrecy of Airy and Challis ever been creditably explained. (Obvious actual explanation: Airy hoped[22] to bag Neptune with the telescope he got for Cambridge (§B5), using the Cambridge-only secret[23] that *two* math-astronomers had pointed to nearly the same spot in the sky. See DA §B5.) Upon publication of Leverrier's first rough prediction-estimate of Neptune's place (1846/6/1), Airy wrote him (6/26), but didn't mention Adams' name! At the time (1846 July) Airy was launching his vast secret sky-search for Neptune, the leading theoretical celestial mechanist of the world (P. Hansen) lived for 3 weeks in Airy's home: no mention was made of Adams. When Airy & Hansen were out walking and accidentally bumped into Adams at Cambridge 1846/7/2, Adams said nothing of his work, even though equations of Hansen (explicitly identified by Hansen's name, in Adams' hand) are right in Adams' Neptune mss. (See DA §B6.) Nor did Airy speak up (on 7/2, or at any other time during Hansen's *3 week* stay at Airy's home: DA §B6), though *just 3 days previously*, Airy had told the RGO board (§B5) that Adams' work co-justified a vast search by the biggest telescope in England! (Tell me nobody was conspiring here.)

B12 But, even before consulting original documents, DR in the 1960s had already seen enough oddities in the traditional saga that when he wrote RGO asking to see the Neptune papers, he felt obliged to express honestly his (non-adamant) skepticism. (See DB §D6.)

B13 Though David Dewhirst of Cambridge openly sent the whole (extremely revealing) Cambridge Observatory Neptune file (CON) in 1967 (DB §D), the RGO's reaction was sinuous and cagey. (E.g., in RGO's correspondence with DR, letters on Neptune were on private stationery, while all other letters were on official RGO stationery: DB §H1.) Details below at §C3 & fn 29. The eventual upshot: the RGON file was never forthcoming. DR had (1966-1972) spent a large amount of labor in research, math,[24] and writing (ordmag 100 pages) towards a book on the Neptune case, a project that ultimately atrophied while he waited patiently and naïvely through two years of RGO sham.[25] (So a high RGO official's hiding of documents stole not only the Neptune papers for many years — but also destroyed a book permanently. Not trifling misdeeds.) Ultimately, RGO in 1969 said the file was lost — without mentioning what DR only began hearing later, namely, that it had been stolen. Later yet, DR learned that the "thief" was extremely close to the Astronomer Royal.

B14 In DA & DB, *DIO* revealed this person's name & high RGO position and published our (unreplied-to) letters requesting his assistance with the RGON file.

C The RGO Neptune File's Startling Reappearance

C1 In the early part of 1999, *DIO* received word that the stolen Royal Greenwich Observatory RGON file on the 1846 discovery of Neptune had been recovered. Despite several knowledgeable parties' reluctance to discuss the matter, it became clear that: the very person fingered (§§B13-B14) by *DIO* as having taken the file had died in Chile — and the file had been found in his home.

C2 As noted (§B14) by *DIO*, this person had been (roughly when the RGON file disappeared: in the 1960s) the Chief Assistant to (and top confidante of) then-Astronomer Royal Richard Woolley — the very party DR had asked (in 1967-1969) for permission to see the file. (We note that — over more than a century of time — no less than FOUR[26] Astronomers Royal have been associated with suppressions[27] of material in this file. But

Adams knew (and explicitly stated: DA fn 5) that since all his 1845 work was based on an *assumed* mean distance for the disturbing planet, he had no "satisfactory" solution until 1846/9/2. (His Hyp 1 mean distance was 38 AU, very different from his final value and from the truth — though, in extenuation, see Rawlins 1970G.) I.e., **he** believed he couldn't reliably predict Neptune's longitude until he had *repeated the work with a different mean distance* (diminished by about 3%) in order to discern two crucial trends: [i] Did this alteration improve or degrade residuals' rms? [ii] What would be the alteration to the all-important *longitude*? — THE answer which telescope-searcher Challis had to receive. But Adams did not conclude work on this until 1846/9/2 — when he got it quite wrong. (Letter to RGO: see §G1. The result, which DA called "Hyp X", was over 10° off.) This was sent to RGO right **after** Leverrier had published his analogous distance-variation-based solution (1846/8/31), the longitude for which was correct to within one degree. Apologists' obsession to promote Adams' proximate 1845 Autumn solutions (as some kind of priority) ignores not only Adams' own nonexcitement (§B7) at this latterly-aggrandized moment (he didn't even note the event in his diary: §D3) but also the little-considered fact that Adams had been developing lots of solutions (both before and after the 1845 Autumn ones), hugely swinging *35 degrees* in longitude, from about 315° up to about 350°. If we only count the solutions he *actually handed to Challis & Airy*, the range is still over 20°: from 315° (Hyp X, 1846/9/2) up to about 336° (Hyp W, 1846/7/20). Thus, Adams' *lodged* predictions covered a range ***more than 20 times larger*** than Leverrier's under-1° error in Neptune's predicted longitude. (See DA §F3 and its Table 1.) True, Leverrier's final 1846/8/31 limits had a range of 14°: longitude 321° to 335°, Leverrier 1845-6 p.436. But this bracket resulted from testing a coherent theory, so it is not comparable to Adams' years of diffuse and somewhat disjointed solutions, each of which had its own unstated imprecision. I.e., if we wish to include solutions' formal errors, this will inflate Adams' already-huge uncertainty-range by ordmag ten more degrees. Analogously, after the 1930 discovery of Pluto, Wm. Pickering also (for post-discovery-promotion) rather shamelessly selected the in-hindsight-better-looking among his years of various disparate predictions of trans-Neptunian planets. Since he even mingled pieces of incompatible solutions, Pickering's post-discovery self-advertisement was less creditable than Adams'; but, at least his predicted data were published before planet-discovery.

[21] An undated Adams memo (JCASJ 20/23/2) similarly explains his not answering Airy's 1845/11/5 letter. [Extant 1845 Autumn Adams outdoor star observations (JCASJ 20/22/1) are nowhere near Neptune.] Yet Galle found Neptune roughly the same time of year. But, unlike the 1845 Adams, Leverrier had a precise conclusion. [See also fn 74 & fn 94.]

[22] Perhaps not only for glory but to atone for his failure to push Adams' math along earlier on.

[23] An oddity about Adams' silence that bothers DR & Standish: yes, one can explain his nonpublication as due to his unsurety. But: wasn't it also awfully convenient for Cambridge? His staying quiet gave a terrific probabilistic advantage to Cambridge in the planet-search. Was Adams *asked* (by Challis? fn 67) to stay quiet about Hyp 1, with reward promised for cooperation? Tempting theory; but, it alone can't explain the §E2 evidence against Adams having had confidence in Hyp 1, assuming it even existed yet: §D4 item [a]. And, if he had a firm solution but kept it back to help Cambridge capture the planet, then why be so slow to commit to any numbers even after the discovery: §H11?

[24] Some appears in Rawlins 1970G and DA.

[25] It was sham by even the present RGO archivist's *least*-discreditable version of events: §C5.

[26] Geo. Airy in 1846 (DA §B2 and below), Wm. Christie in 1893 (DC §§A3-A4), Harold Spencer Jones in 1946-1947 (DA fn 34), and R. Woolley in the 1960s (see §C3 and DB). Note: both RGO archivists Philip Laurie and Janet Dudley correctly identified (privately) who had stolen the file: DB §B6. Are we to assume that they knew — but that the thief's closest confidante Woolley didn't know and (assuming Woolley *genuinely* wanted scholars to see the stolen material) wasn't able to contact **his own chief subordinate** and order him to: put the file back PDQ? Note that Woolley's having deputed his Chief Ass't to get into the RGON file was indicated by the latter's testimony in 1996: see §C7 item 4. Compare to DB §I1.

[27] Despite over a century of high-level efforts (fn 26) to hide or censor the RGON file, most Brit officials close to this case echoed (at least until 1998) the RGO 1967 line that there is nothing-new in

in fairness, we should repeat our DC §A8 note that virtually all British astronomers are innocent of any kind of censorship in connection with the Neptune case.)[28]

C3 Throughout an extended 1967-1969 correspondence, RGO gave out a series of conflicting stories: at first, RGO said it couldn't find the file, then said it was making a list of "letters for examination" (but the lister was ill: fn 29), finally after two years of delays reverting to again claiming it couldn't find the file. (Full phantasmagoric exchange printed in DB. Central oddity stressed here at §C5.)

C4 On 1999/4/14, DR told the current RGO archivist, Adam Perkins (University of Cambridge Library), that it looked like the RGON file had disappeared just about the time known-skeptic DR had asked to see it. Perkins replied that it was believed that the file had disappeared in the early 1960s instead. I pointed out this timetable's contradiction of what P. Laurie, Perkins' own predecessor at RGO, had told DR (in detail) in 1967.[29] Perkins said that, well, Laurie was not being entirely truthful.

C5 DR's reaction: if the only way the RGO archives can deny a contra-DR coverup (i.e., deny that it indeed possessed the RGON file *even while* keeping it from skeptical DR) is by indicating that it was *itself* not being honest in 1967, then: may DR perhaps be excused for not taking the RGO's entire performance[30] quite at face value?

C6 Analogously, the former Chief Ass't to the Astronomer Royal, who was hiding the whole hefty RGON file in Chile — about 500 pages! — along with some other filched RGO archival material, was in 1993 asked[31] by *DIO* for photocopies of the file. His refusal to reply was duly published[32] ("Chile Nonreception") in DB §H.

C7 *DIO*'s two letters to Chile went cc to Ian Ridpath and Charles Kowal. Ridpath followed up and got an e-mail reply from the former Chief Ass't, which Ridpath relayed to *DIO* on 1996/8/26. Since the standard Brit alibi (for the 3-decade hiding of the RGON file) will undoubtedly be of the lone-crazed-Chief-Ass't stripe, we will here quote extensively from the message to Ridpath. Note especially several indicators of the writer's detailed memory — as well as (item 4) his Neptune-involvement's instigation[33] by RGO:

> All I can recall . . . . about the historical files at Herstmonceux [RGO]:
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> 1. They were moved from Greenwich and spread out in several locations at the Castle [Herstmonceux] . . . .
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> 2. Some were in the attic . . . . spread out in piles . . . . The roof leaked . . . . I remember one particular pile because a few letters on top pertained to British reception of Einstein's theories and there was a suggestion that the opposition to Eddington's nomination of Einstein for the Gold Medal of the Society was motivated by other than scientific considerations.
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> 3. No one was officially in charge of these files but the solar observer, Phillip [sic] Laurie, had a personal interest. . . . Eric Forbes . . . worked with Laurie.
>
> 4. As Chief Assistant to the Astronomer Royal, I was requested to prepare biographical and historical material from time to time. These involved a biography of Airy and other British astronomers. Laurie supplied me with data from files for these. I recall his showing me a manuscript that had obviously been incorrectly included in the Maskelyn [sic] file, that proved to be one of the earliest attempts to solve the orbital elements of an eclipsing binary. This was later published in the *Observatory* magazine.
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> 5. My own contributions to the files were (a) some miscellaneous material supplied to me in correspondence with Airy's granddaughter and (b) my interest in the relations between Newton and Molyneux which were held in the Portsmouth Public Record Office and transcripts of which I gave to Laurie.
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> 6. The direct answer to your question[34] concerning the Neptune papers — my knowledge of this matter is based on notes[35] given me by Laurie for (a) a review of a book[36] on the subject by M. Grossner (?) and (b) a biography of Airy.
>
> 7. Certainly Laurie, if he is still alive,[37] can help you infinitely more than I can.

C8 The former Chief Ass't's very coherent[38] denial that he had the RGON file satisfied some. But DC regarded the reply as suspicious, since no response (not even to top Brit officialdom: DC §§A7-A8) had been forthcoming while Laurie lived. So *DIO* proposed to 2 different Brits that the next request to Chile should be for: a copy of the purported Laurie notes. (Suggestion published at DC §A7.) That is where the case was left until it was proved directly (by post-death search of the denier's home) that the foregoing was a bluff: texts of the inquiries he was ducking, as well as his denial that he possessed the RGON file, were evidently found along with the file. We now turn to the hitherto unknown or unconfirmed material in the RGON file itself, the most important parts of which relate to the credibility of the standard excuses (§B8) for Adams' crucial post-1845/11/5 silence.

D Key Adams Document #1: Memo R

D1 One of the two long-lost physical bases of Britain's priority-claim has been recovered in the RGON file. (How it ended up in Chile is a tale that will surely be re-told as long as there are astronomers in our Solar System.) This famous document has been called

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the RGON material. (See fn 29. And DB §D4.) They also affect total neutrality in the matter, which I learned through another source was purely thespian. (See *DIO 2.1* ‡1 §M4.) I was told that the very mention of DR in certain British presences chills the air.

[28] However, it is also unfortunately true that England has not shown much interest in publicizing the Adams legend's manifold peculiarities. E.g., our thanks to the British Astronomical Assoc (DC §A6) were premature: it turns out that the heretical speech there cited was never mentioned in literature sent to BAA members. (BAA guru Patrick Moore cannot abide unorthodoxy in this matter; of course that lamentable circumstance has nothing to do with serious British astronomy.).

[29] See §C3. DB §§E10-E12 (RGO archivist P. Laurie to DR 1967/9/8): "Just after I wrote you at the end of May, Mr. Rickett, who was dealing with the manuscripts, suffered a stroke from which he has not yet recovered. He had drawn up a list of letters for examination although these, I fear, do not appear to contain any new material. I shall try to give these my undivided attention in the near future and send you notes on their contents."

[30] Möbian-merry-go-round summarized at §C3 and DB §§H1&I1.

[31] DB §H13.

[32] Since others were too timid to publish his name in the file-theft connection while he was alive and powerful, *DIO* did so both in DA & DB. Now that he is dead, we can safely leave to the very same folks explicit perpetuation of his infamy in this connection.

[33] Friends of the former Chief Ass't are downhearted that happy memories of his considerable positive characteristics (and kindnesses to colleagues) are being besmirched by revelations that he hid the RGON file for a third of a century. In amelioration of this seriously contra-academic deed it should be stated: *DIO* has argued (§L, §C7 item 4, DA §C5, & DB §I) that he did not do so on his own authority. (Ironically, the theft may have had an eventual helpful upshot: NOAO's openness might not have been fully matched had the papers turned up in England under traditional auspices.)

[34] Note by DR. The question (published in DB §I1 item [f]) which he was answering was: how could the former Chief Ass't have — in a 1971 article — cited letters only available in a file that had (DB §G3) disappeared in the 1960s?

[35] [Note by DR: Catch the resemblance to Laurie's 1967 words (fn 29) — which the former Chief Ass't had recently received via *DIO* (DB §E12; 1994).]

[36] The book was *Discovery of Neptune* by Morton Grosser (Harvard 1962). The review (probably the most perceptive anyone produced, as noted at DA §E7 & DB §H8) was published the following year by *Sky&Tel* (1963/4).

[37] Ridpath notes that Laurie died in 1983.

[38] As Ridpath remarked at the time.

"Memo R" for convenience throughout *DIO*'s analyses. Memo R contains the orbital elements of Adams' Hyp 1 (§B7), which is the core of the whole Adams controversy.

D2 DA §C7 was first to point out the oddity[39] that the only date on Memo R is in Airy's hand, not Adams'. (No cover letter survives for Memo R, nor has Adams' card survived: §D4.) The 1845 Oct date now on the document has to have been added later, since on the date[40] he received Memo R, *Airy must have known what day it was.*

D3 Which puts him one up on Adams, who didn't! Even though Adams was (unlike Airy) absolutely certain to have been in physical contact with Memo R on the very day of the sacred event of its Deposit at RGO, nonetheless, his newly available 1846/10/15 letter to Airy can only guess (p.2) that it was "about"[41] 1845/10/20. Upon our careful consideration, this statement tells us a good deal: [a] This allegedly-epochal event was not entered in Adams' 1845 diary (now conveniently missing: DA §I9) — or indeed into any other continuous record of events. [b] Clearly he reasoned out a rough date from his other dated documents at hand (e.g., the dates of his post-vacation travel from home to Cambridge). What does it tell us about the import (to Adams *himself*, at the time) of the deposit of The Historical Document (1845 Oct unknown-planet elements, purportedly Memo R, containing Hyp 1) that: Adams wrote down the date of his vacation-end but not of his grand document-deposit? — which he later pretended was meant as a kind of "publication".[42]

D4 We conclude this section with two further items in connection with the date of Airy's receipt of Hyp 1: [a] The Hyp 1 math mss (JCASJ W.16 §E IV-E V) leading to it comprise the sole mature Adams perturbational solution on which there are no dates (DA §H1) — except, that is, for a portion which is dated 1845/12/16 (DA §G3) some months after the solution is supposed to have been handed to Airy. (See fn 20.) [b] The Airy home's wispy attestation to 1845 Oct receipt of the immortal Memo R is found in a hitherto-hidden RGON letter, Airy to Adam Sedgwick, 1846/12/8 p.1 (emph added):

> I have no doubt that the facts of Adams' [alleged 1845 Oct] call were as you and Adams have made out. My wife seems to have a notion of his card[43] being brought to her in my absence, but *nothing further is known.* [DR: See §B7 item [b].] I was at the end of October 1845 busy almost every day at the Gauge Commission,[44] and on October 29 my boy Osmund was born. — I am ashamed to mention these things. . . .

With such squishy, gappy testimony in witness to the glorious (if undated: §D3) imparting of Adams' 1845 Oct solution, well — little wonder the full RGON file has never previously been seen by anyone but Cantabs and Astronomers Royal.

[39] For further time-line problems, see *ibid* §G3 & fn 65.

[40] Generally understood to have been a bit later than the date of Deposit. Airy was uniquely notorious (see DA fn 36) for writing dates on anything that came near him.

[41] Adams originally said (RGON 1846/10/15 to Airy p.2) "about the 20th of October 1845". A later Adams memo (newly-found in JCASJ: fn 21) makes it Oct 10 (which, if true, relates to DA §G1). In any case, Adams' recollection of this *central* date is revealingly infirm. Smart 1947 p.19 computes Oct 21 from an Oct 23 Adams letter (JCASJ 16/2/3), describing the Great Day as simply: "Tuesday".

[42] See §K1 & fn 93. The ungrandiose actual purpose of Adams' 1845 Autumn trips to RGO is induced at DA fn 48. (Note: RGO's optical ability to search for Neptune was far less than Cambridge's. See also here at fnn 4, 69, & 74.) If one takes seriously Adams' claim that he was thereby attempting to register (WITHOUT DATE) his discovery for posterity, one is eventually reduced to pleading the client as temporarily inane (Sedgwick to Airy 12/6 p.2, emph added): "Adams tho' a great philosopher in his way [DR: i.e., when Brit-claim-mythology wants him to be brilliant (which he was)], has shown no worldly wisdom — and has acted like a bashful boy *rather than like a man who had made a great discovery*." See also §H12.

[43] Note by DR: Adams gave card & message (fn 89) and "a note for" Airy (JCASJ 16/2/3), vs §D2.

[44] [Nick Kollerstrom (1999/7/24): "I think it's a shame your *DIO* articles never mention the driving passion in Airy's life, *viz.* railway trains."]

E Key Adams Document #2: Memo W

E1 Besides Memo R, the other vital document traditionally brought forth to support the Brit claim on Neptune is an 1846 July document — called "Memo W" by DA — which Adams computed for Challis, to tell him where the Cambridge Observatory telescope should be aimed, to find Neptune. (The full data of the bottom half of Memo W are published for the first time here on p.14: Table 1.) Smart 1947 (p.31) is typical of pro-Adams histories in quoting Challis' argument for Adams, based on Challis' having seen Neptune on 1846/8/4&12 "entirely [due to] my having, on those days, directed my telescope towards the planet's theoretical place, according to instructions given me in a paper Mr. Adams had the kindness to draw up for me."[45] The cited document is Memo W.

E2 However, after analysing Memo W (THE key document[46] revealing the truth behind Adams' nonconfidence and the attendant British confusion on Neptune's place), DR was amazed to find (DA §B4) that it is ***not*** based upon Hyp 1 as Challis had certainly implied (§E1) and therefore all pre-*DIO* historians *had always quite naturally assumed.* (Challis had said at M16:421 that Memo W was "entirely from theoretical data", but he coyly hadn't said *whose* theoretical data: fn 47.) According to the up-to-now-accepted history, Hyp 1 was Adams' theory at this time. But Memo W is instead based on a simple 38 AU ***circular*** orbit (which Adams had indeed once used, but long before his now-famous elliptical Hyp 1). This document's circular calculational basis was induced in DA fn 21, and a full circular-orbit check is provided here in Table 1. (DA fn 19 also notes that this computed ephemeris' central time epoch is based on the Wartmann object we will briefly take up here in §F3, which Challis denied Adams used: §F4.) And all its elements ***and*** limits are basically[47] those of Leverrier's *very* recent 1846/6/1 publication. Further: Adams in Memo W stated that his math-derived planet is at 325° "very nearly". But *DIO* has earlier pointed out (DA §G9) that, for Adams' chosen 1846/10/6 math-epoch, this is the longitude not of the legendary Hyp 1 (329°) but of later-Memory-Holed math-mistake-based[48] Hyp G (325°).

E3 Thus, either Hyp 1 did not yet exist, or Adams had no confidence in it. But the point is that *either* interpretation is lethal to the Adams Hyp 1-priority myth.

E4 In Table 1, we compare Adams' figures (A) to orbits generated by *DIO* from elliptical Hyp 1 (H) and from a circular orbit (C). (Both the H and the C orbits have Bodean mean distance: 38.4 AU; θ = true longitude at 1846/8/29 Greenwich Mean Noon.) One doesn't even need statistics to see that the circular orbit (C) fits far better[49] — an unevadable confirmation of *DIO*'s theory that Adams' (supposedly 1845) Hyp 1 was not his actual belief even as late as mid-1846. Note that in Table 1, the disagreement (of Hyp 1) with the ephemeris grows greater the farther one gets from opposition, mostly because elliptical Hyp 1's heliocentric distance in 1846 was only 32 AU, not 38 AU; so, the parallactic effect becomes increasingly pronounced the greater the time before or after opposition.

E5 Since Adams' allegedly-1845 Memo R was the result of long & famous effort (detailed in Sampson 1904 and Smart 1947) at going *beyond* a circular-orbit to find an ellipse (Hyp 1) which fit the Uranus data much better, it is peculiar & suspicious that (it turns out): as late as mid-1846, Adams still had confidence only in a circular orbit.
(Recall DA §C1: Challis' 1st public claim for Adams put Hyp 1 not before mid-1846.)

[45] SP p.liii or p.6 of the original published report (copy in RGON), 1846/12/12..

[46] Memo W was transmitted by xerox to DR by Cantab David Dewhirst, 3 decades ago.

[47] Leverrier 1845-6 (p.917): heliocentric longtude $325° \pm 10°$ (epoch 1847/1/1). A copy of this statement survives in Challis' hand, partly in French (CON #34, therefore adjacent to Memo W, which is CON #35[a]). The *only* distinction between Memo W's and Leverrier's elements & limits is epoch (Adams' was 1846/8/29): a trifling $1°/2$ constant mean-longitude difference.

[48] See DA §F2. Hyp G (DA §G) was handed Challis in 1845 Sept. Not published by the principals.

[49] The circular orbit residuals (C) are smaller than the Hyp 1 residuals (H) by factors of 2 in decl & 3 in R.A. (A slightly smaller radius can improve C's fit to Memo W; roughness perhaps from misuse of 2nd-order arithmetic scheme in computing Memo W: DA fn 21.) No member of the 25 decl data in the C columns (of Table 1) deviates from the corresponding A column datum by more than $1'$.

Table 1: Checking Adams' 1846 July 20 Ephemeris (Bottom Half Memo W): Hypothesis 1 vs Circular Orbit

For $\theta = 315^\circ$

Date	α_A	δ_A	α_H	δ_H	α_C	δ_C
Jul 20	$21^h11^m.1$	$-16^\circ16'$	$21^h11^m.2$	$-16^\circ15'$	$21^h11^m.1$	$-16^\circ16'$
Aug 9	$21^h\ 9^m.1$	$-16^\circ24'$	$21^h\ 9^m.2$	$-16^\circ24'$	$21^h\ 9^m.4$	$-16^\circ23'$
Aug 29	$21^h\ 7^m.5$	$-16^\circ31'$	$21^h\ 7^m.3$	$-16^\circ32'$	$21^h\ 7^m.7$	$-16^\circ31'$
Sep 18	$21^h\ 6^m.2$	$-16^\circ37'$	$21^h\ 5^m.7$	$-16^\circ39'$	$21^h\ 6^m.3$	$-16^\circ37'$
Oct 8	$21^h\ 5^m.1$	$-16^\circ42'$	$21^h\ 4^m.6$	$-16^\circ44'$	$21^h\ 5^m.3$	$-16^\circ41'$

For $\theta = 320^\circ$

Date	α_A	δ_A	α_H	δ_H	α_C	δ_C
Jul 20	$21^h31^m.3$	$-14^\circ41'$	$21^h31^m.5$	$-14^\circ41'$	$21^h31^m.3$	$-14^\circ41'$
Aug 9	$21^h29^m.5$	$-14^\circ49'$	$21^h29^m.6$	$-14^\circ50'$	$21^h29^m.7$	$-14^\circ49'$
Aug 29	$21^h27^m.9$	$-14^\circ57'$	$21^h27^m.7$	$-14^\circ59'$	$21^h28^m.0$	$-14^\circ57'$
Sep 18	$21^h26^m.5$	$-15^\circ\ 4'$	$21^h26^m.0$	$-15^\circ\ 7'$	$21^h26^m.5$	$-15^\circ\ 5'$
Oct 8	$21^h25^m.3$	$-15^\circ10'$	$21^h24^m.8$	$-15^\circ13'$	$21^h25^m.5$	$-15^\circ10'$

For $\theta = 325^\circ$

Date	α_A	δ_A	α_H	δ_H	α_C	δ_C
Jul 20	$21^h51^m.3$	$-13^\circ\ 0'$	$21^h51^m.5$	$-12^\circ59'$	$21^h51^m.3$	$-13^\circ\ 1'$
Aug 9	$21^h49^m.5$	$-13^\circ\ 9'$	$21^h49^m.7$	$-13^\circ\ 9'$	$21^h49^m.7$	$-13^\circ\ 9'$
Aug 29	$21^h47^m.9$	$-13^\circ18'$	$21^h47^m.8$	$-13^\circ19'$	$21^h48^m.0$	$-13^\circ18'$
Sep 18	$21^h46^m.5$	$-13^\circ26'$	$21^h46^m.0$	$-13^\circ28'$	$21^h46^m.5$	$-13^\circ26'$
Oct 8	$21^h45^m.1$	$-13^\circ33'$	$21^h44^m.7$	$-13^\circ35'$	$21^h45^m.3$	$-13^\circ32'$

For $\theta = 330^\circ$

Date	α_A	δ_A	α_H	δ_H	α_C	δ_C
Jul 20	$22^h11^m.0$	$-11^\circ14'$	$22^h11^m.3$	$-11^\circ13'$	$22^h10^m.9$	$-11^\circ15'$
Aug 9	$22^h\ 9^m.3$	$-11^\circ24'$	$22^h\ 9^m.6$	$-11^\circ22'$	$22^h\ 9^m.4$	$-11^\circ23'$
Aug 29	$22^h\ 7^m.7$	$-11^\circ33'$	$22^h\ 7^m.7$	$-11^\circ33'$	$22^h\ 7^m.8$	$-11^\circ32'$
Sep 18	$22^h\ 6^m.2$	$-11^\circ41'$	$22^h\ 5^m.9$	$-11^\circ43'$	$22^h\ 6^m.2$	$-11^\circ41'$
Oct 8	$22^h\ 4^m.7$	$-11^\circ49'$	$22^h\ 4^m.4$	$-11^\circ51'$	$22^h\ 4^m.9$	$-11^\circ48'$

For $\theta = 335^\circ$

Date	α_A	δ_A	α_H	δ_H	α_C	δ_C
Jul 20	$22^h30^m.4$	$-9^\circ23'$	$22^h30^m.7$	$-9^\circ22'$	$22^h30^m.3$	$-9^\circ24'$
Aug 9	$22^h28^m.8$	$-9^\circ33'$	$22^h29^m.1$	$-9^\circ31'$	$22^h28^m.9$	$-9^\circ32'$
Aug 29	$22^h27^m.2$	$-9^\circ42'$	$22^h27^m.3$	$-9^\circ42'$	$22^h27^m.3$	$-9^\circ42'$
Sep 18	$22^h25^m.7$	$-9^\circ51'$	$22^h25^m.4$	$-9^\circ52'$	$22^h25^m.7$	$-9^\circ51'$
Oct 8	$22^h24^m.1$	$-10^\circ\ 0'$	$22^h23^m.9$	$-10^\circ\ 1'$	$22^h24^m.3$	$-9^\circ59'$

Variables: θ = true heliocentric longitude 1846/8/29 (opposition-date of Adams' Wartmann-based planet); α = geocentric right ascension, δ = geocentric declination.
Subscripts: A = Adams' ephemeris (Memo W); H = Hyp 1; C = circular orbit.
The only portion of this ephemeris hitherto published (M16:421) is that for $\theta = 325^\circ$.
See fn 49 for comparison of residuals' rms (H vs C).
(Note: The fit of Adams' 1845 Sept solution is also inferior to the circular orbit, though very slightly better than that of Hyp 1.)

F Challis Sinks Deeper into Memo W

F1 So, in brief, we have just found that Brit Key Document #1 (§D) is contradicted by Brit Key Document #2 (§E)! In fact, the situation is worse than contradictory — it is anachronistic. (Recall that §D4 item [a] also finds Adamsian time flowing backwards.) But we have still not exhausted the catalog of Brit headaches about Memo W.

F2 First, Challis published (M16:421) only the part of the ephemeris for a 325° planet — omitting[50] the adjacent ephemerides for 315°, 320°, 330°, and 335°. (In Table 1, we check the *whole* ephemeris to show that a primitive circular Bodean orbit fits it much better than Hyp 1 **for all five cases Adams tabulated**.) Second, there's the little matter that: all this is at the *bottom* half of the document.

F3 Every one of Challis' public reports — *even while* (§E1) *publicly founding Britain's Neptune claim upon the bottom half of Memo W* — omitted to mention[51] the entire TOP half, where resided an Adams orbit (*also* circular) based partly upon a then-unaccounted-for object sighted in 1831 by Swiss astronomer Louis Wartmann.[52] This version (called "Hypothesis W" in DA) of Adams' remarkably hyperactive planet was at longitude 336°, over 20° to the east of the solution he issued just 6 weeks later (§G1), which DA called "Hypothesis X" (315° longitude). One can readily understand why Challis was getting increasingly confused[53] about where to point his telescope!

F4 Not only was (as DA §B4 noted) this erroneous Hyp W orbit never mentioned anywhere (until DA, that is), but: we now find (from the newly recovered RGON file) that Challis was stating[54] to Airy (1846/11/3 p.3) that Adams had rejected the Wartmann sightings as irrelevant[55] to Neptune. To the contrary, Challis spent some telescope time[56]

[50] Challis did (M16:421) note the existence of the four other ephemerides. But he never publicly mentioned Hyp W (which was at the top of this very sheet of paper, CON #35[a]); moreover, he actually denied (fn 55) that its part-basis (Wartmann's data) had anything to do with Adams' predictions.

[51] In fairness to Airy: there exists no evidence that he knew the details of Memo W (a secret provably known only to Adams & Challis).

[52] [Since Wartmann's report turned out merely to be a fouled-up series of Uranus observations (fn 55), Kollerstrom notes that Adams' top 1846 July solution (Hyp W) effectively confused the perturbed indicator with the perturbing quarry! I.e., Hyp W (top) inadvertently had Uranus perturbing itself.]

[53] DA (esp. §B4) showed that the traditional tale's fall-guy, James Challis, was not primarily to blame for England's miss of Neptune. (Adams' inconsistent directions were the main problem.) But I am told that Challis' miss is still today commemorated at the Univ of Cambridge, when unhappy Cantab students numb their woes by quaffing grog from a "Challis chalice".

[54] Challis reported (*Cambridge Chronicle* 1846/10/1 and M16:423) that, during his 2nd of three 9′-wide sweeps on the night of 1846/9/29 (just before news of Leverrier's success reached England: London 9/30, Cambridge 10/1), he had noted one "star" (actually Neptune) as looking nonpunctal — and so asked his assistant to write there in the record: "seems to have a disc". (DA fnn 27, 28, & 30 analyze some peculiarities of this claim.) But the actual record (of which *DIO* possesses photocopies) actually commented in the past not present tense: "[last one] seemed to have a disc". (The bracketed words are scratched out.) See p.53 of Challis' sweep book (part of CON). If he is to be believed, then: not only did he fail to engage the clock drive instantly to track and carefully examine the suspect object, but, at the end of the night's 2nd sweep, he went straight on into a 3rd sweep — instead of spending a few minutes searching out the disc he allegedly had just seen. It was still well above the horizon.

[55] The 1971 *DSB* (vol.3) Challis entry omits this intriguing part of Challis' 11/3 letter: "Wartmann's star . . . Adams considered long since, and ascertained that if the observations are at all approximate, it must be much nearer the Sun than the new Planet." (It is possible that this refers to post-discovery conclusions from an internal evaluation of the Wartmann data. If so, Adams was right: the data are of Uranus. See P.Baum & W.Sheehan *In Search of Vulcan* NYC 1997 Chap.7 n.15.)

[56] Nights of 1846/7/30, 8/12, 9/21, 23, 28 (accounting for Hyp W's 12° orbital inclination with node c.130°) and 9/3 (no inclination, thus near the far northeast portion of the search zone). Challis' intense looking into these areas might have no relation to Memo W, but spending serious time on far-east sweeping was contra his realization that the far west region would be the first to become unavailable this season due to the Sun's gradual encroachment into the region. Indeed, considering that roughly half of his sweeping occurred in the eastern half of the Airy-designated (CON #4 p.1, 1846/7/12) search

looking around the isolated & remote far-east regions predicted by Adams' Hyp W. And Adams' Memo W ephemeris — top AND bottom — is temporally-centered upon the 1846/8/29 date of opposition[57] of the Wartmann-based Hyp W.

G Further Insight into Adams' Large Misdirection of Challis

G1 Relative to where Adams was pointing (and how this relates to Challis' miss of Neptune), one of the most helpful documents is that of Adams to RGO 1846/10/15, in which he explains his erroneous extrapolation (from Hyp 1 & Hyp 2) to his final predicted orbit, Hypothesis X — which was sent by Adams to RGO on 1846/9/2.[58] This is the crucial solution which DR has been emphasizing for 30 years (e.g., Rawlins 1969), since [a] no other historian has mentioned it, and [b] it is seriously wrong in longitude (longitude obviously being THE key datum for searchers), over 10° west of the real Neptune, thus fatally misleading Challis. (See fn 20 here; also DA §§B4 & F3.) Historians' incomprehension of the seriousness of the error is largely due to the fact that Adams expresses the result as a *mean* longitude 315° for a virtually circular orbit. Unfortunately, most historians do not understand[59] that null eccentricity automatically requires Adams' final *true* longitude, Hyp X, also to be 315° — at a time when Neptune was actually at about 327°. (The positions of the real Neptune and the several predicted planets of Leverrier and Adams are provided in Tables 1&2 of DA for the entire first half of the 19th century.)

G2 Therefore, the 10/15 letter must finally satisfy those who have found it incredible that: [i] Adams actually made such a huge mistake, and [ii] all previous historians have missed this rather unsubtle slip. Those inclined to question the reality of the Hyp X error, will be enlightened by reading (§G3) Adams' attempt[60] to explain the very mistake they are evidently doubting ever happened. . . .

G3 Fortunately, in the hitherto unknown contents of this private 1846/10/15 letter to Airy, Adams explicitly refers to his final solution (Hyp X), even giving its precise date (1846/9/2). His exact words (1846/10/15 to Airy, p.4, emph added):

> In my letter of Sept$^{\rm r}$ 2$^{\rm nd}$, I inferred [DR: admirably correctly][61] that the mean distance used in my first Hypothesis [Hyp 1] must be greatly diminished, *but I rather hastily concluded that the change in the mean Long. deduced would be nearly proportional to the change in the assumed mean distance.*

[Note added 2011: precise extrapolation-math discovered at www.dioi.org/cot.htm#hpfp.]

area, it is odd that Challis explicitly stated that after 7/30-8/12, "I observed earlier [more westerly] in right ascension, for the sake of being able to go over in the present year as large a portion as possible of the space to be explored."

[57] The Hyp W planet's opposition was at 5 AM of civil date 8/30, which was 8/29, 17$^{\rm h}$ GMT, by astronomer's (pre-1925) reckoning.

[58] Original in RGON; text published at M16:405-408.

[59] I am disappointed to see that Wm. Smart (quite knowledgeable in the relevant math) is so swept up in his championship of fellow-Cantab Adams that he does not mention Hyp X (see Smart 1947 pp.21, 29, 34) — or Adams' own excuses for its considerable error. (See also Smart *Celestial Mechanics* London 1953 pp.253&262.) Instead he elects (*idem*) to promote only the **pre**-final two solutions (Hyp 1 & Hyp 2), which were much closer to the mark. (Isn't this a kind of switch?)

[60] The same excuse appeared at M16:456, in a part of his 1846/11/13 post-discovery presentation which no historian has cited (other than *DIO*: see DA §E8) perhaps partly because it is less clear (than the 10/15 letter) and does not explicitly cite the 9/2 letter (which *DIO* has said right along was the reference: *idem*).

[61] See DA fn 56. Note that in the 1846/10/15 letter and elsewhere, Adams proves far quicker and more supple than Leverrier at realizing that both men's elaborately perturbation-computed orbits were flawed by their mutual initial assumption of the Bode's Law distance 38 AU. In the full 10/15 letter, one can see Adams pressing his case that he had been wiser than Leverrier in this respect. I believe this is the best ground for defending Adams vs. Leverrier — however, it does not answer the question of whether one ought to recognize post-discovery claims, especially after years of deliberate secrecy.

G4 I.e., Adams himself was fully aware that (in addition to several previous already-confusing longitude-predictions: fn 20) he had ultimately pointed Challis way off the correct longitude for Neptune. So (as *DIO* has asked previously) why do Adams' defenders[62] ignore this and pretend that his solution was good to roughly (or at least ordmag) a degree?

G5 A further point relative to Adams' final (Hyp X) solution: one of the most popular Brit alibis for losing Neptune was English nonpossession of the famous Berlin Starchart Hour 21. Quite aside from our surprise discovery that, when the Cambridge search got going, Neptune was moving conspicuously among the stars of Berlin Starchart Hour 22, which Challis had,[63] we have also found that the ecliptical Hyp X solution was south of the southern limit[64] of **all** the Berlin Starcharts. Airy's 1846/7/12 letter to Challis (CON #4, perhaps unpublished before *DIO*) notes at p.4 that Hour 22 covers some of the suspect region, but (he wrongly says) not much.[65] Unambiguous upshot: map-alibi-byebye.

H Big Gaps, Big Eyes, and Perfect Idiocy

H1 There are two suspiciously large gaps in the RGON record. Gap A: 1845/11/5-1846/6/26. Gap B: 1846/9/7-9/25.

H2 Gap A is demonstrably artificial, since Airy's 1846/6/25 letter to Whewell (Rob Smith's crucial find)[66] is conspicuously missing. Moreover, we again (as DA noted) have no information about Adams' Neptune activities for a key period of more than a half-year. Sedgwick's reference (12/6 p.2), to Adams perhaps getting bad advice (from Challis?)[67] is

[62] Virtually every popular history does so (sometimes just implicitly): [a] At the time of discovery, Leverrier was only 1° off. True. [b] Adams' solution agreed with his to within 1°. False for Hyp 1 or Hyp 2 or Hyp X. As one sees from DA's Table 1 for 1846/9/23: Hyp 1 & Hyp 2 were both 2° east of Neptune, and 3° east of Leverrier's predicted planet. Hyp X was 12° west of Neptune, 11° west of Leverrier. Adams' 1846 Sept solution ("Hyp G") agreed with Leverrier almost on the nose (within a tenth of a degree: DA §G9) — but that was Leverrier's early (1846/6/1) solution, and Hyp G (because based on mismath: *ibid* §G4) is not the solution Adams later claimed he had given Airy in 1846 Oct (see *idem*), alleging that he had instead given him Hyp 1 (but without dating this legendary document).

[63] See Rawlins 1984N and DA fn 72.

[64] DC §A6. The south bound of the Berlin Starcharts was declination −15° (epoch 1800.0).

[65] On this (& Airy's also-previously-unpublished 7/21 letter to Challis), see DA fn 72.

[66] See Smith 1989 n.25.

[67] See fn 23. Challis' official 1846/12/12 report just says (SP p.li) that in the crucial period between 1845 Autumn and 1846 midsummer, "I had little communication with Mr Adams respecting the new planet." [That Challis was actually in close regular contact with Adams during this very time (§H6) has been revealed by Kollerstrom, who finds: [a] The 1846/3/14 Adams diary entry (JCASJ) says he walked with Challis all the way to the Observatory even though not visiting it. [b] "Adams was [Challis'] own private friend" (Mrs.Airy to Sedgwick 1846/12/9).] But, then, Challis thought leaving a gap in the record was the only way out: §H10. Even former Chief Ass't to the Astr Royal H. H. Turner was never allowed into the RGON file; see DB §C1 or Turner 1904 p.48 ("pinned") and thus his non-awareness (*ibid* p.65) of Airy's "shadow" sleight (fn 26 or DA §B2). But Turner's intelligence was jostled by central nonsense in the myth's implicit assumptions. Turner 1904 pp.70-71: "Challis never made the most casual inquiry as to the result of [Adams'] visit to Greenwich which he himself had directed Adams to make. *I am judging [Challis] to some extent by default; because I assume the facts from lack of evidence to the contrary* . . . . [Challis] never even took the trouble to inquire on [Adams'] return, 'Well! how did you get on? What did the Astronomer Royal say?' Had he put this simple question . . . and learnt in consequence . . . that this sensitive young man thought Airy's [1845/11/5 radial] question trivial, and did not propose to answer it, ... Even Challis might have been trusted to reply, 'Oh! but you must answer the Astronomer Royal's question: you may think it stupid, but you had better answer it politely, and show him that you know what you are about.' " Though I disagree with Turner's interpretation, I am grateful for his carrying through, to show what bizarre scenarios one ends up with, by not facing the likelihood (DA §I10 item [2]) that, independently of Challis-Airy, Adams was himself (especially after catching his 1845 math error: DA §F2) paralysed into silence (fn 91) by his doubts regarding [a] the general trustworthiness of his math, and [b] the longitude that

the nearest thing to a recorded answer to this mystery. [See also fn 73.]

H3 But we know that Airy-Challis-Adams met[68] in early December, about the time Leverrier's 1st published memoir would have reached England, a temporal connection 1st pointed out in DA (§D4), suggesting Airy could've then warned Adams that Leverrier might be closing in on Neptune. In this context, one of *DIO*'s new findings reveals DA's suspicion as too conservative: for, in fact, *Airy knew of Leverrier's work from the outset*.[69]

H4 On 1845/9/22, Airy was at a meeting of the French Institute,[70] and it was presumably there that he learned that Paris Observatory chief F. Arago had deputed Leverrier, the most capable astronomer in the world (for this type of work), to eliminate the celestial offense of Uranus' nontabular meanderings — a problem that particularly plagued Airy, whose very job it was to track the heavens accurately and who had already long since publicly branded Uranus' intractability as intolerable (G.Airy *Report*. . . [BAAS 1831-1832] London 1833), echoing Laplace's ultimate-theorist dream of banishing empirical terms from the heavens. Airy's famous official public 1846/11/13 Royal Astronomical Society presentation actually cites (M16:395) his attendance at the French meeting — but it does not mention what he learned there. (Recall the revelation of DA §E6 that, right after Neptune's discovery — in early Oct of 1846 — Airy also spied on Leverrier's activities, by getting a pre-publication look at his prediction-math's details, in the office of the *Astronomische Nachrichten*, well before Adams had put a single digit of ***his*** prediction-math before the public. [See fn 69.])

H5 This information guts the standard history, repeated now for 153^y as if (well, what are gaps for?) poor-naïve Adams never even knew of Leverrier's work. How else excuse two glaring glitches in BritMything: [a] Adams was silent for months even *after* Leverrier's 1846/6/1 paper. [b] Adams started work on Uranus well *before* Leverrier (something stressed by Adamsians when it suits their claims of priority) but ended up finishing[71] *after* Leverrier (see §B9). (Item [b] despite Adams' crucial advantage that, entirely due to his secrecy: he [& Airy, fn 69], not Leverrier, ***knew that a race was on***.)

H6 This central question was raised only once in the 1846 record we have when Sedgwick asked Airy (1846/12/6 p.5; quoted by Glaisher at SP xxvi n.1 & Smart 1947 1947 p.42) whether Adams had ever *even been told* that Leverrier was "at his heels". AIRY NEVER ANSWERED THIS QUESTION. He implicitly pseudo-answered it in his incredible hardly-knew-him 1846/12/4 letter to Sedgwick: "My whole epistolary communication with Adams is printed in [M16]; and I never saw him but once somewhere with Challis (I totally forget where) and once, when Hansen and I came for half a day to Cambridge and we were walking across St. Johns' bridge. The interview on each occasion might last 2 minutes.

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would eventually result from it: §K2. Why else give top-of-page rank in Memo W to lost-star-based Hyp W (§F3), if he really believed his own math was firm, precise, and completed?

[68] Question: at this 1845 Dec meeting (or when Adams & Airy were both at the 1846/2/13 R.A.S. meeting: fn 75), didn't either man bring up the matter of the unanswered 1845/11/5 letter?! Another weird inconsistency relating to Adams' incredible lost-period (which includes Gap A).

[69] Airy to Arago 1845/9/29 referred to: "M. Le Verrier's undertaking to examine the theory of Uranus." (Also: "My calculations of some of Bouvard's terms." Which reminds us: Airy was one of the tiny handful of British astronomers who understood the math of the Uranus problem. See §H4 and DA §§E6-E7.) Not in NOAO copy of RGON, but faxed to DR by Elaine MacAuliffe, 1999/5/26.

[70] This is precisely the date of Challis' famous letter of introduction of Adams to Airy. The match is subject to several interpretations. Prominently including, of course: pure coincidence.

[71] Adams claimed (RGON: 1846/11/18 to Airy) that he had little time except during vacations to do the Neptune work. However: [a] The unavailability of most of Adams' diaries for the period in question prevents us from checking this. [b] Some of his 1845 work on Neptune was completed on April 28. (See JCASJ W.16 §E II or Sampson 1904 p.165.) And in 1846 April, he delivered a paper (on another subject) at the R.A.S. (see DA §D1). (From these seasonal data, we can induce — partly by analogy — that at least some of the time period between 1845/11/5 and 1846/9/2 was indeed available to Adams for research.) [Additionally, the extant Adams 1846 March diary material shows that he was doing research work in several directions at this time — but nothing on Neptune. And there is this remark under 1846/3/14: "Did not do much today, feeling rather tired. . . ."]

No other opportunity of seeing him."[72] [Note added 1999 Oct: Yet, Nicholas Kollerstrom has just discovered a portion[73] of Adams' diaries, 1846/3/9-3/28; Nick noted it shows that (during this supposedly-incommunicado period), Adams was in quite frequent touch with Airy's close friend Challis (who knew all about Leverrier's progress), and evidently was checking some atmospheric refraction data of Airy.[74] (All of which only makes the RGON file's Gap A look more incredible yet.) So the orthodox story that Challis & Airy were not communicating with Adams in this period appears to be just another smokescreen.]

H7 And there is yet another (often inadequately separated) burning question put to Airy by Sedgwick in the same letter (also 12/3 letter pp.5-6): why Airy issued no immediate public cry in 1846 June when the Leverrier-Adams agreement was first realized,[75] instead of keeping Adams' prediction a "secret" (Sedgwick's own word for it).[76] Here, Airy did offer a sort-of answer, in his 1846/12/8 letter (***the*** one letter of the Sedgwick-Airy exchange which Astronomer Royal H. Spencer Jones hid from Smart in 1946). But Airy never directly[77] answered it. Both of 2 copies of Airy's 12/8 letter were suppressed. (DA fn 12 had guessed it spoke "of Adams in extremely blunt terms".) [Other copy now available in JCASJ. So both copies resurfaced near-simultaneously: §L8.] It's part of the same Sedgwick-Airy 1846 Dec set which Astronomer Royal Christie had (DC §A3) asked for censorship of.

H8 [This letter's heart was missing from the NOAO copy of RGON. But, at *DIO*'s 1999/7/7 request, Nicholas Kollerstrom (University College, London), with RGO Archivist

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[72] See RGON 1846/12/4 pp.1-2 (emph added), Smart 1947 p.40, or DA fn 75. Smart 1947 p.34 (footnote) dates the 1st meeting to 1845 Dec 4-6; the 2nd, to 1846/7/2.

[73] [Part of Adams' 1846 diaries survives (fn 67), covering most of March, but nothing on Neptune: fn 17. The survival of just non-Neptune diary material, from a period so key to the Adams controversy, only adds to an impression that contra-myth documents have been knowingly filtered out.]

[74] [See fn 67. Adams' entire 1846/3/19 diary entry: "Gave some further developments to my [differential refraction] solution and worked some cases given by Airy." Kollerstrom has also found that Adams and astronomer J. Hind were communicating during 1846 Feb (JCASJ), but not on Neptune; which may help explain why, after Neptune's discovery, Hind expressed (1846/11/12 to R.Sheepshanks: Rawlins 1984N or DA §B5) such disgust for "the inexcusable secrecy" of "the Cambridge people [who] do their best for their own men." Adams' correspondence with Hind (who ran Bishop's Observatory at Regent's Park) bears on the myth that England's Neptune fumble was due to Challis' & Airy's inertia. (Contra this, see Challis' reasonable remarks at §B9.) But if Adams felt that Challis was too slow to act, then he could at any time have told Hind where he thought Neptune was. (Or other astronomers: Lord Rosse [see Smart 1947 p.46], W. Lassell, E. Cooper; all had access to major telescopes.) Hind, soon to become famous as Britain's greatest asteroid discoverer, could've searched out the planet at least as ably as Challis, and would probably have moved more nimbly on the project. (On 1846/9/30, Hind became the first Brit ever knowingly to observe Neptune, having just received a discovery-report from the Continent.) But, then: Hind was not a Cantab.]

[75] The standard Brit story implicitly tries to pretend (for why, see fn 68) that Adams & Airy had no contact from 1845/11/5 until after the discovery. This laughable theory is refuted by their encounters of 1845 Dec (§H3) & 1846/7/2 (§B11). (See also DB §G8: 1846/2/13.) A more intriguing hint: Airy said (M16:405) that in the late summer, Adams "was not aware of my absence from England" — yet the day, on which Adams posts allegedly the first letter he ever wrote to Airy (1846/9/2), was the last when mail could reach Airy (who was vacationing: §I2) at Wiesbaden, which he departed 9/7 (M16:405). We note that Challis wrote Airy the same day (M16:405), also after a substantial silence.

[76] See RGON Sedgwick to Airy 1846/12/6; essentially same question also in Sedgwick's 12/3 letter to Airy (quoted at Smart 1947 pp.42 & 39, resp). Airy's first claim that it was not his responsibility to publish Adams is in his 12/4 reply. (RGON p.4; quoted at Smart 1947 p.40).

[77] Some may say that an indirect answer was Airy's 1846/12/4 (fn 76) and 12/8 (§H8) protests that it wasn't up to him to decide when to publish. (Irrelevant. See fn 91's conclusion.) However: [a] If distribution of Adams' findings was up to Adams, then why was Airy in June telling Herschel, Whewell, Babbage, Peacock, etc? — but not the French or the public. (And were not Challis' & Herschel's 1846/10/1 public letters taking right of publication from Adams? — which could as well have been done in June. *If* Adams approved. Which is the whole issue.) [b] But such revelations in June would instantly have triggered demands for Adams' predicted longitude; thus, one is right back up against the flaw common to all Adamsian excuses: he had as yet no solution he trusted enough to make public.

Perkins' willing help, recovered[78] the letter's startling center, revealing fully at last Airy's private sardonic contempt for his detractors' conception of events (and of ultimate blame). Replying to Sedgwick's 12/6 letter, Airy simply created a spoof upon it:

> I will put your propositions into the form, in which there is not the most trifling exaggeration.
>
> Sedgwick's Theory and Rules thereon founded
>
> 1) Every Cambridge man is a Baby, and cannot walk out except he has a Nurse to trot him out.
>
> 2) Only Extra-Cantabs[79] are eligible as Nurses. No resident, not even a Plumian Professor [Challis], is competent to this office.
>
> 3) Simple nomination of an Extra-Cantab by Baby imposes on such Extra-Cantab . . . all the duties and responsibilities of Nurse.
>
> 4) The regular duty of the Nurse is, to divine the unexpressed wishes of the Baby to walk, and then to take him out.
>
> 5) This responsibility of the Nurse is not removed even though the Baby take a fit of the pique and refuse to answer questions, or though the Baby refuse to clothe himself in what the Nurse considers to be a proper walking dress.]

H9 As for Gap B, 1846/9/7-9/25. This hole is only broken by the 9/25 letter from Berlin Observatory Director J. Encke to RGO: "it is highly probable that Le Verrier's planet is discovered."[80] Gap B may matter because Adams said on 1846/9/7 (RGON p.2, emph added): "I hope by tomorrow [9/8] to have obtained approximate values of the Incl. and Long. of the Node, *which I shall have great pleasure in communicating to you*." (Solutions at JCASJ W.16 §G.)[81] The italicized words were clipped off this quote when Airy published it (without ellipsis) at M16:408, and there is no 9/8 Adams letter in the RGON file. Did the letter (assuming it ever existed) perhaps also offer a guess about longitude that would look even worse than those of Memo W? Or did it express reservations about the whole project? Obvious innocent explanations exist here (e.g., fn 81); but, when a record has been manipulated for so long, dark speculations are invited. Again, natural human selectivity is exactly why post-discovery claims should (as a matter of policy) never be allowed.

H10 There are other gaps. And those in logic exceed those in time. But, then, Challis' 1846/12/19 letter to Airy said that Adams' behavior in not publishing was so peculiar that the public would never understand, so "a hiatus must remain in the history".

[78] [Kollerstrom was the first to make public the 1846/12/8 letter's middle, which will forever be remembered as one of the most striking texts of the whole affair. Nick thus has the privilege of first evaluation (1999/7/24): "My feeling is that Airy's comments here are very fair and well-balanced. The question which I suspect he never asked himself, however, despite his Proposition 3, is why he chose to accept the rôle of 'Nurse'. I'd say this is the nearest Airy ever comes to expressing the Truth . . . ."]

[79] There is an unexpected implication here. DR has previously shown that astronomers such as Hind scoffed at Univ Cambridge for nonpublication of its Neptune researches. But no one previously suspected that Airy (himself an eminent Cantab) was privately doing likewise — and (in hurt from Cambridge's treatment of him) now counting himself as a non-Cantab.

[80] "Highly probable" reflects an odd restraint; Encke, who knew orbit theory, previously doubted the existence of Leverrier's planet. (A fact Encke modestly never hid: C. Bruhns *Johann Franz Encke* . . . 1869 p.303. Note: so Airy was not the only skeptical expert.) Encke goes on in this historic letter, fresh from the discovery-site: "Le Verrier by letter [arriving] 23rd Sept asked Herrn Dr Galle to look for it, and the same evening, during a comparison of the sky with the excellent academical Starchart of Herrn Dr Bremiker (Hora XX1), there appeared to Herrn Dr Galle an 8th magnitude star which was not found on the map. . . . By the 24th Sept, it had gone 1′ retrograde. . . . The motion agrees fully with Leverrier, so that this is a most magnificent discovery." It was Encke who carried through Bessel's Sternkarten idea (fn 5), which aimed at helping to find new planets (Bruhns *op cit* pp.117-125).

[81] Or Sampson 1904 pp.169-170. (See also Adams' 1846/9/10&16 memos, misfiled in JCASJ under 1843.) Adams got several large, disparate inclinations which may've discouraged his writing RGO.

H11 DA was the 1st Neptune history which drew detailed attention (DA §E3) to Adams' reluctance to publish his predicted elements (which would have required but two lines in the following week's *Athenæum*) even ***after*** Neptune's discovery. Again, our remarks in DA (§§D6 & E) turn out to have been too conservative. In a vital RGON 1846/11/3 letter, Challis reports that, after well over a month of silence since the discovery, Adams is STILL reluctant to tell the public what he had allegedly been predicting for over a year. DA §E3 noted the suspiciousness here of Adams' apparent trouble in getting his story straight; now that we have found actual testimony about Adams' reluctance to commit to anything, we are not just speculating about his nervousness — a condition that does not enhance confidence in the reliability and non-selectivity of his eventual well-known 11/13 R.A.S. report. When the person who stole the RGON file wrote the 1971 *DSB* article on Challis, he quoted the part of the 1846/11/3 letter showing *Challis*' reluctance to speak to the R.A.S. on 1846/11/13 — but the part showing that Adams also looked likely to duck out was suppressed[82] (see similarly at fn 55) and is only now being made known in this *DIO* paper.

H12 Which segues us into the standard evasion of the simple (§B9) reality behind the Adams myth: his alleged naïvete. For, the revealing fact that Adams didn't publish *must* be explained-away if one is to preserve the claim of Adams' priority. But his total non-publication was inexplicable without frankly admitting the §B9 truth (a course which would, however, kill Adams' claim to firstness); thus, one had to go to such alibi-extremes that, up to now, these recourses' wildness has never been fully revealed. It got to the point where the loyalest important Cantab defender of Adams (Sedgwick) could only reply to Airy's reasonable §H8 response (that Adams not Airy had to decide when to publish) by admitting Adams had behaved "very like a simpleton". Or so Smart 1947 p.42 renders it. But the original of the letter (RGON) actually puts it more strongly: "like a very simpleton". Question: when your top defender can only explain your actions by pleading you as a *perfect* idiot, can your case be regarded as credible? (See also fn 42.)

I Airy's Slyness

I1 No defender of Airy can convincingly explain away his keeping the French in the dark about Adams in his 6/26 letter[83] to Leverrier (§B11). This was not square dealing (nor was his silence to Hansen: §B11); as Airy's subsequent "desperate" (M16:403) steps show, he was scheming to grab Leverrier's planet for England. And for Geo. Airy (§B11).

I2 A previously-unnoted Airy ethical contradiction: he later said (M16:397) that, had Adams replied to Airy's now-famous 1845/11/5 letter of inquiry (§B8) about the Adams theoretical perturber's radial effects, Airy would have immediately helped Adams with all his power. Well, Leverrier **did** reply (6/28) to the very same question — but Airy didn't help Leverrier a bit. (Quite to the contrary.) His public alibi (1846/11/13, M16:402) was

[82] Same selective suppression at Smart 1947 p.38. Challis to Airy (1846/11/3, RGON file): "I am sorry to say that I can give no hopes of Adams' being able to undertake the [R.A.S.] Astronomical Report. He is moderator this year, & this, with his College duties, takes his time. I am in difficulty about this Report, & should be glad to see some means of getting out of it." (Smart and the *DSB* quote only the final sentence.) Challis' witness to the fact that Adams (contra Adams to Airy 10/15 p.3) shared Challis' reluctance to speak is plainly consistent with Adams' weeks of glaring unwillingness to transmit a single number about his predicted planet to the public (§H11), to understandably-inquiring French scientists, or, indeed, to anyone outside his circle of Cantab conspirators — even while Challis and he were somehow able to find *plenty of their precious time* to fire off letter after letter to the press (§B8) on, e.g., how to give a British name to the new planet. This is the standard hunkerers' lodge&dodge tactic (& see, e.g., conclusion of News Notes, above) a technique which enables its employer to: [a] lodge any story he likes, even while [b] dodging live cross-examination on it.

[83] Airy registered Adams' work by his 6/25 letter to Whewell, which may be absent from the RGON file since it makes Airy's 6/26 silence to Leverrier look scheming. (DA fn 12 wonders if this silence was due to Airy's learning on 6/26 that Adams was still-groping along, unsure of his math's indications.)

that he was off on vacation to the Continent! This almost humorously incredible & bold excuse neglects to explain how Airy managed (while speeding eastward for the Continent) to detour north to Ely (7/9) to conspire with Geo. Peacock and Challis (both of Cambridge), laying elaborate plans[84] for catching Neptune ahead of foreigners and nonCantabs.

J Bloody Beak & Rank Lies: Airy's Ire at Adams' Worst Alibi

J1 When public rage at Airy was cresting in late 1846, Cambridge was abuzz with a lie which had a natural appeal (see DA fn 52) to all those who sympathize with victims of arrogant injustice. The lie (and that was Sedgwick's blunt term for it, e.g., RGON 1846/12/9) was that Airy had "snubbed" Adams in 1845 Autumn. As with alot of lies, it got very fanciful — down to detailed allegations (Sedgwick to Airy 1846/12/6 p.1) that Adams had an appointment at a certain time but was not received.

J2 Airy could hardly believe the fantastic cast of these rumors. He was not in an upbeat mood to begin with: publicly portrayed as a liar by the French,[85] his defense against Cantab anger was hobbled by his own ethically-inexplicable behavior towards Leverrier (§I). As he fought to survive a two-front war somewhat of his own making, his 12/8 letter, trying to stamp out dumb lies about himself, answers juster criticism by stonewalling: "here finishes my Cambridge discussion. The next blow will probably be from Paris." He'd already resigned himself to being abused (12/4 p.1), wryly quoting an ancient warning:
"Those who in quarrels interpose / Must often wipe a bloody nose."

J3 Smart 1947 p.42 had quoted Airy-to-Sedgwick at this tense time: "I must have a very low opinion of those who have so taken it up that my old friend [Sedgwick] has felt himself obliged to question me as if I were a criminal".[86] DA launched the new theory that Airy was referring to Adams. The full RGON file (though superficially ambiguous)[87] contains strongly worded evidence (§J8) encouraging this view.

J4 Sedgwick's letters tried to convince Airy that Adams had no rôle in starting these rumors. Airy knew better. But as a canny politician he also knew better than to counterattack a hero in public, no matter whom the facts actually favored. However, Airy knew the awful truth all along — and he knew it more reliably than anyone has previously suspected.

J5 For, his brother Wm. Airy was reporting directly and specifically to him all of the anti-Airy rumors got up at Adams' college, St. Johns. Even months after the discovery, Wm. Airy writes his brother (RGON 1846/12/9):

> When I was at Cambridge last week I heard so much about the new planet that I actually dreamed about it. You are aware, I know, that the Johnians have taken up the cudgels against you for "snubbing" Adams . . . . the charge was stated by Babington of St. Johns (a friend[88] I believe of Adams) either in Trinity Combination Room, or at Sedgwick's rooms on Audit night.

J6 And no denial by Sedgwick (or anyone else), of Adams' involvement in starting these rumors, could erase from Airy's memory the remarks Adams had made in a recent part-apology part-alibi letter (11/18 p.3) to Airy himself, saying that Adams was "much pained"[89] at not having been able to see Airy during the legendary failed visits to RGO in 1845 Autumn. Adams initially (later retracting) blamed Airy not with vicious intent,[90] but rather in quite natural disappointment (at missing out on a unique discovery) — abetted by an equally natural if unadmirable delusion that someone else was the problem.

J7 Airy's rage peaks in his 1846/12/8 letter to Sedgwick. Again, this is the very letter that *DIO* indicated (DA §A6 and DC §§A3&A5) was deliberately suppressed. [It is now finally public in full (§H8) thanks to the honesty of Nick Kollerstrom and Adam Perkins.] Airy's charge (§H8) was so strong that Sedgwick (12/9) called it "ludicrous". (But he had not seen Adams' unqualified 11/18 blubbery wail-tale: §J6.)

J8 Airy's view of Adams is suggested by a newly revealed 12/11 note to brother Wm.:

> The Johnian story to which you allude was a capital example of the sort of connivance among associated persons which produces rank fibs. Sedgwick went thoroughly into the matter and at length produced the final retraction of the story.

J9 Airy's fury subsided when old-friend-intermediary Sedgwick confronted Adams and at last got him to admit frankly his own blame (for not answering Airy's 1845/11/5 letter — said letter being Airy's does-this-look-like-a-snub? trumpcard[91] when answering Sedgwick). Airy then magnanimously if toothgrindingly tells Sedgwick (1846/12/4) he'll be

[84] Details summarized in DA (e.g., §B4) or M16:404. Main plan elaborated in Airy's 1846/7/12 letter (CON #4) to Challis.

[85] Leverrier (1846/10/16) asks — *quite reasonably* — how Airy could claim England had the slightest convincing priority (or even capability for it) when Airy had had to ask Leverrier (6/26) about the radial effect of the predicted planet. He evidently wondered if the Brits made up the whole story. Evidence contra this idea cannot be found in any continuous record: DA §I9. [And see fn 73.]

[86] Airy's skill at fending off embarrassing questions with a how-dare-you pose helped him survive — but it does not help his credibility with an historian.

[87] Some may dispute DR's interpretation here, so I give full contrary data. Airy's 12/10 letter ends by making peace with Adams, which politician Airy knew he had to do. But the RGON file contains repeated evidences (fn 89) *known to Airy*: [a] that Adams was the source of the "snubbing"-rumor (as if one even needed evidence on such an obvious point, after all, **who else had a motive to tell such a lie?**), and [b] that Adams, even as he finally tried to find innocent explanations for Airy's 1845 Oct unavailability, harbored a deep and almost inoperable regret-resentment over not seeing Airy on that 2nd visit to RGO. Thus, Airy's most private remarks (first revealed to the world in this paper: §J8) express no forgiveness towards Adams and instead contain a crisp and intelligent summing-up of the whole discreditable Adams-circle slander, a lie that has continued into modern histories, e.g., DA §E7. [Note added 2017/6/18. For a gauge of Airy's political hyper-sensitivity, see Diego Medan's unexpected finds (in a long-neglected trove of Arago material) at diemedan@agro.uba.ar.]

[88] He too (like Adams) was forced by Sedgwick to retract the falsehood that Airy had snubbed Adams: Sedgwick to Airy 12/6. [Adams' diary entry of 1846/3/12 mentions a Babington.]

[89] Also Adams' 1846/12/6 remarks to Sedgwick (as he reported in his letter to Airy of that day, finished on 12/7 and postmarked then): "(1) He called at the [Royal Greenwich] Observatory soon after his calculations were finished — the Astronomer Royal away — Bad luck [DR: though, see fn 67 & DA §H4], but no blame anywhere — this was Sept. [1845] — (2) Called again (Oct. the same autumn) & the Astronomer Royal out — left his card — told that Airy would return soon, & therefore left word *that he would call again* — (3) Did call again (I think in a little more than an hour) & was told that the Astronomer Royal was at dinner & had no message & therefore went away . . . . added he did not call by *appointment* — He only took his chance on his way back from Devonshire to Cambridge . . . . I collected that he had been mortified (I am not using his own words) at receiving no message on the second call in October —." Emph in orig. The letter continues (the quotation-marks now become Sedgwick's, as he quotes Adams directly): "I thought . . . that though he [Airy] had been at dinner he could have sent me a reply, or perhaps spoken a word or two to me: but I am now convinced that he never knew of my second call — that the servant had not delivered my message along with my card —" Sedgwick goes on in his own words: "I asked him [Adams] whether [this incident] had any influence in preventing his reply to Prof. Airy's [1845/11/5] note. He said in answer, that had these not happened he possibly might have replied more readily . . . ." (Smart 1947 has already published excerpts from these exchanges, though he could only guess about the most important letter [1846/12/8], since it was kept from him; also from J.Glaisher earlier: SP xxvi-xxvii.)

[90] Which is partly why Airy forgave Adams (& see §J4). Also, Airy valued Adams' rare intellect. (And modesty: see the lovely tale at Smart 1947 pp.16-17, justly ranking Adams as "a prince among Senior Wranglers".) Adams' rumors may've started defensively: as early as 1846/10/10, he was told (by friend E.Spencer, JCASJ 14/15/3) that Airy was laying "the whole blame upon your shoulders".

[91] But it doesn't tell us why Airy didn't ask Adams about the 1845/11/5 question at their three known personal encounters [see also Kollerstrom's recent find in Adams' diary: §H6)] between then and the discovery (fn 68 & fn 75: 1845/12-1846/7/2). The obvious reason? Adams was going back over, shoring up, and extending his work — so he had no firm solution for Airy until later (fn 20: 1846/9/2). But openly admitting this would end Britain's priority-claim, so the ridiculous implicit contradiction pointed out at the beginning of this footnote had to stand (actually hide: no pre-*DIO* account has even

glad to receive Adams — even while gloating that Adams had been "trounced" enough (over his nonresponse to Airy's 11/5 letter) to put him in his place.[92]

J10 So Airy survived — and (just as Adams finally rose to the heights of theoretical astronomy: see DA's happy conclusion), Airy went on to be one of the great figures in the history of astronomy, that rare able administrator who was also a top-notch theorist. (The two types rarely combine, pioneer physicist Arago being a particularly sad example of the norm: though he was Leverrier's 1845 deputer and intensely loyal 1846 champion, he fell from power a few years later — at Leverrier's own hands.)

K Tale-Reduction

Summing-down our newly-informed view of the Neptune-chase history, one can say that preventing the old mythty-eyed tale from leading one astray requires only that one not lose sight of the indisputable key truths:

K1 Adams deliberately, repeatedly, systematically kept his results secret from the world. His later defense, told to Sedgwick (relayed[93] to Airy 1846/12/6), was almost funny: that telling a fellow-secrecy-conspirer constituted the equivalent of publication.

K2 As already noted at §B9 & fn 20 (from DA), Adams' alibis are each demonstrably either irrelevant or false. The truth is simple but is too unromantic for writers to like: while jumping about with various predicted longitudes (fn 20) for Neptune, Adams couldn't feel confident of Neptune's place[94] UNTIL HE VARIED THE MEAN DISTANCE (in his rigorous perturbation math: Hyp 2 vs Hyp 1), and he did not send the result (Hyp X) to RGO until 2 days after Leverrier's last paper was already published. This (perhaps reasonable) caution — and maybe also Adams' (ironically backfired) desire to sneak a march on Leverrier by not even telling him that they were racing[95] — is all there ever was behind Adams' hitherto-mysterious publication-delay, a delay which created both the non-pinpoint-prediction disaster and the pinpoint-prediction myth of Adams' miss of Neptune.

remarked the contradiction) in the place of honest history. (Note resemblance to fn 67.) Obviously, (but see fn 51), Airy was, at some point before 1846 Sept, aware of Adams' math block, unless we assume (as incredibly, the standard history *has* implicitly assumed) Airy set the largest UK refractor on the most intensive sky search ever up to that time without consulting (at least via Challis, with whom Airy was in dense correspondence) the next-door Cantab mathematician whose work co-launched the whole project. NB: Adams' non-reply ***is no excuse*** for incuriosity now regarding his research's status.

[92] Airy to Sedgwick (RGON 1846/12/10 p.2) comments on the latter's recent questions, which "no gentleman if free would ask and which no gentleman could be expected to answer. . . . I am now perfectly satisfied with what you have done . . . . But never let me see the low fellows who have caused it. With Adams, I have no quarrel whatever (he acted discourteously in not answering my [1845/11/5] inquiry, but he has been much trounced for it and so that is over) and I shall be glad to see him at any time." Note that the Adams-circle's ridiculous and refutable slanders against Airy allowed him to concentrate his fire where he was genuinely wronged — and, having shamed Sedgwick a bit, Airy was thus eventually able to distract Sedgwick from the points where Airy himself had behaved badly: §H8 & §I. Airy finally resorted to the ultimate ploy of the cornered: he shut off discussion: §J2 & fn 86.

[93] According to this letter (12/3 p.3), Adams told Sedgwick that he had "done my best" in sending his results to the two national observatories and had assumed Airy had told colleagues about his results. (See fn 42 and Airy's convincing rejoinder [RGON 12/4 p.3 or Smart 1947 p.40]: Adams' non-reply to Airy's 1845/11/5 inquiry shows that Adams "did *not* do his best".) Yet Adams' own failure to mention his research to the R.A.S. (DA §D1) or to speak up when he met (above: §B11) the great celestial mechanist P. Hansen (whose equations Adams was using!) shows that he took a share in the Cantab-circle secrecy [see also fn 74] — which cost him a share in the planet.

[94] [Adams' constant companion (fn 67) Challis believed that in 1845, Neptune's "position was determined but roughly" by Adams' work (§B9). Possibly Adams was only inspired by Leverrier (his 1846/6/1 paper) to perform a solution using a mean distance other than the Bode value 38 AU.]

[95] See §H5. Leverrier & Adams raced to be the first to achieve a solution independent of a pre-assumed mean distance. Leverrier finished in August and published 1846/8/31. Adams sent his solution to RGO two days later. (DA §D5 suggests a possible causal connection, though see fn 75.)

References

Same as in DA & DB [p.4's abbreviations], but with these additions:

RGON file = Royal Greenwich Observatory Neptune file.[96]

JCASJ = John C. Adams papers,[97] St. John's College (Cambr), cited by box/folder/item.

DA = D.Rawlins 1992W. DIO *2.3* ‡9.

DB = D.Rawlins 1994N. DIO *4.2* ‡10.

DC = DIO *7.1* ‡5 §A.

L Note Added 1999 October

As the clenchjawed Brit establishment attempts (e.g., fn 2) to cope with the ghastly disaster of public vindication of *DIO*'s charge[98] (published nowhere else) that *RGO's recent penultimate high official* stole the RGON file, the inevitable establishment alibis (e.g., §C7) are even now building. A warning: If the establishment gets the have-cake&eat-it idea that it can tuck away embarrassing key archival data *for decades* and later pay no price, by shrug-pretending it was all a mixup (similar case at *DIO 10* ⊙1), then no file or historical truth will be safe. And the RGON file's long disappearance was no accident:

L1 The theft undeniably occurred close to the time when an openly skeptical scholar (DR) asked to see the RGON file.

L2 The file had topsecret status long before the RGO official in question was even born. (See, e.g., fn 67.)

L3 The filching official himself said (§C7 item 4) that he got into the Neptune case by request and as part of his official RGO duties. For this reason among others (fn 26), it is obvious that Astronomer Royal Woolley knew who had the file.

L4 Are we to believe that when Woolley was asked (by DR) to see this sensitive file, it is purely coincidental that the person who hid it was his closest colleague? (The Henry II-Becket who-will-rid-me parallel [earlier Brit history] is almost too obvious.) See §C2.

L5 Photocopy machines were common in the 1960s, so there was no need for a researcher to remove a whole file of original mss.

L6 Not even a copy of the 1967-promised RGO *list* of the file's contents was ever forthcoming (see fn 29 and §L9).

L7 All 1960s RGO letters to DR regarding the "missing" file were (§B13) on private stationery, while letters on other matters were on official RGO stationery.

L8 Copies of the long-hidden full Adams-Sedgwick exchange have existed all along in JCASJ, though for over a century this file's letters were largely uncataloged (and therefore unaccessible, as DR learned during a 1996 Sept visit). But in 1998 Nov, right after the RGON file fell into foreign hands, the project of organizing them was begun (& rapidly completed by E. Q. Lawrence). So both copies of the key 1846/12/8 letter reappeared with impressive simultaneity (§H7): within a time ordmag 1/1000 the size of their secrecy-spans.

L9 During the brief period (1967) when RGO acknowledged to DR (DB §E10) that it had the file, RGO was making up a list of selected "letters for examination" (DB §E11) — i.e., it was preparing to hide the most embarrassing letters (just as had been done before Wm. Smart saw part of this material in 1946-1947). In brief, censorship was the 1960s RGO policy regarding these documents; and the file's disappearance at this very time displays a consistency (with that suppressive mentality) which is pathetically obvious.

[96] Originals at University of Cambridge Library. Microfilm available (tel 44-1223-33-3056, Adam Perkins). Copies can also be made by NOAO in Tucson, AZ (tel 520-318-8295, Mary Guerrieri) or NOAO in Cerro Tololo (tel 56-51-225-415, Elaine MacAuliffe).

[97] We are indebted to Kathryn McKee (Technical Services Librarian, St. John's College) for faxing new material from this source — and to Nicholas Kollerstrom for alerting *DIO* to its sudden availability.

[98] See above at §B14; original publication in DA §C 5 (1992) & DB §H6 (1994).

‡2 Evidence of an Ecliptical Coordinate Basis in the *Commentary* of Hipparchos

by KEITH A. PICKERING[1]

A Hipparchos' Spherical-trig Slip

A1 The only surviving complete work of the Greek astronomer Hipparchos (2nd century BC) is his *Commentary on Aratos and Eudoxos,* which contains several hundred partial positions of stars. These positions are expressed in a manner that is quite different from modern forms. For example, Hipparchos will state that a star rises at the same time as a certain degree of the ecliptic rises, or that a star sets at the same time as a certain degree of the ecliptic rises. Positions of this type are collectively referred to as "phenomena." The most common phenomena are those where Hipparchos states that a star culminates (i.e., transits the meridian) at the same time as a certain degree of the ecliptic culminates. There are over 200 stars described this way in the *Commentary* — many of them more than once, and frequently with different (conflicting[2]) results. These simultaneous culmination phenomena he called "mid-heaven" phenomena; here, we call these data "polar longitudes."

A2 There is now ample evidence that the Ancient Star Catalog (ASC) preserved in the *Almagest* of Claudius Ptolemy (2nd century AD) was in fact plagiarized[3] from Hipparchos, after adding 2°40' to the longitudes for precession. But contra these evidences, some believe that because the *Commentary* (unlike the ASC) contains no star positions in the standard ecliptical reference frame, that Hipparchos did not take ecliptical positions — and therefore could not have been the true author of the ASC. So if it can be shown that Hipparchos did in fact take ecliptical positions of stars, the case for his authorship of the ASC becomes even stronger.

A3 Not much is known about how Hipparchos obtained the data in the *Commentary*. It has been frequently assumed, on the basis of comments by Ptolemy, that Hipparchos used a celestial globe to chart his star positions, and may also have used the globe to perform spherical coordinate transformations. However, if Hipparchos used an armillary astrolabe to obtain star positions (and even this has been disputed by Neugebauer), a much simpler method is available. After sighting the star through the pinnules of the astrolabe, Hipparchos simply rotates the astrolabe (around its equatorial axis) until the pinnules are in line with the astrolabe's horizon ring. The astrolabe now is in a position that represents the sky at the time of the star's rising (or setting), and Hipparchos can simply read the position of the ecliptic ring against the eastern horizon ring to get the degree of the ecliptic that rises at the same time as the star. Since the ecliptic ring is not directly adjacent to the horizon ring, however, the result necessarily will be rougher than the star's ecliptical position; and indeed, the positions in the *Commentary* are recorded only to the nearest half-degree, about three times less precise than the positions in the ASC.

A4 But there is another possibility: if Hipparchos observed and recorded the stars ecliptically, then the positions in the *Commentary* could have been derived by using spherical trigonometry. This is especially true of the polar longitudes, because while the spherical

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[2] This despite those apologists for Ptolemy who claim that "standard ancient practice" forbade publishing multiple discordant data for a given phenomenon.

[3] Newton 1977, 211-256; Rawlins 1982; Rawlins 1994.

trig conversions for rising and setting phenomena are cumbersome (and differ by latitude), the spherical trig required for polar longitudes is fairly straightforward.

A5 Obviously, a polar longitude is simply a different way of expressing the right ascension α of a star. Converting ecliptically-observed coordinates into a polar longitude is a two-step process: first Hipparchos computes the star's α from its ecliptical latitude and longitude. Then he finds the degree of the ecliptic that has the same α. This second step was probably done tabularly, and need not have been done at the same time as the first step.

A6 Although the suggestion of spherical-trig conversions may seem speculative, there is evidence in the *Commentary* that this is exactly how the polar longitudes were derived, at least in some cases. This is because sometimes, in making these conversions, Hipparchos slipped up. In the second step of the process, Hipparchos is supposed to find the star's α in a table, and read across to find the degree of the ecliptic with the same α. But occasionally, instead using the star's right ascension α for the lookup, he would use the star's ecliptical longitude λ by mistake. Since α and λ usually have similar values, this error is not immediately obvious.

A7 The clearest case in which this error occurred is that of 32 Cygni, which is listed in the ASC at latitude $\beta = 64°30'$, longitude $\lambda = 302°40'$. After subtracting 2°40' for Ptolemy's incorrect precession, Hipparchos' observed longitude becomes $\lambda = 300°$. Hipparchos converted to equatorial coordinates to get $\alpha = 287°$, a value which he notes in his catalog. (Or, following the intelligent suggestion[4] of Grasshoff 1990, this was more likely a working proto-catalog from which both the *Commentary* and the ASC were derived.) But at this point the α is inadvertently dropped. Looking under the wrong column, Hipparchos used 300° (rather than the correct 287°) to look up the degree of the ecliptic needed for the mid-heaven phenomenon, and arrived at 298°, the exact value that appears in the *Commentary*. If Hipparchos had not slipped in step 2, he would have arrived at (after rounding) 285°30' for the polar longitude of 32 Cygni. The resulting twelve degree error is one of the largest of all polar longitudes in the *Commentary*.

A8 Finding all such errors in the *Commentary* turns out to be rather easy. First, we convert the ASC back to the epoch of Hipparchos by subtracting 2°40' from the longitudes. We then use these positions to compute α for each star with a polar longitude in the *Commentary*[5] (using the Hipparchan obliquity of 23°40'), and determine the "correct" polar longitude. We compare our computed polar longitude to the values given in the *Commentary* to determine the true error T. Next, we recompute the polar longitudes assuming Hipparchos actually made the λ-for-α slip described above. We compare our recomputed polar longitudes to the values given in the *Commentary* to determine the "slip-up" error S. If Hipparchos did not make a mistake, error T will be small and error S will be large; but if Hipparchos made the λ-for-α mistake, T will be large and S will be small. Therefore, dividing T by S will produce a very small number if the mistake was not made (which is usually the case), but a large number if the mistake was made. We plot these values in Figure 1 for all 273 polar longitude data in the *Commentary*, representing 203 stars. For convenience, we use the ecliptical longitude as the x-axis, and plot the absolute values of T/S on the y-axis.

A9 While most of the data hug the x-axis, a few discordant stars leap to attention. It is now easy to see that there may be several cases in which Hipparchos made this mistake. To determine if these are just statistical flukes, we need to find the probability that T/S is greater than a given value V. First we find the mean μ and standard deviation σ of all T and all S in the population. These are: for T, $\mu = -0.05, \sigma = 4.95$; and for S, $\mu = 3.72, \sigma = 23.72$. Next, assuming that T and S are normally distributed, we find the probability that T is **outside** of a given range $\pm t$, and multiply by the probability that S

[4] Grasshoff went out on a limb suggesting that Hipparchos' catalog was ecliptical in nature, so it is satisfying to see his courage vindicated here — a sentiment shared by the author (KP) and publisher (DR) of the present paper.

[5] Grasshoff 1990, Appendix C.

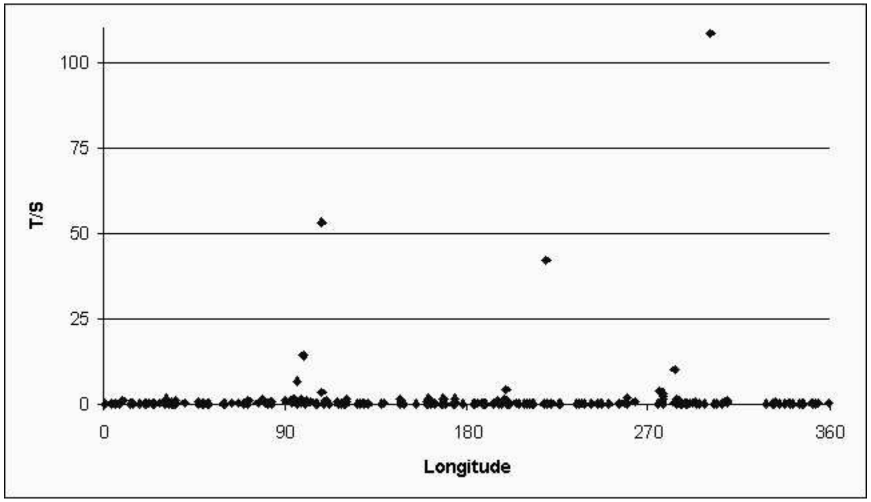

Figure 1: Parameter *T/S* plotted by longitude.

is **inside** the range $\pm t/V$. (As it turns out, in this case the probability is maximized when $t = 3.72$, regardless of V.)

A10 We find that in a population of 273, we would expect to find 1.1 cases where $T/S > 14$ (probability = 4.0×10^{-3}); but the *Commentary* actually has four. And we expect only 0.36 stars where $T/S > 42$ (probability = 1.3×10^{-3}); the *Commentary* has three. This makes it quite likely that these three cases are not random. These cases are 32 Cygni, λ Leonis, and ζ Ophiuchi; the details of each are given in Table 1 below.

A11 It is important to note that there are two requirements for this type of error to occur. First, Hipparchos must have had the ecliptical positions recorded in his proto-catalog. And second, Hipparchos must have converted these positions using spherical trig; this type of error is simply not possible using a globe or an astrolabe as an analog computer.

Table 1. λ-for-α error candidates.

Name	λ	β	α	Apl	Spl	Cpl	T	S	T/S
32 Cyg	300	64.5	287.0	285.7	297.9	298	12.3	0.1	108.3
ζ Oph	219.5	11.83	220.8	223.3	222.0	222	1.3	0.0	42.2
λ Leo	108.5	7.5	111.3	109.6	107.0	107	2.6	0.0	53.2

λ = longitude from the *Almagest* – 2°40'; β = latitude from the *Almagest*; α = RA computed from λ and β, using 23°40' obliquity; Apl = polar longitude computed from α; Spl = polar longitude assuming the slip occurred; Cpl = polar longitude given in *Commentary*; T = absolute value of Apl – Cpl; S = absolute value of Spl – Cpl.

B Theta Geminorum

B1 Another interesting proof of Hipparchos' use of ecliptical coordinates (and of Ptolemy's theft) is the case of θ Gem. This star was observed by Hipparchos with ecliptical coordinates using an ecliptical astrolabe. Hipparchos' originally observed coordinates were very likely $\lambda = 71°$, $\beta = 10°$, a position within a degree of correct in both coordinates, which is not atypical. (The fact that both of these are integers is the dead-giveaway for ecliptical observation.)

B2 But soon after this number was written down, a scribal error was made by Hipparchos or one of his assistants. In ancient Greek, the numeral 1 (uncial alpha) closely resembles the numeral 4 (uncial delta), and this is the most common[6] number confusion in many ancient Greek astronomical texts. So while the longitude $71°$ was observed at the astrolabe, and was written on an observational note, the number $74°$ was recorded in the proto-catalog, a huge three-degree error. Since this is several standard deviations above the mean Hipparchan error, it is a firm indication of a scribal slip.

B3 Hipparchos proceeds as before: converting his (incorrect) ecliptical coordinates to right ascension, he gets $71°.2$; then he converts a to polar longitude, getting $72°.7$, which is also three degrees too high because of the scribal error. He rounds[7] this to the nearest integer as $73°$, the number that appears in the *Commentary*.

B4 Then near the end of his career, Hipparchos again read the erroneous $74°$ position from his proto-catalog and published it in his final star catalog. Three centuries later, Ptolemy adopted Hipparchos' longitude of $74°$, added $2°40'$, and arrived at $76°40'$, the value he published in the *Almagest*. If Ptolemy had really observed θ Gem himself, he could not have gotten a result this bad — not even with repeated observation, the common excuse of Ptolemy's defenders regarding his planetary fabrications. And in Ptolemy's case, no write-1-read-4 scribal slip can be invoked, since his longitude contains neither a 1 nor a 4.

B5 We also know in this case that the polar longitude cannot have been the original observation, in the following way. Suppose that the polar longitude was the original number and the ecliptical coordinates were derived from it. In that case, starting with a polar longitude of $73°$, Hipparchos would have gotten a Right Ascension of $71°30'$. Combined with the latitude $10°$, this would have given an ecliptical longitude of $74°15'$ (after rounding to the nearest fraction in the ASC). Ptolemy would have added $2°40'$ to this, getting $76°55'$, which he would have rounded to $77°$: a full 1/3 degree higher than the longitude ($76°40'$) that actually appears in the *Almagest*.

B6 So the polar longitude can be derived from the ecliptical coordinates, but the reverse is not true: the ecliptical coordinates cannot be derived from the polar longitude. Therefore the ecliptical coordinates must be original, and the polar longitude (which appears in the *Commentary*) must be derived from them.

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[6] Peters & Knobel 1915, 9.

[7] Note that if Hipparchos had rounded to the nearest half- degree, as often occurs in the *Commentary*, he would have gotten a polar longitude of $72°30'$. Why he sometimes rounds to halves and sometimes to whole numbers remains a mystery.

‡3 Continued Fraction Decipherment: the Aristarchan Ancestry of Hipparchos' Yearlength & Precession

The Aristarchos Sidereal Year's High Accuracy
His pre-Hipparchos Knowledge of Precession
Consistency & Cause of Greek Tropical Year's Error

[Eighteen years ago, this paper was doubly refereed & accepted for publication by the Journal for the History of Astronomy *but was then suppressed for its §E heresy on Ptolemy. (Details below at §F.) Substantial changes & additions to the 1981 version are in brackets.]*

A Introduction

A1 Not long ago, Neugebauer republished and considered a couple of largely undeciphered lists[1] (see Table 1) of ancient values for the year's length, taken from the Vatican Greek mss collection.
A2 He comments[2] on the mysterious state of the numbers:

> The first number is the traditional value for the Greek version of the Metonic cycle. The remaining numbers are obviously corrupt; the number of Aristarchus in [list] A could perhaps be rescued by interpreting ξβ′ as [sixtieths of the second power] i.e. "seconds," but the resulting 365;15,20 would still leave [the Aristarchus year in list] B unexplained, nor is there any relation to another supposedly Aristarchean value 365;15,2,13, . . . [365^{d}1/4 + 3/4868 or 365^{d}1/4 + 1/(1622 2/3)].

[The previously puzzling digits of the mss are rendered in both Greek and Arabic in Table 1.]

B Solutions

B1 I have elsewhere already shown[3] that Eratosthenes' famous value for the obliquity (c.200 B.C.), 11/83 of a semicircle ($23°51'20''$), is derived from a continued-fraction process (upon his empirical result, $23°51'15''$).[4] And I find that others[5] have suspected that the continued-fraction technique (for approximating any number by a rational expression, as closely as desired) goes back at least as far as Eratosthenes' Alexandrian predecessor, Aristarchos of Samos (fl. 280 B.C.).

[1] Otto Neugebauer, *History of Ancient Mathematical Astronomy* (3 pts, New York, 1975), p.601. Lists published earlier by W.Kroll and E.Maass. Neugebauer (*loc. cit.* note 1) gives detailed citations, including the information that list A is from Vat. gr. 191 fol. 170^{v}; list B, from Vat. gr. 381 fol. 163^{v}.

[2] *Ibid.*, 601-602.

[3] For details, see Robert Newton, "The Sources of Eratosthenes' Measurement of the Earth", *Q. Jl Roy. astr. Soc.*, xxi (1980), 379-387, pp.386-387; or [Rawlins, "Eratosthenes' Geodesy Unraveled" *Isis*, lxxiii (1982), 259-265, pp.262-263. See also *DIO 2.1* ‡3 fn 26.]

[4] *Almajest* (henceforth [usually] abbreviated *Alm.*), I 12.

[5] Starting with Fortia d'Urban. See Thomas Heath, *Aristarchus of Samos* (Oxford, 1913), 336, and *Greek Mathematical Works* [Loeb C L], English translation by Ivor Thomas, ii (London, 1941), 14-15.

Table 1: The Vatican Mss Lists of Ancient Yearlengths in Days

List A:

Calendarist	Manuscript Data	
Meton, Euktemon, Philip	365 5/19	365 5/19
Aristarchos of Samos	[365] 1/4 κ′ ξ β′	[365] 1/4 20′ 60 2′
Chaldeans	365 1/4 ε′ ζ′	365 1/4 5′ 7′
Babylonians	365 1/4 1/144	365 1/4 1/144

List B:

Calendarist	Manuscript Data	
Euktemon, Philip, Apollinarios	365 5/19	365 5/19
Aristarchos of Samos	365 1/4 ι′ δ′	365 1/4 10′ 4′
Babylonians	365 1/4 ε′ ζ′	365 1/4 5′ 7′
Sudines	365 1/4 γ′ ε′	365 1/4 3′ 5′
———	365 1/4 ρ′ σ′[	365 1/4 100′ 200′?

Table 2: Continued-Fraction Interpretations of Table 1's Data

List A:

Calendarist	Year	Continued Fraction	Restored Year
Meton, Euktemon, Philip	Y_{E}	365 1/(4 − 1/5)	365 1/4 + 1/76
Aristarchos of Samos	Y_{At}	365 1/(4 + 1/(20 + 2/60))	365 1/4 − 15/4868
Chaldeans	Y_{Bt}	365 1/(4 + (1/5 − 1/7))	365 1/4 − 1/c.285
Babylonians	Y_{Bs}	365 1/(4 − 1/(9 + 1/4))	365 1/4 + 1/144

List B:

Calendarist	Year	Continued Fraction	Restored Year
Euktemon, Philip, . . .	Y_{E}	365 1/(4 − 1/5)	365 1/4 + 1/76
Aristarchos of Samos	Y_{As}	365 1/(4 − 1/(10 − 1/4))	365 1/4 + 1/152
Babylonians	Y_{Bt}	365 1/(4 + (1/5 − 1/7))	365 1/4 − 1/c.285
Sudines	Y_{St}	365 1/(4 + (3 1/5)/60)	365 1/4 − 1/304
———	Y_{Ag}	365 1/(4 − 1/(100 + 100/60))	365 1/4 + 3/4868
———	$Y_{Ag'}$	365 1/(4 − 1/100)	365 1/4 + 1/1596

B2 Until the revelations to follow (below), I would say that the starkest hint of ancient use of continued fractions was the Archimedes approximation-bracketing of π:

$$3 + 10/71 < \pi < 3 + 10/70 \tag{1}$$

Why else would the variation appear in the denominator?

B3 Continued fractions are the key that unlocks Table 1. If we presume that the table's data evolved from an uncomprehending scribe's[6] copying numbers which represented continued-fraction expressions (in whole or in part), then we may easily find the solutions set forth here in Table 2 (where I render the unmysterious cases also as continued fractions, just to provide extra illustrative examples).

B4 At first glance, it may appear that the sign-choices are arbitrary. However, upon reflection, it is obvious that without some negative signs, all the years listed in Greek notation would be shorter than $365^{\mathrm{d}}1/4$ (and thus sidereal years would be unrepresentable) by the continued-fraction interpretation of Table 1. And, without some such interpretive hypothesis, the figures of Table 1 are the gibberish that Neugebauer rightly calls them.[7] An obvious point: no normal Greek fractional expression used fractions in non-descending order of size, as in, e.g., the case of list B's Aristarchos year, $365^{\mathrm{d}}1/4$ 1/10 1/4.

B5 This case also illustrates why flexible use of plus or minus signs is efficient (and thus would be attractive to those ancients who worked with continued fractions): if one tries to express $365^{\mathrm{d}}1/4 + 1/152$ using only the usual plus signs, the result is:

$$365^{\mathrm{d}} + \cfrac{1}{3 + \cfrac{1}{1 + \cfrac{1}{8 + \cfrac{1}{1 + \cfrac{1}{3}}}}} \tag{2}$$

But simply permit sign-flexibility, and the outcome is much compacted:

$$365^{\mathrm{d}} + \cfrac{1}{4 - \cfrac{1}{10 - \cfrac{1}{4}}} \tag{3}$$

which is the expression for Y_{As} in Table 2.

B6 Anticipating the inevitable question regarding the influence of preconception, let me say that I started investigating Table 1 in search of values for the *actual* tropical year Y_{t}. [See fn 18 for real ancient tropical and sidereal yearlengths.] I never found a one. The only plausible tropical results instead kept coming out near the old standard (but highly erroneous) Hipparchos-Ptolemy value (eq. 10), $Y_{\mathrm{Ht}} = 365^{\mathrm{d}}1/4 - 1/300$.

[6] An alternate hypothesis is that a deliberate attempt was made to disguise-encode the values of the length of the year so that non-initiates could not understand them. The legendary secrecy of ancient science (or pseudoscience) has been knowledgeably ridiculed by Neugebauer (*op. cit.*, 566): "all these 'secrets' were eagerly written down and have survived in countless copies . . . ." But the inherent sampling-completeness infirmity [*DIO 4.2* ‡9 §I] of this reasoning reminds one of Count Fosco's retort to the confident idea that crimes always out: "Yes — all the crime *you* know of. And what of the rest?" (Wilkie Collins, *The Woman in White*, 1859-1860.) Whatever the degree of success, it is undeniable that some ancient cults treated knowledge as an elitist treasure not to be lightly shared. See, e.g., #143 of *The Letters of Synesius of Cyrene*, English translation by A.Fitzgerald (Oxford Univ, 1926).

[7] See Neugebauer *op. cit.*, p.602. Also R.Gillings *Historia Mathematica*, viii (1981), 456-457 on Egyptian writing of fractions (a different context, though Alexandria is geographically Egyptian): "Scribes never used signs" (brought to my attention by Owen Gingerich in 1982 February).

B7 Some of the Table 2 interpretations of Table 1's data may look arbitrary; however, considering the relatively small number of interpretive options available, one must ask what the odds are on: [a] The exact appearance of two known Aristarchos (calendaric) numbers, 152 (§C8) and 4868 (§C3). [Note that these numbers may be related: 32 cycles, of $152^{\mathrm{y}}1/8$ each, constitute 4868^{y}. Note also that Hipparchos' final lunisolar tabular epoch (*DIO 1.1* ‡6 eq.28), −127/9/24 1/2, was $152^{\mathrm{y}}1/4$ after Aristarchos' −279 S.Solstice, which we know Hipparchos used to found his Y_{Ht}: *ibid* §B4.] [b] The repeated appearance of integer-factor multiples of 76 (Kallippic), namely +1 (Euktemon), +2 (Aristarchos), −4 (Sudines).

C Comparisons

C1 For simplicity in explanation below, we set the convention that the year Y is related to a reciprocal remainder R (reflecting the deviation from the Kallippic-Julian year $Y_{\mathrm{K}} = 365^{\mathrm{d}}1/4$) by the easy equation:

$$Y = Y_{\mathrm{K}} + R^{-1} = 365^{\mathrm{d}}1/4 + 1/R \tag{4}$$

C2 The Kallippic cycle of 76^{y} obviously figures in the Table 2 data. The Euktemon year Y_{E} ($365^{\mathrm{d}} + 1/R_{\mathrm{E}}$) exceeds Y_{K} by an accumulated total of 1^{d} per Kallippic cycle — i.e., $R_{\mathrm{E}} = +76$. Aristarchos' sidereal year Y_{As} (list B): 1^{d} excess every 2 Kallippic cycles (or 152^{y}), i.e., $R_{\mathrm{As}} = +152$. Sudines' (c.240 B.C.)[8] tropical year Y_{St}: 1^{d} defect every 4 cycles (or 304^{y}), i.e., $R_{\mathrm{St}} = -304$.

C3 The Aristarchos tropical year Y_{At} (list A): 1^{d} defect every 18 Saros cycles, where we know[9] that Aristarchos' value for the Saros was

$$18^{\mathrm{y}}10^{\circ}2/3 = (1622^{\mathrm{y}}2/3)/90 = 4868^{\mathrm{y}}/270 \tag{5}$$

Thus, $R_{\mathrm{At}} = -18{\cdot}4868/270 = -4868/15 = -324\ 8/15$. Since the number 4868 has long been associated with Aristarchos (see §A2, fn 11, and Neugebauer *op. cit.* p.603), its appearance in Table 2 under his name is particularly striking.

C4 The Babylonian sidereal year Y_{Bs} (list A) shows an excess over Y_{K} ($365^{\mathrm{d}}1/4$) of 1^{d} every 144^{y}, which may be a rounding of 8 Saros (about $144^{\mathrm{y}}.2$). $Y_{\mathrm{Bs}} = +144$.

C5 The Table 2 expression for the Chaldean (list A)-Babylonian (list B) tropical year Y_{Bt} looks over-speculative at first; however, if we presume an intent to quantify a year which corresponds to a defect of 1^{d} every 15 Metonic (19^{y}) cycles, that is every 285^{y} ($Y_{\mathrm{Bt}} = -285$), then a continued fraction solution would be (rounding 17.5625 to 17 1/2 or

[8] Neugebauer *op. cit.*, p.611 n.31.

[9] See fn 11 below. (*Alm.*, IV 2 has same Saros-length as eq. 5. The Tannery-reconstructed Aristarchos period was half of that equation's 4868. And 2434 years Y = 30105 synodic months M = 32265 anomalistic months A = 32670 draconitic months D = 32539 sidereal months. These figures use the relatively simple Saros relations (*Alm.*, IV 2): $223M = 239A = 242D$ (where $242 = 2{\cdot}11^2$). It is a provocative [though far from conclusive] coincidence that the simplest fractional expression which will yield precisely eq. 13's monthlength M (*Alm.*, IV 2) is 99902/3383, where we note that 99902 = 2·11·19·239. These familiar factors permit simplifying various periods via convenient cancellations:
$Y_{\mathrm{M}} = 235M/19 = 235{\cdot}2{\cdot}11{\cdot}239/3383 = 365^{\mathrm{d}}1/4 - 43/13532$.
$A = 223M/239 = 223{\cdot}2{\cdot}11{\cdot}19/3383 = 27^{\mathrm{d}}1873/3383$.
$D = 223M/242 = 223{\cdot}19{\cdot}239/(11{\cdot}3383) = 27^{\mathrm{d}}7892/37213$.
(In an age of tedious computational means, relatively short fractional expressions were preferable to long sexagesimal ones. [Similarly: since 3383 is 17·199, cancelling 17 in the key eq.1 of *DIO 6* ‡1 finds: $2{\cdot}11{\cdot}17{\cdot}19{\cdot}239{\cdot}251/(17{\cdot}199) = 126007^{\mathrm{d}}9/199$.]) For a completely different explanation of M (also to all sexagesimal precision given in *Alm.*, IV 2), see Noel Swerdlow, "Hipparchus's Determination of the Length of the Tropical Year and the Rate of Precession", *Archive for History of Exact Sciences*, xxi (1980), 291-309, pp.307-308. [Also simple DR solution bracketed below at §D4.]

35/2, to avoid the lengthy exact solution):

$$365^{\mathrm{d}}1/4 - 1/285 = 365^{\mathrm{d}} + \cfrac{1}{4 + \cfrac{1}{17.5625}} \approx 365^{\mathrm{d}} + \cfrac{1}{4 + \cfrac{2}{35}} \tag{6}$$

The ancient preference for unit fractions [& compactness] would quickly tranform the last expression to $365^{\mathrm{d}}1/(4 + (1/5 - 1/7))$ — as shown in Table 2.[10]

C6 Attempts to solve the last item in list B of Table 1 must contend with the lacunae in data and even name. A guess is that this year might be related to the Aristarchan year relayed by Censorinus:

$$Y_{\mathrm{Ag}} = 365^{\mathrm{d}}1/4 + 1/1623 \tag{7}$$

which Tannery reconstructed[11] more exactly as

$$Y_{\mathrm{Ag}} = 365^{\mathrm{d}}1/4 + 3/4868 \tag{8}$$

C7 One possible explanation of the Table 1 version is: $365^{\mathrm{d}}1/(4 - 1/(100 + 100/60))$, which equals Y_{Ag} precisely.[12] Another explanation is: the nearest integral multiple of R_{Ag} in Kallippic cycles is 21 cycles, which is 1596^{y}, the alternate figure found in Table 2 ($R_{\mathrm{Ag'}}$ =1596), where it is implicitly speculated that σ' is not a number but part of a mutilated word on the original record list B was copied from. This value is consistent with the fact that all other years of list B have here been found to be based upon fit-roundings to the Metonic cycle (19^{y}) or Kallippic cycle ($76^{\mathrm{y}} = 4{\cdot}19^{\mathrm{y}}$), not the Saros.

C8 The relating (in Table 2, list B) of the Aristarchos sidereal year

$$Y_{\mathrm{As}} = 365^{\mathrm{d}}1/4 + 1/152 \tag{9}$$

($R_{\mathrm{As}} = +152$) to him is as definite as was the case with his tropical year (above) — since we know[13] that Aristarchos' 280 B.C. Summer Solstice [$-279/6/26$ noon][14] was exactly 152^{y} after the famous Meton 432 B.C. Summer Solstice.

[10] Subsequent to positing this (in the 1st draft of this paper, transmitted for me by Michael Hoskin to the 1980 June Aristarchos conference on Samos [where it was not read]), I realized the likely origin of $R_{\mathrm{Bt}} = -285$. It is clear from *Alm.*, IX 3 that [the ancients quite commonly] rounded to 60ths of a day (i.e., one sexagesimal place) when reporting astronomical period relations. If one expresses the Metonic cycle so, it is [by eqs. 12&13] $6939^{\mathrm{d}}41'$. Divide by 19, and one gets exactly $365^{\mathrm{d}}1/4 - 1/285$. (Note also that 285^{y} [underlies both Ptolemy *Alm.*, III 1 equinox fabrications: *DIO 8* ‡1 Table 1].)

[11] See Heath, *op. cit.*, pp.314-315, and Neugebauer, *op. cit.*, pp.603-604. [It is also possible that the last entry in List B is related to the anomalistic year. See *DIO 6* ‡1 eq.6 & fn 40.]

[12] The precise agreement has of course been achieved only because σ' has been arbitrarily presumed to be a scribe's error for $100'$ or 100/60. (Sole defense: the astonishing agreement with Y_{Ag}.)

[13] *Alm.*, III 1. [Alternate theory for Aristarchos B: $365\ 1/(4 - 1/(10 + 1/4)) = 365\ 1/4 + 1/160$. If this is the right interpretation, it might be based on the $-1079/6/24$ eclipse. See *DIO 6* ‡1 §I2.]

[14] [See *DIO 1.1* ‡6 eq.8. The occurrence of the actual 280 B.C. Summer Solstice was the better part of a day later. See §E3. Thus, the precise observed solstice-time may have been rounded to equal the noon of the calendar day *containing* the event. See Rawlins *Bull. Amer. Astron. Soc.*, xvii (1985), 583, where it is suggested that Aristarchos indeed truncated the observed Summer Solstice's time to diurnal epoch-hour (noon for him) a practice that (the 1985 paper theorizes) would be a natural tradition for calendaric astronomers since Meton — and which obviously tended to cause later astronomers (who had to use older solstice data for computing mean yearlength) to systematically overestimate Y. See also *DIO 1.1* ‡6 §E4.] Proximity to the Dionysios era suggests Aristarchos' connexion to the $365^{\mathrm{d}}1/4$ Dionysios calendar, which was used [by Dionysian heliocentric astronomers: *DIO 1.1* ‡1 §D] to date eight *Alm.* data (3rd century B.C.), mostly observations of Mercury. We take epoch (start of Dionysios year 1 = Ptolemy II year 1) = $-284/6/26$ noon and adopt the reconstruction of August Böckh, *über die vierjährigen Sonnenkreise der Alten* . . . (Berlin, 1863), 286-340. The lone non-fitting *Alm.* date is the Mars-βSco conjunction, off by 1^{d} — but R.Newton has found that the real conjunction was 1^{d} off the *Alm.* report. Thus, Böckh's scheme seems completely vindicated. (Noon epoch is my speculation [based on noon's diurnal analogy to the Summer Solstice's annual effect].) I suspect that the 280 B.C. observation was taken to help establish the Dionysios calendar. By this scenario, the 1^{h}-precise observed time is now lost (and was not even known to Hipparchos or Ptolemy).

C9 Both this value and the Babylonian sidereal year Y_{Bs} ($R_{\mathrm{Bs}} = +144$) are close to that of the tables of the *Almajest* (where $R_{\mathrm{Ps}} = +147$)[15] and to the actual[16] sidereal year Y_{s} of that epoch, when $R_{\mathrm{s}} = +\,153$.

C10 Notice that all the tropical years of Table 2 (Y_{At}, Y_{St}, Y_{Bt}) are very erroneous yet all are quite near the rounded value later used[17] by Hipparchos and Ptolemy, eq. 10. And $R_{\mathrm{Ht}} = -300$, whereas actual[18] $R_{\mathrm{t}} = -133$ in [the former's era, becoming -132 by Ptolemy's time].

D Conclusions

What do we learn from the foregoing?

D1 Table 1 is the oldest extant material expressed in continued-fraction format, albeit corruptly.

D2 The veil has been pulled aside from a flock of long-lost ancient values for the length of the year.

D3 The sidereal-year estimates (Y_{s}), being strictly astronomers' values, are unforced to civil considerations. [Which is part of the reason that ancients' Y_{s} are so much more accurate than their Y_{t}.] These Y_{s} values are probably based [see, e.g., Rawlins, *Vistas in Astronomy* xxvii (1985), 255-268 §5, also *DIO 6* ‡1] upon lunar eclipse observations, which do not require high precision visual measurements to determine Y_{s} to good accuracy. [See *DIO 1.1* ‡6 fn 1.] As seen above, Aristarchos' value, Y_{As}, is astonishingly accurate (R_{As} = 152 vs. actual R_{s} = 153 for his epoch), correct to a few time-seconds. However, this proximity may be much an accident of rounding.

[15] Perhaps $R_{\mathrm{Ps}} = +147$ was also Hipparchos' value. However, there is evidence that he used the nearby value, $R_{\mathrm{Bs}} = +144$. See Neugebauer, *op. cit.*, 293, and Swerdlow, *op. cit.*, 300. [See also *DIO 6* ‡1 §C.]

[16] This figure includes both the secular variation of the Earth's revolution about the Sun and [the greater (but oft-ignored)] effect of the Earth's spin acceleration [*DIO 6* ‡1 fn 53] upon the length of the day itself. [(Gingerich 1981 *Q. Jl Roy. astr. Soc.* 22:44 ignores both.) See also fn 18. It is sometimes helpful to have at-hand rough values for the accumulated effect; so, accurate to ordmag 10^{m}, I give ΔT (and the Besselian date) for each of several calendar epochs: Meton (-430.527) 4^{h}, Kallippos (-328.525) $3^{\mathrm{h}}3/4$, Dionysios-Aristarchos (-283.527) $3^{\mathrm{h}}1/2$, Hipparchos (-126.278) 3^{h}, Antoninus-Ptolemy ($+137.547$) $2^{\mathrm{h}}1/4$. Since empirical estimates of Y were based on data extending backwards in time, the apt R_{s} for comparisons would be a little less than that for the astronomer's own era. For Aristarchos, we make no adjustment, since R_{s} was likely on the high side of 153 in 280 BC.]

[17] *Alm.*, III 1 — though Censorinus (Heath *op. cit.*, 297) rendered Hipparchos' year as $365^{\mathrm{d}}1/4 - 1/304$ (which is Y_{St} of Table 2).

[18] See fn 16. [R_{s} & R_{t} are obviously not exact. Each is figured for the era of Aristarchos and rounded to the nearest integer, being uncertain by a few tenths of a unit in the last place. (Both figures grow more positive in time, roughly 2 units/millennium, sidereal rather more slowly than tropical.) Note that the most applicable R — that for Summer Solstices — is not really -133 (a value based upon mean solar motion) but is instead -124. (So the ancients' values [§D4] were even farther from empirical truth than is indicated by superficial analysis.) This is because all knowledgeable ancients used Summer Solstices to gauge yearlength Y. (See fn 25.) It is seldom appreciated by modern investigators that (due to the variation of eccentricity & apse in the Earth's orbit) each seasonal event has its own yearlength Y (differing from the others by ordmag 1^{m}) and so (by eq. 4) its own R. I here provide these for 280 BC: S.Solstice $R_{\mathrm{t}} = -124$; A.Equinox $R_{\mathrm{t}} = -138$; W.Solstice $R_{\mathrm{t}} = -144$; V.Equinox $R_{\mathrm{t}} = -129$. (The harmonic mean of opposite values of course yields -133 for each pair.) The sidereal year will also differ from star to star, around the zodiac. (And Y_{s}'s periodic variation, as a function of chosen zero-point longitude, will be similar to Y_{t}'s.) So our analyses here of R_{s} implicitly assume a broad enough ancient observational data-base, that mean solar sidereal motion was being measured.]

D4 Ancients' estimates of the tropical year (Y_t) ought to have been based upon centuries of high-precision transit observations.[19] However, the tropical year was the civil and religious year, in an age of strong traditional reliance on lunar calendars. Thus, Tobias Mayer long ago suggested[20] that Hipparchos' yearlength,

$$Y_{Ht} = 365^d 1/4 - 1/300 \tag{10}$$

(6^m too long), was based on a fit to the Kallippic cycle (established 330 B.C.) relating tropical (civil) years Y to synodic (civil) months M, by defining a Metonic year Y_M from the lunisolar equation

$$76Y = 940M \tag{11}$$

which is the same ratio as the older Metonic cycle (established 432B.C.),

$$19Y_M = 235M \tag{12}$$

The length of the month had become [*DIO 1.1* ‡6 fn 1] very well established by Aristarchos' time at the excellent estimate:

$$M = 29^d;31,50,08,20 \tag{13}$$

(accurate to well within 1^s, both in antiquity AND today). [*DIO loc. cit.* speculates that he was responsible for determining this famous value — by the extremely simple method explained at *DIO 6* ‡1 §A5 & fn 18.] So the [Metonic "year"] was

$$235M/19 = 365^d 1/4 - 1/315 \tag{14}$$

i.e., $R_M = -315$ very nearly. That Mayer's theory is no longer just a speculation may be discerned from the tight cluster of tropical year values Table 2 has showered upon us all at once; adding[21] in already-wellknown Y_{Ht} (eq. 10), we have the following tropical R_t values: $R_{Bt} = -285$, $R_{Ht} = -300$, $R_{St} = -304$, $R_{At} = -324$ 8/15. All four are very near the Mayer-Metonic calendaric-numerological value, $R_M = -315$, and all four are very far from the correct ancient value, $R_t = -133$ [actually -124: fn 18]. As we have already seen, each astronomer rounded in a different fashion (using, e.g., 1st or 2nd place sexagesimal roundings *en route* to Y_t, or rounding R_t to the nearest multiple of Metonic, Kallippic, or Saros cycles, or centuries). But obviously all shared the *a priori* prejudice that $R_t = 315$ (or some nearby transformation of it) was about right. [Even accounting for the systematic effect noted at fn 14], it is impossible to believe that all 4 of the R_t values just reprised (-285, -300, -304, -324 8/15) were based upon independent *solar* observations that just happened by chance to arrive in the vicinity of the same (very wrong) *lunar*-based (eq. 14) value, $R_M = -315$. A classic contrast of numerological forcing vs. what was to be expected from [neutral] empirical observation ($R_t = -133$). The overwhelming vindication here of Mayer's supposition ensures that we now know the source of the wellknown $+6^m$ error in Y_t — which accumulated [(*DIO 8* ‡1 ⊙1) to 26^h] in the interval between the solar theories of Hipparchos (c.-130) and Ptolemy [whose date is almost irrelevant here, since all of his alleged +132-140 AD solar "observations" were faked], producing the infamous solar mean longitude error (over 1°, negative) that infected all of Ptolemy's tables (the fundamental astronomy for which was borrowed entirely from Hipparchos) — the fatal error which ultimately revealed the truth behind Ptolemy's pretense that he was a regular observer of the sky.[22]

D5 Annual precession p (the difference between the sidereal and tropical years) [was understood by Aristarchos, though not by Meton or even Kallippos (who proposed only one yearlength each), so Aristarchos was the earliest known scientist to recognize precession. The difference was also evidently (Table 2) accounted for by Babylonians — who are, we note, listed later than Aristarchos, in *chronologically-ordered* Table 1. (See *DIO 1.1* ‡6 §§B11-B13.) Aristarchos gauged p] at very nearly 0°.01 (or 0^d.01) per year (in rough terms: 1/150 + 1/300 = 1/100), the mistaken figure adopted over a century later by Hipparchos (hitherto universally credited with the discovery of precession). We may even derive exact figures for these early precession values by using the following equation for finding centennial precession P:

$$P = 100(360°/Y)(Y_s - Y_t) \tag{15}$$

From this and the numbers developed in this paper, we find for Aristarchos [whose Y_{As} & Y_{At} in Table 2 are fortunately the firmest decipherments there]

$$P_A = 100(360°/Y) \cdot (1/152 - [-1/(324\ 8/15)]) = 0°.952 \tag{16}$$

and for Babylonia:

$$P_B = 100(360°/Y) \cdot (1/144 - [-1/285]) = 1°.03 \tag{17}$$

— both deduced values being close to that of Hipparchos-Ptolemy [$P_H = 1°.00$], but far from the truth for antiquity (1°.38).

E Prejudices [The Part Totally Suppressed by Hoskin]

E1 Hipparchos' lack of originality in his tropical year (eq. 10) and his centennial precession of $P_H = 1°$, should not entirely surprise us.[23]

E2 However, to forestall potential misunderstandings of the implications of this paper, I must here stress that there is little comparison between his failings and Ptolemy's with respect to the issue of non-originality.

E3 His month (eq. 13) was accurate (thus agreement is not suspicious — his own researches indicated a trivially different value).[24] His Y_{Ht} was based upon a comparison of solstices (wiser than using equinoxes):[25] his own -134/6/26 1/4 Summer Solstice [error -1^h] and Aristarchos' -279/6/27 1/2 Summer Solstice [error -16^h] (The total $+15^h$ error[26] of the time-difference, for 1 1/2 centuries, led directly to the fateful error

[19] Transit-observations of ordmag 1′ accuracy were within the capability of some ancient astronomers. See *Alm.*, I 12 [also Rawlins 1982 (of fn 3) n.17, and Rawlins 1985 (of §D3) §§3&5.]

[20] Swerdlow *op. cit.*, 292; other scholars ([including Swerdlow and] myself) have since rediscovered Mayer's finding, primarily Kristian Moesgaard and Raymond Mercier (see *ibid* 293), contra R.Newton.

[21] Swerdlow (*ibid.*, 292) suggests that $R_{St} = -304$ (fn 17, above) might have been rounded to $R_{Ht} = -300$ via a rounding of a Kallippic cycle of $940M$ to $27758^d45'$, followed by a 2nd-sexagesimal-place rounding of a 76th of the result, which would indeed give $365^d14'48''$ (*Alm.*, III 1). [This speculative reconstruction is not at all impossible (similar discussion on rounding at *DIO 1.3* fn 274);] but without the 1st rounding, the upshot would've been $365^d14'49''$, thus $R \approx -327 \approx R_{At} = -324$ 8/15 (Table 2 list A). Alternate theory: 432 B.C. to 135 B.C. ≈ 300^y (fn 14 above).

[22] See fn 29 below. Perhaps the most obvious proof that Ptolemy was not a regular observer is the simple fact that he did not know his own alleged observatory's latitude! (*Alm.*,V 12-13 — an error of $-14'$.) [See Rawlins 1987 (of §E3) p.236 item (2).]

[23] [See, e.g., *DIO 1.3* §N16.]

[24] *Alm.*, IV 2. Note that use of lunar eclipses for finding the synodic month and the sidereal year allows far greater accuracy (from modest visual work) than the use of solar transit observations (*Alm.*, I 12 and III 1) allows for the determination of the tropical year (see above at §§D3&refsec-aD4). See Rawlins (of §D3); also *DIO 6* ‡1.

[25] See R.Newton, *Crime of Claudius Ptolemy* (Johns Hopkins Univ, 1977), 81-82 and 85-86. Also Rawlins here at fn 14 [and at *DIO 1.1* ‡5 fn 20].

[26] See fnn 14&16, above.

in $Y_{\rm Ht}$: $0^{\rm h}.1 = +6^{\rm m}$.) Hipparchos could be faulted for (probably [see *DIO 1.1* ‡6 §E5]) prejudiced selection of the required ancient-to-him [S.Solstice] (Aristarchos'), but nothing more. His own solstice's error was trivial. (Aristarchos' was not. [For cause, see fn 14.]) Hipparchos' 21 [extant] solar observations are of fair accuracy (rms error $8^{\rm h}$) — normal random and systematic[27] errors. By contrast, Ptolemy's 4 solar "observations", which agree with Hipparchan data to within less than a half-hour in all cases, disagree with reality by [$21^{\rm h}$, $33^{\rm h}$ (twice), and $36^{\rm h}$]. The Ptolemaic data's [$31^{\rm h}$] rms deviation from reality is over 50 times [their $0^{\rm h}.6$ rms] deviation from Hipparchos' solar tables. For Hipparchos himself, this ratio is only about 2 (instead of 50+) and even much of that is due to the fact that of course his tables were fit to his observations. [Thus, the common blanket-slander that ancient scientists were non-empirical is simply one more Hist.sci-establishment fantasy. See Rawlins, *Amer. J. Physics*, lv (1987), 235-239, n.12. See also fn 19 here.] (Even so, Ptolemy's "observations" adhere 5 times more closely to Hipparchos' tables than do the Hipparchan observations on which the tables were founded!)

E4 The point to keep in mind here is that whereas gauging the *year* entails dependence upon a predecessor's work (e.g., Hipparchos' use of Aristarchos'), finding solar *positions* has no such dependence. (Thus, the impact of the above-cited ratios.) The reality of Hipparchos' data for position is clear from a glance at his solar theory: despite its fallacious year-length (mean motion), its longitude-at-epoch is very close to the truth for his own time. [See *DIO 1.1* ‡6 §D7.] On the other hand, all of Ptolemy's positional "observations" are consistent with this same theory — a theory that gave correct positions only for Hipparchos' time, but was slow by over 1° by Ptolemy's epoch.[28]

E5 R.Newton's analyses of Ptolemy's data (especially solar and stellar)[29] have concluded that he was not simply prejudiced but that he systematically deceived (to high precision) in support of these prejudices. Newton's conclusion has been attacked with such passionate disbelief in a variety of journals (by commentators all whom had, in earlier publications, prejudged Ptolemy as a great astronomer), that many onlookers may not be aware that a number of scholars agree that Ptolemy has indeed been shown to have been a liar. These include B. L. van der Waerden and myself.

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[27] All of Hipparchos' Vernal Equinoxes are a few hours early; all his Autumnal Equinoxes are a few hours late. (This is perfectly consistent with his transit instrument's equator having been set a few arcminutes low. [But see the attractive alternate theory implicit in Swerdlow's proposed Hipparchos solar parallax: *DIO 1.3* fn 280.]) By contrast, all of Ptolemy's equinoxes (Vernal *and* Autumnal) are late. (Very. The smallest of Ptolemy's three Equinox errors exceeds the largest of Hipparchos' twenty Equinox errors — Ptolemy's rms error was 4 times Hipparchos'.) This circumstance cannot be explained by misplacement of the transit instrument, which Ptolemy says he used, *Alm.*, III 1 (unambiguous ref. to *Alm.*, I 12; see R.Newton, "Comments on 'Was Ptolemy a Fraud?' . . . ", *Q. Jl. Roy. astr. Soc.*, xxi (1980), 388-399, pp.389-390).

[28] This is illustrated, to devastating effect, by Raymond Mercier at p.215 of *British Journal for the History of Science*, xii (1979) 211-217, his review of R.Newton, *Ancient Planetary Observations* . . . (Johns Hopkins Univ, 1976). Note too that almost all of "Ptolemy's" *Alm.* tables end in 82 AD [a fact first broadcast by A.Rome] .

[29] See R.Newton, *opera cit* (fnn 14, 25, & 29) and "On the Fractions of Degrees in an Ancient Star Catalog", *Q. Jl. Roy. astr. Soc.*, xx (1979), 383-394; also Owen Gingerich, "Ptolemy Revisited . . .", *Q. Jl. Roy. astr. Soc.*, xxii (1981), 40-44, p.42.

F *JHA* Editorial Integrity. Again . . . . [Note Added 1999 Dec]

F1 The foregoing is the paper long referred to (semi-facetiously at first) as "Rawlins 1999", in numerous articles appearing in this journal. (Some of the history of its persistent suppression was previously recounted at *DIO 1.2* §B2 & fn 9.)

F2 Since the paper's main math opposed (fn 20) a specific R.Newton contention, Owen Gingerich encouraged the expansion of a brief 1980 version of it for publication in the *Journal for the History of Astronomy*. Almost two decades ago, this was submitted (1981/8/7) in the version printed here at §§A-E and was promptly accepted[30] (1981/9/17) by *JHA* Editor Michael Hoskin. However, the 1981 version ended with a section (§E) that defended Newton's general view. In retrospect, it is obvious that the *JHA* wanted only the anti-Newton part — but didn't want to say so. Thus, there ensued a ridiculous series of publication-postponements (during which time the paper could have been before an honest journal), accompanied by increasingly thespian expressions of sorrowful apology.

F3 DR repeatedly inquired as to whether the *content* was a problem. Consistent denials of this (indicating *JHA* reluctance to *appear* censorial, evidently hoping that enough delay might push DR into angry withdrawal of the paper, thereby neatly solving the *JHA*'s theological dilemma) were followed by a sudden 1982/7/27 demand for *immediate* DR assent to a version chopping off the concluding section (the pro-Newton part: §E above), which had committed the special sin of pointing out (what the *JHA* was loathe to reveal) that some reputable scholars such as van der Waerden (fn 34) did not agree with the *JHA* crowd's rejection of Johns Hopkins Univ physicist Robert Newton's recent charges that Ptolemy was a liar. The *JHA* continues to imitate its hero's ethics in its private explanations of this incident. (See *DIO 1.2* §B1. The attendant ugliness, false slander, and censorship led ultimately to the birth of *DIO*.) [In 2002, *JHA*'s smear was published nationally. See *DIO 11.1* p.2.]

F4 The *JHA* has quite recently shown that its integrity has not changed: it continues to dishonor its own original agreement to publish the full paper, as it *even today* insists that §E5 must be censored. Disturbed that the paper's remarkable results were being kept permanently from the academic community, and increasingly convinced (by the behavior described below) that the *JHA* would exercise interminably its idea of high creativity by continuing to find some excuse or other to impede the prospect of publication (thereby throwing away a perfect opportunity for that journal to improve relations with *DIO* — and, more important, to demonstrate the large-minded universality to which it seems to aspire), DR has decided to publish here in *DIO* the unbowdlerized paper.

F5 Readers who expect (hope?) to be shocked at the original prose will be badly let down. The true shock here is how afraid and one-sided the *JHA* continues to be. It cannot print any paper that says in so many words that Ptolemy was a liar. [But: no-problem for papers opposing that view; see, e.g., §F8.] It cannot even admit that there is a serious controversy about whether he was. (Check for yourself what the *JHA* finds so hideously offensive at §E5.) Instead of falsely laying blame (§F6) on others, for a problem that arose entirely because of its own intellectual and ethical limitations, the *JHA* might consider surprising its critics (e.g., *DIO 4.3* ‡15 §H10), by rethinking its goals, and reflecting on how the devil it became so politically narrow that it would actually suppress key new findings

[30] DR had even gone to the trouble of putting the paper into the *JHA*'s inexplicably-preferred style. Some of the additions to it here are done *DIO*'s way; thus the odd mix of styles in the present version. The paper has been improved and augmented here&there. Serious additions are in brackets. None of these alterations have anything to do with the offending sentence in §E5.

(certified as such by both of its own eminent referees)[31] and *for years* hold these results hostage to an insistence that DR betray an honest scholar (R.Newton) and the obvious truth — and consent to **join in the *JHA*'s obsessive decades-long try at misleading academe regarding the state of the Ptolemy controversy**. (How can Hist.sci be taken seriously as a discipline, when one of its most prominent journals resembles a church? — banning ideas, even researchers. When did the Hist.sci community begin uncomplainingly taking it for granted — as an enduring & unalterable Reality — that a politically prominent academic journal will rigidly promote a certain viewpoint, boosting only one side of a controversy while handicapping another?)

F6 A good start at reform here would be a retraction of the behind-the-back falsehood (*DIO 1.2* §B1) that DR was "impossible to deal with" regarding the present paper. (Check *idem* or here at fn 31 to see just how perversely opposite the actuality was.) Several years ago DR warned in *DIO* that (with the millennium ending), the paper's increasingly-unhumorous tag as "Rawlins 1999" meant that time was running out for Hoskin to "fulfill his written agreement" (*DIO 6* ‡3 fn 24) to publish the paper. Honest Hoskin made no reply. (Since *DIO* began, Hoskin has just mailed back all issues, unopened. He thinks this proves something. And it does. Question: Is anyone in the Hist.sci community concerned at what is done to the reputation of academe, when the publisher of the leading British journal for astronomical history refuses to communicate with his US counterpart? For over 16 years now.)

F7 With 1999 passing, and while there seemed to be growing hope[32] of encouraging peace, mutual tolerance, and *open discourse* between the formerly warring camps and journals, DR asked *JHA*-Number-Two-Leader about the "on-ice" paper (as he had come to

[31] The paper was submitted in final form 1981/8/7. Hoskin soon reported (1981/9/17) that the "paper is accepted in principle but we need to work at *getting your message across to the reader*. Moesgaard has very kindly agreed to try his hand at a draft modified version for your consideration and I hope to send this to you in ***a few weeks***." (Emph added.) The referees were Willy Hartner & K.P.Moesgaard. Hartner had (after initially nixing the paper) directly informed DR of his positive verdict on 1980/8/15, but Moesgaard's prompt referee report on the 1981 version was long held private — even when its content was requested. The report recommended that the paper be softened. (Readers here of the original 1981/8/7 DR version [keeping in mind that brackets indicate post-1981 changes & interpolations] can now judge for themselves the validity and purpose of that suggestion.) This intent was kept private for a half-year — despite DR's direct 1981/11/27 questions about what Moesgaard was to do. Instead of imparting his plans, Hoskin denied to DR that any tampering with the content was to occur (a denial Hoskin maintained right up to the 1982/7/27 moment when the *JHA* rush-demanded tampering, and simultaneously relayed Moesgaard's report at last). After DR offered in late 1981 to have a friend (the former Editor of the American Geographical Society) do the ever-elusive clarity-enhancing revision, that was allegedly delaying the paper month after inexplicable month, Hoskin declined, saying (1981/12/27): "It's totally a matter of making the message clearer to the reader. That's all." And: "The paper won't present any problems. . . . everything's OK and there's no problem. And you'll be hearing from me within the next month." More months passed, along with two more apologies. (E.g., "especially to apologize for my delay in revising your paper & to thank you for your patience. It must certainly go in the October issue . . . . Many apologies!") DR treated Hoskin with such extreme politeness throughout this charade that it bordered on obsequiousness. DR actually offered (seriously) to visit Cambridge to assist the work of revision; Hoskin declined the help, but replied (1982/5/30, emph added): "I feel very guilty about this. It's simply a pressure of other things. . . . *it's nothing more than that* . . . . You've been extraordinarily patient. . . . again my thanks for your patience."

[32] See inside cover of this issue. Also, the *JHA* had in 1998 published (without the slightest interference) John Britton's erudite torching (*JHA 29*:381-385) of the gas in Swerdlow's 1998 P.U. book, though Swerdlow is one of the *JHA*'s "Advisory Editors". And a paper from Russia (an earlier version of which was commended at *DIO 4.3* ‡14 fn 4) giving evidence for Hipparchan authorship of the Ancient Star Catalog is said to be in the offing at *JHA*. One hopes that the authors will not be cajoled into warping the logical conclusions of their own research — as has happened in the recent past in Hist.sci. (See, e.g., *DIO 1.2* §I8. Process satirized [barely] at *DIO 6* ‡3 fn 11.)

call it). He said (1999/7/3) that THE problem with the paper was a single terrible sentence — though he couldn't quite remember what it was. Unable at first to find his original copy, he wrote DR, carefully attempting to commit the *JHA* to nothing (forgetting that an honest journal would already be committed, after it had thoroughly refereed and accepted the paper — especially in light of Hoskin's airs about being bound by referees' advice, whenever this suits *JHA*'s wants). Number Two said (1999/10/7, emph added): "I would certainly be willing to urge a *reconsideration* of [the paper] upon receipt of a clean copy of it with the offending sentence removed." (This is an important clarifier of what had originally gone on behind the scenes, because Hoskin had never even been frank enough to admit that this sentence was the problem. He'd originally gotten rid of the sentence by just wide-broom-sweeping-out the whole pro-Newton section [§E] that included it.) DR sent along a photocopy of the paper, knowing that no sentence in it was improper — and therefore challenging Number Two to find such (1999/10/15): "I look forward to learning what the intolerable sentence in it is." Reading the paper, Number Two soon learned that no sentence in it could be defensibly condemned. He then dug out his original 1981 copy — but still found no impossible sentence. So, he shifted to a new ploy: he excerpted just a portion, a **phrase**, from the sentence in question! — and used that as an excuse to keep on[33] arguing for censorship (1999/10/25): "You will not have a ghost of a chance of getting your paper published in the *JHA* as long as it contains the phrase [at §E5] 'that Ptolemy has indeed been shown to have been a liar' ". However, the full sentence is not a claim of proof (as the excerpting makes it sound), but is rather a description of the Hist.sci sociological situation on the issue. (Must the *JHA* mutilate a sentence, in the cause of mutilating the journalistic truth of its attempted mutilation of an article on a long-mutilated historical

[33] Hmmm. Why does such censorship mean so much to the *JHA*? Is the intent to aggravate DR until co-existence breaks down? (And then blame the result on him?) Or is the *JHA* just now so deeply into this wringer that it thinks it must resort to any available means of conjuring up a non-existent problem with the paper, because the journal can't admit that it never had cause to censor this article in the 1st place? Keep in mind that Hoskin's 1983 severance of communication with DR was caused not by the paper's contents but by DR's openly — not behind Hoskin's back — and gently (note old-proverb at *DIO 1.2* p.96) pointing out the *JHA*'s poor refereeing of a miscomputed paper it published late in 1982 (*DIO 1.2* §B2 & fn 8). But the heaviest underlying factor behind *JHA* censorship of the DR paper is elementary: keeping control of centrist-forum discussion of the Ptolemy-as-liar issue can be accomplished by the simple scheme noted at *DIO 2.1* ‡3 fn 8: "tape one side's mouth shut." For the unfettered other side, see here at §F8; see also the gratuitous false attack on Newton's character (pointed out by Thurston at *DIO 8* ‡4 §A19), appearing in a 1998 book that was vetted and aggressively promoted by two *JHA* archons. [Remainder of note added 2000 Jan.] This attack is in fact based upon 1993 remarks first appearing in the *JHA* itself: "Newton's arguments were based on apparently dispassionate statistical tests, but the concluding sections of the book [*Crime* 1977] were marked by a personal animosity of surprising intensity toward Ptolemy" (*JHA 24*:145). Note that neither of these *JHA*-inspired attacks on Newton's character (& of course his competence — it doesn't get any more ironic) has given us *any quotes whatever* from a pure-*JHA*-fantasy hatemonger-Newton, to justify *JHA*'s charge of "personal animosity". I.e., this is simply more *JHA* smearing of Newton, intended to convince the reader that Ptolemy's fraudulence is nothing but the figment of a lone (§F8) nut's enraged & unbalanced imagination — when the sharply ironic truth is that the mirrorlessly-projecting *JHA* is itself the party whose undeniable personal animosity is doing the imagining. No one has ever presented any evidence to support the lie that Newton's work was motivated by personal anger — though dislike of massive cheating wouldn't merit condemnation, anyhow — or was (*ibid* p.146) "ahistorical", except insofar as the *JHA* (again the actual fantasizer here) megalomaniacally supposes that it blesses its lessers by defining for them what is and isn't legitimate history. (The *JHA*'s central historical Principles [!] are ever so solemnly set forth at *JHA 11.2*:145; 1980 June; reactive comments at *DIO 2.1* ‡3 §B2.) While Newton presented proof after proof (see H.Thurston's summation at *DIO 8* ‡1) of his theory of Ptolemy's character — *which is a legitimate and major science-history question* (fn 34, contra *DIO 2.1* ‡3 §B7) — no proof at all is required by the *JHA* whenever the urge seizes it to trash Newton's character again. For the ***explicit*** smear-'em-back logic that governs the *JHA* circle's behavior in this controversy, see *DIO 2.1* ‡2 §H17.

truth? See ‡1 fn 10.) The actual, entire §E5 sentence: "Newton's conclusion has been attacked with such passionate disbelief in a variety of journals (by commentators all whom had, in earlier publications, prejudged Ptolemy as a great astronomer), that many onlookers may not be aware that a number of scholars[34] agree that Ptolemy has indeed been shown to have been a liar." That's a very different statement than is indicated by the snippet-phrase.

F8 So: does the even-handed *JHA* stifle *all* scholars who try to describe in its pages what the academic community thinks about the issue of whether Ptolemy was a liar? Wellllllll — no. And certainly not if a scholar is "safe" — i.e., can be trusted to conclude in favor of the *JHA*'s two granite religious tenets in this connection: [a] Ptolemy was not (provably) dishonest, and [b] Reputable academic consensus agrees. E.g., in a book review, the *JHA*'s most servile pawn on the Ptolemy debate, is permitted to discuss the *very same issue* — general academic opinion on Ptolemy's lying — which the (rather more important) above paper was long suppressed for mentioning! His comment at *JHA 24*:145 (1993): "Newton denounced Ptolemy as a liar and a plagiarist, and called into question his competence as astronomer and mathematician. . . . Very few historians have accepted Newton's conclusions in their entirety."

F9 We conclude with questions which deserve provident consideration.

Question 1. What should we think of a journal that would ever-so-deftly[35] resort (while suppressing vital research) to excision-sleight and inequitably-applied pseudo-rules — as a means of convincing onlookers that it is (of all things!) playing fair with contributors?

Question 2. Do any of the *JHA*'s decorative "Advisory Editors" or its occasional high-quality contributors[36] even care?

[Note added 2001: In the foregoing, there lurks (at §B7) yet another precise appearance of the Aristarchan number 4868. (This one was missed by all previous scholars, including two decades of DR's own researches.) Simply check the number of years between Meton's famous bedrock −431 Summer Solstice and the Hipparchos Ultimate-Orbit epoch −127 Autumn Equinox: it's $304^y1/4$. This number is *exactly* one sixteenth the number of years in the "Great Year" (fn 9 & §A2) of Aristarchos. That is, the Meton-Hipparchos epoch-interval is $4868^y/16$ — on the nose.]

[Note added 2013: The full, astonishingly extensive Aristarchan-Hipparchan geometric-series context, into which $304^y1/4$ neatly dovetails, is set forth at *DIO 11.1* ‡1 fnn 14, 16, & 17.]

[34] One of these scholars was the great B. L. van der Waerden. Before concluding (in his final book, 1988) that Ptolemy lied (see also *DIO 1.1* ‡6 fn 37), a previously neutral van der Waerden was shocked that O.Pedersen's otherwise valuable 1974 book on Ptolemy had not even dealt with the issue; van der Waerden commented in *Annals of Science 32.6*:603 [1975]: "the question of whether Ptolemy was a liar is important for everyone who wishes to appreciate the value of Ptolemy's work. Pedersen does not even tell the reader that Ptolemy's sincerity has been doubted by serious scholars, and that extensive calculations have been made to check his statements." (Similar defense tactics discussed at *DIO 4.3* ‡15 §§D2&D7.)

[35] Shades of the ineptitude of another of History-of-science's anti-DR censorial moves (by Robert Kargon), described at *DIO 2.1* inside cover. (Note: *DIO* has since been restored to the library shelves at Johns Hopkins University.)

[36] R.Stephenson is both.

DIO

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