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DIO

&

The Journal for Hysterical Astronomy

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Journal for Hysterical Astronomy:

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‡1 Prologue: by Dennis Rawlins

A Countdown

A1 It has been leftly remarked that a free press exists only for the person who owns one. But the onsets of photocopier & computer have lately assuaged this traditional plaint, by bringing publishing costs within reach of the nonwealthy, at least for scholars in a small field. Curiously, the Gutenbergian potential for avoidance of (if not outright rebellion against) overproprietary & overconfident authority has heretofore been little realized.

A2 An attractive opening in this regard is afforded by the field of ancient astronomy, an area with which *DIO*, the periodical here launched, will be intimately (though far from exclusively) involved. As will be evident following this prologue (p.12), I intend also to enjoy the eclecticism & fun of an upfront general commentary column, "Rawlins' Scrawlins" (nonPascalian title credit to my mutually-deflating friend Quiglet). But the more specific purpose of the journal will be scientific history. (As against "history of science".)

A3 *DIO* is fortunate that the figure who has been the most able[1] of the world's scholars in ancient astronomy, the great mathematician & statistician Bart van der Waerden, concludes his capstone book on the subject, *Die Astronomie der Griechen* (van der Waerden 1988 p.307), by passing to *DIO* the flickering torch of openminded and mathematically competent critical analysis in this exquisite field.[2]

A4 Early numbers of *DIO* will present various newly recovered details of the ingenious & refined astronomy of the legendary ancient figures: Kallippos, Timocharis, Aristarchos, Aristyllos, Hipparchos[3] — and still other highly creative ancient Hellenistic figures whose magnificent work can be reconstructed but whose names are lost to us. And we will be proud to publish the final academic contributions of 2 of the world's most gifted and wellknown ancient science specialists. One appears in this premier issue of *DIO*: ‡5. (Other scholars who may wish to contribute papers to *DIO* are asked to read this entire Prologue carefully.)[4]

A5 For the last 2 decades, by far the warmest controversy in the ancient astronomy field has been that surrounding the cascade of revelations of pervasive fakery[5] in Claudius

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[1] It is typical of van der Waerden that (in a 1988/12/20 letter to DR) he denies the charge — instead attempting to convince me that O.Neugebauer is the most respected of such scholars. I'm sure Neugebauer would agree. But I disagree with both men, regarding van der Waerden as the better scholar on at least 3 grounds: [a] mathematical facility (where his superiority would not be denied even by Neugebauer), [b] openness to new evidence, [c] advice & assistance to scholars entirely regardless of personal agreement or academic politics. Whatever Neugebauer's former contributions, he has long since become the don-of-the-dead to a mob of truth-*possessors* (borrowing Pat Truzzi's incisive phrase), while van der Waerden has become an inspiration to truth-*seekers*.

[2] However, van der Waerden is in no fashion responsible for the sometimes journalistic tone of *DIO*, and he is far too gentlemanly to approve of *DIO*'s blunt exposures of those who took advantage of his kind & retiring scholarly disposition by publishing wild attacks on a few of his historical papers. I should add that R.Newton, BvdW, & DR all have serious disagreements with each other on a wide array of subjects — disagreements which have never for a moment affected the amiability of our relations.

[3] See ‡6 here for Hipparchos' admirable final discovery. In the gratifying context of rescuing this treasure, it's worth remarking (for contrast) the necessarily critical or "negative" thrust of so much of the best work that rewrites history. (After all, nobody says, e.g., he didn't reach the Pole when he actually did.) Nonetheless, one can find numerous DR redemptions (some already published) of unjustly treated figures: e.g., Aristarchos, Aristyllos, LeMonnier, Morton, Papanin, Amundsen, Ellsworth, Nobile, Plaisted, Diller, van der Waerden, R.Newton, & others. See also (fn 24) DR's macro-defense of the previously-misunderstood majority of ancient scientific scholars.

[4] My first (nonfacetious) recommendation will be, simply: start your own journal! But if you instead wish to send a paper to *DIO* for consideration, then: [1] mail a *spare* xerox (not to be returned) to [a *DIO* referee, & [2] phone him in 40 days]. *DIO* will publish very few papers not by DR's acquaintances. [DR 1998 note: original 1991 policy has long since broadened. Contributors are asked to consult the most recent *DIO*s' inside-back-cover publisher's statement. *DIO* also] welcomes readers' notices of [a] errors & [b] prior publication of matters presented here as new. And its departments of Unpublished Letters and Referees Refereed seek input from all quarters.

[5] Britton 1967 is now much cited by the O.Neugebauer-Muffia's capos (though never in this connection, before R.Newton's arrival on the scene) to prove that the Muffia knew all along that Ptolemy's outdoor "observations" were in strangely consistent agreement with his indoor tables. Certainly the Muffia knew. So why wasn't the public told? (And why has the proprietary Muffia become so enraged when alien nonMuffiosi publish the obvious implications?) Instead,

Ptolemy's *Almagest*,[6] which is the central text of the entire field of ancient astronomy. (Ptolemy's dishonesty[7] has been openly suspected for at least 1000 years, most notably before recent times by the great astronomers Tycho, Delambre, and Peters.)

A6 The revelations which have so scandalized Ptolemy's censorial modern cadre of defenders were first published by Robert Newton (especially R.Newton 1977), while he was Supervisor of the Space Sciences Division of the Johns Hopkins University Applied Physics Laboratory. The other central modern developer of such evidence is myself. These analyses have appeared in some of the leading science journals of the world since 1969.

A7 Nonetheless, during this time, the seemingly most apt "centrist" historian-dominated journals, the extremely handsome *Journal for the History of Astronomy* (*JHA*) and *Centaurus*, have systematically suppressed the skeptical side of the Ptolemy dispute, providing their readers with only a minimal, controlled, and warped glimmer of what has transpired in this, the most critical ancient astronomy controversy ever.[8] (Revealingly, Ptolemy's defenders have fled every suggestion of public debate, e.g., the challenge issued by DR in the *American Journal of Physics* 1987/3.) *DIO* will provide an antidote to this skew. Those unfamiliar with science journals may not know my output, so they may understandably conclude that I am starting a journal simply due to Reputable journals' rejection of my papers. Actually, I cannot recall having a refereed paper rejected in over 7 years — not since 1983/7/23, when *QJRAS*-appointed referee O.Gingerich of Harvard (Ptolemy's #1 p.r.-man) evaded his way out of a written judgement. (However, this dearth of rejections is no doubt due in part to my refusal since 1984 to send anything to Hist.sci journals, for reasons explained below — often in overlarge footnotes, which the reader oughtn't to have to be explicitly urged to skip, during first readthrough here!)

A8 Since OG's embarrassingly indefensible flipflop,[9] I have issued a stream of discoveries in ancient astronomy through: *Queen's Quarterly* 1984 (invited); *Vistas in Astronomy* 1985 (invited: Greenwich meridian centenary symposium); *American Journal of Physics* 1987; *Bulletin Amer Astron Soc* 1990 (invited). These publications have been possible only due to the courageous assistance of a few decent, highly placed scholars who are not

Britton 1967 was not published — except for the sole portion (Britton 1969) that seemed to exculpate Ptolemy! (The Muffia later unsuccessfully tried a similar ploy with DR regarding Rawlins 1999: fn 11.) By contrast, Britton 1967 was uncited even in Neugebauer 1975, Toomer 1975, & Toomer 1978H — though Toomer 1984 now suddenly brings forth Britton 1967 repeatedly as the best discussion available of these matters! (See Toomer 1984 pp.viii, 135 n.12, 138 n.21, 253 n.58, 334 n.64.) Convincing sleight of hand isn't a Muffia strong suit. (See also ‡5 fn 7.)

[6] From the Arabic "almajasti" (Toomer 1984 p.2). So RRN asks: why does Ptolemy's modern alibi-contingent object to DR's spelling *Almagest* as *Almajest*?

[7] *DIO* will frequently examine shady scholarship. Whether this is seen as showing that the journal has a critical tendency or is founded upon an ethical base, will depend upon the observer. (See fn 3.)

[8] Not one of the dozens of new evidential findings against Ptolemy has first appeared in a Hist.sci journal. (Most debuted in journals of astronomy & physics, an inappropriate burden on them.) This impressively pristine record only deepens Hist.sci's stake in now discrediting skepticism of Ptolemy. [But see Jones' huge success (noted at *DIO 11.2* ‡4 §F3) and Graßhoff's thorough vindication by D.Duke at *DIO 12* ‡2.]

[9] Evidently, the hypothetical inner OG never really did reverse his enthusiastic (if maltimed) initial written 1983/1/14 praise for the later-rejected material. (OG's original 1/14 reaction: mailed just before his *JHA* co-Editor's 1983/3/3 rage necessitated DR's ostracism from *JHA*&co: fn 25 & ‡6 fn 15.) For, without a word to me, OG in 1985 encouraged another astronomer, Sam Goldstein (UVa), to publish this same paper's central equations (solutions of the *Almajest*'s mean motions) under Sam's name in OG's *Journal for the History of Astronomy* — a venture that never came off because Sam (an admirably honest and knowledgeable scholar) fortunately contacted me (1985/9/21) before proceeding. These new equations were originally sent privately to OG by DR on 1980/4/13 ultimately published in the *Amer J Physics* (Rawlins 1987). . . . OG spent 6 months (1983/1/14-7/23) piling up heavy reams of computer readout (proudly shown DR 1983/6/4) . . . [yet OG's] 1983/7/23 [*QJRAS*] referee report could find no computational errors anywhere in the paper's 91 equations. . . . [and] acknowledged (privately) that the *Almajest* mean Mars motion [a] had received a [non-fitting] solution published by OG in the very same journal (Gingerich 1981 p.41-2), while [b] the DR paper's Mars solution [fit & was accurately computed] . . . . OG's negative verdict on DR did not prevent OG's 1985 attempt to publish the very same Mars equation in *JHA* (SG's ms, Table IIa, Mars' mean motion). [Note added 2008. This adds yet another irony to the odd 1980s Mars-mean-motion history: when headline-crediting OG (*DIO 11.2* [2003] inside-front-cover), for correctly suspecting an alternate possibility for DR's 1980 Mars solution, DR had forgotten that OG had in 1985 tried to publish the identical unhistorical DR solution under another's name. I.e., OG's objection was not intellectual but personal.]

personal friends but who privately acknowledge that the behavior of the eminent "prima donnas" of this field has been "horrible", adding that there is no question that certain Hist.sci archons are trying to "blackball" DR. (Hardly the chosen sport of scholars busy enjoying their own intellectual creativity.) But they also acknowledge that it could be unhealthy careerwise if they came forth publicly. Thanks to them, I have since 1984 had access to an assortment of non-Hist.sci journals; however, none among these nonhistorical journals can be expected to publish a long succession of papers outside their field. Thus, to continue along this laborious path requires one new journal after another to consider and evaluate subjects alien to immediate areas of expertise. The aggravations of pursuing such a nomadic publication circus are hardly much more than that of starting my own periodical. I do not thereby bar my contributions from other journals' use, since any scholarly journal has *DIO*'s permission to re-publish, verbatim, portions or all of articles published in any issue of *DIO*, so long as: [a] *DIO*'s name & address are printed with the excerpts; [b] it is stated that replies to appended comments will be published in *DIO*.

A9 During the same period, I have also had papers accepted at *Archive Hist.Exact Sci.* (several), *Isis*, & *Archives Int.Hist.Sci*. *AHES* is a very high quality journal, run by someone exceedingly competent in science, but whose editing priorities are unshared by DR. The *Isis* & *AIHS* acceptances included conditions barring certain statements in the papers. (And *Isis* exceptionally added that if my conclusions were attacked, I would get no reply space.) Since these demands were politically motivated censorship, I could not honorably comply and have published much of the same material instead in *QQ* & the *Amer J Physics* (Rawlins 1984A & Rawlins 1987). On 1987/7/14, I was invited to write a feature article for *Sky&Telescope*, but the proposal was not attractive, given that journal's wellknown editorial record.[10]

A10 More than once, I have had the stirring experience of sending a new academic discovery to an editor or journal and then later finding the same result published under another scholar's name — sometimes in the very journal I'd sent it to. (In 1984, I entirely ceased sending material to Hist.sci people. By coincidence, no subsequent DR discoveries have mysteriously diffused.)

A11 So I am starting *DIO* at least in part because I prefer to publish without negotiating the perils of [1] censorship & [2] finding my results in print but with my name randomly misspelled as "B.Goldstein" or somesuch odd anagram.

B Originality

B1 I am prepared to believe that many cases of apparent plagiarism actually involve nothing worse than innocent intellectual osmosis. (A simple test: after any publication

[10] As noted in Rawlins 1984N, a fresh Neptune-Scandal theory (namely, the critical import of J.Adams' 1845-6 calculations being sequestered by a tiny UCambridge clique, until after Neptune's 1846/9/23 discovery in Berlin from U.Leverrier's published math) was included in a DR review for *Sky&Tel* in 1980 but then suppressed. Soon after, much the same theory surfaced without citation in a speech by R.Smith (protégé of *JHA* Editor-for-Life, whose co-editor, O Gingerich, is close to *S&T*). Smith's exceedingly valuable 1989 *Isis* article ("The Cambridge Network in Action: The Discovery of Neptune") nowhere mentions DR in the main text in this key connection. The Smith paper's thanks & notes acknowledge access to (& nonevidentially reject the secrecy-aspect of the theme of) Rawlins 1984N & a DR 1966-1972 Neptune ms (basis of 1966/5/11-20 presentation at Johns Hopkins); but few readers will know that the Smith article's central (titular!) thesis is essentially the keystone of both the 1966-1972 ms & Rawlins 1984N and was pioneered by DR (running counter to all other pre-1980 modern discussions), and that Smith's hard-earned new evidence consistently confirms DR. (As does vacation-bound Cantab Airy's 1846/8/6 parting advice to Cantab Challis, Cambr Obs Nept file item #6: "write to Mr.Main [Airy's RGO ass't] who is fully in my confidence and understands the position of the whole matter.") In the 1989 Smith paper (& in Smith's intimately related 1983 *JHA* paper), Liverpool's Smith thanks longtime friend (& 1984 *JHA* co-author) R.Baum of nearby Chester for comments; Baum was the only scholar on Earth who possessed the 1966-1972 DR ms (since 1972/6/16) before Smith found the clique-silence key to the Neptune affair. As for the 1980 DR ms (containing this discovery) written at *S&T*'s request: *S&T* has lost its entire file. Anyone wishing to lodge an original find in the pages of *S&T* (or a similar journal), might ponder these events: [a] The discovery is not published. [b] It soon reappears elsewhere. [c] The prior transaction vanishes; thus, no certifiable copy of the original submission survives — or so it may have seemed.

of your findings without proper citation, give the author or journal the opportunity to acknowledge the actual order of priority. The reaction will indicate the degree of guilt involved.) However, there is also no question that original scholars have had to contend with intellectual piranhas since antiquity. In the First Century AD, Pliny (Preface 21) remarked that verbatim plagiarism was practiced by the *majority* of the *best known* writers, adding (*ibid* 21, 23):

> it is a pleasant thing and one that shows an honourable modesty, to own up to those who were the means of one's achievements . . . . Surely it marks a mean spirit and an unfortunate disposition to prefer being detected in a theft to repaying a loan . . . .

B2 Internationally respected U.Minn. astronomer Willem Luyten's 1987 *Autobiography* (collected, prefaced, & published by my late friend, the courageous scientist-explorer Rob't Lillestrand, with Anton LaBonte) notes at pp.115-8:

> I can recall something like 20 occasions where another astronomer "discovered" a star (previously discovered by me) with some unusual properties and where he announced his "discovery" in an accredited scientific publication. In several cases these investigators had received support from the National Science Foundation. NSF took a particularly dim view of my critical pronouncements because they cast a shadow on the recipients of their research grants. In all of these cases I had published a description of these stellar objects many years earlier, so the issue of concurrence did not exist.
>
> . . . the outright unwillingness of many scientists to give credit to an earlier discoverer even though [the discovery] is already published. . . . seems to border on deliberate intellectual dishonesty and is far more pervasive than most people believe. Also, this characteristic is not restricted to the lesser Achilles of astronomy.
>
> I suspect that many of these professors are so accustomed to taking ideas from their graduate students and research assistants that they don't even regard this practice as dishonest.
>
> . . . I have done . . . my best to stick to the truth. In some cases this has made life difficult for my colleagues, in other cases it has made life difficult for me, but in every case it has been basic to my life.

C Evaluating the Evaluators

C1 Who are the academic-businessmen-politicians that control Hist.sci journals and thereby assume god-like prerogatives both as censors of information flow and as arbiter-bestowers (upon the Less Fortunate) of the "prestige"[11] that is said to attach to publication in their incestuous forums? These gentlemen allegedly evaluate incoming manuscripts. But: who evaluates the evaluators? Are these editors and-or their referees capable in the very disciplines where they pretend to measure others? Even in high school math?

[11] On 1983/6/6, *JHA* #2 Editor O Gingerich urged me to accede to the sudden late attempt of the *JHA* Editor-for-Life, Lord Hoskin, to excise the sole, brief pro-R.Newton section of a paper long previously accepted (even advertised in the 1982 March *Isis*) — a section which included, e.g., the little-known information that Ptolemy's solar "observations" agreed 50 times better with his indoor tables than with the outdoor sky. OG explicitly recommended I tolerate Lord H's censorship (typical of that which has prevented *JHA* readers from knowing the truth of the Ptolemy situation) because publication in the eminent *JHA* would enhance my "prestige" in the field. For the record: the dirty business surrounding this affair (see fn 25 & ‡6 fn 15) is what led directly to the inception of *DIO*. I'm sure establishmentarians everywhere will be grateful to OG & Lord H for that achievement.

C2 Well, one of the rôles of *DIO* will be the investigation of precisely these matters, to which *DIO* will devote a regular supplement. I believe these little forays will enlighten, perhaps surprise, & certainly entertain *DIO*'s readers. The supplement will be called the *Journal for Hysterical Astronomy* (*J.HA*).[12]

C3 This is the right place to state that DR is an apt publisher of scientific folly, since his own 1988 release of R.Peary's Betelgeux Document was inexcusably careless and stupidly overconfident.[13] Especially for one who insists on high scholarly standards, in his own work above all.[14]

C4 Please note that criticisms & satires in *DIO*'s supplemental *J.HA* will be primarily aimed not at the small or the powerless, but at [1] the lordliest archons of academe (largely reviewing their Hist.sci effusions), and-or [2] he who tries to kiss these lords' brains, by attempting (safe in their captive journals) to bully-trash dissenters' creativity, though himself being not especially original or infallible.

C5 I have in mind most particularly the O.Neugebauer cult's ongoing war (examples:[15] ‡3 §D; ‡6 fn 6) upon the discoveries of such civil, gentlemanly scholars as R.Billard, D.Dicks, A.Diller, W.Hartner, R.Newton (the bravest of all Greek astronomy analysts), & B.van der Waerden. The cohesiveness, vitriol, & accuracy-quotient of Neugebauer-clique slanders is such that I have taken to calling it The Muffia. (For samplings of truly epic Muffiosi struggles with the mysteries of elementary arithmetic, see DR's exposures in the *American Journal of Physics*: Rawlins 1987 nn.30 & 35. Previously suppressed by Hist.sci: ‡6 fn 4. See also Captain Captious' Muffia math at ‡5 fn 7.)

[12] A prime cause of the poor interdisciplinary communication discussed above has been numerous Hist.sci professionals' doubtless unbiassed conviction that mere scientists are ill equipped to contribute to the field. As we shall see, some among these superior Hist.sci folk can indeed be class entertainers when attempting, e.g., astronomical calculations. (See below: §C5.) However, *DIO* shall nonetheless appreciate their talented facets, as well as their occasional contributions to our knowledge. (E.g., ‡6 fn 35.) In defying the gods of the field, I have no wish to join them (in either power or omniscience). *DIO* is being launched to enhance knowledge, not the writer's political influence. Thus, at least initially, most *DIO* copy will be generated internally, except for occasional pieces by friends (as well as the dep'ts of Unpublished Letters & of Referees Refereed, both of which specifically seek others' input), whose appearance in *DIO* carries no implication of anything *but* friendship. I.e., *DIO* is operating just as numerous journals do, but is being upfront about it. Indeed, DR is not even calling himself "Editor" of the journal.

[13] Regarding DR's original 1988 BetDoc error and his unqualified retraction (*Wash Post* 1989/2/16) just 2 weeks after evidence against it appeared: DR saw the experience as principally a test of character, and attempted (under an intense and frequently hostile spotlight) to set an example of rigorous integrity and severe self-censure. I was gratified that the scientific community responded by itself setting an admirable example, treating DR with fair criticism and balanced attention to the full range of evidences bearing on the Peary Controversy. (See *Science* 1989/3/3 & 1989/12/22, *Scientific American* 1990/3 & 1990/6.) The result has been a joint behavior-model which one hopes will henceforth encourage other temporarily mistaken scholars (regardless of the prominence & depth of their previous commitment) not to fear frank retraction when the weight of evidence turns out to be against them. (DR's openness so infuriated primo-Peary-apologist & National Geographic chief G.Grosvenor 2, that G2 has even publicly fanned the flattering rumor that DR's error was intentional. With enemies like Grosvenor, who needs friends?)

[14] DR's restoration on this issue was accomplished by: [a] Total DR retraction (previous fn) of his egregious 1988 error. [b] DR's surprise announcement (1989/12/11) of the BetDoc's correct solution (1894/12/10 Betelgeux & Vega 3-wire transit data observed at $77^\circ 40'$N), along with detailed demonstration of the impossibility of the elaborate "time-sight" solution published in NGS' 1989/2/1 pressrelease (also overconfidently promoted in *NatGeogMag* 1989/6), *unanimously* validated by NGS' experts. The NGS was mistaken on virtually every detail: observation-type, altitude-type, altitude-purpose, instrument, orientation, unnamed star, date, place. (The truth of DR's solution and the falsity of NGS' has been unqualifiedly certified by several expert astronomers.) [c] DR's release of numerous independent evidences, including some startling finds in the Peary Papers (US National Archives) showing that Peary's 1906 discoveries and 1909 N.Pole fable are riddled with contradictions and data-alterations that render these claims scientifically unacceptable. (See in this *DIO*: ‡4.) [d] Perhaps most important: the courageously open intercession (on the skeptical side of the Peary Controversy) of the famous astronomer Chas.Kowal at the very time the massive NGS' p.r.-blitz was trying to stampede the press into unquestioning acceptance of its inept hired consultants' 1989/12/11 verdict. [e] DR's photogrammetric demonstration (22-unknown least-squares fit) that Peary's 1909/4/6-7 position was about 100 mi (3 standard deviations) from his claimed N.Pole (Amer Astron Soc 1990/10/22 presentation).

[15] As one may see from these quotes, the most frantic missman for the Muffia has been its Captain Captious: N.Swerdlow. Two decades of similar output have helped earn historian Swerdlow: [a] a prime seraphic place directly beneath the oscufied throne of O.Neugebauer, [b] a professorship in the U.Chicago Dep't of Astronomy & Astrophysics, [c] a MacArthur Foundation grant, & [d] a place on the board of no less than the *Journal for the History of Astronomy*.

C6 The most curious aspect of these violent attacks is that (unless they represent a conscious effort to save the faces of Muffia archons' precommitted reputations and-or to hog all power & grants in the field as the exclusive property of a restricted clan),[16] they appear to be inspired by nothing more than *disagreement* over scholarly questions. Before observing the Muffia at work, I had mistakenly supposed that the idea that error was sinful had somewhat declined since the Dark Ages.[17]

C7 When describing those who doubt that Ptolemy observed outdoors (a class which has included some of the finest astronomers in history: §A5), Muffia-circle folk use such pleasantries as: "incompetent"*, "crank", "silly"*, "unreliable"*, "absurd"*, "disreputable"*, "insults the intelligence of the most naïve reader"*, "pipsqueak", "Velikovskian", "con-man", "crazy"*, and . . . well, you get the drift. Just the sort of terms rational & intellectually secure scholars use to describe persons with whom they merely happen to disagree.[18] Oh, I forgot one other Muffia term applied to a skeptic: "abusive".[19]

C8 Are we dealing here with an absolutely precious unselfconsciousness — or with a sense of humor even more warped than my own?[20]

C9 Note that this behavior must be just fine with — often useful to — certain archons of academe, since most of the abusive scholars quoted here (§C7) have advanced to prominence, while some among the polite opposite numbers mentioned above (§C5) have not done so well politically. In controversies embarrassing to entrenched institutions, baseless high-archon slander (e.g., §C7) is freely employed to defend orthodoxy. Note: [1] The perpetrators pretend to eschew such abuse, and seek to punish any who speak against their own

[16] An academic clique's members can achieve prestige, regardless of scholarly ability, just by loyally promoting each other, counting on naïve onlookers never to sense the circularity of the proceedings. Auxiliary tactics: [a] Discredit *and attempt to utterly destroy* all competitors, as threats to inevitably finite fiscal resources. [b] Most observers cannot understand technical details well enough to tell who's right in a disagreement, so forget evidence and concentrate on *ad hominem* attacks. [c] A critical argument is without effect if its expounders are not heard. The natural issue of such approaches might be expected to resemble the pack snarls of the Muffia quoted at §C7. (General principle: a clique attempting to kill, starve, or isolate an intellectual opponent, betrays inward fear of that party's evidence.) It would be unfair and libellous to make comparisons here to the hyena, which is known for its intelligence, good spirits, & pleasant laughter.

[17] W.Lecky *History of . . . Rationalism in Europe* 1865 Chap.4 (1873 NYC ed., 2:26-28, emph added): in the 4th & 5th centuries AD, "the pagans were deprived of offices in the State, . . . the entire worship condemned . . . [though their leaders] had exhibited a spirit of tolerance . . . . [this in decent contrast to the orthodox's] doctrine of exclusive salvation, and the conceptions of the *guilt of error* and of ecclesiastical authority."

[18] The published bouquets are starred. Sources: Toomer 1975 p.201, Gingerich 1984P (echoing Swerdlow ‡3 §D2), HamSwerdlow 1981, Toomer 1984 p.viii, *Balto Mag* 1989/7 p.80. The unpublished expressions are from private communications of 1976/3/9, 1978/11/30, 1979/2/7, & 1983/6/2 (‡3 §D). (Equating topflight physicist R.Newton with bigtop zany Velikovsky is tophole something-else, even for the Muffia.)

[19] O Gingerich 1983/8/26, referring to DR, whose prose is admittedly not *quite* so staid as that of Diller & RRN. But the catch with blaming Muffia rage on DR is that §C7-style Muffia treatment of dissent had been going on for about *7 years before* DR entered the Ptolemy Controversy in late 1976. Indeed, Diller received a similar Neugebauer letter in 1934, reviling Diller 1934's seemingly unoffending discovery of Hipparchos' obliquity. ON's comments were published at Neugebauer 1975 p.734 n.14 and were soon proved to be as valid as they were polite (see ‡6 fn 21). (I have long tried, not always successfully, to apply abusive remarks only to my own mis-scholarship. Strong self-criticism encourages scrupulous investigation.)

[20] When first involved in the Ptolemy Controversy, I attempted amiably to encourage O Gingerich's feeble attempts to refute R.Newton, since Neugebauer's clonies were pursuing a policy of noncitation. (They've never cited DR. Up to now.) This policy's reality was freely acknowledged by all parties. An earnest Muffian grad student joined me when I first met RRN (at his home 1976/3/29) but later told me NEVER to tell the Muffia about that heinous indiscretion. Neugebauer himself defended the freeze-out of R.Newton to me 1976/8/14, even alibiing his having attacked Velikovsky (in *Isis*), but not R.Newton! I spoke openly of this shameful policy and continued trying to bring out the putative best in OG, but then learned to my amazement from a number of scholars that OG was, behind my back, slandering me by characterizing such common-knowledge (which OG privately shared) as symptomatic of paranoid insanity. For instance, OG wrote (1978/2/2, alibiing his rejection of an editor's invitation to debate DR) that an "exceedingly paranoiac" DR has been "suggesting that a cabal has been suppressing the consideration of [R.]Newton's work" on Ptolemy. OG omitted to quote another cabal-inventing nut, who wrote (to DR 1976/9/15, commenting on Gingerich 1976; emph added): "So far the Neugebauer camp has not been heard from. *Perhaps my merely mentioning [R.]Newton in a review of Neugebauer has placed me beyond speaking terms*." The identity of this, my partner-in-paranoia? O Gingerich! Stand aside, Machiavelli. [More at *DIO 4.3* ‡15.]

self-deified persons, even though [2] The unmighty's occasional reactive slanders are [a] not secret and [b] of featherweight concern compared to potentially lethal (often-clandestine & evidentially unsupported) institutionalized gossip-judgement that a given scholar is Impossible or Not-Reputable. (A famous & able now-deceased US Antarctic explorer was often falsely so vilified, which may explain his omission on the recent USPS polar stamps that included some lesser figures.) Such an evaluation, uncheckable in 2 senses, can spread like an invisible cancer, throughout the ill body of an institution detached from reality, triggering the customary self-fulfilling-prophecy action-reaction circles.[21] (The mass of scholars fear power-operators' editorial or fiscal revenge, and so are cowed into tacit or even active assent to archons' misbehavior. Perhaps *DIO*'s independent voice will rekindle once-cherished ideals in at least a few among those ashamed of silent acquiescence in tyranny.)

C10 Similarly, Muffia public attacks exhibit all the legendary courage of the hit&run driver, since they are attempted exclusively in forums where reply is not believed to be possible, thanks to the protection of power-priority editors whom these attacks often serve. As noted (§A7), suggestions of face-to-face debate are routinely ignored. Likewise the case for the first *eighty two years* of the long Peary Controversy — now finally to be debated on 1991/4/19 at the US Naval Institute, Annapolis. (The 5 panelists: T.Davies, W.Herbert, W.Molett, R.Plaisted, & DR.) Note that no university has ever sponsored a debate on the Peary 1909 North Pole claim, which is now widely suspected of having been the most successful science fake of the century. (Debates' outcomes cannot be rigged so easily as captive, politician-edited journals' contents & reviews; thus, power operators abhor debates as wildcard threats to the Conventional-Wisdom fantasy worlds they prefer to promote in more controlled fashion.) And all academic forces that matter may be counted upon never to call anyone to account for this pathetically transparent record of behavior — meanwhile advertising academe to the public as an entity that thrives on intellectual openness.

C11 For contrast, we may note that [a] the AAAS in 1974 held an official session to debate Worlds-in-Collusion[22] Velikovsky (whom the AAAS regarded as a nut); [b] the Muffia's late expert cuneiformist A.Sachs debated Velikovsky 1965/3/15 face-to-face at Brown Univ (though Sachs' admirer Toomer says Sachs wasn't sincerely debating but merely trying to make V look ridiculous); [c] debates pitting biologists against creationists are fairly common; [d] astrologer-vs-astronomer debates are just as routine (e.g., *Nightline* 1988/5/3, with the otherwise pre-occupied and thus ineffectual Dr.Squareza representing skepticism; see ‡8 §A6). For some scholarly groups, kicking mental cripples' crutches from under them is evidently preferable to dueling with forces intellectually capable of defending themselves on at least equal terms.

C12 Muffia tactics against Robert R. Newton & DR are worth comparing to the implications of some satirical articles that may occasionally adorn the *J.HA*. The Muffia's essential attitude is that RRN & DR are not *ever* right. (See fn 17 & §C7.) By contrast, the *J.HA* will merely show that Muffiosi are not *always* right. I recommend careful attention to this distinction. (Though, admittedly, I am not denying the tenuous possibility that the inverse of these statements is nearer the truth.)

C13 When any of his subjects opposed his high-handedness, Boss Tweed used to scoff: "What're you going to do about it?" It was a humorist (artist & cartoonist Thos.Nast) who answered the question. Some of modern academe's dispensers of patronage (grants, publication, review-treatment, review assignments, conference-invites) also operate by fear & intimidation. The predictable upshot has of course been classic Lord Acton. An

[21] E.g., secretly calling someone paranoid (fn 20) indicates a remarkable insensitivity to irony, even aside from the ethics of the matter. DR happens not to be an ultimate casualty of such warfare since: [a] I've stayed pretty well informed regarding slanders about myself, and [b] I have primarily certain snakebit detractors to thank for handing me a legitimate measure of world fame (e.g., *NYTimes* front page 1989/12/12, editorial 12/15). But things do not always come out so well, and scholars who are simply trying to discern & proclaim the truth oughtn't to have to waste effort on such trivia, nor to tolerate strains which some cannot survive and shouldn't have to.

[22] Credit: Ira Wallach.

unexpected but equally inevitable upshot is *DIO*.

D Unearthing the Unearthly

D1 The new periodical's name, *DIO*, is a merging of numerous themes. Dionysos was the god of fertility. Dio can mean twoness: apt for a journal attempting to fuse competent science and competent history into a progressively more accurate & just understanding of the precious period at the birth of science, when predictive intellect was first achieving and revelling in astoundingly correct & ingenious success. (These earthly raptures were first made possible by the inherently controlled & virtually frictionless mechanism of the heavens.) Dio Cassius was a valuable ancient historian. Diogenes sought an honest man. Bart van der Waerden's longtime nickname for me is Dionysios (Greek for Dennis).

D2 Also, both van der Waerden & DR believe that the $365^d 1/4$ Dionysios calendar[23] was founded by ancient scientists who had unseated the Earth from the center of their universe (van der Waerden 1984-5 p.130).

D3 Future issues of *DIO* will unearth the precise orbital parameters of a vital, well established ancient heliocentric astronomy: admirably accurate, mathematically sophisticated, and improving over at least 2 centuries, between the epochs of Ptolemy II and Cleopatra (the end of Greek rule at Alexandria).[24] Pre-eminent among these heliocentrists was Aristarchos of Samos (c.280 BC), who defied the threat of prison or worse (as we are told at Plut *Mor* 923A) to broaden the vision of men infected with that intellectual narrowness & sterility which so often pairs with intolerant, ungenerous arrogance.

D4 Aristarchos' book, one of the most important in the history of human intellect, is lost. (Not even a textual excerpt survived classical antiquity.) But his orbital data and their empirical bases are, by amazing good fortune, largely reconstructable. And his central truth was inextinguishable. A publishing scholar could not wish for a finer model.

References

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
John Britton 1967. *On the Quality of Solar & Lunar Param in Ptol's Alm*, diss, Yale U.
John Britton 1969. Centaurus *14*:29.
Aubrey Diller 1934. Klio *27*:258.
O.Gingerich 1976. Science *193*:476.
O.Gingerich 1981. QJRAS *22*:40.
O.Gingerich 1984P. Nature *308*:789.
K.Moesgaard 1983. JHA *14*:47.
O.Neugebauer 1975. *History of Ancient Mathematical Astronomy* (*HAMA*), NYC.
R.Newton 1976. *Ancient Planetary Obs . . . Validity . . . EphemTime*, Johns Hopkins U.
R.Newton 1977. *Crime of Claudius Ptolemy*, Johns Hopkins U.
O.Pedersen 1974. *Survey of the Almajest*, Odense U.
Pliny the Elder. *Natural History* 77 AD. Ed: H.Rackham, LCL 1938-62.

[23] I propose that this calendar (epoch 285 BC) was probably named by Aristarchos (fl. 280 BC) for his elder contemporary, the courageous heretic Stoic & philosophical hedonist, Dionysios the Renegade (c.330-250 BC), [a] devotee of the poet Aratos (who authored the great contemporaneous astronomical poem, Φαινομενα), [b] schismatic opponent of Cleanthes (who asked for Aristarchos' arrest), & [c] pupil of his fellow Heracleian, the famous geomobilist Heracleides of Pontos (fl. c.360 BC; temporarily head of Plato's Academy). Which suggests that Dionysios was a link connecting Heracleides & Aristarchos, part of a precious heliocentrist chain that appears to go back at least to the time of Plato. The Dionysios connection suggests a philosophical bent in Aristarchos, of which we previously had no hint.

[24] It has long since become Conventional Wisdom to accept that the ancient Greeks were poor empirical scientists. (The belief that Ptolemy was a mainstream scientist of his era is an important founding father of that general misconception.) A consistent theme of *DIO* will be the undoing of this long un-re-examined blanket libel of ancient scientists. See also Rawlins 1982G, Rawlins 1987, & here at ‡5 fn 13.

Plutarch. *Moralia* c.100 AD. Eds: Babbitt, etc., LCL 1927-.
D.Rawlins 1982C. Publications of the Astronomical Society of the Pacific *94*:359.
D.Rawlins 1982G. Isis *73*:259.
D.Rawlins 1984A. Queen's Quarterly *91*:969.
D.Rawlins 1984N. BullAmerAstronSoc *16*:734.
D.Rawlins 1987. American Journal of Physics *55*:235. [Note *DIO 11.2* §G & fnn 26-27.]
D.Rawlins 1999. DIO *9.1* ‡3. (Accepted JHA 1981, but suppressed by livid M.Hoskin.)[25]
N.Hamilton-Swerdlow 1981. JHA *12*:59. Review of R.Newton 1976.
Gerald Toomer 1975. Ptolemy entry, DSB *11*:186.
Gerald Toomer 1978H. Hipparchos entry, DSB *15*:207.
Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.
B.van der Waerden 1984-5. ArchiveHistExactSci *29*:101, *32*:95, *34*:231.
B.van der Waerden 1988. *Astronomie der Griechen*, Darmstadt.

[25] Paper recovers several revealing, longlost ancient Greek & Babylonian values of the yearlength; ms accepted in entirety for publication by *JHA* Editor-for-Life (1981/9/17, and several times thereafter in response to pointed DR inquiries following repeated arbitrary deferrals of publication). Publicly stated to be forthcoming in 1982 (*JHA* ad: 1982/3 *Isis*), and material cited in Moesgaard 1983 (p.57). Failing in sudden late attempt to rush DR into agreeing to bowdlerization of paper (to Muffia specifications: fn 11), EfL then became furious & threatening in response to accurate 1983/2/9 criticism of EfL's disastrous over-riding of *JHA*'s own referees (for a non-DR paper published in *JHA*, later retracted & recomputed): ‡6 fn 15 & ‡8 §G6. EfL did not allege problems with Rawlins 1999's validity (already approved by both *JHA*-appointed referees, K.Moesgaard & W.Hartner). Nonetheless, *JHA* severed correspondence with DR 1983/3/21. Paper not in press. Will not be withdrawn.

‡2 Rawlins' Scrawlins

A Germs

A1 The more widely trusted an institution, the less trustworthy it is.
A2 The Middle Class: the one not on welfare.
A3 Gov'ts tend to permit free speech only if it's ineffectual.

B Guess Whether I Read the 2nd Half

B1 History instructor Ludlow Baldwin[1] (Gilman School), who 1st instilled in me a love for ancient history, was the most memorable teacher I encountered at any educational level.
B2 An incident of my senior year will illustrate why. In American History at Gilman, students were required to read a certain number of supplemental related books of their choice. I proposed to Ludlow that *Gone with the Wind* would be apt — but was so long that it ought to count as 2 books. He said: you're right, so just read the first half of *GWTW*, and that'll count as 1 book.

C Doubletakes

C1 The light side of heavy maternalism: "My [Irish] mother won't let me marry an Italian. She says Italians are too dominating."[2]
C2 Entertainment-world superplug-implosion: "He's a wonderful actor. And there's no pretense about him."[3] (Hey, didn't Reagan already pull that one on us for 8 years?)

D How to Soak the Rich & Have Them Like It

D1 There is a peaceful means for lowering interclass hatreds and simultaneously redistributing wealth, a means so simple and so inexpensive (as regards taxes) that its very mention is banned from all US media (right or left wing).
D2 This radical approach is: simply do whatever it takes[4] to ensure that middle and especially upper income groups have more kids, while the poor have fewer. This approach (inverting the usual trend) would also ensure that, statistically,[5] more children than not would grow up surrounded by affection, toys, books, computers, optimism, intellectual stimulation, and gentility. Less frequent foetal-alcohol-syndrome infants, and premature cocaine-snowbabies.[6] More homes with two parents. And no rats. Little things like that.

[1] Ludlow is one of the oldest & dearest friends of my wise stepfather, John Williams Avirett 2nd, and of myself. All 3 of us are fortunate to have married extraordinarily kind, bright, and cultured women.

[2] Conversation-fragment witnessed by Monika Mathews & DR, Loyola College (DS 215) 1990/3/9.

[3] Stated verbatim by C.Bernsen, of friend T.Berenger: *Entertainment Tonight* 1990/5/11.

[4] If this sounds drastic or unfeeling, then ask: is a temporarily-impolite but effective & relatively rapid solution more brutal than perpetual degradation? (Were Margaret Sanger or Bertrand Russell alive, there's little doubt: [a] they'd urge intercepting this cycle with aggressive population control, and [b] network TV would ignore their very existence.)

[5] This probabilitistic argument should not be construed as ignoring or belittling the remarkable, hard-earned exceptions that occur among numerous poor families. On the other hand, such exceptions are too often mis-adduced in order to suggest that no foresighted demographic policies are required, to lower the high statistical incidence of poverty breeding poverty.

[6] Unfortunately, Marie "Snowbaby" Peary & DR were never friends. But I am glad for her that she did not live to see the desecration of her lovely nickname, which now refers to children (of cocaine-addict mothers) who are pre-addicted to cocaine at birth. (Another cycle. See fn before last.)

D3 Also, by this means, concentrations of wealth would become diluted naturally & relatively painlessly, instead of by the current gov't policy of (forceably) taxing provident couples (who thus can afford fewer well-cared-for children themselves) in order to pay (inadequately) to feed the overnumerous children of someone else (usually improvident and single) — children whose depressing home-lives virtually kill their future-chances from the outset, so that the same gov't that encourages such a mess then taxes the middle class all over again, for eternally-ineffective band-aid "head-start" & "JOBS"-style programs.[7]
D4 Why not simply give our *entire society* a headstart, beginning right now (instead of collecting "data" interminably): ensuring that the children of the next US generation are born predominantly into caring, decent homes — rather than our going on inertially accepting (as *faits accompli*) birth after birth to poor, semi-literate, and-or addict parents living in hopeless slums, so that we must forever be trying to patch up (belatedly) the inevitable resulting disaster: illiteracy, crime, drugs, and the whole by-now-drearily-familiar show? (What would we think of the Dutch people's smarts, if they'd never built dikes but instead just tried bailing the sea out of Holland forever?)
D5 Such selfevident social ideas (or something like them) have been around for decades. Yet one now never hears them at all in the media, which is [see *DIO 2* ‡1 fn 38 & ‡6 fn 23] run by parties who (while themselves religiously avoiding going anywhere near slums) proscribe such approaches as "elitist" and thus intolerably offensive to the poor's "dignity" & "ethnic pride" (and other similarly patronizing pseudo-sensitive word-stroking). Politeness is, after all, much more important than alleviating generation after generation of mass misery and despair.

E You Are Getting Verrrry Sleepy . . .

E1 US "news" outlets (especially TV, which forms most voters opinions: see §A1) ever-increasingly act as propagandists for our Rulers and for the ever-shrinking spectrum of Conventional tenets they tolerate.
E2 The result is a spectacle which I recommend henceforth labelling: 'SNEWS. This because: [a] TV 'snewsprograms are boring & repetitive. [b] So are the ads (overt & covert) which clutter them up. [c] The network trinity 'snewscasters, through incessant repetitions, lullaby the public into accepting explicit or implicit viewpoints useful to the gov't, and dissenters are given virtually no space. [d] As the nation sleepwalks into decline, the public is pacified-hypnotized into accepting that this is occurring despite the media's best efforts to reverse the trend. [e] Each network 'snewsdepartment is *owned* (thus the conspicuous apostrophe). It is owned and controlled by a power-dealing, ad-catering organization — whose interests are not your interests (borrowing a Vidalism from a slightly different context).

[7] E.g., see "Breaking the Welfare Cycle That Destroys Our Children" (which starts deceiving us right in the title: the next-last word), signed by Senator Moynihan (D, NY) (*Wash Post Nat.Weekly Ed.* 1990/12/3 p.23). The article (to which I add occasional astonished emphases) states that, after Aid to Families with Dependent Children (AFDC) began in 1935, surprisingly: "we experienced a vast, *still little understood* social change involving a huge increase in the number and proportion of children born out of wedlock. . . . among children born in the years 1967-69, the first cohort . . . tracked. . . . 72.3 percent of black children and 15.7 percent of nonblack children were supported by AFDC at one point or another during childhood. . . . Congress [in 1988 established] extensive provisions for the evaluation of the impact of the programs, especially the Job Opportunities and Basic Skills training program (JOBS). . . . that *will tell us over time* the extent to which child dependency is increasing or decreasing — and what if anything, government programs are doing to affect that dependency. . . . high rates of births to young, single women *may be with us for a long time*. We need to . . . . collect the right data. . . . We will not even begin to know whether [Congress' 1988 bill] is having any effect until the year 2000 at the earliest, perhaps the year 2010. (To those who may wish to protest that is too long, I would answer that *they* should have thought of that a quarter century ago when *we* first spotted this social change.)" Comments: [a] No wonder politicians drink. [b] When's the next one-way flight to Japan or Europe? [c] *Animal House* rulership's reproach to Flounder: Face it, you screwed up; you trusted us. (Parapsychologists, UFOlogists, & Ptolemists also prefer unending data-collection, thereby avoiding confronting the shame of having pursued & promoted a false path for decades.)

F The Roundest Possible Number

Barring horrible (and, of course, inevitable) intervening consequences, natural world population growth will just roll along at around 2%/annum. A round number I've never seen computed in print: at this well-established growth rate, how long will it take before the entire population of the world is crowded shoulder-to-shoulder? (We'll know when the day approaches, because gov't-TV 'snews will be advertising the benefits of sleeping erect and of the wondrous new physical closeness of the brotherhood of man.) The land area of the Earth is around 10^{14}meters^2, and a standing human occupies roughly 1/10 m^2. So 10^{15}persons will literally cover the Earth's land with a solid 2 m-thick layer of human protoplasm. The current world population is about 1/2 of 10^{10}persons, thus growth by a factor of around 200000 will do the trick. Since the above 2% figure corresponds to a growth-factor of 1.02 every year, we simply divide the log of 200000 by the log of 1.02 to get the answer: roughly 600 years[8] hence — or, about 2600 AD. That is, in less time than merely the span of history that has passed since the Crusades or Dante, our planet is scheduled to turn into a round human-sardine-can.

G Shorts

G1 Though most great academics are religiously unorthodox, dedicated scholars are akin to a priesthood: eschewing crude hedonism for a higher calling. And partaking of an elite priestly succession: preserving, purifying, and hopefully augmenting a precious and beloved heritage, even while passing it on down to those who come after.

G2 The Solar System has 2 pairs of twin planets (near-identical mass): Venus-Earth and Uranus-Neptune. A peculiarity (evidently hitherto unremarked) that may provide a clue to the system's origin: both pairs involve contiguous planets (in order of mean distance from the Sun). Also: V-E is the closest pair of terrestrial planets, while U-N is the closest Jovian pair.[9] Finally: the only retrograde-rotating known planets in the Solar System are the inner members of these 2 planet-pairs: Venus and Uranus.

G3 In the post-World-War-2 period, race-integration became the prime US goal for achieving social justice and equality. Meanwhile, it's been all downhill in the US for populism, the New Deal tradition, socialism, unions, and the intellectual left.

G4 While wincing at the shams in what popularly passes for democracy, I am at least cautious about desiring instant pure democracy here, upon considering what the US public would do to the Bill of Rights if it could. (Polls indicate it would be more than 2/3 dismantled if put to popular vote.) Certainly, I would like a fuller slate than the pair we get to choose from in our Plunkittesque US Presidential "elections". And I regard no election as valid that does not have a none-of-the-above lever. But then I realize what sort would win here in a *truly* open contest. President Elvis? Lucky he's alive to accept.

G5 When a criminal is to be tried (especially for murder), advocates have been known to protest that [a] the perpetrator was at the mercy of impulse & without internal selfgovernance, and [b] his punishment will not deter other criminals since they're just as irrational. But, when it's Oscar-time before the parole board, one instead hears: this is a sane person, who won't-do-it-again — because he's is in control of his actions . . . .

G6 Refereeing is the key to equity & progress in the modern academic community. I am happy to say that (in my experience) the majority[10] of referee reports in US scientific journals are both well-intentioned & competent. Given the state of fairness & expertise in numerous other realms, this record is something — and something important — to feel good about.

G7 The fine print of FBI statistics show (*World Almanac 1991* p.848) that in 1989 there were 120 times as many murders *per capita* in the District of Columbia as in North Dakota. Not 120 more murders. Not 120% more murders. No, *one hundred and twenty TIMES more murders*. (For context: the D.C./N.Dakota ratio for all types of crimes is "merely" 4-to-1.) Are there perhaps sharp, relevant demographic differences between D.C. and N.Dakota, from which we might learn something regarding how to start lowering murder rates? TV 'snews isn't even slightly interested in this issue, so I guess the answer is: No.

G8 Pragmatic cynics are clever enough to realize the usefulness of if-you-can't-say-something-nice-don't-say-anything sentiments, while genuine idealists (aghast at the resultant hypocrisy in the passing societal scene) are driven to overtly cynical observations. Superficial public perception thus easily reverses the two types.

H Some Neglected Modern Saints: the Angelmaker Paradox

H1 Those who condemn abortion fail to understand that abortion is, ethically speaking, the purest of deeds. A traditional French nickname for abortioners is "angelmakers". For, what the abortioner accomplishes is a grievous sin on his own celestial scorecard: he goes to hell for murder. But he catches the foetus at a perfect moment: an utterly sinless soul. Every abortion-murder the angelmaker commits sends another pure soul to heaven. What could possibly be more selfless? How can the ideal of ethical sacrifice have a purer expression than: the *eternal*-hellfire-pain destruction of one's own soul, in exchange for the *eternal*-paradise salvation of thousands of one's fellow souls? Not even the Battle of Britain offers a better example of so many owing so much to so few. When technology produces test-tube foetus-farms and so finally realizes the progressive ethicist's awesome futuristic dream of mechanized mass-foetus-murder, heaven will be stormed by such an unprecedented wave of sinless souls that the deity's cup — and abode — may finally run over . . . .

H2 The pious life has traditionally been formed with the primary aim of the salvation of one's own eternal soul. In the context of our angelmakers, how embarrassingly self-centered this now seems. According to the purity & volume of those Saved, even the holiest long-ago saint's accomplishments pale by comparison to the esteemed work of these modern paragons of self-effacement. Until I see the Beatitudes and Dante revised, to atone for the neglect and misunderstanding abortioners have endured for centuries — until I see canonization proceedings initiated — I will know that the world still languishes in a primeval Limbo of pre-angelmaking ethics.

I The Immortal 535

I1 Cokey Roberts (ABC-TV 1990/9/16 David Brinkley 'snews-hour): why, Congressmen aren't re-elected automatically, as has been commonly stated of late; indeed, 93% of those who sat in the House when Speaker T.Foley first entered it are gone!

I2 Ms.Roberts' misrepresentation is a classic instance of an increasingly omnipresent problem: journalism-as-lobbying. Take a close look at the data: Foley won his seat in 1964, 12 congressional races before her statement. Ms.Roberts emphasizes that only 7% of his colleagues are left — but the advocate in her omits the relevant math: the 12$^{\text{th}}$ root of 7% is 80%; so 4/5 of Congressmen have survived each election, on average. And the mean annual 20% casualty-rate includes deaths & retirements for other causes. Thus, the actual 24 year-average re-election rate is likely nearer 90%. Ms.Roberts' most obvious qualification as one of ABC 'snewspersons is that her father was the late Congressman Hale Boggs (who died in the congressional saddle). (ABC's promotion of such as Roberts tells us just how trustworthy it is.)

[8] For those who can't handle logs: multiply 1.02 times itself 616 times to see that the product is about 200000. [Note added 2008. Factor 1.02 is presently too high, so 1000^y may be nearer the (hypothetical anyway) mark.]

[9] Speculation: if Me is an escaped satellite of V, while P is an escaped satellite of N, then each pair may bound the true planets of the Solar System. (Note: in §G2, "closest" refers to distance-ratio, not absolute distance.)

[10] R.Newton and I have estimated similar proportions: about 3/4. I must add that other scientists whom I respect report less fortunate experiences. But there is no question that the fraction of creditable referee reports in US science is substantial.

I3 Whatever its reason,[11] *Time*'s 1990/11/19 cover stated the truth: even at a time of outrage at congressmen, 96% of incumbents got re-elected in 1990.

J Practicals

J1 How does one find an up-to-date roadmap? Most don't bear dates anymore. (Penny-counting publishers want to sell a mass-printing indefinitely.) This is an example of an abuse theoretically best handled by legislation, but which will probably not end until consumer journals start listing ratings & warnings.
J2 With some exceptions, tape-decks display a digital "counter" which usually indicates revolutions n of one of the deck's reels; n is aggravatingly unproportional to time t, so it may be useful to provide the general relation of t to n, which is: $t = A \cdot n + B \cdot n^2$. For the now-ubiquitous Video Cassette Recorder, n records the takeup-reel's revolutions. At the customary 6-hour speed, with the US standard VHS tape, taking t in timeminutes, we have (to an accuracy of a few timemin)[12] $t = 0.0309 \cdot n + 0.0000057 \cdot n^2$.

K Blinders

K1 There is a wellknown legend that certain 17th century churchmen adamantly refused to look at the sunspots revealed in Galileo's telescope, allegedly because they could not believe there were blemishes on the solar disk.
K2 However, was the churchmen's actual concern simply: possible eye-damage? Incidentally, Galileo later went blind.
K3 Galileo at least took some precautions to dim the sunlight he observed; but many ordinary citizens today staunchly ignore warnings and stare right at the Sun during solar eclipses' partial phases. Result: every solar eclipse produces lots of retinal-damage cases. Lesson: never stare at the Sun; all you'll see is a doctor.

[11] Boggs was Dem (majority party). *Time* is traditionally GOP (minority party). Which party wants numerical status quo? Which doesn't?

[12] The constant A depends upon the takeup reel's inner radius (pretty standard). The constant B is a function of tape-thickness, and will vary by a few percent. Note that an absolute difference in Δn does not correspond to a time difference Δt unless one knows what the mean n is; thus, it is useful to find the rate dt/dn as a function of n: $dt/dn = 0.0309 + 0.0000114 \cdot n$. Near the end of the reel, this rate is close to 1 min/per 10 revolutions. (The tape usually runs about 6h10m and ends with n about 5800.)

‡3 Unpublished Letters

A The Secret of Safford's Prank

To: *Harvard Magazine* 1983/1/11 [rev. 2/9]
From: DR, Class of 1959

A1 I am rather surprised at the unskeptical nature of your article (1982 Sept-Oct pp.54-56) and the only letter published [in response] (1983 Jan-Feb pp.23 & 54) regarding "Lightning Calculator" T.Safford [Harvard Observatory astronomer]. Though Safford worked for many years among academicians, the article's centerpiece tale proving his rapid calculational abilities is a (posthumous) account by a bible salesman (Rev. H.Adams) of Safford's circuslike performance during an 18-digit-by-18-digit multiplication problem, said to have been completed in one minute:

$$365365365365365365^2 = 133491850208566925016658299941583225.$$

A2 Performed normally (as Rev. Adams and the reader were led to believe was the case), such a computation requires 18^2 (that is, 324) smaller multiplications (not to mention a mass of additions), which in 60 sec allows less than 1/5 sec per! This is so patently fantastic in itself that it should not be necessary for me to illustrate that this specific "multiplication" is in fact no more complicated than balancing a chequebook [merely a simple 3-digit-staggered addition of an arithmetic pyramid of low integral multiples of 365^2 (133225)]:

```
                              133225
                           266450
                        399675
                     532900
                  666125
               799350
            666125
         532900
      399675
   266450
133225
------------------------------------
133491850208566925016658299941583225
```

A3 A few years ago, as an instructional part of an anti-mystic effort, I convinced a number of temporary victims (including a well-known Cambridge astronomy editor and the whole physics department of a large west coast university) that I was a genuine idiot-savant, by performing 8-digit-by-8-digit multiplications in roughly a minute — without Safford's giveaway repeated integers and without any props (hidden scratch-papers, radios, or calculators). However, I always made it clear afterwards that it was just an illusionist's trick. This is more than a matter of personal integrity. It is simply cruel to mislead people by convincing them that their normal attributes are far beneath what is in fact an unreal standard of superiority. Whether that standard is a comic's secretly rehearsed "ad-libs" or *Playboy*'s silicones or a pseudo-lightning brain, the result is the same in the naïve observer: discouragement though a baseless self-impression of inferiority.

A4 Retrospective DR remarks: The 7th digit in the Harvard Magazine rendition of the solution is misprinted (3 instead of 8). The same 18x18 Safford-Adams fable is also repeated in James Newman's *World of Mathematics* NYC 1956 p.466, where the next-to-last digit is printed as 5 instead of 2. And Petr Beckmann's *History of* π (NYC 1971 p.104) tells the same tale, including Newman's misprint. Which is just one more illustration of how much care is exercised by those whose casual hand-me-downs generally determine academic history. I have yet to see an account that correctly printed the solution, much less realized Safford's easy method of actual computation, provided above for the first time.

A5 *Harvard Magazine*'s failure to expose the undeniable truth behind a Harvard astronomer's most famous hoax is not much of a mystery when one realizes that, in 1983, *HM*'s resident astronomical expert was the unavoidably ubiquitous O Gingerich (Harvard Observatory), then on the *Magazine*'s Board of Directors. (OG finds it difficult to doubt anyone but doubters. He believes in Ptolemy, archaeoastronomy, and Jesus.) A 1983/2/23 letter from *HM* Copy Editor Gretchen Friesinger claimed that the DR letter was set in type, and "there's a good chance we'll publish it in May." Never happened. (It may not be irrelevant to note that DR's 1983/3/3 banishment from OG's enraged *Journal for the History of Astronomy* occurred between Friesinger's letter and May. See ‡6 fn 15.)

A6 Unlike some Harvard astronomers, Safford was a highly capable mathematician. (See, e.g., his 1862/3/14 Royal Astron Soc papers on finding the mass of Neptune from Uranus' residuals and on the declinational proper motion of Sirius.) So why did even he feel the need to exaggerate his considerable computing abilities? Not long after the above letter, I got an inkling of the problem — while observing a young modern mental whiz's public exhibition of his skills: he executed a variety of swift genuine mental tricks, but then ended the show by faking an impressive computation, using a simple device. When I asked him about it after the performance, he readily admitted his little humbuggery. (We then had dinner and spent a pleasant evening amiably trading techniques & tales.) He explained that, unfortunately, audiences were more impressed with the easy fake trick than his real ones.

B A Progressive Obituary

To: *Time* Magazine 1981/3/9
From: DR

B1 Howard Hanson was guilty of the crime of composing music for beauty not fashion. Thus, nearly half of his *Time* Milestones obituary (1981/3/9 p.74) is the statement, "Also a teacher and conductor, he fought tirelessly, if unsuccessfully, against progressive trends in American classical music." Whatever the romantic Hanson's private view of "progressive" music (an ad-man sort of term, masking personal taste in natural-law garb), he in fact conducted *and promoted via recordings* [by his Eastman Rochester Orchestra] the music of: Carter, Ginastera, Hovhaness, Ives, LaMontaine, Piston, Riegger, Sessions, and Thomson. Evidently, Hanson was less narrowed by his artistic preferences than some Progressive obit writers are.

B2 The same day, I wrote to Donald Shetler (Inst Amer Music, Eastman School of Music, Rochester): "So soon after Barber's passing, I was saddened to hear of the death of Hanson.[1] His music was among the loveliest memories of my youth and will help keep that youth from quite passing away. . . ."

[1] This letter also commented: "*Time*'s obituary was too brief . . . but, considering its slant, perhaps that's a blessing. . . . [*Time*'s music column] actually placed its . . . [1975] obituary for Shostakovitch second behind a bigwetkiss promotional piece for Phoebe Snow".

B3 For introducing me long ago to the unabashedly romantic music of Hanson (& other moderns), I have always been thankful to my lifetime friend (& Harvard roommate), Ted Defandorf. Hanson's greatest contemporary popular success has been the use of his "Romantic Symphony" (1930) at the peaceful conclusion of the classy and scary (& feminist) scifi film, "Alien". On 1990/11/27, I suddenly wondered whether he had lived long enough to know the joy of realizing that "Alien" had brought his work (in such a heavenly setting) before the widest public he had ever achieved. I quickly learned that the answer was: Yes. The film appeared in 1979, two years before Hanson's 1981 death.

B4 But my next question was: why had I cared enough to look up these dates with such fervor? After all, the happiness I was hoping Hanson had experienced was in the past, and he was now long dead. (To Orwell's "O'Brien", the past is a chimera, since it exists merely as infinitely manipulable collective memory.) But it mattered anyway.

B5 True historians are defined by their not caring if the past is unreal. It is real to us.

C PseudoPrediction

To: Joe Ashbrook, Editor *Sky & Telescope* 1967/7/3
From: DR

C1 Now that Pluto is approaching us, mightn't another check be in order sometime for possible satellites . . . ?

C2 The foregoing, written 11^y before the discovery of Charon (Pluto's satellite), looks prescient. And it could be made to look more so, by reference to a paper of DR & Max Hammerton (*Mon Not Royal Astron Soc 162*:261; 1973) where, at p.263, it is carefully noted that "Pluto has no *observed* satellite" (emph added). But the truth is unfortunately quite otherwise: between 1967 & 1973, DR had come to believe that there was no Plutonian satellite; so, the original 1973 ms lacked the word "observed" — an adjective that was wisely inserted before publication (probably either by Max or by David Dewhirst).

C3 DR is telling this tale on himself because: [1] The foregoing exhibits excellent raw material for enabling the wise reader to discern typical opportunities which professional predictors make quite different use of. [2] Editors often inadvertently degrade a paper's accuracy. It is just, pleasant, and beneficially humbling[2] to recall an opposite experience.

D The OverConfidence Artist as Hitman

D1 Among the more striking aspects of the following grossly libellous letter are that: [a] it was written on University of Chicago stationery, and [b] so far as the public record shows, the University has no misgivings about the writer. Handwritten and signed by Univ of Chicago professor Noel T. Swerdlow, the letter was mailed to physicist Robert R. Newton (Johns Hopkins University Applied Physics Laboratory). Though repulsively malignant, the letter is in truth a precious document, in providing a firsthand inside-look at the sanity & equanimity that have characterized the Hist.sci crowd's reaction to RRN's skeptical writings on Ptolemy. Thus, despite the letter's highly offensive contents, RRN has agreed to its publication in *DIO*. (Similar slanders against dissenters on Ptolemy — DR included — have been circulated for decades by Hist.sci archons.) Barely less feral Swerdlow attacks (against physicist RRN and mathematician van der Waerden) have repeatedly been published by Hist.sci journals, and not a word of disapproval has ever appeared in these turf-protective, incestuous forums. (They and Swerdlow clearly deserve each other.) The Swerdlow letter follows:

[2] A little humility is needed for balance, since the authors are naturally more fond of recalling that this paper's proposed value for Pluto's mass (1/40th of the Earth+Moon mass) is now known to have been the most accurate ever published — during the 4 1/2 decades that passed after the planet's discovery (1930), until the Pluto controversy was resolved in 1976-1978 by direct evidence.

THE UNIVERSITY OF CHICAGO
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5640 SOUTH ELLIS AVENUE
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Dear Mr.[3] Newton: June 2, 1983

D2 Thank you for your book on Ptolemy, which I have looked through and am now returning since there is probably someone else who would rather have it and make better use of it. As I have read your various Ptolemy pieces over the years, they have come to seem to me not just wrong-headed and careless, which they are, but positively crank. And the more you go on and on with your crusade, the more of a crank you appear, not merely to me, but to anyone who simply keeps count of the extraordinary number of books and articles you have written trying to prove your silly accusation of someone dead no less than 1800 years. You are really much of a kind with the people that used to carry on about Francis Bacon's writing Shakespeare, and that sort of thing, or to bring it up to date, the Velikovsky people.

D3 The most remarkable thing about your work, to me at least, is that you manage to get it funded by the U.S.Navy on the preposterous grounds that it is "intimately connected with the precise measurement of time." This is the kind of flim-flam, indeed out-and-out lie, that would make P.T.Barnum blush. And you call Ptolemy a fraud? It is far more likely that you are a crank and a con-man, whose principal accomplishment has been extracting money from the government on false pretenses.

Very truly yours,
Noel Swerdlow

D4 Since RRN & DR had long been debating who the Ω among Ptolemy's apologists was, RRN's 1983/6/10 reaction (when sending DR a copy of Swerdlow's letter) was simply: "This definitely promotes Gingerich to Ψ." RRN later responded for *DIO* as follows:

To: *DIO* 1988/2/3
From: R.Newton

D5 Two astronomical phenomena have been used to furnish standards of time. One is the earth's spin on its axis, which furnishes the standard that we call solar time or universal time. The other is the orbital motion of the earth around the sun, which furnishes the standard called ephemeris time. The relation between solar and ephemeris times is of high importance for fundamental astronomy and in particular for the determination of time, which, in the United States, is the responsibility of the U.S.Naval Observatory.

[3] Though RRN has a Ph.D., Swerdlow's 1979 *American Scholar* (Phi Beta Kappa) attack referred throughout to "Mr.Newton", due to Swerdlow's inability to get even that simple fact straight. O Gingerich, who was on the *Amer Schol* board responsible for publishing this embarrassment (and for, as usual, preventing any printed reply), claims to have been simply appalled. This did not stop OG from publishing more of Swerdlow's frothings against RRN, in the 1981 *JHA*! (Examined at ‡5.) Though OG was the sole member of the *Amer Schol* board with the slightest interest in defending Ptolemy and attacking RRN, OG nonetheless claimed in a 1979/12/10 letter that he was not the *Amer Schol* party who invited Swerdlow to write the 1979 review. Same OG letter: "I am happy to say that the original pugnacious tone of the article was considerably ameliorated before publication." (A glance at what was printed suggests that the original must have been nearly on the order of the Swerdlow 1983 letter displayed here.) OG continues: "I strenuously objected to the condescending use of the expression 'Mr.Newton,' which was changed to 'Prof.Newton,' but when they found out he was not a professor they put it back to 'Mr.' at great expense in the typesetting." Insistence (for years) upon ranking a dissenter falsely — not to mention deceiving Phi Beta Kappa readers (regarding RRN's actual degree) — evidently meant more to Swerdlow than it would to a scholar of normal emotional composition.

D6 If both kinds of time flowed uniformly, we could simply find the ratio of their rates, and then use whichever was convenient. The more convenient one for regulating our lives is clearly solar or universal time. It turns out that the two kinds of time do not flow uniformly with respect to each other, so that at least one kind of time is flowing non-uniformly. At present, we assume as a working hypothesis that ephemeris time flows uniformly, and we must then determine how solar time varies with [respect to] ephemeris time. A study of ancient and medieval astronomical observations is of great value in determining this variation.

D7 DR will offer two comments: [a] When Swerdlow scoffs at RRN's criticizing someone who died 1800^y ago, he appears to be implying[4] cowardice. This is slightly odd, coming from the chief attack animal of the Neugebauer Muffia, which has ducked face-to-face debate of the Ptolemy Controversy for over 20 years. [b] Before unsheathing its obligatory hatchet, the 1981 review which Swerdlow co-authored (on R.Newton's 1976 book)[5] states in its opening sentence: "For over a decade Robert R. Newton has engaged in the *laudable* project of analysing ancient and medieval astronomical observations in order to obtain improved determinations of the retardation of the Earth's rotation and of the Moon's secular acceleration." (N.Hamilton & N.Swerdlow *JHA 12.1*:59, emph added.) This sentence pretends to the reader that the authors approached the work with an amiable disposition but then became regrettably "*compelled* to point out" its inadequacies (p.60, emph added). However, the Swerdlow letter's actual belief (§D3) is that RRN's entire "laudable" work in this area is worthless & crank. The contrast here (between letter and review)[6] leaves us in little doubt as to why Swerdlow[7] confidently regards himself as an expert on confidence-men.

[Note added 2012. Swerdlow's attempt, to paint R.Newton as a crank for contending that Ptolemy faked & plagiarized, is as ironic as his ill-chosen initial example of supposed crankitude: disbelief that Shakespeare was a writer. Common factor: unalloyed-careerist Swerdlow always sides with relevant RichEstablishmentThink. (See *DIO 4.3* ‡15 §A3 & *DIO 20* ‡2 §B.) Sadly for his repulsively rendered verdict on R.Newton, the intervening 3 decades have seen an opinion-shift in the very hist.astr establishment Swerdlow counted upon for eternal-verity: except for such cementalities as himself & J.Evans, it is now universally understood that Ptolemy indeed faked & stole. As for the ongoing controversy over actor & loan-shark Wm.Shakespeare, the 19th century authors who lauched scholarly questioning of Shakespeare were such "cranks" as Twain, Hawthorne, James, & Whitman. More recent skeptics include Westminster Abbey, several US Supreme Court justices, crime-expert Colin Wilson, & hoax-detective DR. In light of our Ptolemaic lesson on the mortality of Eternal-Sacred-Moneycows, readers may share our foreseeing eventual general realization of C.Marlowe's authorship of the works now generally attributed to Shakespeare: see *DIO*'s "BardBeard" at http://www.dioi.org/sha.htm.]

[4] If the implication is rather that one cannot prove anything about someone dead 1800^y, then why is Swerdlow pursuing ancient astronomy?

[5] R.Newton *Ancient Planetary Observations and the Validity of Ephemeris Time*, Johns Hopkins Univ Press, 1976.

[6] The review quoted is the same one (HamSwerdlow 1981) which is atomized by author R.Newton later in this *DIO* at ‡5. No wonder Swerdlow didn't send it to DR (see following fn).

[7] DR's last letter to Swerdlow (1981/4/5, just after DR had phoned NS on that date) well illustrates Ptolemy-skeptics' legendary viciousness, which presumably accounts for Muffia noncitation of DR: ". . . Thanks for filling me in on the background of the theory that Hipparchos-Ptolemy's year comes from multiplying the ['Babylonian'] month by 235/19. . . . It's inspiring to encounter such historical acumen in so fine a theorist and observer as Tobias Mayer. Despite our [NS's & DR's] large disagreements on the value of Robert Newton's and of Ptolemy's work, I'm glad we talked. (And I repeat [DR's telephone suggestion] that it would be nice to get all of us — [Robert] Newton, you, I, and some of the others interested in the [Ptolemy] controversy — together at an informal gathering to chat and learn from each other's viewpoints.) Looking forward to seeing the papers you're sending." Swerdlow's reply? No reply. (Similar to ‡6 §H5.) Unless one counts the 1983 letter to RRN (§D2-D3).

‡4 Peary, Verifiability, and Altered Data

A Melting Myth

A1 The greatest of US polar explorers, Robert Peary, claimed to have reached his long-sought grail, the North Pole, on 1909 April 6-7, after 5 weeks of arduous dogsledging over the rough, broken, and drifting ice-floes of the Arctic Ocean. For 8 decades, the claim has been allowed, though [1] Peary did not provide normal specific, verifiable scientific proof or fruit of it and [2] his reports are riddled with anomalies. He was initially believed due to his brilliant previous explorations, which included his and Matt Henson's unquestioned 1900 discovery of the world's northernmost point of land, Cape Jesup (latitude 83.7 degrees).

A2 The case against Peary's 1909 claim rests on numerous firm and independently self-sufficient lines of evidence, many presented in my 1973 book, *Peary at the North Pole: Fact or Fiction?* Most polar explorers[1] have agreed with the negative verdict of *Fiction*, long the premier skeptical volume on the subject. (Cited in *Encyclopedia Americana*. And see *Science* 1989 March 3 [SCI 243:1131]; this article also severely dispenses with one document I misfired on, but it details how convincing *Fiction*'s case is and provides welcome indication that the science community will now apply standard verifiability criteria to all scientific claims, no matter how sacred.) And much agnostic evidence appeared in the epochal centenary 1988 September *National Geographic* [NGM 174.3:387] (surprising many, since National Geographic had co-sponsored the 1909 trip).

A3 I will précis *Fiction* below, adding various startling new confirmatory materials, most not in the 1988 article.

B Late Claims

B1 In Peary's 1907 book, *Nearest the Pole*, he reported as his 1906 expedition's 2[nd] most important achievement [PY 280 & map] the discovery of "Crocker Land", perhaps the northernmost land on Earth, supposedly seen (from north Canada) by him and his Eskimos upon the distant northwest horizon on 1906 June 24 and 28 [PY 202, 207]. Crocker Land does not exist. *Fiction* noted its absence in Peary's then-known 1906 records (including his handwritten June 30 description of his June 28 "clear view of northern horizon") and so argued [F72-77] that Crocker Land wasn't seen in 1906 June but materialized only in 1907 to reward banker George Crocker for a $50,000 contribution.

B2 Peary's 1906 June diary has now been recovered. It never mentions Crocker Land. To the contrary, the June 24 entry says [PC 1906/6/24 p.39]: "No land visible west of [nearby] Jesup Land." Peary's 1907 book (*Nearest . . .*) is for 1906 June 24-28 copied virtually verbatim from the diary.[2] Except for 2 passages, inserted whole into the account. These contain both the later-alleged sightings of Crocker Land.[3]

B3 Also newly available in the Peary Papers (US National Archives) is a typescript copy of Peary's diary for 1906 April 2-20. Explorer Wally Herbert revealed [NGM 174.3:398] in the 1988 *National Geographic* that the typescript stops just 1 day before Peary's long-suspect alleged Farthest North (87.1 degrees), with his party way too far south to cover in a day the enormous last-minute [distances] required by his book's account [PC 1906/4/20

[1] [E.g., Umberto Nobile, Martin Lindsay, Finn Ronne, Joseph Fletcher, Bertrand Imbert, Ralph Plaisted, Wally Herbert. See also explorer David Roberts' perceptive look into the exploration-hoaxer's mentality (e.g., RH viii).]

[2] [For 1906/6/23-7/1, compare PY 200-212 to PC 1906/6/24-28 pp.35-56. Note that Peary deletes from PY 211 his diary's p.55 reference (1906/7/1) to his having on 1906/6/30 left at Cape Hubbard a handwritten cairn record (later recovered only by chance: FC 187, F75). This record, photoreproduced at F77, contains the above-quoted (§B1) devastating reference to the clear northern-horizon view on 1906/6/28, the very moment at which Peary later stated (PY 207) he resighted Crocker Land.]

[3] Peary advocates cannot regard his 1906 exaggerations as irrelevant to his 1909 Pole claim since the Peary Arctic Club used Cook's 1906 fake climb of Mt.McKinley to discredit Cook's 1908 Pole story [FC 175f].

vs. PY 133-139]. The diary reveals other serious misreportages in his book: alleged Peary steering by compass at head of party all day (April 14, Henson way ahead [see below, fn 12]); 10-20 percent exaggerations of marching time (April 14), distance (April 14, 15), and speed (April 16).[4] The April 14 entry says the pace "I think has been nearly three miles an hour" for 9 hours (less than 27 miles or about 25 miles);[5] the April 15 entry says explicitly that the day's march was 25 miles. But both days' distances are reported in his 1907 book as 30 miles [PY 131]. Such Peary-critics as geographer Rev. James Gordon Hayes (1929) have long scoffed at these claimed 1906 distances as incredible [HR 61]; and all doubters of the subsequent Pole claim have believed that Peary stretched his estimates of the unverified 1909 march-distances [e.g., HR 87]. The 1906 diary entries now provide black-and-white proof that critics' suspicions were justified.

B4 In 1911, Peary's arch-rival, explorer Frederick Cook (who claimed he reached the Pole in 1908, a year before Peary), reported [CM 542] that Peary's sextant was too damaged in 1906 for accurate navigation. Peary's 1906 April 13 diary entry confirms this.[6] Cook stated [CM 490, 559; C54A:54, 59] that Peary's 1906 Eskimos said Crocker Land was never seen. Peary's 1906 records verify that. Cook was the first to publicize Peary's natural children [CM 493 opp., 601], now displayed in the 1988 *National Geographic* [NGM 174.3:417-429]. *Fiction* deemed Cook's 1908 Pole trip a fantasy [F79f] (triggered by inside knowledge of nonverification of the Peary 1906 Farthest [CM 542, F82]) but regarded these 3 Cook reports as accurate [F69, 73, 74, 201]; and all new evidence backs this position.

C Final Shot

C1 With Peary's years and funds running out, 1909 was his last chance for fame eternal [PZ 9-10, 192]. On February 28-March 1, his sledge parties left land at Cape Columbia (Canada's north tip, 83.1 degrees latitude), heading for the Pole, 413 nautical miles distant. On April 1, Peary sent back his last navigator-witness, Capt.Bob Bartlett, at least 135 miles from the Pole (Bartlett camp). Accompanied only by loyal Matt Henson and 4 Eskimos, Peary then proceeded on, allegedly to the Pole.

C2 Peary, a highly skilled surveyor, had on all his previous Arctic Ocean trips (1900, 1902, 1906) brought back theodolite data for the magnetic compass' "variation" from true north [RR 36 n.34, F130]. But in 1909 he observed none [F130, 226-228], though his theodolite was ever at hand [PZ 288n]. New physical data are the prime scientific justification for exploration [F130-132]. (And Peary had in 1908 requested proof be demanded if Cook claimed the Pole [F126].) Along Peary's route, the needle points somewhat nearer south than north. So frequent observational checks of the large and changing compass variation are required for steering north. But Peary denied at his 1911 Congressional hearings that he took such data in 1909 [SPH 299, 310; F226-228; & note F128-131].

C3 He also there admitted another Peary first: no astronomical observations (solar altitudes via sextant) for longitude to check his 1909 left-right deviation from the intended path to his goal [SPH 317, F136, 140, 143, 231]. His diary and reports never mention or even contemplate [F136] veering leftward or rightward (short of the elusive Pole) in order to correct [F117, 142] for the inevitable misaim all real North Pole trips experience, going over icefloes that drift a few miles a day, with frequent interruptions between these floes,

[4] [PY 132 has "not less than two and one-half miles per hour." But the diary (PC 1906/4/16) says instead: "Hope we have come at rate of two miles an hour at least, and am sure we did as long as it was clear."]

[5] His book [PY 131] has 10 hours at "not less than three miles an hour." I.e., total at least 30 miles.

[6] The 1906/4/20 diary entry also reveals Peary's understandable concern that calculation from his April 19-20 sunsights has given a falsely high latitude (86°.5 N) since these data weren't observed at local noon. This point is crucial to his 1906 Farthest [since the reported Abruzzi-Cagni 1900 latitude record Peary aimed to beat with a long 1906/4/21 march was 86°.6 N] and to his later Pole claim — but is unnoted in either trip's reports [e.g., PY 133, PZ 268, 284].

from "pressure-ridges" (high barriers of ice) and "leads" (lanes of open water) [PHm 25:11, F146]. The resultant detour-zigzags take one east-west as often as north.[7]

C4 [The] key point here: Peary had ample and repeated opportunity in his official hearings and reports to explain how he aimed towards the Pole;[8] but he steadfastly avoided doing so whenever such questions arose [e.g., SPH 299, 310, 316, 317].

C5 Despite attempts at compass-adjustment [HE 88], Peary's sole extant 1909 compass course [PZ 232, F131, 138] was off by about 8 degrees; and from various such aiming errors (as well as detouring and drift), his previous shots at the Pole lurched hugely left and right, misaimed by about 10 to 15 degrees [F135]. But in 1909 he alleged a straight-line path, with about 1 degree aiming accuracy [F140, 154], hitting only 4 miles left of the goal, a navigational pole-in-one.

C6 Peary's 1909 diary is now declassified. Its April 2 entry records his actual regular seat-of-the-pants steering [PM 0051]: "setting course by moon, our shadows, etc." (Hardly the stuff of 1 degree aiming precision.) The most vital portion of the entire diary, these 7 words are Peary's sole inexplicable omission [SPH 302] when reading the diary at his 1911 hearings.

C7 In 1911, individualist writer W.Henry Lewin remarked [LF 8; also Cook: CM 505-506n] that Peary's speed allegedly doubled as soon as he was out of sight of navigator Bartlett (April 1-2). *Fiction* noted [F159] a matching oddity: during the return southward, Peary's speed suddenly halved (reverting to normal) after he passed Bartlett camp again (April 9). Which suggests that the trip was genuine south of Bartlett camp, while north of it (April 2 to 9) all the reported mileages were roughly doubled [F158].

C8 Arctic Ocean ice slows travel both from roughness and detouring; for full sledges over an unbroken trail, 10 to 15 miles (net northing) per day is excellent speed. (Peary diary, 1906 April 4: "It takes more travelling to cover a given distance up here than anywhere else in the world.") But Peary's 1910 book *The North Pole* says that on 1909 April 1 he planned [PZ 269] "my program . . . . five marches of at least twenty-five miles each" from Bartlett camp to the Pole. (Peary's first detailed telegram inadvertently says instead "fifteen miles each".)[9] But the April 1 diary just hopes for 6 or 8 better-than-average marches while saying that for typical daily distances (15 miles, as in his first wire) it will take 9 marches to get to the Pole.[10] Nine is glaringly near double 5; notably, Peary read the key datum "Nine" as "Eight" at his hearings [SPH 301].[11]

D Fudge

D1 The Pole claim's 2 starkest fictions are aim and speed. We have now found that Peary misread or altered his diary in 2 key places, one related to aim, the other to speed. And when J.Eddie Weems' 1967 standard (family-approved) Peary biography published

[7] Ralph Plaisted and Wally Herbert, the genuine 1st attainers of the Pole by surface (1968 and 1969, respectively), report that each mile of net northing actually required 1 3/4 miles of weaving travel. Thus, their left and right angular deflections from the ideal northward path averaged roughly 1/2 a right angle [F136].

[8] [The original 1989/4/16 preprint attributed this point from memory to Carnegie Institute astronomer Harry Raymond. A brief subsequent search failed to uncover such a remark in the Raymond correspondence with Bowman; a memo of American Geographical Society expert Oliver Maitland Miller (BPJ 1935/8/12) is perhaps the source I recalled. Miller just notes that Peary did not explain his aiming procedures "when he had the opportunity of stating them at the Congressional investigation."]

[9] [NYT 9/11:1:5.] Relic-glimpse of a prior trial-version of events? The context suggests that 25 miles is meant; but it's odd that a verbal typo agrees with such a realistic expected mean distance, and with the diary's expectation of 9 normal marches: 9 times 15 miles is virtually the distance he says he was from the Pole on April 1.

[10] The diary [PM 0049]: "Nine marches same average as our last 8, or 8 equal to the 3 from [85.8 degrees] or 6 like yesterday's will do the trick." (At his hearings, Peary read this as [SPH 301] "Eight marches same average as our last 8, or 8 equal to the 3 . . . .") "Nine" is written in the diary as a word, not a digit; so there is no question of accidental misreading.

[11] "Eight" agrees with Bartlett's 1909/4/1 written certificate [PZ 360-361]: "At the same average as our last eight marches Commander Peary should reach the Pole in eight days."

the diary's text, it neatly omitted these same 2 revealing passages [see WP 265].[12] (Peary's 1910 book dropped the part in the diary about 9 marches like the last 8 but used the rest of the same sentence [PZ 270]. Weems [WP 264] gives this truncated version from Peary's book while on the opposite page [WP 265] Weems' quotation from the original April 1 diary entry ceases exactly 1 word before the word "Nine". For the final 5 marches, 1909 April 2 through 6, Weems quotes [WP 265-268] every single word from the original diary except the key 7 words "setting course by moon, our shadows, etc.")

D2 Peary unquestionably altered yet another key document: the very statement he claimed to have left in a bottle at the Pole. He retained a copy, the text of which appears in Peary's 1910 book [PZ 296]. It begins: "90 N. Lat., North Pole, April 6, 1909. Arrived here today, 27 marches from Cape Columbia." But Helgesen in 1916 found that Peary's own handwritten statement [C53A:1628, F284-285] (otherwise identical) read "28 marches from Cape Columbia."

D3 Putting these 1909 items together with the 1906 exaggerations already noted, we discern an impressive collection of provable late-career data-alterations[13] by Peary: [a] Crocker Land (1906-7), [b] 25 miles vs. 30 miles (1906; twice), [c] 7 deleted 1909 diary words on aiming, [d] "Nine" marches vs. "Eight", and [e] 28th march vs. 27th. Such a record is inconsistent with claims worthy of acceptance by scientific societies.

D4 Peary defended his controversial 1909 April 1 jettisoning of the powerful if overworked[14] Capt.Bartlett (age 34) by calling [the considerably older] Henson uniquely indispensable. [PZ 272, SPH 311, RD 7, F103-107.] Every defense of the Pole myth [e.g., WR 180] leans upon this essential foundation. But Peary's newly found 1906 April diary cruelly defaces it. April 2: Henson "not turning out as I expected." April 5: "Was not surprised at the end of six hours to come upon Henson humped up in camp . . . his [Eskimos] belly aching about being so far away [from land], and the hard travelling, etc. and he as bad as any of them, though of course he would not admit it. . . . fallen down badly on his job and if he does not do better very soon I shall make a change."[15] Diary, 1906 April 6: "the delays [some unlike 1909] and Henson's sluggishness have cut our advancement down to five miles per day." This is the same Henson (now 3 years further past physical prime at age 42) which Peary's 1909 fable alleges he must choose to have with him in order to make 25 to 50 or more unverified miles per day.[16] Diary, 1906 April 15: "came upon [Henson] camped beside a *closed* lead, where he had been for some 20 hours. He claimed that it had just closed, but that is a lie, and if I had not come up, he would be there still. The truth is, he is worse than the eskimos in being frightened to death with these leads." I question Peary's view of Henson's veracity. (And stamina-loss with age deserves sympathy not abuse.) But

[12] Herbert's 1988 *National Geographic* article publishes both [NGM 174.3:402], but without noting their previous multiple suppressions. [(PZ 276, SPH 302, WP 265; PZ 270, SPH 301, WP 265.) Note that the 7 diary words on rough aiming by shadows & Moon are replaced at PZ 276 by the precise-sounding report: "Our course was nearly, as the crow flies, due north, across floe after floe, pressure ridge after pressure ridge, headed straight for some hummock or pinnacle of ice which I had lined in with my compass." (How convenient that a distinctive pinnacle was always just due north — and remained recognizable even after huge zigzags en route to approaching it.) Note that Peary alleges at PY 131 that he was leading with the compass for 10 hours on 1906/4/14, though that day's diary entry says that he came upon trail-breaker Henson's igloo after 83% of the day's 9 hour march! Thus, we have two demonstrable instances where Peary has published statements that he was steering precisely by compass, though the diary says otherwise.]

[13] [Others include: time spent riding & time in the lead.]

[14] Henson claimed ([SPC] 1926/6/11) that Bartlett was worn out. (Peary had made Bartlett break most of the 1909 March trail.)

[15] This was to prove impossible since Peary had lost contact with all non-Eskimo members of the expedition but Henson. Peary was first certain he was isolated from navigator-witness verification on 1906 April 14. ([PY 130:] "It was evident that I could no longer count in the slightest degree upon my supporting parties, and that whatever was to be done now, must be done with the party, the equipment, the supplies which I had with me.") And it is on precisely this date that his diary's estimated marches suddenly became enormous (25 miles/day), exactly as later happened on 1909 April 2 — though at a very different latitude: 85°N in 1906 vs. 88°N in 1909.

[16] [See above citations. However, note Peary's PZ 240 remarks on the 1909 Henson — very like those of PY 124, which is a much-muted version of what appears in the corresponding 1906/4/5 diary entry just quoted above.]

if you plan — as Peary did in 1909 — to choose one companion as the prime sledgemaster and witness to a daring, swift polar miracle, then your claim is necessarily undercut when your own doubts of his drive and integrity surface.

D5 In 1916, Congressman Henry Helgesen's speeches [F247-248] doubting Peary's 1909 success anticipated numerous evidential points later rediscovered by others' researches, mine included. (Weems suggests [WR 200, WP 310, 346-347] that upright explorer Adolphus Greely of National Geographic may have given Helgesen much of his material.) E.g., Helgesen noted [C53A:282, 1636] that Peary in 1911 renounced[17] his most crucial 1909 sight (April 5), his only zeroing-in navigational datum. *Fiction* found [F150] that as early as 1913, this sunshot alone was missing from the 1909 records. It still is. (Some Peary supporters hope eventually to recover it. But this would not now save the unsalvageable Pole claim because Peary disowned the sight and such data are fakable anyway. Again, crucially, veering to aim at the Pole is never mentioned by Peary.)

D6 At his hearings, Peary defended his pole-in-one by saying [SPH 317-318] he'd accurately paced large distances on the smooth Greenland icecap [1892-1895]. But this is irrelevant to drifted and detoured sea-ice travel, and Peary rode on a sledge most of the 1909 trip anyway. [See below, §E7.] (His icecap distance-estimates were by odometer-wheel [PG 1:280 n.2]; but his 1910 book notes [PZ 211, F232] that such a wheel could not be used in 1909. Incidentally, riding affects not only distance-estimation but steering, since [a] one must be in the lead to steer, and [b] proper use of the magnetic compass required repeatedly removing it some distance from his sledges' ferrous metal [MH 185].)

D7 Thus, Peary's 1909 yarn in brief: he paced distance from a sitting position and steered north by compass without measuring its variation from north.

E Eyewitness

E1 In 1917, disbelieving ship's captain Thomas Hall noted [HH 66f, 143f], evidently 2nd hand, that an obscure written Henson account (*Boston American* 1910 July 17) reported that reaching the purported Pole from Bartlett camp in merely 5 marches was a "surprise" to Peary who himself had underestimated his superspeed until after arrival [HA 1]. (Recall that Peary's own April 1 diary entry expected more than 5 marches [above §C8].) *Fiction* recovered the original of Henson's article and found in it the lethal direct eyewitness testimony: Peary's face was "long and serious" [HA 1] after the April 7 sextant observations gave his position (likely about 350 miles from land, admirable but well short and right of the Pole). Without warning Henson, Peary had snuck out of the northernmost camp for just an hour [HA 1], not enough time for significant northing, in order to make his first post-Bartlett sunsights; the 2 Eskimos with him told Henson that Peary's face showed "disappointment" [HA 3] when he completed the observations.

E2 Henson saw this [HA, HE iv-v 1969 ed] as a Peary funk over sharing the Pole. He told Peary that they had both already gone far enough to be there [HA 2]. Was this: [a] navigational advice? [b] hope? or [c] expression of a prudent consensus for instantly heading back to Bartlett camp and home, before the ice was scattered by storm, tides, or spring, cutting off Bartlett's freshly-knitted southward trail (pre-broken and pre-iglooed)?

E3 From the moment of the "Pole" sextant sights, Peary for the rest of his life ceased conversing with Henson [HA 1-2, 4, HE v 1969 ed], his faithful companion of 22 years. No other still-accepted Pole attainment has such a peculiarity attached to it. And no other rests entirely upon the leader's unsupported word: though Henson could take sunshots [HA 2, F128], Peary shared none [*idem*]. (The Poles are the easiest places on Earth to fake sextant data for: simple arithmetic [RR 35, F154]. But data for aiming *toward* the Pole are not so easily faked.) *Fiction* revealed skeptic C.Henshaw Ward's 1935 discovery that in 1909 Peary had, before showing his "Pole" sextant data to his official judges, pre-checked them out for consistency, using a surveying expert he kept secretly at his home that Autumn [F285-289].

E4 Set Henson's testimony beside Peary's final navigational story (which only came out under 1911 crossexamination [SPH 316-317]): no precise sextant data [1] for aiming (left-right) during the whole 413 mile trip, or [2] for gauging forward progress over the last 135 miles, the 5 northward marches (April 2-6) after leaving Bartlett.

E5 *Fiction* induced the simple and nonconspiratorial solution [F149f] to these oddities: Peary 1st took 1909 aiming data on April 7, but they showed he was way too far from the Pole to reach it (roughly 100 miles away); so within hours he was wisely speeding southward.[18] Therefore, his eventual 1909 story had to put *at* the Pole the very camp where he 1st took data to aim himself *to* the Pole. Thus the origin of Peary's incredible 1909 pole-in-one navigational fantasy.

E6 Henson has been quoted as saying various things, some under Peary's dominance, others in dotage. For the historian, the premier Henson accounts must be his 2 independent written 1910 articles, in *Worlds Work* (April) [HW] and *Boston American* [HA], based on his now-lost 1909 diary.

E7 In these articles, Henson makes 3 crucial statements contradicting Peary's 1909-1910 reports. In each case, we find that Henson told the truth. [1] Henson [HA 1, 4]: no observations from Bartlett camp (1909 April 1-2) to the "Pole" camp, "Camp Jesup" (April 6-7). Peary at first reported an April 5 shot [PM 0061, NYT 9/11:1:7, PZ 284] but later dropped it [§D5]. [2] Henson: Peary, age 52, rode most of the time [HA1-2, SPC 1926/6/11]. (This is noted by Hall [HH 67, 116, 143-144], who also shows [HH 67-70] that, north of Bartlett camp, rougher ice and more transverse leads than Peary recalls were reported by Henson's first account, though not later ones [e.g., HA 4].) Peary: "there is no riding when you go hunting the pole" [NYT 9/20:2:4]; starting April 2, "whatever pace I set, [the others] would make good. If anyone was played out, I would stop for a short time." [NYT 9/11:1:4, PZ 271.] (And [PZ 274] "I took my proper place in the lead." In fact, Henson usually led. [HA 2. Note the similarity to Peary's revision of the 1906/4/14 situation: above, §B3 & fn 12.]) But the Eskimos testified [HP 366 n.15] that Peary rode on the sledges most of each day, customarily for hours at a stretch. (Peary at the 1911 hearings: "never over 5 minutes at a time" [SPH 303].) [3] Henson: the ice-drift near Camp Jesup was to the east (rightward as seen from Cape Columbia [HW 12837]). In a hitherto secret highlevel 1926 June 11 document, Henson describes in detail a systematic ice-drift to the east throughout the 1909 trip, revealed by shearing ice-breaks in the trail [SPC 1926/6/11]. (This alone sinks the Pole claim because of the effect on unchecked aim.) Yet Peary and his defenders say there was no east-west ice-drift in 1909 [PZ 307, WR 173]. (The nonfantasy 1906 diary worries about east drift, even — as on April 9 — when not visible as local eastward ice-shear, which it often was anyway.) But the Transpolar Drift Stream, 3-4 miles per day, in the direction Henson (and Helgesen [C53A:273, also 3-4 mi/day]) described, is now on National Geographic's excellent maps [e.g., NGM 170.3:297]. (And the brevity of Bartlett's April 1 sunshot hints that he was way east of [where it was noon].)

E8 When Henson's revealing accounts were published, Peary knew that openly challenging Henson to produce his diary would be suicide. Instead, Henson was privately damned to devalue his testimony. Isaiah Bowman ([BPJ] 1935 July 30): "Mrs.Peary says Matt was a 'snake in the grass' in that he would apparently say a complimentary thing and

[17] [Helgesen quotes Peary's SPH 317 statement that there were no observations taken [between] Bartlett Camp (4/1-2) & Camp Jesup (4/6-7). The hitherto-unnoted Peary statement of SPH 316 is equally important in certifying that the only observations were taken on 1909/3/22, 3/25, 4/1, 4/6, 4/7. Thus, as at the 1913 IGC presentation [F150]: no 1909/4/5 observation.]

[18] This in order to elude the death he had so nearly met in 1906, when warming weather set adrift the ice, almost stranding him and Henson permanently. The story of the Peary party's 1906 May southward escape from the central Arctic Ocean pack-ice (gingerly snowshoe-shuffling over 2 miles of weak, undulating rubber ice covering the ocean depths) is a must-read both as harrowing adventure and as entirely-sufficient explanation of Peary's understandable 1909 decision not to commit suicide by going all the way to the Pole. [See PY 145, F118-123.]

take it back in the next phrase and that he was vainglorious and boastful."[19] These are unworthy reflections upon a remarkably versatile, little-rewarded explorer, who gave much of his own life to the Arctic. And attacking the credibility of his only literate Camp Jesup witness hardly boosts Peary's case.

F Perspective

F1 Peary's pioneering contributions to geography and to exploring technique have not always been properly appreciated by his critics.[20] Some wrongly doubted Peary had gotten even half way to the Pole; explorers like Greely knew better: "That Peary entered regions adjacent to the Pole is unquestioned by any Arctic expert" [HP 416]; Greely added however that many (including Greely [C53A:1645, WR 176]) believed Peary did not go all the way. But most scholars stayed silent, while Greely and Ward had the courage to speak their well balanced skepticism.

F2 Peary sacrificed, suffered, and devoted his life to seeking undying fame. And he has won it, by his magnificence in exploration and prankery. Grand success at either takes skill and courage. He had both in epic proportions.

The source-abbreviations used above are listed at *Fiction* pp.308-313, with these additions:

BPJ Bowman Papers, Johns Hopkins University Library.
LF W. Henry Lewin *The Great North Pole Fraud* London 1935.
PC Peary 1906 records, US National Archives.
PM US National Archives' 1971 microfilm: Peary 1909 records (numbered by frame).
F Dennis Rawlins *Peary at the North Pole: Fact or Fiction*? Washington 1973.
RH David Roberts *Great Exploration Hoaxes* Sierra Club, San Francisco 1982.

The foregoing paper was written in 1989 Winter, mailed to several persons 1989/3/20, revised 4/16, and distributed (in the days immediately following) to numerous parties, including NGS Peary project supervisor Joe Judge (then Senior Associate Editor, later abruptly canned by NGS: early 1990). Other than extra bibliographic items (& section titles), material added since 4/16 is contained in brackets. (These additions completed 5/11. Several typo-corrections & brief clarifiers inserted 1990-1991.)

Notes added 1991:

[A] The foregoing is printed just as circulated the better part of a year before photogrammetry was publicly brought to bear on the Peary case. It will illustrate why DR has held that enough evidence already existed to justify scientists' nonacceptance of the Peary North Pole legend. (NGS' desperate resort to photos is embarrassingly akin to the UFO cult's tactics for defending claims which are equally dubious on their face.) The bottom line here is stark: DR's 1973 book pointed to 4 probable hoaxes by Peary (Jesup Land 1899, Crocker Land 1906, Farthest North 1906, North Pole 1909) and 2 genuine records (discovery of northernmost land on Earth 1900, W.Hemisphere Farthest North 1902). A decade later, when the Peary Papers were finally opened to the public, the continuous diary records exhibited blanks (at the moment of discovery) for all 4 DR-doubted claims, but contained full documentation for the 2 DR-accepted claims. Most scientists would regard such a 6-fold one-to-one correlation as something of a confirmation for the skeptical side. Not the wealthy & diehard publishing outfit run (for 5 generations) by the Hubbard-Bell-Grosvenor family under the ambitious title: the "National" Geographic Society.

[19] It is not pleasant bringing forth such material; but it is now part of the publicly-accessible record, so it cannot stay secret, regardless. Moreover, since Henson's testimony is an important member of the set of independent evidences against Peary's claim, I cannot suppress charges against his truthfulness simply because I disagree with them.

[20] Hall and Hayes were far too sympathetic to Cook's [1908] claim; Helgesen was initially part of the Cook lobby [F248]. Still, that is no reason to ignore their considerable rôle in establishing the truth of the 1909 imposition.

[B] In late 1989, NGS attempted resuscitating[21] Peary, issuing a pristine whitewash of all his exploration claims: an impressive-looking 1989/12/11 Report (NG) by NGS' hired consultant, the "Navigation Foundation" (NF). The Report: [a] Uses shaky 2-D photogrammetric analyses (NG 127f) to prove Peary was indeed at the Pole on 1909/4/6-7, allegedly by showing that the Sun was at the correct altitude above the ice-horizon in photos from that time. (These analyses' claimed precision has met with general skepticism in the scientific community. See, e.g., *Scientific American* 1990/3 & 1990/6.) [b] Straightfacedly explains (NG 166) that the reason Peary forgot to record his crucial 1906/6/24 discovery of Crocker Land (§B1-B2) in his diary of that date was: because he fell asleep in mid-diary-entry! (Evidence? He was tired the next day. That's proof enough for anyone.) [c] Suppresses (NG 85) the same key 7 diary words (revealing Peary's actual crude 1909/4/2 navigation) previously suppressed by Peary (§C6) and by official biographer Weems (§D1). . . .

[C] On the day NGS announced this Report, DR was quoted nationally as charging that it contained "more fiddle factors than the NY Philharmonic", pointing, e.g., to *NGM* 1990/1 p.45, where NGS had unwittingly reproduced key photo E5 with 2 successive (& seriously discrepant) NF-drawn ice-horizons visible! (On the same day, DR announced that the Report's author, "Navigation Foundation" President Adm.Tom Davies, had in 1984-1985 publicly defended, in elaborate pseudoscholarly detail, yet another dubious explorer, Amerigo Vespucci. Davies' Vespucci-apology math analyses were based upon grossly bungled astronomical calculations, as confirmed by several astronomers; the most famous of these astronomers, Chas.Kowal, was quoted in the *Wash Post* of 1989/12/12 as commenting that Davies' math was based on a mistake which a "freshman astronomy student wouldn't make". Davies has since demonstrated his integrity and ability to admit errors by refusing to discuss his Vespucci work with any inquiring reporter. The largest Davies error here, omission of lunar parallax, affected his deduced position by a trifling 2000 mi — placing Vespucci in Africa rather than S.America as claimed. See *J.Hysterical Astronomy* 1990 preprint: "Incontinental Drift". The purported precision of Davies' Vespucci analysis was what caused NGS to select Davies to head its investigation-renovation of Peary's N.Pole claim.) Subsequently, DR circulated detailed exposures of the NF Peary Report's amateurish & remarkably overneat[22] statistics and photogrammetry, as well as producing his own (3-D) photogrammetric analyses[23] of Peary's 1909/4/6-7 photos (American Astronomical Society presentation 1990/10/22), showing that the explorer turned back approximately 100 mi from the Pole (‡1 fn 14 item [e]), which at this point will probably surprise almost nobody outside the immediate Peary & Grosvenor families.

[21] NGS' closedminded re-turnabout adds credence to the theory (suggested by DR in *Science* 1989/3/3) that longtime stalwart NGS had (shockingly: §A2) published doubts of Peary (1988/9 *NGMag*) solely due to recent frightening rumors that documentary disproof of the Pole claim might have surfaced from the newly-opened Peary Papers.

[22] Noted at *Scientific American* 1990/6. In the NF photogrammetric work, DR finds repeated serious inconsistencies, affecting deduced solar altitude by amounts running as high as about 100 mi. The NF's relative azimuths are funnier yet, exhibiting errors of as much as 44° (100 standard deviations off!) and even 135°.

[23] If the azimuthal orientations of these photos are what Peary stated, then Camp Jesup was well west of the 70°W meridian, as W.Herbert has steadily maintained (contra DR).

‡5 The Scholarly Integrity of Book Reviews

by Robert Russell Newton[1]

A Introduction

A1 Appearing in the *Journal for the History of Astronomy*, a review of one of my books reads, after a few preliminary sentences: "The object of this book is to determine whether the rate of the rotation of the Earth is subject to long-period variation independent of the retardation produced by lunar tidal forces." I will cite and discuss both the book and the review later in this paper, but first I want to call attention to the quoted sentence.

A2 It has been known for a long time that the rate of rotation of the earth "is subject to a long-period variation independent of the retardation produced by lunar tidal forces." For that matter, the rotation of the earth is also subject to short-period variations independent of the lunar tides. The oddity in this context is that the book in question has nothing to do with the forces responsible for the variation in the earth's rotation. Instead, as its preface clearly states and its [very] title implies, the object of the book is to enquire whether the force of gravitation (including the modifications due to general relativity) is sufficient to account for the observed orbital motion of the earth and the other planets, or whether there may be other effects that affect the orbital motion at the present level of observational accuracy.

A3 If the reviewers can make such an outstanding error in even understanding the object of the book (and their review shows many other failures of understanding as well), it is clear that they are not competent to review the book. The question then arises: how can such an incompetent review appear in a scholarly journal? This paper will be concerned with documenting the incompetence of the review in question, and with suggesting a method of improving the quality and integrity of book reviews.

A4 I should point out one editorial matter in this introduction. I identify a reference by giving the name of the author or authors, followed by the year of publication, in square brackets. When necessary, I follow the year of publication by a specific point of reference, such as page number or a section number. For example, the book in question will be cited as [Newton, 1976]. If I need to refer specifically to page 532, for example, I add "p.532" after "1976" within the brackets. If the author's name occurs naturally in the text, it is not put within the brackets; otherwise it is.

B The Integrity of Research Papers

B1 Most scholarly journals have done a pretty good job of maintaining the scholarly value and integrity of the research papers that they publish. This applies both to journals published by professional societies and to those which are published independently of

[1] Note by DR: Robert R. Newton is the former Space Sciences Division Supervisor of The Johns Hopkins University's Applied Physics Laboratory. He is one of the world's foremost experts (and a prolific author) on the variation of the Earth's rotation-rate and on the uses of ancient astronomical observations in this and other research areas relating to secular time-measurement.

professional societies. The same cannot be said of the book reviews published in many scholarly journals.

B2 Let us look at the method of handling research papers. In the usual procedure, a paper submitted for publication is sent to the editor or to his office. From there it is sent to two referees who submit independent reports to the editor. The contents of the paper are kept confidential between the editor and the referees at least until the paper is accepted for publication, and all parties are in honor bound not to discuss even the existence of the paper with any other person until this happens. A few journals disclose the identity of the referees to the author but most do not.

B3 If both referees recommend that the paper be published, the paper is published as soon as possible, with due attention paid to earlier accepted papers that are still awaiting publication. If both referees reject the paper, the editor returns all copies of the paper to the author, with a note that it has been rejected. If one referee recommends publication while the other recommends rejection, practice varies, but we do not need to go into this complication.

B4 When an editor returns a rejected paper, he does so along with copies of the referees' (or referee's) reports that caused rejection. Several options are now open to the author, of which we have room to discuss only two. First, he may rewrite the paper to conform to the objections that have been raised, and resubmit it for publication. Second, he may write a letter of rebuttal to the referees, send the rebuttal, adverse referees' reports, and the original paper back to the editor, and request that all this material be sent to a third referee for another opinion.

B5 So far as I can discover, no journals, even those of the utmost probity, take any such safeguards with regard to the integrity of book reviews. Before I discuss this situation, I want to describe the book and the review in question.

C The Book

C1 The book being reviewed is *Ancient Planetary Observations and the Validity of Ephemeris Time* [Newton, 1976]. The term "Ephemeris Time" is well known to astronomers but is probably not known to the general reader. The need for ephemeris time arises from the variation of the earth's rotation, but the measurement of ephemeris time does not require measuring the earth's rotation.

C2 Suppose for the moment that the motion of the solar system is completely governed by gravitation, within the accuracy of present observation (about 1 part in 10^8). We can then establish a time scale based upon, say, the orbital motion of the earth (or the apparent motion of the sun). If the entire motion of the solar system is dominated by gravitation, the same time scale can be used to describe the motion of all the planets. This time scale is known as ephemeris time.

C3 However, it has been known for more than a century that gravitation is not sufficient to account for the orbital motion of the moon. It is believed that friction in the lunar tide is the only cause for the deviation from gravitational motion, at the present accuracy of measurement, but this is only a belief which there is no current way to test.

C4 If there are forces other than gravitation which affect the motion of the moon (at the level of about 1 part in 10^8), it is natural to ask if there are forces other than gravitation which affect the motion of the planets about the sun. There is a way to answer this question that is simple in principle but that is difficult to carry out because of the accuracy required. In order to describe this way, it is necessary to discuss the variations in the rotation of the earth.

C5 At the same time that friction in the lunar tide affects the orbital motion of the moon, it changes the rate of rotation of the earth. We do not know enough about the tide to calculate its effect upon the orbital acceleration of the moon or upon the acceleration in the earth's rotation, but we can calculate accurately the ratio of the two accelerations. (The ratio depends only upon such matters as the moment of inertia of the earth and the mass of the moon, which are known with reasonable accuracy.) The ratio of the two accelerations shows clearly that there are forces other than friction in the lunar tide which affect the rotation of the earth. On the whole, the length of the day is increasing and the number of days in the year is decreasing.

C6 However, until quite recent times, the rotation of the earth (the length of the day) was used in astronomy to furnish the standard of time. I shall use "solar time" to denote the time defined by the length of the day, because this kind of time was measured by observing the transits of the sun across the observer's meridian, or by some equivalent observations. Now if the number of days in the year is decreasing, and if the length of the day is taken as the standard of solar time, it is clear that the sun has an acceleration with respect to solar time. The planets must also have an acceleration with respect to solar time in their heliocentric motions. Further, if the concept of ephemeris time is valid, the acceleration of the planets with respect to solar time must be in the same ratio as their heliocentric mean motions.

C7 To see if there are forces other than gravitation which affect the motion of the planets (that is, to see if the concept of ephemeris time is valid), we simply calculate the heliocentric acceleration of each planet and see whether the accelerations are in the same ratio as the mean motions. In the book under review I do this using only surviving observations up to the year 1019. Unfortunately, the surviving observations do not let us find the planetary accelerations with enough accuracy to apply this simple test. However, I found another way to test the validity of ephemeris time, but one that does not allow us to infer the planetary accelerations.

C8 The investigation was based upon old observations; I actually used observations dating from -567 to $+1019$. During this period, the planets beyond Saturn were unknown. Further, the mean motions of Jupiter and Saturn are small, and the expected accelerations are also small, so I decided not to estimate their accelerations. As a result, I confined the immediate purpose of the book to studying the accelerations of Mercury, Venus, Earth, and Mars. I did so by using timed measurements of the position, such as the times of equinoxes and solstices, the rising and setting times of the planets, and the times of conjunctions of the planets with other celestial objects.

C9 Unfortunately it is not always a straightforward matter to extract the data from the ancient and medieval sources. To start with, we cannot always read the calendar used by the observers. The Babylonian months were determined by the time when the moon first became visible in the evening after passing the sun in longitude. Islamic writers used two different fundamental dates for the start of their calendar. Thus, even when Babylonian or Islamic dates are explicitly stated in their own calendars, we are not always able to translate the dates into unique dates in our calendar.

C10 Most of the sources have not come down directly to us but have come to us only through the medium of many copyists. Thus large errors which have probably been introduced by copyists, are frequently present in the sources. In some sources, c.1/5 of the observations contain scribal or copying errors so large that they are useless. In other cases, one astronomer used the work of another and forgot to record where the original observation was made. We cannot automatically assume that the observation was made where the writer worked, and sometimes it takes considerable research to establish the site of an observation.

C11 And, unfortunately, there are some outright forgeries, hoaxes, and fabrications. As an example, the Islamic astronomer Abu Sahl al-Kuhi claims to have made a thorough study of the solstices around the year 990. This study comes to us through al-Biruni [1025], and I have discussed it in some detail [Newton, 1976, pp.226ff]. al-Kuhi claims to have gotten exactly the same value for the obliquity of the ecliptic as Ptolemy [ca.150] although, as al-Biruni points out, all other contemporary measurements give a result that is about a quarter of a degree smaller. al-Biruni concludes that al-Kuhi's claimed study is a hoax, and I agree with him. There are many other fabrications in the ancient astronomical literature, particularly in the Greek.

C12 Thus it frequently takes considerable historical research in order to decide which ancient and medieval observations can be used in modern astronomical research.

D The First Part of the Review

D1 The review in question is by Hamilton and Swerdlow[2] [1981], which I shall denote by *HS* for brevity in the rest of this paper. I have already pointed out the enormous error that *HS* make in understanding the purpose of my book. Shortly after they make this error, *HS* write of my results: "The results found are not positive, nor are they negative, nor are they inconclusive; they are simply meaningless. . . . the standard deviations of the estimates are so large as to make all the numerical estimates, including those for the Sun, without value."

D2 This shows that *HS* fail to understand either the object of the book or its results; since they fail to understand the object, it is probably not surprising that they should fail to understand the results. It is simply not true that the numerical estimate for the acceleration of the sun, for example, has such a large error that it is without value. My final result for the acceleration of the sun is (page 534 of the book)

$$2.52 \pm 0.35 \qquad (1)$$

seconds of arc per century per century. This was probably the best estimate that had been made of the solar acceleration at the time of of the book, and it may still be the best estimate available.

D3 Shortly before the quotation just given, *HS* make the peculiar remark that "the accelerations of Mercury and Venus are, when compared to the solar acceleration, far too low, the acceleration of Mars is too high. . . ." This again shows a failure to understand the object of the book. If the accelerations of Mercury and Venus are too low compared with the solar acceleration, while the acceleration of Mars is too high, then the planets are subject to forces other than gravitation at a measurable level. As I said in the second paragraph of this paper [§A2], answering this question is the main object of the book. Results showing the quoted property would not be meaningless, as *HS* imply. Instead, they would be of the utmost importance for the theory of the solar system, and my results would be highly meaningful.

D4 As an aside, while I cannot be certain what *HS* had in mind, their writing about meaningful results suggests that they share a miscomprehension about statistical results that is common among scientists and many other people who have to deal with statistical results. To illustrate this miscomprehension, let us take an example from Table XV.1, page 532 of my book. There I find from Babylonian measurements of the times of conjunctions of Venus with other objects that the acceleration of Venus is

$$1.45 \pm 3.39 \qquad (2)$$

seconds of arc per century per century. Here, as in the acceleration of the sun just given [eq. 1], the first number is the "central value" or "best estimate", while the second number is the standard deviation. The combination of the two numbers means that, with a probability of about 2/3, the acceleration of Venus lies between $+4.84$ $(1.45 + 3.39)$ and -1.94 $(1.45 - 3.39)$. That is, the uncertainty[-range] in the acceleration of Venus is $2{\cdot}3.39$, or 6.78.

D5 Many people think that a result such as this is meaningless because the standard deviation exceeds the central value. This is not so. If there were no measurement of the acceleration of Venus, the uncertainty in the acceleration would be ∞. With the quoted measurement, the uncertainty has been reduced to about 6.78. Surely, reducing the uncertainty from ∞ to 6.78 is a meaningful accomplishment.

[2] Note by DR: No doubt aided by the sort of attack (upon dissent from Neugebauer-Muffia orthodoxy) here under dissection, Swerdlow (Hist.sci member UChicago astronomy dep't) has ascended to the board of the extremely handsome *Journal for the History of Astronomy*, the very journal in which *HS* appeared.

D6 It is true that the uncertainties in the accelerations found for Mercury, Venus, and Mars are too large to let us simply use the ratios of the planetary and solar accelerations in testing the validity of ephemeris time. This does not make the results meaningless in the sense used by *HS*; it merely makes them inconclusive (contrary to the statement made by *HS*). For example, the acceleration of Venus should be 1.6255 times the solar acceleration if ephemeris time is valid. Using the solar acceleration given above [eq. 1], then, the acceleration of Venus should be 4.10 ± 0.57, while the range found for Venus [eq. 2] lies between -1.94 and $+4.84$. The central value 4.10 does lie within the range found, but the range of uncertainty found is so large that the result is inconclusive. This is not the fault of the book, however. It is a consequence of the data available, and it is useful to know that the available data cannot give us a conclusive answer by simply using the ratios.

D7 As I have already mentioned, I found a way to test the concept of ephemeris time in spite of this difficulty. *HS* do not mention that I found a way to test the concept, perhaps because they do not understand that this testing was the purpose of the book.

D8 After their incorrect comment about the accelerations, *HS* go on to say: "But more must be said for the book also seems to be intended as a contribution to the history of astronomy in that the author evaluates ancient Babylonian and Greek, and medieval Arabic observations. . . ." (I think the reader can understand this sentence more easily if he will put a comma after "said". Without this comma, it may take several readings to understand the sentence.)

D9 Again *HS* show their failure to understand the nature and purposes of the book. As I have already explained, there are many historical problems in trying to extract the valid astronomical data from the ancient and medieval sources. There are problems in trying to tell which statements represent [outdoor] observation and which represent only [indoor] calculation. There are a number of innocent fabrications of data in the literature, as well as a number of outright hoaxes, which have been taken as serious observations in earlier astronomical literature; these must be detected and eliminated from the corpus of accepted observations. There are many scribal and copying errors in the existing forms of the old literature, in some cases amounting to about a fifth of the total observations. There may also be problems in determining the time and place of an observation.

D10 Thus of course I had to spend quite a bit of space in evaluating "ancient Babylonian and Greek, and medieval Arabic observations" in order to carry out the main purpose of the book which, to say it again, was to test the validity of ephemeris time. However, I did only the minimum amount of historical research necessary to find a body of valid data. If I could not establish the validity of a datum after a moderate amount of effort, I dropped the datum from my body of observations. I did not necessarily search the historical literature for the most recent historical analyses which might admit other valid observations; I merely tried to find a body of observations, of reasonable size, which I could accept as valid observations with reasonable confidence. I am sure there are many research works on the history of the old sources which I did not consult and which contain much information that is important to the historian. Such a failure does not impair the value of my book, which merely required a reasonable body of valid observations.

D11 Thus my book was not intended as a contribution to the history of astronomy and any criticicm of it as such a work is automatically invalid. To be sure, it is valid to criticize the historical research that I had to do, but such criticism must be within the framework of the book and its purposes, and not upon the basis of a professional historian being criticized by other professional historians.

D12 Of course I have tried to be accurate in every historical statement that I have made, but I have not tried to give a complete scholarly discussion of every historical subject that has come up. I have only tried to give evidence for the acceptance of valid data. When I have rejected data, I may or may not have explicit reasons for doing so. I do not need such explicit reasons for rejecting data; for my purposes I should reject or omit data if I cannot find explicit reasons for accepting them.

D13 *HS* then really start to play hardball; up to now their comments have been mild compared with what is to follow: "Newton's work is careless and unreliable to the point that it can be recommended, if at all, only to the reader who is prepared to examine every source of observations and check every computation, *i.e.*, to do all the work over again, and this is far from easy as Newton's exposition is often far from clear. In order to defend this admittedly harsh judgement, we can do no better than to give examples of Newton's understanding and use of sources in making the crucial decisions about whether a report represents an observation or a computation."

D14 *HS* then give two examples of places where I have presumably failed "in making the crucial decisions about whether a report represents an observation or a computation". Both their examples are interesting. They concern matters of controversy, but *HS* do not mention this point. Instead, they choose one side without mentioning the other side, and then show my alleged lack of understanding by demonstrating my differences from their viewpoint, which they present as established fact. [See fn 15.]

D15 I shall now take up their examples in order.

E A Babylonian Text Dated to the Year −424

E1 Kugler [1914, pp.233-242] gives a thorough analysis of a Babylonian text which he describes as "a text allegedly originating from the middle of the second millenium B.C." The text is (or at least was in 1914) in the museum of the University of Pennsylvania, and is identified as CBS 11901. The text applies to the Babylonian months IV through IX of some year that is not identified. For these months, it gives the number of days in each month, the dates of the full moons, of the first and-or last visibilities of the planets and the star Sirius, of the summer solstice and of the autumnal equinox, and of one solar and one lunar eclipse. It also says that the lunar eclipse began 40 minutes after sunset.

E2 Before the work of Kugler, CBS 11901 had been dated at about −1500, for reasons that I have not studied. Kugler noted, however, that, because of the precession of the equinoxes, the date of the first or last visibility of a star (in this case, a first visibility of Sirius) moves steadily throughout the solar year. In the text CBS 11901, the summer solstice comes on day 1 of the month called Duzu and the first visibility of Sirius comes on day 20 of the same month.[3] After paying due attention to the rounding of the data, and to the uncertainty about the date of a first rising of a star, Kugler concludes that the stated interval between the solstice and the first visibility of Sirius could only have happened between the years −800 and −400.

E3 By comparing the dates of the autumnal equinox, the lunar eclipse, and the solar eclipse, which all fall within the Babylonian month called Tisri, and aided by the fact that the lunar eclipse started 40 minutes after sunset, Kugler finds that the only Babylonian year between −800 and −400 which fits is the one which began in the spring of the year that we call −424. From this, it follows that the lunar eclipse is the total eclipse of −424 October 9 and the solar eclipse is the penumbral eclipse of −424 October 23; both dates are in the Julian calendar, with the day beginning at midnight.

E4 However, there is a difficulty. Because we do not know the accelerations of the sun and moon well enough, we cannot calculate accurately the times of the individual eclipses. Luckily, as Kugler himself pointed out, we do not need the individual times here; we need only the time interval between two eclipses that were only two weeks apart. If we use any plausible set of accelerations that makes the beginning of the lunar eclipse visible in Babylon, we find that the solar eclipse was not visible there. In a test of the situation (page 129 of the book), I chose acceleratons which made the lunar eclipse begin at sunset

[3] There is a difficulty here that is almost surely of typographical origin. In his translation of the text, Kugler puts the rising on day 21 of the month. In his main writing, and in his calculations, Kugler puts the rising on day 20. I think the day 21 that occurs in the translation is probably a typographical error, but someone with access to the text should check the matter.

rather than 40 minutes later; this is the extreme assumption we can make if the beginning of the eclipse was visible at all. Under this assumption, the solar eclipse was not visible at Athens or any point east of there.

E5 Thus, as Kugler concludes, it is not possible for both eclipses to have been visible in Babylon, and thus the text is not a record of observations. It is instead a record of calculations or predictions. It is wishful thinking to claim that the lunar eclipse was observed while the solar eclipse was merely calculated.

E6 There are other reasons for concluding that the text CBS 11901 is a record of calculations and not of observations. One reason is given by Neugebauer [1948], who makes a special study of equinox and solstice times in Babylonian astronomical texts. His conclusion is: ". . . *no* solstitial or equinoctial date which is found in (Babylonian astronomical) texts can be evaluated as an observation . . ." [Note by DR: CBS 11901 contains such data: §E1.]

E7 Another reason is given by Sachs [1948], who classified all the Babylonian astronomical texts known when he wrote. He finds only two classes of text that contain observations; all others contain only calculations. One class that contains observations is called a "goal-year" text; it concerns mainly the planets, and CBS 11901 is clearly not of this class. The other is called a "diary". A diary gives a variety of astronomical information, usually for a period of six months, and it devotes a separate section to each month. So far, CBS 11901 sounds like a diary, but it is not. In addition to the kind of information found in CBS 11901, the diaries typically give conjunctions of the moon and planets with major stars near the ecliptic, and matters which we would consider non-astronomical such as the weather, the height of the river, the prices of various agricultural products, and occasionally some important political events.

E8 We should note two other kinds of information that are not present in CBS 11901. There is no remark that the solar eclipse did not occur, and we are not told on what part of the moon the darkness first occurred. When we have to deal with observations of eclipses that were planned with the aid of prediction, we frequently find one or both of these remarks, depending upon the circumstances.

E9 Thus, CBS 11901 does not read like a diary that contains observations. Instead, it reads like the class of text that Sachs calls an almanac, which contains only calculation or prediction.

E10 However, this conclusion has been a matter for controversy. The earliest dissent from Kugler's conclusion that I have read personally is by de Sitter [1927], although he cites an earlier dissent by Carl Schoch that I have not read.[4] van der Waerden [1974, p.102] says that "the Mars and Mercury dates coincide much better with modern tables than is otherwise normal in the case of Babylonian calculations". He also says that "The lunar eclipse too coincides with modern calculation to within a few minutes". Thus he also dissents from Kugler's conclusion.

E11 A few sentences before, van der Waerden writes that Kugler "believed he could conclude that all the dates were calculated, because there is a complete absence of meteorological observations and because the text shows an eclipse not visible in Babylon without the comment, customary in the observation texts, 'It was missing'." I cannot find any place in the cited text where Kugler mentions either of these matters, but perhaps I overlooked it.

E12 Instead, as I read him, Kugler based his conclusion entirely upon astronomical calculations and upon such paleographic matters as vocabulary. I am not competent to judge the paleographic matters, but I do feel competent to judge the astronomical calculations and the points that I quoted from van der Waerden in the preceding paragraph.

E13 Actually, I do not see any way to settle the controversy. We can prove that only one of the two eclipses in CBS 11901 could have been visible in Babylon; let us say for the sake of argument that it was the lunar eclipse that was visible. Even so, we cannot say that

[4] de Sitter cites this as Berlin, 1926. [It later appeared in *Astronomische Abhandlung 8.2*, Kiel, 1930 (*Die säkulare Acceleration des Mondes und der Sonne*).]

the Babylonians observed it; perhaps it was cloudy that night. Nonetheless, we must always admit the possibility that it was observed. This is a different matter from admitting the alleged observation to our body of accepted and accurate astronomical observations. We accept observations with high weight only if we have strong reason to believe that they are genuine, but here we have strong reason to believe that the "observations" in CBS 11901 are actually calculations.

E14 On page 128 of my book, I wrote that ". . . I do not see any reason to assume that the text contains an observation of a lunar eclipse at all." On page 129, I wrote: "The reader may assume, if he wishes, that the lunar eclipse was observed. If he does so, he must recognize that this is a matter of opinion with no supporting evidence, and hence he is not entitled to give high weight to his assumption."

E15 I do not see any reason to change these statements. I do not believe that CBS 11901 contains any observations at all, and I certainly feel that one cannot use the data from it in astronomical calculations.

E16 I cannot attach much weight to the arguments by van der Waerden that were quoted a moment ago. He says that the Mars and Mercury dates "coincide much better with modern tables than is otherwise normal in the case of Babylonian calculations," implying that the dates are observed ones. He does not say anything about the other planets. Furthermore, the planetary dates in question are those of first or last visibility of a planet before or after its conjunction with the sun. These dates are uncertain matters to observe.

E17 Finally, he says: "The lunar eclipse too coincides with modern calculation to within a few minutes." He does not say how the modern calculations were made, and in particular he does not give the accelerations of the sun and moon that were used. (These accelerations are not important in comparing the two eclipse times with each other, but they are important in calculating the individual times.) The uncertainty in calculating the elongation of the moon at any time after the year -700 is, on a standard deviation basis, about $1''T^2$, in which T is time measured in centuries from 1800 [Newton, 1979, p.464]. For the eclipse of -424 October 9, T is about -22.2 [centuries], and the uncertainty in the elongation for a given instant is about $493''$, or about $0^\circ.137$. It takes about 16 [time]minutes for the elongation to change this much, so this is the uncertainty in a modern calculation of the eclipse time. I do not know if this comes within van der Waerden's meaning of a few minutes or not. Even if it does, this does not prove that the eclipse was observed; it merely means that the calculation of it was accurate.

E18 After mentioning Kugler's conclusion (that the material in CBS 11901 was calculated rather than observed), *HS* go on to write: ". . . while Newton agrees in this judgement, his analysis shows little understanding of what Kugler wrote and contains a rather strange result. Kugler dated the text to -424 . . . . but Newton says he questions the dating, although why is not made clear."

E19 This is a shot with no supporting evidence. I certainly did not fail to understand what Kugler wrote, and *HS* present no evidence that I did. I can see only two possible bases for the claim that I did not understand Kugler's work. One is that, because I could use a modern large-scale digital computer, I could perform certain calculations more accurately than Kugler could take the time to do; this is a refinement, not a misunderstanding, of Kugler. The other is that I questioned Kugler's dating "although why is not made clear."

E20 My reason is made quite clear and explicit on page 130 of my book. There I write: "Now it is likely that the errors in the Babylonian ephemerides, the ones upon which the information (in CBS 11901) is based, are greater than Kugler thought. It is certain that the errors in the modern ephemerides that he used are greater than he thought.[5] I have not

[5] Every attempt that I have seen to date a text by using modern theory has greatly exaggerated the accuracy of the modern theory. The most extreme example I have seen is by Schoch [1928], and his exaggeration was accepted by Langdon and Fotheringham [1928]. Schoch claims that his calculations of the times of the new moon are accurate to 3 [time]minutes for times back to -2000! I showed a minute ago [§E17] that the uncertainty in calculating a full moon (lunar eclipse) was already about 16 [time]minutes for the year -424, and I showed on page 38 of my book

attempted to discover whether the errors are enough to bring the year into question."

E21 In other words, if there were sizeable errors in the basic document (CBS 11901) or in the modern calculations, there may be years other than −424 which fit the stated conditions. Again, *HS* have failed to understand the situation.

E22 I will take up my calculations relating the eclipses of −424 in a moment [§E29], but first I want to take up another related point. *HS* disagree vigorously with my calculations about the eclipses, and they then go on to say: "And this example is not an isolated[6] aberration. In the course of spot checking we have noted instances of incorrectly computed sunrises, confusion of tropical and sidereal year, and other suspicious syzygies. We need hardly point out that in research of this kind, in which the goal is to isolate very small cumulative errors in modern theory, precision in computation is crucial if the work is to have any meaning at all."[7] Here again *HS* give no examples of my alleged deficiencies.

E23 It is probably unnecessary to say that I understand[8] the difference between the tropical and sidereal years, and I have done so at least since my freshman year in college. It is always possible, of course, that I inadvertently used one word somewhere when I meant the other.

E24 The remark "precision in computation is crucial" again shows the failure of *HS* to understand the situation. The precision needed in computation depends upon the use to which the result will be put. As it happens, in the book under discussion, I did not need high precision in either the times of sunrise or of syzygies.

E25 Take the matter of sunrise (and sunset). Many of the older observations, particularly the Babylonian ones, give the time by means of a time interval from either sunrise or sunset. Therefore we need the time of sunrise or sunset in order to convert the recorded time into some kind of astronomical time. Now the difference between ephemeris time and solar time

that the uncertainty for −2000 is at least 3 hours, not 3 minutes.

[6] Note by DR: The implication, that an occasional alleged error is typical of numerous other unstated ones, is standard for a Capt.Captious Swerdlow attack. See also his equally competent (fn 20) diatribe against R.Newton in Phi Beta Kappa's *American Scholar 48*:523, 1979. (Also discussed at ‡6 fn 6. One notes that O Gingerich was on the *Amer Schol* board at the time. The private details of this review's production are even more repellant than what was printed.) Who is naïve enough to believe that, had *HS* found even 5 instances, they would not have laid out every one in gleeful detail? (Co-reviewer N.Hamilton, U.Ill at Chicago mathematician, has told DR in so many words that he derived pleasure from attacking Newton in *HS*.) A lengthy itemized list of author-fluffs in a Muffia review is not without precedent, as witness Gerald J.Toomer's review of O.Pedersen's 1974 *Survey* (*Archiv Internat Hist Sci 27*:137-150; 1977/6), which features exactly 100 errors. (The review immediately preceding *HS*'s review in the 1981/2 *JHA* lists roughly 50 errors.) Curious contrast: Toomer nonetheless calls Pedersen's *Survey* "useful" and "warmly recommended" (opinions DR concurs in); so how can merely 2 questionably-relevant alleged errors *in a 749 page book* (by R.Newton) be held by *HS* to destroy the credibility not only of the book under review but of the entire historical corpus of the author? Two mistakes in 749pp? Heck, *HS* achieve more than that in 4pp.

[7] Note by DR: Attacking others' alleged slips is particularly ironic coming from Swerdlow, whose 1968 Yale U Hist.sci thesis is infected, at its vital part (p.82), by math which is bungled with Ptolemaic neatness & republished (unchecked) by *Centaurus 14*:287-305 (1969): in eq.1 (p.298), Swerdlow needs 67;20 sin($360°/1300 = 16'36''55'''$) to be 0;19,30, though it's really 0;19,32. No problem: [a] Capt.Captious miskeys the argument as $16'36''.55$ & so multiplies 0;0,17,23,34,50 times 67;20, yielding 0;19,31,7,45,26,40, which he then [b] mistypes as 0;19,30,7,45,26,40 = 0;19,30. Cute. The Hipparchos distance-ratios thus found by Swerdlow (UChicago) are highlighted in RiceU Hist.sci archon A.Van Helden's *Measuring the Universe* (UChicago! 1985, pp.11-13), whose p.168 calls Swerdlow's thesis "the definitive work" on ancient distance-schemes, though Swerdlow's main new result requires that Hipparchos believed: [a] half-Moons occur c.0.1 radians from quadrature, & [b] the Sun's diameter is merely twice the Earth's, seriously inconsistent with what we know (from Cleomedes & Theon of Smyrna) of Hipparchos' estimates of the Sun-Earth ratio. See, e.g., p.140 of G.Toomer's plausible attempts at a compromise solution: *Arch Hist Exact Sci 14*:127 (1974). This entire area of research is murky. Some of the confusion can perhaps be alleviated by speculating that Hipparchos' values of 62 & 67 1/3 Earth radii might have been his figures for the Moon's mean & greatest distances, not least & mean distances (as Pappos had it). Using 62 Earth radii in the basic equations of Toomer pp.130-131 produces a solar distance of about $1/(1/59 - 1/62)$ = 1200-to-1300 Earth radii, near the value of *Alm* 5.15.

[8] Note by DR: The only serious confusion of this sort known to me is O.Neugebauer's amazing and fateful mislabelling of the Babylonian tropical year as a sidereal year (*HAMA* p.528 eq.2). (This yearlength value has now been directly connected to the solstices of Meton and Hipparchos. See ‡6 in this *DIO*. It is thus unquestionably a tropical year. An elementary point, requiring no induction, which had obvious implications even before the mystery was solved: the Babylonian yearlength in question is roughly 3 times closer to the tropical yearlength than to the sidereal yearlength.) [Original printing wrongly had "5 times". *DIO* thanks John Britton for the correction.]

was several hours when the Babylonian observations were made, and we will be lucky if we find the difference with an accuracy of ten percent. Thus we can tolerate a precision of, say, 30 [time]minutes in calculating sunrise or sunset, particularly if our errors are periodic with the time of year, so that they tend to average out.

E26 In spite of the low precision required, I adopted a simple method of calculating sunrise or sunset, whose error is periodic and which has, I believe, a maximum error of perhaps 3 or 4 [time]minutes. This exceeds the precision needed by roughly an order of magnitude. The method is described on page 342 of my book.[9]

E27 With regard to the syzygies, I could find only five syzygies that I used in the book other than those in −424 October, all being connected with lunar eclipses. I used these eclipses only for dating purposes, that is, for finding the relation between a particular Babylonian month and our calendar. For this purpose, a precision of half an hour is surely adequate, except in critical cases. (As it happens, none of the cases was critical.) Because of the low precision required, I did not find these syzygies from my highly accurate lunar eclipse [computer] program. Instead, I listed the positions of the sun and moon for times around the needed times, and found the syzygies by a simple hand calculation. I did not record the precision I kept in the results, but it was certainly greater than that required. When *HS* claim errors in my times of syzygy, they should state the size of the errors found and compare them with the required precision.

E28 It is worth spending a word about how I calculated astronomical positions when they needed to be calculated precisely, and how I tested the precision of the [computer] programs. I will not take the space to describe the programs here, but they are described in Chapter IX of my book. I will give the results of one test, which is typical. The Naval Observatory calculated a number of positions of the sun with dates ranging from −1062 to +590. When I compared my results with theirs, as well as with present-day results from the *American Ephemeris and Nautical Almanac* (annual), I found no discrepancy as large as 1 second of arc [$1''$]. *HS* of course make no mention of my precise programs which are used when precise results are needed.

E29 Now let us turn to my calculations regarding the eclipses in −424 October, and start by reviewing what Kugler did. He first narrowed the year to the range −800 to −400 by using the first visibility of Sirius, and found the approximate days of the year for the eclipses by the interval between their dates and the autumnal equinox. He then searched through a canon of eclipses looking for a pair of eclipses that would meet the conditions just found, plus the condition that the lunar eclipse should start about 40 minutes after sunset. After finding a unique pair in −424 October, he calculated the local circumstances at Babylon, using an odd mixture of numbers [Kugler, 1914, p.237] taken from canons of eclipses by Oppolzer [1887] and by Ginzel [1899]. For example, he took the middle of the lunar eclipse from Ginzel, even though he had found the date of the eclipse by using the time from Oppolzer, and he then took the half-duration from Oppolzer. He also took the middle of the solar eclipse from Oppolzer.

E30 Under these circumstances, I wrote that Kugler took the time of both eclipses from Oppolzer, and that he made a mistake in copying the time of the lunar eclipse. I am sorry that I made this error, but it is certainly an easy error to make, and one that has no effect on my conclusions. In fact, it is so easy to make that *HS* make an exactly similar error[10] in

[9] It is always possible that there is a typographical error in the results of a computation listed in the book, but this does not imply an error in my final result. I first combined all the results from computer calculations on work sheets, triple-checked them, and then had to prepare the book. This required two copyings and proof-readings, and errors could have crept in here even though the results used were accurate. I will take this matter for granted in the rest of this paper.

[10] Note by DR: In fact, [Ginzel 1899] does not even provide a time for the −424/10/23 solar eclipse! (Ginzel's *Handbuch der Mathematischen & Technischen Chronologie* vol.1, Leipzig 1906, p.552, provides a rough value, 20:29, but this is not identical to the 20:31 Oppolzer figure adopted by Kugler. The same book's p.537 gives 20:11 for the −424/10/9 lunar eclipse, altering the [Ginzel, 1899] value by -3^{m}. Kugler, *HS*, & Newton all use the [Ginzel, 1899, p.137] lunar eclipse time. There is no question of Kugler's source for 20:31; he explicitly states [1914, p.237]

saying that Kugler took the times of both eclipses from Ginzel.

E31 I also wrote that there are errors in Oppolzer's times, because of the approximations that he had to make in the solar and lunar theories in order to carry out his enormous body of calculations in the days before large-scale computers. Before I take up this point, let me take up the opinion of *HS* about accepting the lunar eclipse as an observation. They do not state their opinion explicitly, but I think it is suggested by the following passage: "It is clear that the solar eclipse was not visible in Babylon, but the lunar eclipse, which was total, certainly was, and that the text gives no time for the solar eclipse but a specific time for the lunar eclipse suggests some difference in their reports. But Newton writes that the text offers no grounds for a distinction." From this, I conclude that *HS* think that the reference to the lunar eclipse constitutes an observation.

E32 Whether this is so or not, the remark of *HS* needs comment. The presence of the time in the lunar report but not in the solar report does not constitute a distinction between calculation and observation. The Babylonians at the approximate time could reliably predict the occurrence and the time [of day] of a lunar eclipse, but they could not reliably predict the time and place of a solar eclipse. Thus CBS 11901 contains only information that could be calculated, and it contains no information that indicates observation. It does not even remark that the eclipse was total.

E33 Now let us turn to the errors in Oppolzer's (and Ginzel's) times. As *HS* state, the time of the lunar eclipse's beginning, in Babylonian [mean] time, is 18:25 according to Ginzel and 18:45 according to Oppolzer. This is a discrepancy of 20 minutes, which illustrates the errors in Oppolzer's (or Ginzel's) times arising from the approximations they had to make. I also wrote (p.129 of the book): "According to Oppolzer, syzygy for the solar eclipse occurred 3 minutes earlier, mean time, than did syzygy for the lunar eclipse. According to my calculations, it should be 55 minutes earlier."

E34 *HS* write about this point that this difference dwarfs "the differences of the sources we have compared and of our own computation. This is an extraordinary result, and if it is true, Newton knows something about calculating syzygies that no one else knows." I no longer have the computing programs I used and cannot check the matter myself. However, Dennis Rawlins of Loyola College, Baltimore, [has written] the necessary programs, and he has kindly checked the matter. [For the book's lunar and Earth-spin accelerations], he finds[11] that the solar eclipse [invisible at Babylon] was about 56 minutes later rather than about 55 minutes earlier. Apparently I committed the equivalent of a sign blunder when I reported the time difference.

E35 *HS* refer to my figure of 55 minutes as an aberration. Even if it were, this would not have anything to do with the main point that *HS* claim to have made. The figure of 55 minutes was not used *in any way* in my decision [see §E6-E9] not to use the record of the lunar eclipse as an observation. It was used only to illustrate the need to repeat Kugler's calculations, using a highly precise program carried out on a modern digital computer.

E36 I suspect that the difference between *HS* and Rawlins-Newton comes from the difference between the middle of a solar eclipse as seen at Babylon and [as seen at] the center of the earth. Rawlins and I have used the difference as seen at Babylon, since that is the time that governs the visibility of a solar eclipse at Babylon. The times at the center of the earth are irrelevant.[12]

E37 In my calculations, which are described above, I found that the solar eclipse was not visible at any point east of Athens if the beginning of the lunar eclipse was visible at Babylon. This calculation, and not the figure of 55 minutes emphasized by *HS*, was the basis of my decision not to use the time of the lunar eclipse. This point is made quite clear in the book. As we may expect by this time, *HS* ignored this central result and focussed their attention on a side issue.

F The Parapegma of Geminus

F1 Now let us turn to the other example which *HS* give of my lack of understanding[13] "in making the crucial decisions about whether a report represents an observation or a computation." This example concerns the document called the parapegma of Geminus [ca. −100]. A parapegma is a document that gives the lengths of time the sun spends in each sign of the zodiac, the times of the heliacal risings and settings of various stars, and the weather conditions. All these are presumed to repeat at the same time each year. The

that it is Oppolzer.) In the midst of the same frenzy of accusations against another scholar's purported unreliability, *HS* also err in charging that Newton gives no absolute time. In fact, [Newton, 1976, p.130] says that Kugler's −424/10/23 solar eclipse time is 72^{m} nearer sunset than he thought. The Kugler and Newton sunset times should be virtually identical, so, since Kugler follows Oppolzer in using 20:31, this tells us that Newton's time for the 10/23 solar eclipse syzygy was 19:19. Then, subtracting -55^{m} fixes Newton's adopted time of the 10/9 lunar eclipse as 20:14, identical to Kugler's figure (from Ginzel). This precise agreement, as well as the correctness of the sign and the proximity of 19:19 to 19:14 (the DR-computed geocentric solar eclipse time: see following footnote), suggests an alternate explanation for the 55^{m} discrepancy of §E33, namely: in rough preliminary scratch-work (CBS 11901 was ejected from RRN's sample *very* early on), RRN accidentally compared his own 19:19 solar eclipse syzygy time to the Ginzel-Kugler time (20:14) for the lunar eclipse. Regardless, it is revealing that *HS* had to resort to such patently peripheral RRN figures (not even used in computing his book's results) as a basis for denigrating his hated conclusion on Ptolemy.

[11] Note by DR: Using RRN's value ET−UT = 5 hours for that epoch, and the standard *AENA* lunar acceleration ($-22''.44$) adopted by the book [Newton, 1976, p.315], I calculate Babylon mean solar time of conjunction: lunar eclipse 19:11; solar eclipse 20:07 topocentric (19:14 geocentric), a time difference of 56^{m}. By contrast, DR's adopted earth-spin acceleration (fractional $f = -19\text{x}10\text{e}{-09}/\text{cy}$) yields ET−UT = 4 hours; and, using this with the $-25''.1$ lunar acceleration of Dickey & Williams (*EOS 63*:301; 1982), I calculate: lunar eclipse 20:32; solar eclipse 21:13 topocentric (20:35 geocentric), a time difference of 41^{m}.

[12] Note by DR: Newton is too merciful here. The times at the center of the earth are relevant to the question of the caution & expertise of the loftily sarcastic charges by *HS* (§E34) that the 55^{m} gap proves that "Newton knows something about calculating syzygies that no one else knows." What Newton knows is simply: one must of course include lunar parallax (accounting for the difference between the earth's center and the observer's location: 6400 km!) in a precise solar eclipse calculation for a specific place (such as Babylon) — a procedure which is familiar to every positional astronomer in history, even that bumbling old faker C.Ptolemy. Incidentally, remark here and elsewhere the difference in RRN's & *HS*'s attitudes regarding error-apprehension: RRN admires Kugler (and excuses his errors), merely hoping to improve his accuracy. By contrast, *HS* approach RRN as fundamentalists approach Darwin: the slightest perceived slip is leapt upon, with tyrannosaurian gentility, as happy proof that a hated general theory is entirely false and abhorrent.

[13] Note by DR: *HS*'s 2nd attack boomerangs. RRN's Geminos seasonal values are used to get a rough figure for very ancient Greek accuracy; so *HS* must denigrate these because it is *HS*'s job to portray ancient accuracy as terrible — *in order to make Ptolemy's errors seem not so ghastly as Newton has shown them to be*. Yet *HS* lack the minimal integrity to note the ironic fact that their own argument actually *increases* our estimate of early ancient Greek accuracy. For, when attacking Newton's acceptance of Geminos, they prefer the Eudoxos Papyrus — where the season-lengths of Kallippos (330 BC) are *remarkably correct*. Why do honest-scholarship-wardens *HS* never note the wonderful accuracy of Kallippos' work? (Neugebauer remarks it in his *HAMA* p.627: "These values agree so well with the facts that their origin from observations can hardly be in doubt." The Kallippos seasonlengths referred to are given below in fn 15. See the excellent analysis of Kallippos' data by van der Waerden in *Archive Hist Exact Sci 29.2*:115f.) Since Ptolemy worked nearly 500^{y} after Kallippos, I hardly see that *HS*'s carping undoes RRN's basic point (regarding prePtolemy Greek solar data accuracy) or supports the cutely sardonic remarks at their p.62 wherein *HS* try to present Euktemon's errors as a triumph for Ptolemism. (Keep in mind *HS*'s defense-lawyer-ploy: if Euktemon was inaccurate, then Ptolemy's huge errors look less inexcusable.) To the amusing contrary: all that *HS*'s attack on RRN has accomplished is to replace Euktemon with Kallippos and thus move (§F19) accurate early Greek observational astronomy merely 100^{y} later (less than 20% of the Euktemon-Ptolemy interval) *while making it far more accurate*. Brilliant *HS* strategy. (For solar data: Ptolemy's rms error is about 32 hrs, so RRN's estimate of Euktemon's rms error is only about 4 times better — but Kallippos' 5 hr rms error is 6 times better.) *HS*'s fantasy gets so out of control — and so oblivious to Kallippos' accurate work — that the review's concluding paragraph states (while snidely asserting the truth to be so obvious that the reviewers are almost too bored to comment): "There is no evidence for [Newton's alleged] 'vast body of accurate Hellenistic observations' . . . . except for Hipparchos *and Ptolemy himself*, there was little concern for observation and less for accuracy." (Emphasis added. With a smile.) Even Ptolemy-adulator O Gingerich asserts (*JHA 21.4*:364-365; 1990/11) that, since ". . . Ptolemy's parameters . . . seem generally more accurate than his data base", then "there is an invisible data base behind" his work. (So *HS*'s insults apply to OG.) Regarding the quality & integrity of genuine Hellenistic scientists' admirable accuracy (vs. that of Eratosthenes & Ptolemy), see D.Rawlins papers at: *Isis 73*:259 (1982), *Vistas in Astronomy 28*:255 (1985), and *American Journal of Physics 55*:235 (1987). Accurate observers ignored by *HS* include Timocharis (300 BC) & Aristyllos (260 BC).

parapegma is divided into twelve parts, which correspond to the times that the sun spends in each [zodicial] sign. Day 1 in the parapegma is the first day the sun spends in Cancer, which is also the day of the summer solstice.

F2 As a minor but illustrative point, *HS* date Geminus as "first century A.D." without qualification or justification. On the other hand, the article on Geminus in the *Dictionary of Scientific Biography* [Dicks, 1972] says that he flourished about 70 B.C., while *Pauly-Wissowa* [1894] gives his dates only as lying between −100 and +200. *HS* say that I date Geminus to ca.−100, although on one of the very pages they cite [Newton, 1976, p.162 n.2] I explicitly write that I only take his date to lie between −100 and +200, and that I use "ca.−100" only as a date to use in citation.

F3 The parapegma is obviously not based upon the personal observations of Geminus, since each entry is explicitly attributed to some earlier astronomer. For example, the entry for day 3 of Scorpio reads: "Stormy weather according to Dositheos." By far the greatest number of entries are taken from either Callippus, Euctemon, or Eudoxus.[14] The times of most phenomena are taken from only a single source. However, the entry for day 25 of Cancer says that it is the day of the morning rising of Sirius according to Meton, where the entry for day 27 says that it is the morning rising of Sirius according to Euctemon. In addition, the times of the equinoxes and solstices are given according to both Euctemon and Callippus. However, it is the lengths of time between the equinoxes and solstices, as attributed to Euctmon, that concern us here. That is, we are concerned with the lengths of the seasons implicitly attributed to Euctemon.

F4 Beginning with summer, the lengths of the seasons attributed to Euctemon are 92, 89, 89, and 95 days. On the other hand, there is a [papyrus] called *Ars Eudoxi* [Dinsmoor, 1931, p.317], written apparently about −200, which gives the seasons according to Euctemon, presumably as preserved by Eudoxus in a writing that is now lost in the original. Note that *Ars Eudoxi* is about a century and a half later than Eudoxus. *Ars Eudoxi* says that the seasons according to Euctemon are 90, 90, 92, and 93 days.

F5 *HS* write: "Newton considers the parapegma the work of Geminus, . . . and finds some very important information in it (pp.162-73, 291-97) that no one seems to have found before." Anyone can find the information which I used (which is limited to the lengths of the seasons attributed to Euctemon) who can read either the Greek text or the German translation in the edition published by Manitius, which is cited in the references as [Geminus, −100].

F6 *HS* also write flatly that the parapegma is a composition unrelated to the writing of Geminos. Other writers are not so dogmatic. Dicks [1972], for example, says more cautiously that the parapegma "probably" represents older material. Many other writers simply use the parapegma as if it were due to Geminus, without comment.

F7 I do not understand the point of the argument. The parapegma certainly represents earlier material, since it is composed entirely of quotations from earlier writers. I do not see any way to decide if such a compilation of quotations was made by Geminus or some other writer. Further, at least for our purposes, the point is unimportant. The important point is whether certain quotations about Euctemon are accurate.

F8 On this point *HS* write: "Now the durations . . . have *no* relation to any of the authorities named[15] . . . . But Newton, by reasoning he does not explain and we cannot fathom, decides that they must be the work of Euctemon . . . and that they must be the result of observation. Never mind that the Eudoxus Papyrus [that is, what I have called the *Ars Eudoxi*] gives altogether different intervals for Euctemon; these are dismissed in a footnote."[16]

F9 To take up the last point first, it is true that I discuss the *Ars Eudoxi* only in a footnote, but this footnote is half a page long [pp.164-165]. In this footnote, I show that the seasons derived from the *Ars Eudoxi* are consistent with my main conclusions. This is hardly the same as "dismissing" *Ars Eudoxi* "in a footnote."

F10 The reasoning that I do not explain and which *HS* do not fathom is so simple that I saw no need to mention it. I simply took the seasons to be the work of Euctemon because they are derived from the dates of the equinoxes and solstices[17] explicitly attributed to him.

F11 We should also note that *HS* criticize my treatment of the durations (which means the lengths of time the sun spends in each zodiacal sign). This is another example of their carelessness; I barely mention the durations, and my discussion is limited to the seasons.

F12 As I have already mentioned, *HS* cite my discussion of the "durations" as an example of my lack of understanding "in making crucial decisions about whether a report represents an observation or a computation." Actually, my discussion about the durations, or rather the seasons, played no part[18] in deciding whether to admit or exclude any data, and the entire criticism of *HS* is irrelevant to their point.

F13 Since I did not use the lengths of the seasons in deciding whether to admit or reject data, I should mention why I did use them. I used them, in conjunction with other data, in order to estimate the standard deviation of a Hellenistic measurement of the time of an equinox or solstice. I used the standard deviation in turn to estimate the probability that certain errors in measurement could have happened by chance, but this probability did not enter into my decisions. I made the decisions before I calculated the probability in question.

F14 Most writers I have seen take it for granted that the seasons (and durations) given by Geminus are not those due to Euctemon while those given by Eudoxus are. I presume this is why *HS* write that the durations in Geminus have no relation to the authorities named. I also presume that the unstated reason for preferring Eudoxus is that *Ars Eudoxi* is older than Geminus.

F15 I do not see the reason for such assurance. Both *Ars Eudoxi* and Geminus are late writings presumably based, in the part that concerns us, on the writing of Euctemon. If there are errors in quotation, they are just as likely to be in the earlier quotations made by Eudoxus as in the later quotations made by Geminus.

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[14] Euctemon is usually credited, along with Meton, with having measured the time of the summer solstice of the year −431. Callippus and Eudoxus apparently belong to the following century.

[15] Note by DR: Personally, DR tends to agree with *HS* that the Geminos seasonlengths are not Euktemon's. However, I concur with RRN (§D14) that this point does not in the least undercut the RRN book's conclusions (quite the reverse: see fn 13). Moreover, there is a hilarious irony (which, again, RRN is too nice to mention) implicit in *HS*'s superior cocksureness that the Geminos durations are unrelated to authorities, and that this alleged error proves RRN to be *careless, unreliable, & intelligence-insulting*. For, *HS* have forgotten a little something written by their very own don-mentor O.Neugebauer. (S's decades of hitherto-flawless sycophancy & dutiful hatchetry in ON's service have earned S his rightful place as ON's recognized intellectual heir.) ON says, while contrasting the Geminos parapegma's data with those in the Eudoxos Papyrus: "From the *dates and intervals* given in the 'Geminus' parapegma (cf. above p.581 [in *HAMA*]) one finds, however, for Callippus $s_1 = 92$ [days], $s_2 = s_3 = 89$, $s_4 = 95$ (cf. below p.1352, Fig.4). The explicit statement in the papyrus seems to me the more reliable source." (*HAMA* p.627 n.9; emph added. See also reference to this useful ON discussion in fn 13.) Thus, no less a figure than O.Neugebauer himself entertained the idea that the Geminos seasonlengths related to at least one of the authorities cited. (Even *HS* admit at p.61 that the Geminos data for Kallippos are not far from his Eudoxos Papyrus values, but this resemblance does not cause *HS* to qualify their emphatic certainty that there is no relation.) Did the late ON ever know that his toppe syc has decreed that one of ON's own hypotheses is typical of the worst excesses of the intelligence-insulting Evil One's carelessness & unreliability?

[16] The emphasis in this passage is in the original. The "authorities" are Euctemon, Eudoxus, and Callippus.

[17] Note by DR: The Summer Solstice is attributed to Kallippos.

[18] Note by DR: At p.294 of [Newton, 1976], RRN mentions his Geminos-based 7 hr standard deviation for Euktemon in connection with the credibility of a supposed −28 hr error in his −431 Summer Solstice, but RRN adds that a smaller standard deviation could be induced independently. See also the discussions below at §F13, §F20-F21, & §G2. Thus, RRN is correct in stating that he did not depend upon Geminos in rejecting the reality of the grossly false Euktemon S.Solst time given by Hipparchos & Ptolemy (an innocent explanation of which is proposed here in ‡6 §E5). I must add that *HS* fail to note certain important points: [a] When RRN suggested that this S.Solst was fabricated, he knew that Ptolemy could not be responsible for the date and was explicitly cautious in leaving it an open question as to whether Ptolemy fabricated the hour [Newton, 1976, pp.296-297], [Newton, 1977, p.96], & here at §F20. [b] As regards Euktemon, his conveniently false (SS) datum is isolated and is from secondary sources (centuries later) — while Ptolemy's suspiciously agreeable data are by the dozen and are all found right in his own magnum opus.

F16 Now let us look at the durations given. Those given by Geminus, starting with the duration in Cancer, are 31, 31, 30, 30, 30, 29, 29, 30, 30, 31, 32, and 32 days. Those in the *Ars Eudoxi* are 30, 30, 30, 30, 30, 30, 30, 31, 31, 31, 31, and 31 days. *HS* write that the durations given by Geminus "may be partially based upon observation, but are still mostly schematic. . . ." I do not see how *HS* can possibly have the information needed to make this statement about the durations in Geminus. It is probably true of the durations given in *Ars Eudoxi*. They seem to be based upon observation to the extent that they yield a valid estimate of the length of the year (365 days), and that they put the sun's perigee reasonably close to the right place. Otherwise they are clearly schematic, and they may well come from computed Babylonian ephemerides.

F17 On the other hand, in spite of *HS*'s statement, the durations in Geminus are almost surely observed. Their variation is too great for them to be mostly schematic. In addition, they place the solar perigee more accurately than the durations from *Ars Eudoxi*. Now we have good reason to believe that Euctemon observed, or at least participated with Meton in observing, the summer solstice in the year -431. It is plausible that he observed other solstices and equinoxes if he observed this one, and thus it is plausible that his durations, or at least his seasons, are based upon observation.[19] If so, his seasons are not those derived from *Ars Eudoxi*.

F18 In sum, Euctemon's lengths of the seasons are more likely to be those in Geminos than those in *Ars Eudoxi*. In this connection, the work of Pritchett and van der Waerden [1961] is interesting. They take all the quotations from Euctemon in Geminos to be genuine except the durations. I suppose this is a possible situation, but I would not wish to uphold it as dogma.

F19 Even if the seasons given in *Ars Eudoxi* should prove to be those due to Euctemon, this would not affect any important conclusion or decision that I reached. The seasons given by Geminus are still an ancient Greek set of seasons which show the accuracy that I stated. The only change needed would be that I could not attribute this accuracy to the time of Euctemon but only to the time of Callippus about a century later.

F20 When I attributed this accuracy (a standard deviation of about 7 hours) to Euctemon, I used it for only one purpose. I had already concluded from an analysis of ancient Greek solstices that the exact time of the solstice attributed to Meton and Euctemon was fabricated by someone about the year -100 for an entirely different purpose, and that this fabricated time is the only one that has survived in the literature. [See fn 18.] I used the standard deviation only to calculate the statistical confidence level that we can attach to this analytical conclusion. However, the statistical confidence level is quite high no matter what we assume about the accuracy of Euctemon's (and Meton's) measurements. The reason for this seemingly paradoxical statement is given by me in another work [Newton, 1977, pp.343-344]. [Note by DR: the reader is urged to consult the important discussion here cited. See also §G5.]

F21 One final remark should be made. It is possible that both sets of seasons attributed to Euctemon were actually used by him. He might have used the schematic seasons given by *Ars Eudoxi* in his early work before he had done much observing. Then, after he had made measurements of the seasons (perhaps in conjunction with Meton), he adopted the set of seasons, based upon measurement, which we find in Geminus.

F22 In summary of this section, it is likely that the lengths of the seasons given by Geminus are due to Euctemon, in spite of the dogmatic statement by *HS* that they are not. Even if they are not due to Euctemon, this would not affect any important conclusion that I reached. In particular, contrary to the claim of *HS*, this would not illustrate my lack of understanding the sources "in making the crucial decisions about whether a report

[19] It is possible that Euctemon measured only the lengths of the seasons but not the individual durations. In this case, his durations would be schematic ones made to fit the lengths of the seasons. They are still based upon much more observation than the durations in *Ars Eudoxi*.

represents an observation or a computation." Attributing the seasons to Euctemon did not play any part in chosing whether any report represents an observation or a computation.

G Ptolemy's Alleged Observations

G1 I shall take the space to mention one other *HS* error. This concerns the astronomical observations that Ptolemy claims to have made himself. *HS* say of these that Newton "does not think highly of Ptolemy's observations, in fact, he believes they are all fraudulent. His reasoning is based mostly upon demanding rather great precision [of the ancient observations] . . . and then saying that Ptolemy's observations, which do not meet such standards, must be fraudulent." After some remarks about the equinoxes and solstices[20] "observed" by Ptolemy, they go on to say: "Through similar reasoning, but not much evidence, the argument is extended to *all* [emphasis in the original] of Ptolemy's observations, and Newton 'proves' [quotation marks in the original] his point by computing probabilities like 10^{200} to 1 that the observations are fraudulent."

G2 This misrepresents my reasoning in almost every respect. To start with, my reasoning is not based in any way upon demanding great precision of ancient observations. It is based upon the exact agreement, to the level of rounding used, between Ptolemy's alleged observations and the theories he pretends to derive from them. For example, consider the observations of the [three] equinoxes and the summer solstice that Ptolemy claims to have made himself. On the basis of these [four] observations, Ptolemy claims then to prove that Hipparchus's theory of the sun, derived almost three centuries earlier, is still valid in Ptolemy's own time, even to the exact values of the parameters.

G3 However, if we calculate the times of the equinoxes and solstices in question from Hipparchus's theory, maintaining a precision of more [better] than an hour in the calculations, and then round to the nearest hour, we get exactly the times that Ptolemy states. This is so, even though Ptolemy gives his results with a precision of an hour while Hipparchus gave his results [DR note: on which the whole solar theory is based!] with a precision of only a quarter of a day. Our intuition tells us that such agreement is impossible, and this conclusion does not depend upon the precision "demanded" of ancient Greek observations.

G4 In spite of this, numerous writers have claimed that Ptolemy's measurements were the result of chance errors in observation. To counter these claims, I felt it necessary to estimate the probability that Ptolemy's results could have happened by chance. Now, compared with modern theory, Ptolemy's [four] solar observations are all in error *in the same direction* by the order of 30 hours, but they all agree with preassigned values (from the [solar] theory of Hipparchos) within half an hour (half the rounding level used). I have estimated the probability[21] that all this could have happened by chance at 10^{-92}.

[20] Note by DR: One of the more amusing moments in the *HS* review, which RRN is too polite to note, is *HS*'s sarcastic mock astonishment while commenting upon a key RRN discrimination: "most remarkable of all, that solstices could be observed with more accuracy than equinoxes." That RRN is correct (in the very judgement which *HS* attack as "remarkable" folly) is obvious to any unprejudiced scientist familiar with the instrumental problems involved. (See the lucid discussion at [Newton, 1977, pp.81-82].) One notes that *all* known ancient astronomical observers (excluding Ptolemy, who did not observe) depended primarily upon solstices for gauging the year's length: Meton, Euktemon, Kallippos, Aristarchos, Archimedes, Hipparchos. (Hipparchos observed numerous equinoxes, but even *his* yearlength was based upon solstices: see ‡6 eq. 8.) However, Swerdlow, an historian with the official rank of professor in the Dep't of Astronomy at the Univ Chicago, cannot understand this elementary point: during a gloriously delirious passage (p.527) in his prominent 1979 attack on Newton (*American Scholar 48*:523; and see fn 6 & ‡6 fn 6), Swerdlow argues: "At the time of the solstice, the meridian altitude of the sun changes by less than fourteen seconds of arc per day, and measuring this quantity, let alone any fraction of it, was obviously ridiculous." The only ridiculous aspect of this astounding piece of reasoning is that a member of *the University of Chicago's Dep't of Astronomy* should so conspicuously exhibit his touching innocence of the implications of first-year calculus and of the standard technique known as "equal altitudes". It is easy to see that Hist.sci archon Swerdlow's reasoning is essentially equivalent to insisting that the time a vertically oscillating body reaches maximum altitude cannot be determined since at that moment it lacks vertical motion!

[21] The probability of 10^{200} to 1 that *HS* quote (see p.149 of the book under review) is based upon a larger set of data.

G5 I have pointed out at [Newton, 1977, p.90] that I do not mean for probabilities like 10^{-92} to be taken literally. For one thing, I used the normal law of error in calculating the probability, but there is no reason to assume that the normal law applies in these extreme circumstances. For another, I assumed a specific standard deviation for a single measurement, and one may question the standard deviation used. However, as I pointed out on page 92 of [Newton, 1977], a work available to *HS* [and cited by them] when they wrote their review, it does not matter much what law of error we use or what standard deviation. [See §F20.] The probability of chance occurrence is vanishingly small, far beyond the level of ordinary experience. This conclusion does not come, as *HS* claim, from my "misuse of probability" that "insults the intelligence of the most naïve reader."

G6 *HS* write that I extend my argument (about the solar observations) to all of Ptolemy's observations. This is not correct. My conclusion (page 493 of the book under review) is: "All of his own 'observations' that Ptolemy actually uses, and that are subject to test, prove to be fraudulent." The two qualifications are important. First, there is not enough information to let us test some of the observations he claims to have made for each outer planet. While personally I have no doubt that he fabricated these observations, this feeling is based upon his usual method of doing business, and I exclude these observations from my general finding. Second, there are some stellar observations [Newton, 1974] which Ptolemy claims to have made but which he does not use [12 declinations]. These observations disagree with his theories, but he does not use them in any way, and they pass all the tests for genuineness. However, the fact that he included these discordant observations in his work, without pointing out that they are discordant, increases the evidence that Ptolemy's work is a deliberate fraud.[22] It also suggests that Ptolemy did actually make some observations but he does not use them.

G7 Finally, *HS* claim that I extend my argument to all of Ptolemy's observations without much evidence. They could not have written this if they had read my book with any attention. Altogether, I base my conclusion upon a detailed analysis of the following sets of observations:

[a] measurements of the times of equinoxes and solstices,
[b] a measurement of the lunar evection,
[c] several measurements of the obliquity of the ecliptic,
[d] a measurement of the latitude of the site where Ptolemy claims to have made his observations,
[e] several measurements of the inclination of the lunar orbit,
[f] a measurement of the maximum lunar parallax,
[g] several measurements of the apparent solar diameter, and
[h] all of the planetary observations.

This includes almost all of the observations that Ptolemy claims to have made, and I included all of the others in a work [Newton, 1977] that was available to [& cited by] *HS* when they wrote their review. This cannot be seriously described as "not much evidence".

H The Integrity of Book Reviews

H1 In summarizing the quality of the review by *HS*, I cannot do better than to paraphrase one of their statements (§D13) about my book: The review by *HS* "is careless and unreliable to the point" that it should be read only be someone "who is prepared to examine every source". In other words, no statement in the review, no matter how simple, can be taken as accurate, although a few minor statements are correct.

[22] Ptolemy pretends to choose [the stars he uses] at random from a table containing many stars. Yet "by accident", the ones he uses are the ones that agree with his theory while the others are ignored.

H2 Since the review appeared in a scholarly journal, many people who are not particularly acquainted with the [Ptolemy controversy] situation[23] will probably take the review as valid. That is, the appearance of this review does damage to the field of learning involved, rather than promoting it. It is what a friend of mine calls "a subtraction from the sum of human knowledge."

H3 The problem is how to inhibit the appearance of such incompetent reviews in the scholarly literature.[24] I see only one general way to do this. This is to require that book reviews, like research articles, be subject to refereeing. That is, a book review, before it is accepted for publication, should be refereed, and the editor's decision to accept or reject the review should be made in light of the referees' report. I do not say that the editor should necessarily follow the referees' recommendations, but he should at least know what they are.

H4 Further, the author of the book in question should receive a copy of the review, and be given an opportunity to comment. He should be particularly on the lookout for factual errors such as those committed by *HS* in their review. If he wishes, the author should have the opportunity to write a rebuttal to the review, to be published immediately after the review and in the *same* issue of the journal.

H5 It will probably take much discussion to decide upon the way in which this policy should be implemented, and I can only make some suggestions. I suggest that a review should be sent to at least two referees, just as a research paper is sent by the best journals. In addition, a copy of the review should be sent to the author under review. The editor should not make his decision until he has received and studied the comments of the author and referees. Of course, if the referees and-or the author fail to send in their comments in a reasonable time, the editor should proceed without them. When he sends out the copies to the referees and the author, the editor should make the time limit known to them; I suggest it as reasonable to require that comments should be sent to the editor within three months.

H6 In summary, book reviews should be subject to the same scholarly standards that research articles are, with the additional requirement that the author of the book should have an opportunity to comment on the review, and if he sees fit, to write a rebuttal.

References

al-Biruni, Abu al-Raihan Muhammad bin Ahmad, *Kitab Tahdid Nihayat al-Amakin Litashih Masafat al-Masakin*, 1025. There is a translation into English by Jamil Ali, with the title translated as *The Determination of the Coordinates of Positions for the Correction of Distances between Cities*, published by the American University of Beirut, Beirut, Lebanon, 1967.

American Ephemeris and Nautical Almanac, U.S.Government Printing Office, Washington, D.C., published annually. [This publication has been succeeded by the *Astronomical Almanac*.]

de Sitter, W., On the secular accelerations and the fluctuations of the longitude of the

[23] Note by DR: the Book Review Editor at the *JHA* who commissioned the review is O Gingerich, Ptolemy's prime public hagiofish, 2nd only in *JHA* rank to the unique Editor-for-Life.

[24] Note by DR: There is another issue relevant to this matter. Numerous journals' correspondence columns will not print replies to book reviews. (Indeed, last time I looked, the egregious *JHA* did not even *have* a correspondence column — as befits a journal that is operated by an Editor-for-Life whose response to dissent has included such openminded behavior as that noted at ‡1 fn 25, ‡6 fn 15, & ‡8 fn 35.) That being the case, an organized assault (upon a dissenting view), carried out in centrist-journal book reviews, permits the dissenter little or no reply space. This technique is one of the neatest (among those which certain embarrassed cliques employ) for protecting cherished nonsense, even for decades on end — e.g., the sacred Muffia tenet that a clumsy faker like Ptolemy was "the greatest astronomer of antiquity", as O Gingerich has publicly decreed (echoing Neugebauer's *HAMA* p.931) in OG's 1976/8/6 *Science* book review of *HAMA*. This review's gratuitous cracks at R.Newton (mild compared to Swerdlow's) were, as usual, protected from the slightest printed reply.

moon, the sun, Mercury, and Venus, *Bulletin of the Astronomical Institute of the Netherlands, IV*, pp.21-38, 1927.

Dicks, D., article on Geminus in the *Dictionary of Scientific Biography*, Charles Scribner's Sons, New York, 1972.

Dinsmoor, W.B., *The Archons of Athens in the Hellenistic Age*, Harvard University Press, Cambridge, Massachusetts, 1931.

Geminus, Εισαγωγη εισ τα φαινομενα, ca.−100. There is an edition under the title *Elementa Astronomiae*, with a parallel translation into German, by K.Manitius, B.G.Teubner, Leipzig, 1898. The date −100 is only a guess used for purposes of citation; the real date may be as late as +200. [There is a Greek-French edition by G.Aujac, Paris, 1975.]

Ginzel, F.K., *Spezieller Kanon der Sonnen- und Mondfinsternisse*, Mayer und Müller, Berlin, 1899.

Hamilton, N.T. and Swerdlow, N.M., Judgement on Ptolemy, *Journal for the History of Astronomy, 12.1*, pp.59-63, 1981.

Kugler, F.X., *Sternkunde und Sterndienst in Babel, Ergänzungen zum Ersten und Zweiten Buch, II. Teil*, Aschendorffsche Verlagsbuchhandlung, Münster, Westphalia, 1914.

Langdon, S. and Fotheringham, J.K., *The Venus Tablets of Ammizaduga*, Oxford University Press, Oxford, 1928.

Neugebauer, O., Solstices and equinoxes in Babylonian astronomy during the Seleucid period, *Journal of Cuneiform Studies, 2*, pp.209-222, 1948.

Newton, R.R., The authenticity of Ptolemy's eclipse and star data, *Quarterly Journal of the Royal Astronomical Society, 15*, pp.107-121, 1974.

Newton, R.R., *Ancient Planetary Observations and the Validity of Ephemeris Time*, Johns Hopkins University Press, Baltimore and London, 1976.

Newton, R.R., *The Crime of Claudius Ptolemy*, Johns Hopkins University Press, Baltimore and London, 1977.

Newton, R.R., *The Moon's Acceleration and Its Physical Origins, Vol.1, As Deduced from Solar Eclipses*, Johns Hopkins University Press, Baltimore and London, 1979.

Oppolzer, T.R. von, *Canon der Finsternisse*, Kaiserlich-Königlichen Hof- und Staatsdruckerei, Wien, 1887. There is a reprint, with the explanation of the tables translated into English by O.Gingerich, Dover Publishing Company, New York, 1962.

Pauly-Wissowa, 1894. This is a conventional citation for all the volumes of Wissowa, Georg, *Paulys real-Encyclopädie der Classischen Altertumswissenschaft*, J.Metzler, Stuttgart, 1894 and later years.

Pritchett, W.K. and van der Waerden, B.L., Thucydidean time-reckoning and Euctemon's seasonal calendar, *Bulletin de Correspondence Hellénique, LXXXV*, pp.17-52, 1961.

Ptolemy, C., Μαθηματικης συνταξεως βιβλια $\overline{\iota\gamma}$,[25] ca.150. The standard edition is generally considered to be the one by J.L.Heiberg[26] in *C.Ptolemaei Opera Quae Exstant Omnia*, B.G.Teubner, Leipzig, 1898 & 1903. [There is a useful English edition by G.Toomer, New York etc., 1984.]

Sachs, A., A classification of the Babylonian astronomical tablets of the Seleucid period, *Journal of Cuneiform Studies, 2*, pp.271-290, 1948.

Schoch, C., Astronomical and calendarial tables, 1928. This work is printed as Chapter XV in the reference Langdon and Fotheringham 1928.

van der Waerden, B.L., *Science Awakening: II. The Birth of Astronomy*, Noordhoff International Publishing, Leyden, 1974.

[25] Note by DR: This work is the famous *Syntaxis*, otherwise known as the *Almagest* or (DR) *Almajest*. In the Greek title, $\overline{\iota\gamma}$ (13) is inadvertently misprinted as $\overline{\tau\gamma}$ (303) at Pedersen *Survey* 1974 p.15.

[26] Note by DR: In a quite different arena, Denmark's Heiberg was the scholar who (even while he was working on Ptolemy) Latinized the text for Danish composer Carl Nielsen's glowing choral work, *Hymnus Amoris* (1896).

‡6 Hipparchos' Ultimate Solar Orbit

& the Babylonian Tropical Year

Summary

The sole extant Babylonian tropical yearlength value is found to be based upon Hellenistic observations, one of which — a 135 BC Summer Solstice — was performed and used by Hipparchos (a few years before his death) to improve his wellknown erroneous 146 BC solar theory (PH orbit). His new Ultimate Hipparchos (UH) orbit (epoch 128 BC) was that from which he set the astrolabe for his last 3 surviving dated observations & for the nearly contemporaneous Ancient Star Catalog's zodiacal longitudes. The elements of this UH orbit are completely reconstructed and are found to be almost twice as accurate as the famous standard (PH) Hipparchan solar tables preserved in the *Almajest*.

A The Initial Cuneiform Clue

A1 The only surviving explicit[1] Babylonian estimate of the tropical year's length is found on the well known Astronomical Cuneiform Text (ACT) #210 [BM55555] Sect.3 (Neugebauer 1955 1:271-3, 3:243a; Neugebauer 1975 p.528). ACT #210's yearlength is:

$$Y_{\mathrm{B}} = 365^{\mathrm{d}}14'44''51''' \tag{1}$$

(precision: 2/5 of a timesec), a much discussed but previously unexplained datum. (See ‡5 fn 8.) If we run a continued fraction analysis on this value (& truncate before the

[1] Rawlins 1999 reconstructs a Babylonian tropical (civil) year of $365^{\mathrm{d}}1/4 - 1/285$, evidently arrived at by ancients' division of 19 into $235M_{\mathrm{A}} = 6939^{\mathrm{d}}41'$. ($M_{\mathrm{A}}$ is from eq. 10; note that 285 is an integral multiple of 19.) At least as early as Meton (432 BC), $235M/19$ was a politically useful civil year (bringing lunar & solar priests together under a single calendar, a scheme still used to compute Easter's date). But equating this amount to an empirical *tropical* year was a fateful blunder, apparently originated (from early, shaky evidential indications: Rawlins 1985H) by Meton, Kallippos, & Aristarchos, later adopted by Hipparchos & Ptolemy. However, the fact that Aristarchos was the earliest (Rawlins 1999) to use a year near eq. 7 also imparts the vital information that *he was the first known astronomer to possess a highly accurate value of the month*, a value we may virtually recover just by multiplying 19/235 times his tropical year (giving $29^{\mathrm{d}}.530602$; see Rawlins 1985H and Rawlins 1999's decipherment of the mss data listed at Neugebauer 1975 p.601). Aristarchos (280 BC) was specifically the originator of the remarkably correct "Babylonian" month M_{A} (see §B10). On the other hand, $19Y_{\mathrm{K}}/235 = 27759^{\mathrm{d}}/940 = 29^{\mathrm{d}}.530851$; thus, in 330 BC, Kallippos' month was (Dinsmoor 1931 p.409) 22^{s} longer than the real month (then equal to $29^{\mathrm{d}}.530597$, according to the Earth-acceleration of §B2). In 432 BC, Meton's month was $19\cdot(365^{\mathrm{d}}5/19)/235 = 6940^{\mathrm{d}}/235 = 29^{\mathrm{d}}.531915-$, which is 114^{s} longer than reality. By contrast, Aristarchos' M_{A} (eq. 10) is correct to a fraction of 1^{s}. Since determining M required possession of [a] reliable ancient lunar eclipse records & [b] an accurate theory of the syzygial Moon's non-mean motion [however, see *DIO 6* ‡1 fn 18], remarkable improvement in both categories seems to have occurred during the 1 1/2 century interval: 432 BC to 280 BC. Regarding [a]: Kallippos was among the very first Greeks with access to the ancient lunar records of Babylon (van der Waerden 1974 p.290; note the Theon of Alexandria testimony there cited in n.3, from Rome 1931-43 p.839-840, and its conflict with Rawlins 1985H); Kallippos obviously used (and so helped immortalize) the −330/9/20 Arbela eclipse (his most recent) as a prime contemporary empirical anchor for his lunisolar theory & calendar (Rawlins 1985H), whose epoch was the latter of a millennially unique pair of close approaches of Summer Solstice & New Moon, −348/6/27 and −329/6/28. Simply by comparing monthlength accuracy (22^{s} vs. 1^{s}), we can date [b] to the 1/2 century between Kallippos & Aristarchos. This allows us to pinpoint (at least within a few decades) just when the amazing flowering of the full genius of Hellenistic empirical astronomy occurred. A measure of that genius: Aristarchos' sidereal motions of Sun (Rawlins 1985S, Rawlins 1999) & Moon (*idem* plus eq. 10 & §B10) were both accurate to about 2 parts in ten million; Rawlins in-prep A.

remainder-denominator becomes outsize) we get the close approximation:

$$Y_\mathrm{B} = 365^\mathrm{d} + \cfrac{1}{4 + \cfrac{1}{15 - \cfrac{1}{2 + \cfrac{1}{2}}}} = 365^\mathrm{d}73/297 \tag{2}$$

A2 We may also express this result with respect to the familiar Kallippic (Julian) year, which is equal to

$$Y_\mathrm{K} = 365^\mathrm{d}1/4 \tag{3}$$

Combining eqs. 2 and 3:

$$Y_\mathrm{B} = Y_\mathrm{K} - 5^\mathrm{d}/1188 = Y_\mathrm{K} - (5^\mathrm{d}/4)/297 \tag{4}$$

(Eqs. 2 or 4 will easily produce the attested Y_B of ACT #210 to full sexagesimal precision, since eq. 4 differs from eq. 1 by less than $0^\mathrm{s}.04$.) An alternate way of rendering eq. 4: 297 Babylonian tropical years are cumulatively $5^\mathrm{d}/4$ shorter than 297 Kallippic years:

$$297 \cdot Y_\mathrm{B} = 297 \cdot Y_\mathrm{K} - 5^\mathrm{d}/4 \tag{5}$$

Empirical ancient solstices & equinoxes were customarily rounded to the nearest quarter day. Such data could be the basis of Y_B.

A3 Ancient astronomers Meton, Kallippos, Aristarchos, Archimedes, & Hipparchos evidently used Summer Solstice (SS) observations for determining the tropical year's length because equinoxes are subject to vexatious systematic errors[2] (VE & AE: same magnitude, but opposite sign; Britton 1967 p.29) from misplacement of instrumental equator. (See below, §C1.) The hypothetical solstices producing Y_B would have been recorded 297^y apart, with the 2nd datum occurring (as shown by eqs. 4 & 5) $5^\mathrm{d}/4$ ahead of the time predicted by just adding $297{\cdot}Y_\mathrm{K}$ onto the 1st datum.

A4 Understand: besides 297^y, no other span of time (relatable to a not too long interval between observations)[3] can yield eq. 1 via standard ancient $1^\mathrm{d}/4$ precision solstice data.

A5 So, now one goes fishing: are there extant ancient solstice observations that are 297^y apart? Well, since there are only 3 real examples of such data whose observers and years are directly attested, the *a priori* odds certainly are not encouraging. These three records are mentioned in *Almajest* 3.1: the solstices of Meton (−431/6/27 1/4 = dawn or 6 AM), Aristarchos of Samos (−279/6/26), and Hipparchos (–134/6/26). (Ptolemy does not provide either Aristarchos' or Hipparchos' solstice hour — nor even day, though the dates are fortunately not in dispute. I thank the late Willy Hartner for bringing Ptolemy's silence to my attention in a letter of 1980/8/15.)

A6 We know that something quite remarkable has been revealed when we find that: *the Meton and Hipparchos observations are in fact* 297^y *apart*. The likelihood of this being a chance agreement with the 297^y interval of eq. 5 is ordmag 1%. (It was on 1982/1/28, while typing a letter to R.Newton, that I hit upon eqs. 4 & 5 and the astonishing connection between ACT #210 and the Meton & Hipparchos data. The discovery was reported briefly in, e.g., Rawlins 1984A p.989 n.43, and Rawlins 1985G p.256 n.3.)

[2] Unlike these astronomers, Ptolemy was utterly unfamiliar with actual outdoor observing (see, e.g., fn 24) and so preferred equinoxes (*Almajest* 3.1). See also ‡5 fn 20.

[3] Only sub-500^y alternatives (to eq. 4 remainder) are: $(7^\mathrm{d}/4)/416^\mathrm{y}, 2^\mathrm{d}/475^\mathrm{y}$. (Each yields an adequate approximation to eq. 1, though not so close as eq. 4.) But either requires availability before c.68 BC (see §B9) of empirical solstices over 4 centuries old, i.e., from c.500 BC. (As for Babylonian solstices, see Neugebauer 1975 p.363.)

A7 The tracing of a Babylonian cuneiform parameter back to wholly Hellenistic sources is a watershed, marking the commencement of our awareness of how heavily Seleukid-era Babylonian astronomers (more likely astrologers) depended upon the science of the superior civilization that had under Alexander conquered Babylon. (Subsequently discovered details of extensive Babylonian use of Greek lunar and planetary orbital work will appear in Rawlins in-prep A.)

B Hipparchos' Accurate Solstice & the Date of ACT #210

B1 Scholars have long conjectured regarding the hour of the Hipparchos –134/6/26 solstice, commonly presuming[4] it to be noon because that is consistent with the Hipparchos-Ptolemy (PH) *Almajest* solar tables (see §B3). Now at last the hour may be firmly reconstructed just by adding $297{\cdot}Y_\mathrm{B}$ to the Meton time (−431/6/27 6 AM; *Almajest* 3.1):

$$-431/6/27\ 1/4 + 297 \cdot Y_\mathrm{B} = -134/6/26\ 1/4 = 6\ \mathrm{AM} \tag{6}$$

(Rawlins 1985H. This equation merely rearranges the original process whereby Y_B was found by its ancient inventor: dividing 297 into the time-interval between these 2 solstice-data.)

B2 The actual –134/6/26 solstice was about 7 AM Rhodos local mean time (if one adopts Earth mean fractional secular spin acceleration $-19\mathrm{x}10^{-9}$/century;[5] Tuckerman 1962&64 makes it 6 AM); therefore, the observation was accurate within rounding error ($\pm 3^\mathrm{h}$), as such data will usually be (fn 13; Rawlins 1985H).

B3 Hipparchos' observed Summer Solstice (SS) hour 6 AM (eq. 6) does not agree with the *Almajest* 3.2&6 Hipparchos (PH) solar tables (which give 11 AM); this presumably explains why Ptolemy in *Almajest* 3.1 neither states the hour nor compares his own tabular 140 AD solstice "observation" to this discrepant Hipparchan datum, in order formally to establish the tables' yearlength, which was his procedure earlier (twice in the very same chapter: *Almajest* 3.1) regarding Hipparchos' equinoxes. He instead compares his 140 AD datum to Meton's agreeable old solstice. This inconsistency is especially odd because

[4] E.g., Britton 1967 pp.23, 56; R.Newton 1977 p.83 n.3; van der Waerden 1986. However, in a generous 1986/9/20 letter to DR, van der Waerden, whose desire to adjust his opinions to new evidence is legendary, has withdrawn his paper's conjectured Aristarchos & Hipparchos solstice-hours. (BvdW's letter also proposed to send a retracting note to *Isis* on the basis of the UH orbit. This noble offer I regretfully declined, having experienced a succession of weird encounters with *Isis*. I instead made plans to publish the UH orbit discovery in *DIO*. Of course, *Isis* is always free to republish *DIO*'s findings. We're not holding our breath.) In a 1988/12/20 letter to DR, van der Waerden objects to the foregoing word "legendary", protesting that no such legend exists. If he is right, I hope to change that situation. He also objects to the word "generous". Clearly, his logic is: one should follow the truth wherever evidence leads, and that is not a matter of personal generosity: van der Waerden will praise a detractor or criticize a friend without favor, a virtue which he has inspired in others and which I have pledged will long survive him in *DIO*. So, I accept & support the interpretive correction, but wish to add that I call it not merely proper but *additionally* generous when one acknowledges the rightness of a scholar who is correcting a published work of oneself. And, if there is any word that succinctly describes the Neugebauer clique's attitude toward R.Newton, Diller, Billard, and sometimes even van der Waerden, it is: ungenerous. Incidentally, the frequently entertaining math of the Neugebauer gang is sampled at Rawlins 1987 n.30. (In the *American Journal of Physics*: undeniably accurate but highly embarrassing material which pathetic *Isis* had previously refused to publish.) See also fns 9 & 35 there, and here at fn 6, fn 21, & fn 33; also ‡1 §C5 & ‡5 fn 7.

[5] I use this figure here throughout. It is accurate to better than 10%, and is based upon [a] modern lunar places & gravitational theory, [b] the tidally-induced lunar acceleration of Dickey & Williams 1982, and [c] taking the successful *Almajest* 4.4 lunar mean elongation tables as correct for anytime between epochs Phil 1 (324 BC) & Ant 1 (137 AD). The fit is so smooth that any chosen epoch in this semi-millennial range produces the same result. (If the pre-Ptolemy solar equinox data of *Almajest* 3.1 are trusted to $1'$, then $-19\mathrm{x}10^{-9}$/cy might be a few percent on the small side; but an alteration of even 10% would require the existence of an unsurvivably flagrant asymmetry in errors of ancient eclipse-time predictions from the tables, i.e., comparable to their 16^m rms scatter: §E1.)

Almajest 3.1 correctly describes Hipparchos' solstice as "accurate", while twice calling Meton's solstice "crude". (Thus, I doubt that the doubly greater antiquity of Meton's solstice would justify using it in preference to Hipparchos'.)

B4 Placing Ptolemy's reticence into context: in *Almajest* 3.1&7, he provides 28 solar data (24 equinoxes and 4 solstices, helpfully tabulated in full by Britton 1967 p.23). Of all these data, the *only* ones for which he omits the time of day are the above-mentioned solstices of Aristarchos (−279/6/26) & Hipparchos (–134/6/26), where instead he merely quotes Hipparchos' statement that the *interval between* these 2 solstices was $1^{\rm d}/2$ shorter than $145{\cdot}Y_{\rm K}$, in close accord with the standard Hipparchos-Ptolemy tropical year used throughout the *Almajest*:

$$Y_{\rm J} = 365^{\rm d}14'48'' = Y_{\rm K} - 1^{\rm d}/300 = 54787^{\rm d}/150 \tag{7}$$

B5 In retrospect, we really didn't need the foregoing ACT #210 discovery to tell us: if Ptolemy evaded giving these two solstices' times, it was because *they did not agree* with the Hipparchos (PH) solar tables his discussion was trying to establish (*Almajest* 3.1-7). Obviously, the Aristarchos and Hipparchos solstices were jointly offset by roughly $1^{\rm d}/4$ from the PH solar tables of *Almajest* 3.2&6; and Hipparchos most likely differed in the direction of accuracy, given the surety with which the solstice can be determined, within about $2^{\rm h}$ (Rawlins 1985H, contra R.Newton 1982 p.42) of the truth.[6]

B6 And that is exactly what we have found in §B1, since (to the nearest $1^{\rm d}/4$) Hipparchos' observed –134 SS time (deduced in eq. 6) was rightly earlier by $1^{\rm d}/4$ than the time given by the *Almajest* solar tables (PH).[7]

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[6] Simply accomplished by the exceedingly elementary method of equal altitudes, which appears to be known to everyone in the universe except Ptolemy (fn 2) & U.Chicago's Noel Swerdlow. For the latter's epochally entertaining preschool anti-solstice argument, see p.527 of Swerdlow 1979 (lowlights: ‡5 fn 20), a review whose demeanor toward Ptolemy-skeptics is apt to the same educational level. This precious gem was published in the journal of the Phi Beta Kappa honor society, *American Scholar*. Of course, it goes without saying that Swerdlow questions the integrity of the author under review (as also in Swerdlow 1973): on p.528, he charges R.Newton 1977 with hiding his use of the French (Halma) 1813-6 translation of the *Almajest*, though Halma's edition is in fact cited at p.146 of R.Newton 1977, as well as at p.121 of R.Newton 1973-4 (the very paper where the proposal Swerdlow 1979 is assaulting was 1st published, at p.112). Similarly, Swerdlow 1973 p.243 (in *Isis*) accuses van der Waerden 1970 of noncitation of works disagreeing with him, a charge contradicted 2 paragraphs previously, and in any event a neat trick for a work with a 42-item bibliography, since, at the time, no one agreed with (or had even thought of) van der Waerden's central new proposal, which has since been proven correct (fn 36). Note: [a] van der Waerden 1970 cites 4 works from the Neugebauer clique that loathes the theory under discussion; [b] not a single inner member of this clique has ever cited any work by DR. (Watch Neugebauer's clonies handle the lovely UH discovery by: [a] ignoring it, [b] attacking it, or [c] trying to grab prime credit for it.) Swerdlow 1979 appears in the journal of ΦBK, whose editorial board included Ptolemy's most public defender, power-operator O Gingerich (on whose scholarly ability Swerdlow has somehow never gotten around to publishing his strong private opinion). Throughout, Swerdlow 1979 falsely treats R.Newton as if he does not have a PhD, by deliberate & consistent reference to "Mr.Newton". (Details at ‡3 fn 3. Question: why bother being accurate, in a field where one can ascend anyway by catering to power and taking care to attack only the pet hates of the influential?) Since Swerdlow's behavior suffers no public criticism by Hist.sci's other archons (to the contrary: ‡1 fn 15), one assumes that his output is regarded by them as exemplifying the scholarship & credit this field's leaders generate when they are placed at the best-known universities.

[7] More accurately: $5^{\rm h}$ earlier; from §B1 & §B3: observed-minus-PH = 6 AM − 11 AM = $-5^{\rm h}$. Hipparchos' PH tables agree with his observation (virtually exactly) for the –145/9/27 1/4 Autumn Equinox. This is also the 1st year for which Hipparchos leaves us 2 cardinal-point solar data. (And he adds another –145 VE observation from Alexandria; all 3 data are in *Almajest* 3.1. There was probably also a –145 SS-time: §C1.) Thus, it is reasonable to suppose that Hipparchos' contemporary epoch for his PH tables was –145. If so, this exact epoch was (just $54^{\rm h}$ after his AE observation) at: Pot 1 = Physkon 1 Thoth 1 = –145/9/29 noon. (The astronomical 1st regnal year of Ptolemy VII Physkon; φυσκων is Greek for pot-belly.) Proposed in Rawlins 1985K (though not necessary to that abstract's rounded-ϵ theory). (Note the oddity that the AE occurs at Thoth 1 noon in –136 for PH orbit, –135 for UH orbit & reality. Hipparchos' formal PH lunisolar epoch: Philip 1 Thoth 1 = −323/11/12 noon, likely borrowed from Kallippos and-or Aristarchos; Rawlins 1985K.) Since the PH (& UH) tables are based on yearlength $Y_{\rm J} = Y_{\rm K}$ $-\ 1^{\rm d}/300$ (eq. 7), these tables must depart from Kallippically spaced $1^{\rm d}/4$ precision data by $1^{\rm d}/300 = 4^{\rm m}48^{\rm s}$ per year after –145 (when the PH error for SS was over $+2^{\rm h}$). By –134, this departure had accumulated to more than $1^{\rm h}$; by –127, to $2^{\rm h}$, bringing the PH error in SS to over $+4^{\rm h}$, a discrepancy which was later revised by the new UH theory (see fn 12).

B7 Hipparchos' information (*Almajest* 3.1), that there was (between Aristarchos' solstice & his own) an interval of 145 of his yearlengths $Y_{\rm J}$, now additionally permits our reconstruction[8] of Aristarchos' solstice-time (using the result & the method of eq. 6, again ignoring the small geographical longitude difference between the observations, as does *Almajest* 3.1); rounding to the nearest $1^{\rm d}/4$:

$$-134/6/26\ 1/4 - 145 \cdot Y_{\rm J} = -279/6/26\ 1/2 = \text{noon} \tag{8}$$

(The elementary source of the illusory huge errors in this solstice & Meton's is revealed in Rawlins 1985H.)

B8 Though too long (vs. reality) by almost 5 timeminutes ($5^{\rm m}$), $Y_{\rm B}$ (eqs. 1 & 4) is nonetheless the best of a rather poor lot of surviving ancient estimates of the tropical year's length (Rawlins 1999). It was likely a Hipparchos value in some sense, though whether it was his own attempted late improvement (subsequently neglected by Ptolemy and Censorinus; Neugebauer 1975 p.624) upon the traditional and tabular value (eq. 7: $365^{\rm d}1/4\ -1^{\rm d}/300$) or was due to a later disciple, one cannot now be sure. I prefer the latter theory, partly because eq. 8 shows that a late Hipparchos work (after –134) justified his yearlength value by comparing his own –134 solstice not to Meton's (which would have given $Y_{\rm B}$, eq. 4) but rather to Aristarchos' (yielding $Y_{\rm J}$, eq. 7); and an even later self-summary (cited *Almajest* 3.1) of all his works still stands by $Y_{\rm J}$.

B9 A nice byproduct of the foregoing findings is a major temporal restriction upon the hitherto undated ACT #210 (Systems A & B): it was written after 135 BC. And since no System B lunar text is dated to later than 68 BC (Neugebauer 1955 pp.xvi & 182), we have the probable range:

$$\text{date of ACT } \#210 = 100 \text{ BC} \pm 35^{\rm y} \tag{9}$$

This tablet is one of the very few explicitly exhibiting the famous and highly accurate "Babylonian" monthlength (System B):

$$M_{\rm A} = 29^{\rm d}31'50''08'''20'''' = 29^{\rm d}.530594 \tag{10}$$

which Ptolemy attributed to Hipparchos (*Almajest* 4.2).

B10 It has long been assumed (starting with the epochal work of F.Kugler S.J., who first elicited $M_{\rm A}$ from cuneiform material: Kugler 1900 pp.24, 53, & 111) that Ptolemy was wrong and that Hipparchos instead just appropriated $M_{\rm A}$ from Babylon. . . . ACT #210 is now revealed here as post-Hipparchos [DR 2008: I thank A.Jones for a correction here]; I have already published evidence that $M_{\rm A}$ originated with neither him nor Babylon but instead is due to Aristarchos. (Rawlins 1984A p.987 n.25, Rawlins 1985G p.267 n.3, Rawlins 1985S & Neugebauer 1975 p.603; full details to appear in [*DIO 11.1* ‡1].)

B11 My impression has been that, from Kugler through Neugebauer, orthodox scholars have at least tacitly been assuming (e.g., Aaboe 1955; Britton 1967 p.iii; Neugebauer 1975 pp.4, 309, 351-5, 622) that parameters common to Babylonia & Greece show that Babylonian theoretical astronomy was a source for Greek, not vice-versa[9] — even though

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[8] Rawlins 1985H. (I here withdraw that paper's explicitly speculative Hipparchos $30400^{\rm y}$ precession cycle.) Note probable use of a nearby eclipse-anchor (−279/6/30, Rawlins 1985S; as also in the case of Kallippos: fn 1). This and Aristarchos' −279 solstice observation (only a few days earlier) were presumably the empirical foundation-stones of the astronomical calendar named for Dionysios the Renegade (the philosopher whose name is one of the inspirations for the title of this journal: ‡1 fn 23).

[9] Neugebauer once flirted with the idea that Meton's cycle was original (Neugebauer 1957 p.140; Samuel 1972 p.21) but later rejected this (Neugebauer 1975 p.622).

(until now) the evidence adduced actually favored neither alternative.[10] (However, see the two ancient tables of astronomers' yearlengths at Neugebauer 1975 p.601: both's hitherto-unremarked chronologies support Greek priority.)

B12 Our previous uncertainty regarding who got common (high-level astronomy) parameters from whom is eliminated by ACT #210, since it is a Babylonian text providing a parameter which is dependent upon and thus subsequent to a specific, dated twosome of famous, purely Hellenistic instrumental *observations* (Meton & Hipparchos). This is vastly more informative than a sharing of common parameters of unknown empirical origin, which might have been transmitted in either direction or be from an earlier mutual source.

B13 The Kugler-Neugebauer Babylonia-to-Greece presumption may ultimately have been due to little more than the very natural and human hopes of those making discoveries (among Babylonian cuneiform material) that their ingenious, hard-wrought finds represent original not merely secondary science. Another possible unconscious contributing factor: the greater antiquity of Babylonian civilization; but late Babylon had no sophistication in observational instruments or astronomical mathematics — which presumably explains why virtually all (if not precisely all) worthwhile orbital data on cuneiform texts date from after the Greek conquest of Babylon. (See Neugebauer 1955 1:xvi, 2:xii.)

C Hipparchos' Improved Solar Observations & Ultimate Orbit

C1 It is well known that at his career's peak, Hipparchos' instrumental equator (IE) was a few arcminutes low (see fn 13, and Rawlins 1982C p.370 & sources there cited), causing his Vernal Equinox observations to be early, his Autumn Equinoxes late. He also evidently observed a Summer Solstice (record not directly extant) in –145. (A solstice time measurement is unaffected by IE error; §A3. For an elucidating discussion of the distinction, consult R.Newton 1977 pp.81-82, 90.) Shortly thereafter, using this solstice and the 2 recorded equinoxes (*Almajest* 3.1) of the same year (3 empirical data), he founded his solar tables by the method explained in *Almajest* 3.4-7. The solar orbit thus established I am calling: the PH (Prime Hipparchos) orbit. The PH theory was identical[11] to the orbit preserved in the tables of *Almajest* 3.2&6, and treated by Ptolemy as the only Hipparchos solar orbit — mistakenly, as we are about to see.

C2 Hipparchos' last extant Autumn Equinox observation (–142/9/26 3/4) crucially snapped his equinoxes' pattern of systematic error (a point emphasized in R.Newton 1970 p.15): it was correctly observed as having occurred a $1^{d}/4$ notch earlier than indicated by the PH tables, themselves 7^{h} late at this moment. (The PH tables predicted Autumn Equinox at –142/9/27 0^{h} = midnight; for Earth-acceleration of §B2, the actual Autumn Equinox was at –142/9/26 17^{h} = 5 PM, within about an hour of the recorded Hipparchos observation.)

C3 Putting this notable –142/9/26 equinoctial improvement together with the fact that (as discovered above, §B3) Hipparchos' last known Summer Solstice (–134/6/26 1/4) was also rightly discordant by about $1^{d}/4$ with respect to the PH tables: we have a double suggestion that an astronomer as energetic as Hipparchos might well have tried to use his fresh data (both now more correct than his corresponding earlier material) for improving his original PH solar orbit and thereby creating an Ultimate Hipparchos orbit, a momentarily-hypothetical entity which I will henceforth refer to as the UH solar orbit.

C4 Fourteen Hipparchos Vernal Equinoxes survive (*Almajest* 3.1): first, –145/3/24 1/4; last, –127/3/23 3/4. (Note: the bounds are in the years ending at the PH & UH epochs, which independently suggests that those two VE data were utilized in the empirical foundations of the respective theories.) Unlike his Autumn Equinoxes: all are spaced Kallippically, i.e., at exact integral multiples of Y_{K}. Due to the 11^{m} excess of Y_{K} over the actual $365^{d}.2423$ interval between Vernal Equinoxes at that epoch, these Hipparchan Vernal Equinox observations got 11^{m} more accurate every year: 9^{h} early in –145, but only 6^{h} early in –127. Thus, since the –142 Autumn Equinox and –134 Summer Solstice were both correct to about 1^{h}, Hipparchos by –134 had in hand solar data averaging only 4^{h} off reality (rms) — vs. his prior (PH) orbit's foundation, where the errors were nearly 2 times larger.[12]

[10] The Greeks used noninstrumental Babylonian observations of eclipses and stations; but none of these borrowings establish parametric dependence on Babylon; to the contrary, all the old Babylonian data were used with current Greek observations to deduce new Greek parameters.

[11] The *Almajest* used epoch Nab 1, while Hipparchos formally used Phil 1 (fn 7), as did the Handy Tables (fn 12). The constant difference is under $0'.1$.

C5 The gist of the foregoing is that Hipparchos' last fundamental observations shifted (vs. the $365^{d}1/4$ interval Kallippic-Julian calendar) his Summer Solstice time & Autumn Equinox time back by $1^{d}/4$ each, while producing no such shift in the Vernal Equinox. Since Spring (V) lasts from the VE to the SS, Hipparchos had found his final value for Spring's length, V_{U}, to be shorter by $1^{d}/4$ than his PH orbit's value, V_{P}; since Summer (S) lasts from the SS to the AE (both shifted identically), he found no change in Summer.

C6 The famous season lengths from which Hipparchos had elicited his PH orbit's eccentricity e_{P} and apogee A_{P} were (*Almajest* 3.4):

$$V_{\mathrm{P}} = 94^{\mathrm{d}}1/2 \qquad \& \qquad S_{\mathrm{P}} = 92^{\mathrm{d}}1/2 \tag{11}$$

(Actual season lengths then: $V = 94^{d}$, $S = 92^{d}1/3$.) But the above-discussed shifts tell us that the UH figures Hipparchos later settled upon were:

$$V_{\mathrm{U}} = 94^{\mathrm{d}}1/4 \qquad \& \qquad S_{\mathrm{U}} = 92^{\mathrm{d}}1/2 \tag{12}$$

C7 Using the simple procedure of *Almajest* 3.4 (well explained by Neugebauer 1975 pp.58, 308, & 1221 Fig.53), one may find (from these 2 season lengths) the eccentricity e_{U} and apogee A_{U} of the final Hipparchos solar orbit, just as he would have derived it. His process started with the conversion of the Spring and Summer arcs from days into degrees of mean longitude, using mean solar motion F; from eq. 7:

$$F_{\mathrm{J}} = 360^{\circ}/Y_{\mathrm{J}} = 54000^{\circ}/54787^{\mathrm{d}} \tag{13}$$

Multiplying this motion times eq. 12 gave:

$$V_{\mathrm{U}} = F_{\mathrm{J}} \cdot 94^{\mathrm{d}}1/4 = 92^{\circ}54' \qquad\qquad S_{\mathrm{U}} = F_{\mathrm{J}} \cdot 92^{\mathrm{d}}1/2 = 91^{\circ}10' \tag{14}$$

Next were found (using Ptolemy's conventional 60^{p} = unity):

$$x_{\mathrm{U}} = 60^{\mathrm{p}} \cdot \sin \frac{V_{\mathrm{U}} - S_{\mathrm{U}}}{2} = 0^{\mathrm{p}}54' \tag{15}$$

$$y_{\mathrm{U}} = 60^{\mathrm{p}} \cdot \sin \frac{V_{\mathrm{U}} + S_{\mathrm{U}} - 180^{\circ}}{2} = 2^{\mathrm{p}}08' \tag{16}$$

So the UH eccentricity e_{U} was:

$$e_{\mathrm{U}} = \sqrt{x_{\mathrm{U}}^2 + y_{\mathrm{U}}^2} = 2^{\mathrm{p}}19' = 2^{\mathrm{p}}1/3 = 7/180 \tag{17}$$

And the UH apogee A_{U} was:

$$A_{\mathrm{U}} = \arccos \frac{x_{\mathrm{U}}}{e_{\mathrm{U}}} = \arccos \frac{0^{\mathrm{p}}54'}{2^{\mathrm{p}}19'} = 67^{\circ}08' = 67^{\circ} \tag{18}$$

[12] Errors of PH orbit in –145: VE, -10^{h}; SS, $+2^{h}$; AE, $+6^{h}$; rms = 7^{h}. (Due to rounding during the *Almajest* 3.4 mathematical deduction of the PH orbit, some of the $1^{d}/4$-rounded founding data's errors are slightly different: VE, -9^{h}; SS, $+3^{h}-$; AE, $+6^{h}$.) Parallel UH errors in –127: -6^{h}; 0^{h}; $+4^{h}$; rms = 4^{h}. For any year, the UH−PH differences are: VE, $+1^{h}$; SS, -4^{h}; AE, -5^{h}. If we have $\epsilon_{\mathrm{U}} = 180^{\circ}05'$ at –127/9/24 noon (eq. 28) and $\epsilon_{\mathrm{P}} = 227^{\circ}40'$ at −323/11/12 noon (Phil 1; see Neugebauer 1975 p.984), then for all time the mean longitude difference $f_{\mathrm{U}} - f_{\mathrm{P}}$ is $+4'.1 = -1^{h}.7$ (found from eqs. 13 & 24).

C8 By comparison, *Almajest* 3.4 has for the PH solar orbit (after applying the foregoing procedure to the data of eq. 11):

$$e_{\mathrm{P}} = 2^{\mathrm{p}}1/2 \qquad A_{\mathrm{P}} = 65°1/2 \tag{19}$$

And the real –130 values were:

$$e = 0.0351 = 2^{\mathrm{p}}1/10 \qquad A = 66°1/2 \tag{20}$$

All these e are defined as double what is modernly called e, since Hipparchos' solar theory used the eccentric model. The UH values for e & A are both more accurate than the PH values. Also, A is better than e, in both orbits (PH & UH).[13]

C9 The ancients reckoned mean solar anomaly g from the apogee A; thus (using eq. 18):

$$g = f - A \quad \text{so} \quad g_{\mathrm{U}} = f_{\mathrm{U}} - 67° \tag{21}$$

where f = mean longitude. The eccentric-model equation of center E is (using eq. 17)

$$E = -\arctan\frac{e\cdot\sin g}{e\cdot\cos g + 1} \quad \text{so} \quad E_{\mathrm{U}} = -\arctan\frac{\sin g_{\mathrm{U}}}{\cos g_{\mathrm{U}} + 180/7} \tag{22}$$

where, of course, the true longitude ϕ is:

$$\phi = f + E \tag{23}$$

and where

$$f = \epsilon + F\cdot d \tag{24}$$

(ϵ = mean-longitude-at-epoch; d = days since epoch.)

[13] Toomer 1984 p.153 n.46 defends Ptolemy's copying Hipparchos' A_{P} (65°1/2, in error by $-6°$, because obsolete after 280^{y} of equinoctial & apsidal precession), recommending the analyses of Petersen & Schmidt 1967, who assert (pp.74-83) that A_{P}'s original accuracy (at Hipparchos' epoch) was coincidental, as e_{P} was so poor. The point made is essentially true; however, the expected A error was under 4°, only 3/4 the expected e error. (See discussion below.) Thus, [a] Ptolemy's A error ($-6°$) was less excusable than indicated; and [b] the smallness of Hipparchos' A error (–1°) was fortunate, but not so unlikely as suggested on *ibid* p.83, which proposes at least a 14° interval in which A_{P} could easily fall by chance. This is a useful paper, but its pp.81-2 assume equal & independent (& large) errors for SS, VE, & AE, ignoring [a] IE error (which connects VE & AE errors; see above §A3 & §C1) as well as [b] superior SS accuracy (Rawlins 1985H). For predicting expectation-errors, we may compute using IE-related equinox error (from randomly mis-set IE) $u = 4^{\mathrm{h}}$ (R.Newton 1970 pp.11 & 15) and intrinsic SS random error $rs = 2^{\mathrm{h}}$ (Rawlins 1985H; also, contrast solstice & equinox accuracy in fn 12), adding in rounding errors (for $1^{\mathrm{d}}/4$ precision) rr = rms of deviations (uniform density in the interval $\pm 3^{\mathrm{h}}$) = $\sqrt{3}$ hrs. Since raw visual error in an equinox observation is trivial in the context of $1^{\mathrm{d}}/4$ rounding, it will suffice to set (the random equinox errors independent of u) $r_{\mathrm{V}} = r_{\mathrm{A}} = rr$; but for solstices, $r_{\mathrm{S}} = [rs^2 + rr^2]^{1/2}$hrs $= \sqrt{7}$ hrs. Empirical-observation expectations: $de/e = (F_{\mathrm{J}}/e)\cdot[(u\cdot\sin A)^2 + rr^2/2 + (r_{\mathrm{S}}\cdot\cos A)^2]^{1/2}$; $dA = (F_{\mathrm{J}}/e)\cdot[(u\cdot\cos A)^2 + rr^2/2 + (r_{\mathrm{S}}\cdot\sin A)^2]^{1/2}$. Thus, for Hipparchos' epoch (rendering overprecisely): de/e = 4°.7 & dA = 3°.7. (For Ptolemy's: de/e = 4°.8 & dA = 3°.6. Note that A is more accurate than e from A's proximity to SS, which lowers dA sensitivity to the dominant error-source u.) These standard deviations are statistically consistent with the actual UH orbit, where de/e = +6°, and dA = +1°/2. But the error in e_{P} is statistically significant for both epochs. (PH errors: de/e = +11° & dA = –1° for Hipparchos; de/e = +11° & dA = $-6°$ for Ptolemy.) The difference here is that Hipparchos eventually corrected his PH errors by years of honest outdoor labor (resulting in the UH orbit), while Ptolemy couldn't be bothered to do more than plagiarize the PH orbit (unaware that it was doubly obsolete). It should be added that Kallippos' 330 BC solar theory was superior to either the PH or the UH orbit (Neugebauer 1975 p.627 n.9, van der Waerden 1984-5 p.116).

C10 From eq. 21-23, f at the cardinal points of the UH solar orbit may be calculated:[14] $f_{\mathrm{VE}} = -2°03'$, $f_{\mathrm{SS}} = 90°52'$, $f_{\mathrm{AE}} = 182°03'$, $f_{\mathrm{WS}} = 269°08'$. (PH: $f_{\mathrm{VE}} = -2°10'$, $f_{\mathrm{SS}} = 90°59'$, $f_{\mathrm{AE}} = 182°10'$, $f_{\mathrm{WS}} = 269°01'$.)

C11 Thus, we know the mean longitude f for any observed cardinal time. (E.g., once the UH orbit is adopted, an observation placing the SS at –134/6/26 6 AM empirically sets f for that moment equal to $f_{\mathrm{SS}} = 90°52'$, only 0°.1 from the truth: 90°46'.) And the mean-longitude-at-epoch ϵ is thereby determined through eq. 24. (See fn 14. This is effectively the method of *Almajest* 3.7.) Since Hipparchan solar mean motion departs so little (under 2′) from Kallippic during a decade, ϵ is only slightly affected by the exact choice of epoch among Hipparchos' final few years of observational labors.

C12 We recall (§B8) that Hipparchos defended his famous yearlength $Y_{\mathrm{J}} = 365^{\mathrm{d}}14'48''$ (eq. 7) on 2 different occasions near the end of his life; thus, his UH value for F was very likely that of eq. 13, namely F_{J}.

C13 So we have now four UH orbital elements (e_{U}, A_{U}, ϵ_{U}, F_{J}) empirically established and-or adopted by Hipparchos late in his career. These constitute a complete determination of the UH solar orbit.

D The UH Orbit Restored to Life

D1 When I first noticed the fact that two of Hipparchos' 3 solar orbit cardinal cornerstones had shifted (some years after he had in –145 arrived at his PH orbit), I performed some of the above UH calculations (eqs. 12-18) in rough fashion (1985/3/12, scribbling right on p.58 of my copy of Neugebauer 1975) — but was too dumb & ignorant to see any way of testing the outcome, lazily supposing at the time that any evidence would have been interred along with the UH orbit itself (since Ptolemy preserves only the PH tables & parameters).

D2 But on 1986/5/15, while examining a list containing 3 very late Hipparchos lunisolar observations (R.Newton 1977 p.148), I was struck by some glaring discrepancies between Hipparchos' solar positions and values calculated from the PH tables. The magnitude (c.1°/4) of the discords (and the fact that they peaked in the Summer) naturally reminded even me of the UH theory. These three solar position data are provided in *Almajest* 5.3&5, and each is there subsequently recomputed (seemingly by Ptolemy; vs. §H5), virtually correctly, to agree with the PH tables. The 3 Hipparchan data ϕ_{i} are as follows [with Ptolemy's corresponding reported PH recomputations beside in brackets]:

$$\phi_1 = 128°7/12\ [128°1/3] \ \text{ at } -127/8/5\ 1/4 \tag{25}$$

$$\phi_2 = 37°3/4\ [37°3/4] \ \text{ at } -126/5/2\ 1/4 \tag{26}$$

$$\phi_3 = 100°9/10\ [100°2/3] \ \text{ at } -126/7/7\ 2/3 \tag{27}$$

D3 These 3 (unbracketed) solar true longitudes were Hipparchos' own calculated values, each used for setting ring 5 of his astrolabe (reference-object ring; see Fig.1 and Appendix A of Rawlins 1982C)[15] for a daytime measurement of the lunar longitude.

[14] E.g., $f_{\mathrm{AE}} = 182°03'$ in eq. 21 produces g_{AE} = 115°03'; this in eq. 22 yields $E_{\mathrm{AE}} = -2°03'$. Therefore, from eq. 23, we obtain $\phi_{\mathrm{AE}} = f_{\mathrm{AE}} + E_{\mathrm{AE}} = 182°03' + (-2°03') = 180°$, which is the very definition of the AE. Presuming an accurate Hipparchos AE observation at –127/9/26 1/2: from eq. 24, mean-longitude-at-epoch $\epsilon_{\mathrm{U}} = 182°03' - F_{\mathrm{J}}\cdot 2^{\mathrm{d}} = 180°1/12$ for UH epoch Phil 197 (eq. 28), 2^{d} earlier. (I suggest in §F4 that this is the Star Catalog's formal epoch. Compare *Almajest* 7.3, 5.3, and 3.1 dates.) PH's ϵ_{P} from –145/9/27 1/4 AE: $\epsilon_{\mathrm{P}} = 182°10' - F_{\mathrm{J}}\cdot(-6572^{\mathrm{d}}1/4) = 180°$ exactly (instead of $\epsilon_{\mathrm{U}} = 180°05'$) at –127/9/24 epoch (correct within 1′), a neat number which could help explain later general preference for the PH orbit.

[15] Doubtless without the slightest relation to vengeance, the 1987/8&11 issues of the allegedly space-tight *Journal for the History of Astronomy* (*JHA*) spent a chaotic 81 pp. (using contributions by 3 authors) — over 25% of the entire *JHA* regular 1987 output! — attacking Rawlins 1982C (& R.Newton 1977 pp.245-254). All this was arranged and

D4 The solar $\phi_{\rm i}$ are the only such records we have from Hipparchos that were computed at a known date[16] and all are from the conclusion of his empirical work. Indeed, they are embedded in the very last three precisely dated observations we have inherited from him. So they are ideal for testing the theory of the existence of the UH orbit.
D5 From any ϵ determined by $1^{\rm d}/4$-rounded Hipparchan cardinal-point observations (Kallippic-interval-accordant with the improved data of §C4) for about the year –130, we calculate $\phi_{\rm i}$ values from the UH orbit (for the 3 times given in eqs. 25-27) and thereby encounter the delightful result that in all 3 cases the computations agree to about $1'$ with the values given by Hipparchos and relayed in *Almajest* 5.3&5. For context, it is important to realize that 2 of these 3 longitudes were formerly believed to be grossly discrepant (ϕ_1 by $+15'$ and ϕ_3 by $+14'$; see eqs. 25 & 27) because they were supposed to have been calculated from the PH solar tables of the *Almajest*.
D6 Though computations of E can be rough by about $1'$ from tabular interpolation, I will nonetheless be precise (using the rigorous eq. 22) while here seeking the epoch Hipparchos adopted for the UH orbit. Examining the reported fractions of degrees (eqs. 25-27), we can see that $1'$ differences are important in this search because: had ϕ_1 come out equal to $128°36'$, it would have been expressed as $128°3/5$, not $128°7/12$ as reported in *Almajest* 5.3; were ϕ_2 equal to $37°47'$, *Almajest* 5.5 would have $37°4/5$ rather than $37°3/4$; had ϕ_3 been $100°52'$ or $55'$, *Almajest* 5.5 would say $100°5/6$ or $11/12$, instead of $100°9/10$.
D7 These considerations, and awareness of the ancient practice of adopting mean-longitude-at-epoch ϵ rounded to the nearest $1°/12$ (a point much developed in Rawlins 1985K), assist in delimiting possible epochs. The most probable candidate[17] occurs in 128 BC (noon here refers to Alexandria or Rhodos local apparent noon):

$$\epsilon_{\rm U} = 180°1/12 \ \text{ at } \ -127/9/24\,1/2 = \text{Nab 621 or Phil 197 Thoth 1 noon} \tag{28}$$

This $\epsilon_{\rm U}$ was off reality by $+4'\pm1'$ in –127; same error as PH's $\epsilon_{\rm P}$ in –145. (See fn 13 & data of fn 12. Mean equinox error is in both cases about $-1^{\rm h}1/2$, which is $+4'$ in f.)
D8 Note: –431, –279, & –127 are at two-Kallippic-cycle intervals. So, Hipparchos presumably intended to found his own calendar: $304^{\rm y}$ after Meton, at the epoch –127.[18]

published (at vast expense in effort, funds, & page-space) just as an unexpected new independent proof of Rawlins 1982C's central thesis appeared (§F5). Nice timing. The self-evident flaw, in the sole coherent pro-Ptolemy point made by the *JHA* assault, was swiftly exposed by K.Hertzog *QJRAS* *29*:279; 1988/6. (This only goaded Ptolemists into 3 more try-anything rear-guard meanderings, attempting to alibi Ptolemy on the Star Catalog matter, all 3 appearing in the 1990 output of O Gingerich's incurably partisan *JHA*. See also Graßhoff 1990.) *JHA*'s massive 1987 offensive was launched though: [a] no undoing DR errors are found, and, in a perfect expression of the wellknown British sense of fair play, [b] DR is barred from appearing in the very *JHA* that attacks him. In a 1983/3/3 letter, the *JHA*'s coolheaded Editor-for-Life (EfL) told DR never again to submit a paper to the extremely handsome *JHA* and intimated a libel action — all because DR had committed the unforgivable offense of pointing out the baselessness of a 1982/10 *JHA* paper. (See ‡8 fn 35, and ‡1 fn 25.) To replace *JHA*-referee-approved-&-accepted Rawlins 1999 (which the EfL personally despised & so had already held up for nearly a year), the recently-received 1982/10 paper (suitably mild in its criticism of Ptolemy) had been suddenly rushed to press by the EfL over the protests, of *JHA*'s own 2 referees, that its conclusions were unbelievable. (Yet further prescient EfL timing: just after EfL's suit-threat, the honest author's creditable retraction arrived on the desk of a now-even-further-enraged EfL. At this contretemps, the EfL's formerly hurried pace suddenly went glacial, thus postponing the retraction's publication until the 1984/6 *JHA*!) Just another enlightened episode in a proud Hist.sci community's ongoing demonstration of its academic idealism.

[16] The specialized Hipparchos equinox-solstice data of *Almajest* 3.1 are observed, not calculated. Previously, we did not know when the six solar positions of *Almajest* 4.11 were computed. They are all consistent with the PH solar orbit, so we may now say that these calculations preceded –127/8/5 (see §E1). [Misread corrected *DIO 1.3* fn 198.]

[17] Hipparchos' computation of $\epsilon_{\rm U}$ is reconstructed in fn 14. Note that the *Almajest* wrongly assumes Rhodos' longitude equals Alexandria's (Toomer 1984 p.225 n.16) and uses the equation of time solely for the Moon (not the Sun, though this habit was perhaps inadvertently reversed for the –126/7/7 observation; Toomer 1984 p.230 n.23).

[18] Hipparchos' cycle = 4 Kallippic cycles = $304^{\rm y}$ = $111035^{\rm d}$ (Heath 1913 p.297 or Neugebauer 1975 pp.297 & 624). If this cycle started at the epoch of eq. 28, then he figured it & Kallippic cycles from Thoth 1, as suggested at

D9 Below, I calculate (via eqs. 13, 21-24, 28) the UH solar longitude $\phi_{\rm i}$ ($f_{\rm i}$ & $E_{\rm i}$ computed precisely before $1'$ rounding), for each of the 3 times given in eqs. 25-27 (result then rounded according to ancient astronomical convention):

$$f_1 + E_1 = 130°33' - 1°58' = 128°35' = 128°7/12 = \phi_1 \tag{29}$$
$$f_2 + E_2 = 36°41' + 1°05' = 37°46' = 37°3/4 = \phi_2 \tag{30}$$
$$f_3 + E_3 = 102°08' - 1°15' = 100°53' = 100°9/10 = \phi_3 \tag{31}$$

D10 Each of these UH results is identical with the corresponding reported Hipparchos value (eqs. 25-27), which leaves no doubt that the UH orbit really existed and that Hipparchos himself used it to compute these three $\phi_{\rm i}$ at the time — just before going outside to observe the Moon. And we mustn't forget that our success in connecting Hipparchos' three $\phi_{\rm i}$ data to the UH orbit also reconfirms the dependence of ACT #210 upon him, since it was that invaluable Babylonian text which provided us (§B1) the hour of the Hipparchos –134 solstice and thereby made possible our complete reconstruction here of the UH orbit.[19]
D11 Likewise, the false hour 6 AM (eq. 6) reported in *Almajest* 3.1 (c.150 AD) for Meton's solstice is shown to have been accepted between 135 and 68 BC (§B9), though (Rawlins 1985H) it was not known to Kallippos (330 BC) or Aristarchos (280 BC).[20]

E The UH Orbit's Accuracy & Fate

E1 The UH theory of the Sun was adopted by Hipparchos sometime between –134/6/26 (eq. 6) and –127/8/5 (eq. 25). It roughly halved the rms error of the old PH solar tables relayed in the *Almajest* — and virtually eliminated the prime source of error for eclipse-times, since the periodic error in the UH solar motion was very nearly matched by the then-unknown annual term of the lunar motion. The impressive accuracy of the UH eclipse theory must (if the solar orbit empirical foundation was indeed equinox-solstice observations) be partly just chance; but it is striking nonetheless. During eclipses, the largest term of the lunar theory's longitude error (sign convention: Hipparchos-minus-real) was annual: $-14'\sin g$, where g = solar anomaly. The next-biggest missing syzygial lunar terms possess amplitudes $5'$, $4'$, $3'$, and $2'$. The predominant term of the UH solar longitude error was $-13'\sin g$ (vs. the corresponding PH orbit error term: $-23'\sin g$; see §F3 & §F1); and no other UH solar error term's amplitude exceeds $1'$. Thus, since the $-14'$ and $-13'$ terms virtually cancel, the UH theory predicted eclipses with (noting the other terms, & using eq. 10):

$$\text{error} = \sqrt{(5'^2 + 4'^2 + 3'^2 + 2'^2)/2} \cdot M/360° = 10^{\rm m} \tag{32}$$

(vs. $16^{\rm m}$ rms error for the PH orbit's eclipse predictions).
E2 Whether the UH theory was ever published is doubtful. Ptolemy's innocence of it proves nothing.[21] But there is other evidence.

Toomer 1984 p.214 n.72 (though with a $1^{\rm y}$ base discrepancy: fn 27). If the traditional SS was used instead, then the epoch was the UH (& real) SS at $-127/6/26\ 0^{\rm h}$. (Against SS-base: [a] The entailed $\epsilon_{\rm U} = 90°52'$, which is not near a rounded fraction of a degree. [b] The interval since –431/6/27 1/4 is $1^{\rm d}/4$ short of $111035^{\rm d}$. [c] Fn 27.)

[19] The foregoing analyses, down to this point and through §E1, were briefly set forth in a 1986/5/19 letter to Curtis Wilson, 4 days after the discovery of eqs. 29-31.

[20] The eq. 6 Meton date was known to both men (Rawlins 1985H). Also known in 109 BC (R.Newton 1977 p.95). The original Meton solstice was correctly recorded as occurring on the Athenian day starting –431/6/27 6 PM; but typical calendar-convenient adoption of the day-start as SS (rather than the actual SS hour, –431/6/28 10 AM) produced the usual negative truncation-error in the recorded SS (a practice 1st recognized at Rawlins 1985H): $-16^{\rm h}$ in the –431 instance. (See below at §E5.)

[21] E.g., he also never knew that the mature Hipparchos had recomputed his prior klimata table on the basis of a correct obliquity value, not the erroneous one Ptolemy attributes to him: Rawlins 1982C p.368. Note that even

E3 We know (§B9, §D11) that someone in roughly 100 BC used the Meton solstice of −431, with the exact same (terribly incorrect) dawn hour later reported by Ptolemy. There is a problem here (justly emphasized by R.Newton 1977 p.95): how could the famous Meton solstice's hour (eq. 6) have been in perfect agreement with the PH tables (even while in outrageous discord with the real sky: -28^h error!) — though the PH tables did not exist and were not accurate until nearly 3 centuries later? The coincidence has suggested to some (R.Newton 1977 p.96 & Rawlins 1985H, contra §E5 here) that the Meton solstice's conveniently false hour was not observed but was fabricated sometime after –145 from the PH tables.[22]

E4 Regardless (& I now doubt fabrication here: §E5), the Meton date & hour of eq. 6 existed well prior to Ptolemy (as found in §B1), who is not responsible for any of the confusion regarding the Meton solstice. (R.Newton 1977 p.96 earlier guessed he was not. Rawlins 1985H demonstrated it.) And, though the eq. 6 date was used continuously, the eq. 6 hour first appeared between 280 BC and 68 BC (§D11), probably about 146 BC (Hipparchos: §E5).

E5 Rawlins 1985H innocently explains (& thus accepts as real) the date of the Meton solstice. (See above, §D11.) I have since decided that it is not necessary to assume fabrication for the hour either, because this can be accounted for as merely a Hipparchan warp of prejudice. When constructing his PH solar orbit (146 BC; fn 7), Hipparchos would have been delighted to confirm the lunisolar-calendar-convenient false tropical yearlength of Aristarchos-Sudines (Rawlins 1999; Hipparchos later rounded this value trivially, to eq. 7). That encouraged Hipparchos to read "morning" for Meton's reference to his solstice having occurred at the "start" (αρχην) of the day[23] (by which Meton meant 6 PM, since the Athenian day began at dusk). This hypothetical Hipparchos miscue would append a -12^h misinterpretation-error to the -16^h truncation-error (Rawlins 1985H) that had already attached to the Meton solstice, probably from the outset (−431; fn 20) and certainly by 330 BC (*idem*). All of which left the now-notorious total of -28^h off: a gross error — but the 6 AM Meton hour adopted (eq. 6) was attractively consistent with the PH solar theory (which was based on Hipparchos' solar observations in –145, and the by-then long-established 3^{rd} century BC Aristarchos-Sudines yearlength effectively preserved by Hipparchos in eq. 7; see fn 22).

E6 When he died c.127 BC, Hipparchos was presumably working at an improved lunar theory (thus the quadrant observation of *Almajest* 5.3 and the octant data of *Almajest* 5.5),[24] perhaps planning to publish it and the UH solar orbit together as a lunisolar unit. Instead, his PH solar tables became standard throughout the pagan world community, even as late as the 4^{th} century era of Julian the Apostate and Theon of Alexandria. Had Hipparchos ever issued something so basic as an improved solar orbit, such would likely have long since been generally adopted in place of the PH calendar. It is regrettable that Hipparchos probably never published the UH orbit, since its periodic errors were barely half those of the PH solar tables that became canonical among astrologers for the worst part of a millennium.

nonmathematician Strabo was aware of the later klimata table: see the admirable analysis by nonmathematician Diller 1934, which Neugebauer 1975 p.734 n.14 typically damned as incompetent & "absurd" — a cocksure denigration published, ironically, just before Diller's triple independent vindication by Rawlins 1982C p.368 and Nadal & Brunet 1984 p.231 n.17.

[22] Pre-empirical Hipparchan adoption of PH's eq. 7 was perhaps via Sudines, c.240 BC (Rawlins 1999, & see Neugebauer 1975 p.624 & 574).

[23] See, e.g., the possibly-revealing *Almajest* 3.1 language at Ptolemy's 2^{nd} mention of this solstice's hour. Toomer 1984 p.139 innocently obscures the matter by presumptively translating αρχην as "dawning" (just as I suspect Hipparchos did). All other translators scrupulously retain the original meaning: see Manitius 1912-3 1:144; also Halma 1:163, and Taliaferro (Great Books v.16) p.82.

[24] Can one imagine a genuine observer (which Ptolemy pretends to be, throughout the *Almajest*) using 3-century-old data to establish fine details of the Moon's oscillations about its mean motion? Equally obvious giveaway symptoms of Ptolemy's innocence of real astronomy (e.g., fn 2 & fn 37; and Rawlins 1985G & Rawlins 1987) make equally little impression on the equally indoor Muffia.

F Unexpected Fruit

On 1986/11/20, about 2 months after sending the foregoing discussion (nearly as it appears above) to B.van der Waerden & R.Newton, I followed up with a letter to R.Newton (copy to BvdW), from which most of the rest of this section (& the next) is taken, with some revision. The letter carried news of a pleasant discovery: fresh confirmation (1986/10/29) of the Ultimate Hipparchos Orbit.

F1 The prime dubious point in my detailed analysis of Hipparchos' Ancient Star Catalog (Rawlins 1982C)[25] was its attribution (pp.366-371) of the Sample A (zodiacal stars) longitude error curve (solid line in Fig.3, *ibid*) to pre-solar-theory use of raw equinox observations for zero point: if intelligently applied, this method would more likely produce a zigzag or step-function error curve, not the sinusoid that is the case (a point I found puzzling at the time: Rawlins 1982C p.370). But the Hipparchos (PH) solar theory periodic error was about $-23'\sin(f-62°)$, while $(-12' \pm 1')\cdot\sin(f-92° \pm 3°)$ was the Star Catalog's periodic error (*ibid* p.376 Table IV). The amplitudes were incompatible. So, believing (when I wrote Rawlins 1982C) that there was but one Hipparchos solar orbit, I could make no progress in relating the Sample A longitude error curve to a Hipparchos solar orbit error curve.

F2 [Subsequent to this 1986/11/20 letter, a fresh DR study of the Catalog zodiacal stars, using the constellations as weighted normals and dropping discordant Cap, finds (for –127): mean error $z = -10' \pm 1'$ (vs. $z = -8' \pm 1'$ from the analyses of Rawlins 1982C pp.367&9), periodic error $(-14' \pm 1')\cdot\sin(f-101° \pm 6°)$. This solution is slightly different from — though statistically consistent with — the Rawlins 1982C solution just given in §F1. Both solutions are quite incompatible with the PH orbit's error curve (§F1), though their amplitudes are close to that of the UH orbit.]

F3 The Rawlins 1982C incompatibility problem now evaporates. The Ultimate orbit (UH) periodic error curve was about $-13'\sin(f-71°)$. The amplitude's match to that of the Star Catalog error curve is lovely! Also, I see that the epoch I proposed (eq. 28: –127/9/24 1/2), for the UH orbit, in the ms (i.e., the above paper, §A-§E, sent RRN & BvdW 1986/9/16), is almost exactly the anciently accepted date of the Star Catalog.[26]

F4 For, in a previously disputed[27] passage, *Almajest* 5.3 says –127/8/5 is in the 50^{th} year of the 3^{rd} Kallippic cycle, which is the very same Kallippic calendar year Ptolemy believes

[25] Revision to another overt Rawlins 1982C speculation: most of the Catalog (outside of Samples C & A) was observed using Hipparchos' pre-135 BC solar theory & obliquity. (And a few areas' star positions were based on rounded transit data, e.g., Ara, PsA inf, and parts of Argo & Cen.)

[26] The UH epoch, –127/9/24 noon (eq. 28), was 264 Egyptian yrs (264^E) before Antoninus 1 = +137/7/20 noon, Ptolemy's star data epoch (*Almajest* 7.4), whereas Ptolemy says in *Almajest* 7.2 that the interval was about 265^E. *Almajest* 7.1 says about 260^E; *Almajest* 7.3 says 265^E, but the concomitant use of $2°2/3$ precession implies 267^E & thus a Star Catalog epoch of –130 (which I believe was the Catalog epoch effectively adopted by Ptolemy when he dealt with precession corrections; see Rawlins 1985K & fn 14).

[27] Since –127/8/5 is in the 51^{st} year of the 3^{rd} of Kallippos' original tropical-year cycles, calendar-specialists have been tempted to alter the *Almajest* 5.3 text. Toomer 1984 p.224 n.13 (& p.13) carefully makes it clear that these attempted emendations of the unambiguous text have no support in the *Almajest* mss. As I realized only very recently (1988/12/5), the discrepancy that has upset scholars for so long entirely vanishes if, in Hipparchos' calendar, the 50^{th} Kallippic year was Egyptian in length & ended in –127 not at the SS but at Thoth 1 — which is a natural consequence of eq. 28 (see fn 18). Toomer 1984 p.214 n.72 perceptively makes just such a suggestion for Hipparchos' Kallippic dates of 201-200 BC (*Almajest* 4.11) — but is forced by the data to set forth a scheme which (unlike that I propose above for –127) has the tropical & Egyptian versions of the same-number Kallippic years only barely overlapping, which suggests that it is off by 1^y. By coincidence, as Toomer 1984 p.224 n.13 rightly realizes, his numbering-scheme differs by 1^y from the foregoing one — which exhibits far better same-number overlapping and, as we found (above), perfectly explains the hitherto-troubling –127 Kallippic date at *Almajest* 5.3. Toomer's 1^y calendaric discrepancy may just be from an ancient confusion about the 201-200 BC data (presumably due to the switch from SS to Thoth 1). Or, conceivably, a Hipparchan numbering shift occurred between 200 BC & 128 BC, due to the difference in length of Egyptian & Hipparchan years (possibly with respect to a longer cycle, say 8105555^d). In any case, we cannot now improve on the closing remark of Toomer 1984 p.224 n.13.

was the Star Catalog's epoch, as we see from the *Almajest* 7.2 date of Hipparchos' Regulus longitude (119°5/6, identical to the Catalog value).

F5 The relating (§F3) of these error-curves: [a] adds yet another obvious proof[28] to the overflowing arsenal of evidences (e.g., R.Newton 1977 p.250, Rawlins 1982C) that Hipparchos (UH), not Ptolemy (PH), was the Catalog's true observer, and [b] has made possible the completion of my reconstruction of Hipparchos' Catalog compilation process. I noted (Rawlins 1982C p.373) that Ptolemy's alleged use in *Almajest* 7.2 of a huge elongation from Sun to Regulus (when determining Regulus' longitude) was folly since it only accentuates (by accumulation) the physical imperfections in the astrolabe's ecliptic ring.[29] (And, of course: had principal stars — or ordinary catalog stars — been fixed using elongations of large and thus virtually random size, as Ptolemy falsely indicates in *Almajest* 7.2, then there would be virtually no periodic error at all in the Catalog.) Hipparchos did the job the right way, keeping the elongation to a minimum — thus unwittingly preserving the UH solar theory error curve's amplitude (as we saw above in §F1-§F3: $13'$ agrees very nicely with $12'$ or $14'$, both $\pm 1'$), as well as keeping the UH-to-Catalog phase shift fairly small (c.20°-30°).

G Hipparchos' Observing Routine

Hipparchos' astrolabe procedure for locating his principal stars' positions with respect to the Sun (using the Moon as a stepping stone, as described in *Almajest* 7.2):

G1 Hipparchos virtually always found his Sample A principal stars at sunset, not sunrise.[30] (That accounts for the phase shift being positive with respect to the Star Catalog phase of §F1: $92°-71° = +21°$; or, for the alternate solution of §F2: $101°-71° = +30°$.) Which tells us something about his sleeping habits!

G2 In RA, the principal star being observed was (on average) about $1^{\rm h}1/2$ (= 22°1/2) or $2^{\rm h}$ (= 30°) east of the Sun. This explains very nicely the shift in phase from 71° (UH orbit) to 92° or 101° (Star Catalog). And it tells us that the stepping-stone Moon was ordinarily a very young waxing crescent (c.$2^{\rm d}$ old), right next to the desired star.

G3 Each step in the principal-star-fixing-process involved setting ring 5 on the reference object (Sun in Step 1, Moon in Step 2 — see Rawlins 1982C App.B), then clamping the unit comprising rings 3 & 4, i.e., freezing axis dd and quickly turning ring 2 to line it up so that the desired star (being located by this procedure) seemed visually to "adhere" to ring 2's side (as *Almajest* 5.1 speaks of ring 5's use). (No need for sighting the star through pinnules; too time-consuming, and latitude already known from older Sample A′: Rawlins 1982C pp.367 & 369.)

G4 The longitude of each catalog star is based on 3 astrolabe observations (except the few principal stars: 2 observations each):[31] Sun to Moon; Moon to principal star; principal star to ordinary star being cataloged. (See Rawlins 1982C App.B.) For 128 BC (eq. 28: more exactly, –127/9/24 1/2 = Besselian date –126.278), there is a systematic longitude discrepancy (between the Star Catalog & the UH orbit) of about $-13'\pm3'$: the $-9'\pm2'$ mean Star Catalog error[32] (average of the two z estimates of §F2) minus the UH orbit's own mean $+4'\pm1'$ error (§D7). According to eq. 1 of Rawlins 1982C p.361, the longitude differential $-13'\pm3'$ corresponds to systematic net lateness $57^{\rm s}\pm13^{\rm s}$. So, since there are 3 observations involved for each star (as just explained), we see that Hipparchos' average time between clamping axis dd and fixing ring 2 onto any desired celestial object was $19^{\rm s}\pm8^{\rm s}$. (I have here conservatively tended to round these random error calculations on the high side.) Reasonable.

G5 It's remarkable that all this detailed knowledge about a wellknown Hellenistic astronomer might never have come to light, were it not for a single precious Babylonian cuneiform text: ACT #210.

H Postscript

Two prior scholars deserve credit for getting close to discovering the UH orbit.

H1 Hartner 1979 p.18 analysed[33] $Y_{\rm B}$ and went as far as realizing that a factor of 99 was involved in its remainder, supposing that the originators of $Y_{\rm B}$ had founded it by using data separated by $99^{\rm y}$ and expressed to a precision of $2^{\rm h}$ (or $8^{\rm h}$). Had he added in the Greek habit of rounding to $6^{\rm h}$, he would have tripled $99^{\rm y}$ to find $297^{\rm y}$, which would have probably led to his finding the dependence of ACT #210 upon Greek data.

H2 I only recently noticed that Britton 1967 pp.45-47 actually proposed that the 3 solar data of *Almajest* 5.3&5 show that Hipparchos had a different solar orbit than that used by Ptolemy. But Britton then for some reason states (p.47) that these 3 data do not provide enough information to reveal the orbit. So I tried a solution based just upon the 3 Hipparchan $\phi_{\rm i}$, & thereby discovered that some idea of the UH orbit could in fact have been attained from them alone.

H3 Any alteration in the mean motion F would have a negligible effect upon the spacing of these data; thus, the precise values of only 3 solar orbital elements are contingent upon the three $\phi_{\rm i}$ (unbracketed values in eqs. 25-27), a situation which permits a determination of these elements from the data. Allowing for $1'$ error in the 3 data, our solution finds: eccentricity $e = 2^{\rm p}19'\pm02'$; apogee $A = 69°\pm2°$; mean-longitude-at-epoch $\epsilon = 180°01'\pm04'$.

H4 The results of the solution are statistically consistent with the UH orbit. However, since all 3 data are bunched in only about 1/4 of the zodiac, the unavoidable $1'$ data uncertainties introduce disappointing lassitude into some elements. (One cannot expect here the precision attained above in §C7, where we used sharply defined seasonlengths covering 1/2 the zodiac.) However, the solution for e is utterly incompatible with the PH orbit. (Thus, just from the 3 *Almajest* data he was commendably the first to propose the significance of, Britton could have found at least this element of the UH orbit to high precision, and could additionally have realized that this orbit's A was probably higher than the PH value.)

H5 I do not criticize either Hartner or Britton, especially since a 2nd look at these 3 data, triggered by reading Britton's near-miss, turned up (1988/7/7)[34] a highly revealing

[28] Which millennium will see Muffia acknowledgement of this in any of its various captive journals?

[29] Ring 3. See Rawlins 1982C Fig.1 (or Toomer 1984 p.218 Fig.F, where ring 3 is unfortunately drawn not quite perpendicular to axis ee). And note that near-syzygy is the region where Hipparchos best knew the Moon's motion (though his lunar theory is used only differentially for astrolabe star-locations, as in *Almajest* 7.2).

[30] Tiny Sample B (14 principal stars: Rawlins 1982C pp.366-7) is much less consistent than Sample A, so it may be hybrid. The poor definition of Sample B's phase may also have been affected by separate (non-A) positionings of some stars (e.g., Regulus) and-or by a hypothetical traditional demand that the longitudes of Aldebaran & Antares (cardinal ecliptical stars) be exactly 180° apart — which, incidentally, they really were, within $1'$, for roughly $1500^{\rm y}$ starting about 300 BC. Since these 2 stars can never be seen simultaneously from the Mediterranean area, this striking knowledge (precisely embedded in Hipparchos' Star Catalog) provides yet another hint suggesting the existence of accurate empirical ancient astronomy.

[31] For –127, the z for Samples A and B are indeed roughly in a ratio of 3 to 2; however, Sample B is not large enough to permit us to call this a statistically significant confirmation.

[32] I tentatively used a similar constant on p.369 of Rawlins 1982C to determine (assuming null systematic error in z) the "formal" epoch of Sample A as about –136, a figure I now withdraw.

[33] On 1982/1/15, O Gingerich stated approvingly that Hartner 1979 was regarded by the Muffia as symptomatic of its author's incipient senescence. (Like van der Waerden, Hartner always tried to ignore such clutter, to credit Neugebauer for his contributions.) Just customary Muffia intellectual generosity — and this from a clique that still (Neugebauer 1975 p.528) thinks $Y_{\rm B}$ is sidereal!

[34] Looking back to my first discovering the connection of ACT #210 to Hellenistic data, I see that my evolving awareness of the evidence for the UH orbit encompassed at least 6 independent discoveries, accomplished over more than 6 years of research: §A6 1982/1/28, §B1 1985/1/25, §D1 1985/3/12, §D10 1986/5/15, §F3 1986/10/29, & §H5 1988/7/7. The molassian slowness of wit thus revealed, is still another reason why I am disinclined to be unsympathetically critical of predecessors working at problems which turned out to be related to the UH orbit.

item which I had myself previously overlooked.[35] Ptolemy provides what purport to be his own PH orbit computations of the three *Almajest* 5.3&5 solar data, finding agreement with Hipparchos' values in but 1 case, as already noted. But he here makes an awful blunder: in this single agreeable instance (observation #2), the mean longitude f_2 he displays (at *Almajest* 5.5, in a context of $1'$ precision) is $36°41'$, which is the UH value (eq. 30) on the nose! (The UH theory gives precisely $36°40'.6$; eqs. 13, 24, & 28.) And this f_2 is patently incompatible with the PH tables Ptolemy allegedly used in his computations. (The PH tables of *Almajest* 3.2 give mean longitude $36°36'.4$; using the Phil 1 ϵ_P of fn 12 yields $36°36'.5-$.)

H6 The truth of the matter is self-evident: Ptolemy, a plagiarist of occasionally catastrophic carelessness (R.Newton 1977, Rawlins 1985G p.266, Rawlins 1987),[36] learned ahead of time that the 2nd of the three *Almajest* 5.3&5 Hipparchos solar data (for ϕ_2; eq. 26) did not disagree with the PH orbit. (The UH-minus-PH discrepancies in f & E happen to nearly nullify each other at this point in the solar orbit. Sheer accident, but likely seen by Ptolemy as just a case where Hipparchos didn't miscompute, since Ptolemy clearly saw the discrepant values, ϕ_1 & ϕ_3, eqs. 25 & 27, as mere calculating errors by Hipparchos.) Believing therefore that ϕ_2 didn't require recomputation, he in this sole case simply copied Hipparchos' figures (for ϕ_2 & f_2) directly into the *Almajest* without alteration.

[35] Yet another illustration of my manifold limitations is afforded by my persistent blindness to the explanation for Ptolemy's $1°$ shift in the Hipparchan lunar nodal motion (over his 311784^d interval, an equation I imparted to O Gingerich on 1983/6/6), given right in *Almajest* 4.9, which I prejudicedly ignored until having it pointed out by the generally excellent analysis of Hamilton, Swerdlow, & Toomer 1987. (Equation & interpretation preview-published in Toomer 1984 p.205 n.51 and Swerdlow & Neugebauer 1984 p.405 n.5. My 1986/5/19 & 6/19 notes to Hamilton, voluntarily acknowledging the superiority of HS&T's interpretation and requesting the date of Hamilton's discovery of the 311784^d equation, have not been replied to.) Toomer&co were unquestionably the 1st modern scholars to realize that Ptolemy's Canobic Inscription preceded the *Almajest*. This recent paper's perspicacity is blemished only by repetition (p.65) of the suggestion by Swerdlow & Neugebauer 1984 p.405 n.5 that the Canobic Inscription's Mercury model might be normal (i.e, no crank), which if true would require this model to be utterly incompatible with Ptolemy's own crucial (*Alm*-model-foundation-stone) *Almajest* 9.10 Mercury position of 139/5/17, allegedly observed (1st-hand by him) more than 7 years before the CanInscr model was published (see Rawlins 1987 pp.236-237). Indeed, without a crank, tabular Mercury never attains (within $0°.4$) geocentric longitude $77°1/2$ (reported in *Almajest* 9.10) at *any* time during its 139/5 swing around its stationary point. Thus, as a check would swiftly have shown HS&T, the math of *Almajest* 9.10, applied with a null crank radius, necessarily produces an imaginary solution for Mercury's synodic longitude ($24°56'$ + arcsin$[22^p56'/22^p30']$).

[36] For a potentially agnostic interpretation of the Mars period relation of Rawlins 1987, see Swerdlow 1973. This review (in *Isis*, the US' most prestigious Hist.sci journal) is so joyfully busy with the cute details of expressing its author's characteristically amiable & openminded reaction to dissent that, despite his distaste for "careless and unreliable" , "absurd", and "intelligence-insulting" scholarship (HamSwerdlow 1981 p.60-2 — published in *JHA*, which pretends to reject mss for strong language!), Swerdlow inexplicably [a] inverts his central conditional, and [b] attacks van der Waerden 1970 for taking Indian use of s as suggesting heliocentrism, though van der Waerden 1970 actually (p.30) instead points to λ.

[Note added 2003. The 1991 original of this footnote used DR's now-discredited argument that the *Almajest* 9.4 Mars tables were based upon an integral number of longitudinal (heliocentric) instead of synodic cycles. Though the tables indeed turn out (*DIO 11.2* p.30 & ‡4 fn 21) to be based upon longitudinal (not synodic) revolutions, the mechanism is not that originally proposed by DR.

For firmer indication of ancient heliocentrism's vitality (and counter-evidences), see: *ibid* §§G3-G4. See also Rawlins 1987 p.238 item IV[c], or here at ‡7 fn 8 & §§F3-F4, and *Almajest* 3.1's reference to "the school of Aristarchos".]

H7 The upshot is embarrassing for Ptolemy & the unfalsifiably ineducable[37] Hist.sci archons who have (originally with the best of intentions, one assumes) by now spent decades irrevocably committing their insecure reputations for sound judgement to the outlandishly ironic proposition that Ptolemy was the Greatest Astronomer of Antiquity (Princeton Institute's Neugebauer 1975 p.931, echoed verbatim by Harvard-Smithsonian's Gingerich 1976 & Gingerich 1980 p.264) and who have consistently fled a decade of challenges (e.g., Rawlins 1987 p.236) to face-to-face debate of the Ptolemy Controversy. But Ptolemy's giveaway f_2 oversight is fortuitously useful in that [a] it demolishes the sole glimmer of a potential last-ditch counterargument to the UH orbit's reality (namely, that at least one of Ptolemy's PH calculations agrees with Hipparchos: ϕ_2, eq. 30), and [b] it preserves unsullied the original rendition of Hipparchos himself — and this is a wonderful further verification of the UH orbit's use by Hipparchos: we actually glimpse the details of his UH mathematics, as he converted (eq. 30, using eqs. 21-23) a mean longitude ($f_2 = 36°41'$; §H5) into a true longitude ($\phi_2 = 37°3/4$). This is the sole surviving fragment of such eccentric-model solar computation by the very astronomer whose better-known PH solar tables (also eccentric-model) were used longer than any others in history.

DIO preprint distributed 1990/10/22 at American Astronomical Society meeting (Planetary Sciences Division), Charlottesville, VA. (Minor revisions since.) Basis of talk at AAS meeting 1991/1/14 (Philadelphia). Abstract in *Bulletin AAS 22.4*:1232 (1990).

References

Asger Aaboe 1955. Centaurus *4*:122.
Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
J.L.Berggren & B.Goldstein 1987, Eds. *From Ancient Omens to Stat Mech*, Copenhagen.
John Britton 1967. *On the Quality of Solar & Lunar Param in Ptol's Alm*, diss, Yale U.
J.Dickey & J.Williams 1982. EOS *63*:301.
Aubrey Diller 1934. Klio *27*:258.
Wm.Dinsmoor 1931. *Archons of Athens* . . . , Harvard U.
Gerd Graßhoff 1990. *History of Ptolemy's Star Catalogue*, NYC.
O.Gingerich 1976. Science *193*:476.
O.Gingerich 1980. QJRAS *21*:253.
N.Hamilton, N.Swerdlow, & G.Toomer. At Berggren & Goldstein 1987 p.55.
W.Hartner 1979. JHA *10*:1.
Thos.Heath 1913. *Aristarchus of Samos*, Oxford U.
Franz Kugler 1900. *Babylonische Mondrechnung*, Freiburg im Breisgau.
Karl Manitius 1912-3, Ed. *Handbuch der Astronomie* [*Almajest*], Leipzig.
R.Nadal & J.Brunet 1984. ArchiveHistExactSci *29*:201.
O.Neugebauer 1955. *Astronomical Cuneiform Texts*, London.
O.Neugebauer 1957. *Exact Sciences in Antiquity*, 2^{nd} ed, Brown U.

[37] The Greatest Astronomer of Antiquity's fabrication of allegedly empirical data was so frequent, flagrant, & inept that he even perpetrates the nonpareil hilarity of assigning 2 different dates to the same "observation". (The 136 AD greatest evening elongation of Venus: 136/12/25 & 136/11/18, *Almajest* 10.1&2; see R.Newton 1985 p.10 & van der Waerden 1988 p.292. Muffiosi are typically impervious to their contextual problem: is it just coincidental that the very same astrologer whom skeptics have been pointing to for centuries as astronomy's most obvious faker has now been newly caught at the funniest muffed "observations" in the history of the field?) As I put it recently in the *American Journal of Physics* (Rawlins 1987 p.236): "That is, Ptolemy . . . states that he *observed* first-hand the *same celestial event* on two different occasions *thirty seven days apart* — a blunder unique in astronomical annals, and the coup-de-bloop for the notion that Ptolemy was a legitimate scientist." (A 1987/4/12 van der Waerden letter comments on this paper's detailing of a few among Ptolemy's various deceptions, emphasis in original: "*excellent*. The arguments — some of which are new . . . — are exposed with such a force and [clarity] that from now on nobody can shut his eyes to the clear facts." See also van der Waerden 1988 Chaps. 14, 19, & 20.) Nonetheless, Swerdlow & Neugebauer 1984 (p.377) and Toomer 1984 (p.469 n.1) swear that, when double-dating his Venus "observation", *Ptolemy knew exactly what he was doing*. Aren't they just adorable?

O.Neugebauer 1975. *History of Ancient Mathematical Astronomy* (*HAMA*), NYC.
R.Newton 1970. *Ancient Astronomical Observations*, Johns Hopkins U.
R.Newton 1973-4. QJRAS *14*:367, *15*:7, 107.
R.Newton 1976. *Ancient Planetary Obs . . . Validity . . . EphemTime*, Johns Hopkins U.
R.Newton 1977. *Crime of Claudius Ptolemy*, Johns Hopkins U.
R.Newton 1982. *Origins of Ptolemy's Astronomical Parameters*, U.Maryland.
R.Newton 1985. *Origins of Ptolemy's Astronomical Tables*, U.Maryland.
Viggo Petersen & Olaf Schmidt 1967. Centaurus *12*:73.
D.Rawlins 1982C. Publications of the Astronomical Society of the Pacific *94*:359.
D.Rawlins 1984A. Queen's Quarterly *91*:969.
D.Rawlins 1985G. Vistas in Astronomy *28*:255.
D.Rawlins 1985H. BullAmerAstronSoc *17*:583.
D.Rawlins 1985K. BullAmerAstronSoc *17*:852.
D.Rawlins 1985S. BullAmerAstronSoc *17*:901.
D.Rawlins 1987. American Journal of Physics *55*:235. [Note *DIO 11.2* §G & fnn 26-27.]
D.Rawlins in-prep A.
D.Rawlins 1999. DIO *9.1* ‡3. (Accepted JHA 1981, but suppressed by livid M.Hoskin.)
A.Rome 1931-43, Ed. *Comm Pappus & Theon d'Alex*, Studi e Testi *54*, *72*, *106*.
Alan Samuel 1972. *Greek & Roman Chronology*, Munich.
Noel Swerdlow 1973. Isis *64*:239. Review of van der Waerden 1970.
Noel Swerdlow 1979. American Scholar (ΦBK) *48*:523. Review of R.Newton 1977.
N.Hamilton-Swerdlow 1981. JHA *12*:59. Review of R.Newton 1976.
Noel Swerdlow & O.Neugebauer 1984. *Mathematical Astronomy in Copern*, NYC.
Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.
B.Tuckerman 1962&64. *Planetary, Lunar, & Solar Pos*, AmPhilosSocMem *54*&*56*.
B.van der Waerden 1970. *heliozentrische System . . . griech, pers & ind Astron*, Zürich.
B.van der Waerden 1974. *Science Awakening II* (contrib. Peter Huber), NYC.
B.van der Waerden 1984-5. ArchiveHistExactSci *29*:101, *32*:95, *34*:231.
B.van der Waerden 1986. Isis *77*:103.
B.van der Waerden 1988. *Astronomie der Griechen*, Darmstadt.

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Figleaf Salad

Royal Cometians

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‡7 Figleaf Salad

Ptolemy's Planetary Model as Funny Science

The following is the textual basis of a DR talk given by invitation before the American Astronomical Society (Charlottesville, VA, 1990/10/22).[1]

A Cranking Up

A1 I offer no hard definition of what constitutes crank or funnyfarm science. But a few examples will convey the odor of the animal better than a dictionary-definition can. [a] When parapsychologists are faced with favorite subjects' consistent failure under scientific controls, their standard conclusion is not that ESP is a chimera but rather: tight controls upset the subject and destroy the effect. [b] Those astrologers & psychics who claim to predict the future must face an obvious contradiction: why do they charge their clients, when, after all, they ought already to be rich from playing the stock market or the nags? The usual excuse: mystic powers always fail when applied to the possessor's own benefit. [c] Gore Vidal said of those who still believed in Dick Nixon at the pit of his Watergate fortunes: if the Nixon faithful saw him strangling his wife Pat, they'd say — well, she must have fainted and Dick was just helpfully holding her up by the neck.

A2 Conventional wisdom of Historians of science (Hist.sci) holds that, though Ptolemy's model of the planetary system seems inadequate today, it was highgrade science for its own era, and those who think otherwise are inferior scholars: "whiggists", nonempathetic with a different time's "paradigm" (my least-favorite pseudo-scholarly word), and incompetent (‡1 §C7) when compared to Hist.sci's elite archons.

A3 Below, I will show that the very opposite is true. Indeed, I will reveal follies & figleaves in Ptolemy's scheme which are so blatant that one soon realizes: [a] Geocentric astronomy was about as crackpot for ancient scholars as for modern. [b] Modern Hist.sci archons deserve a medal — preferably struck from their own magnificent brass — to reward Hist.sci's heroic protection of the academic community from exposure to the embarrassingly ludicrous secrets of Ptolemy's *Almajest*, which will be laid open below.

B The Heliocentric "Illusion"

B1 All ancient astronomers knew that the planets Mercury and Venus visibly swing to&fro around the Sun (and are indeed never seen far from the Sun). Even geocentrists had to assent to the undeniable fact that the Sun is *the center* of these 2 planets' oscillating celestial patterns. So, anyone with the slightest openness of mind would have perceived the unsubtle hint that both planets physically orbit the Sun. Not Ptolemy. He instead effectively maintained that: the provocative appearance of their circling the Sun was simply AN ILLUSION. Ptolemy hid the frightening truth under a delightfully imaginative figleaf, to wit: Mercury and Venus each actually circle *a point between us and the Sun*, so it only *appears* that each planet goes around[2] the Sun. Yes, just holding Pat up by the neck . . . .

[1] Abstract in *Bulletin of the American Astronomical Society 22.3*:1040 (1990).

[2] The early 17th century discovery of the phases of Venus disproved this particular Ptolemy figleaf; however, Theon of Smyrna (1st century) and Tycho (16th century) had both already admitted that Venus circuited the Sun — so both men then just made the Sun (with attendant planets) go around the Earth! The Earth may move, but the pre-committed mind cannot.

B2 Understand, this is the glorious Ptolemaic system, which Hist.sci unceasingly tells us was the intellectual epitome of ancient astronomy. (Ptolemy may indeed have been brilliant, but hardly in the sense implied.)[3] Neugebauer 1957 p.191: "one of the greatest masterpieces of scientific analysis ever written" — composed by "the greatest astronomer of antiquity" (Neugebauer 1975 p.931 & Gingerich 1980 p.264).

B3 To a mind not yet purified by Hist.sci propaganda, there might seem to be something a little, well, Funny about an astrologer (fn 4) like Ptolemy, whose model-construction labors went so outlandishly far beyond necessity and sanity, in his religio-fanatical pursuit of a plausible-looking cover story for Mercury and Venus — one which would alibi away their paths' inconsiderately blatant (§B1) heliocentricity.

B4 Not a single Hist.sci professor has ever for a moment intimated to his trusting students that: Ptolemy's ploy here is peculiar — and revealing. Hist.sci's openmindedness is such that: this heretical if common-sense re-evaluation (of Ptolemy's Mercury-Venus nonheliocentricity) is not even broached as a possibility, much less a probability. No, to the ripe Hist.sci mind, the true crackpots (the *genuinely* dangerous enemies of accurate scholarship) are those modern scientists who think that Ptolemy should have gotten real.

B5 One of Galileo's greatest anti-Ptolemaic discoveries was the Jupiter family of satellites: 4 hitherto-unknown moons *obviously circling a body other than Earth* — a clear microcosm of the Copernican vision. And how would Ptolemy have reacted, had he known of the jovian moons? Surrender? No chance. Since Hist.sci archons' amusing sense of superiority to mere scientists stems largely from their supposedly uncanny ability to put themselves in the place of past investigators, let's here demonstrate how easy it is for lesser scholars like ourselves to do so: we see immediately that Ptolemy would just protest that the seeming joviocentricity of Galileo's 4 new bodies was merely *an illusion* — actually, they circle (on their appointed epicycles) respective points between us and Jupiter: four new figleaves. Crazy?[4] Yes, but no more so than Ptolemy's identical ploys for Mercury & Venus (§B1) — which Hist.sci's most respected authorities trumpet as the constructs of genius!

C Those Geocentrist Wags

C1 An experiment attributed to the immortal heliocentrist Aristarchos (280 BC) attempted to gauge the ratio of the Sun's & Moon's distances by observing the angle between these 2 bodies at half-Moon.[5] The figure he is alleged to have measured was $87°$.[6] This may have been a lower bound. Regardless, the vital points here (often lost sight of when details are overemphasized): [a] The fact that half-Moon occurs nearly at luni-solar quadrature

[3] See §G4. I am reminded of my old Harvard prof, the refreshingly blunt skeptical philosopher Henry Aiken, who once shocked his students by asserting that the smartest philosopher was Aquinas. Aiken then explained: sure, you'd have to be a genius to defend Aquinas' incredible (inadvertently anthropocentric) edifice.

[4] Keep in mind that Ptolemy wrote astrology's bible, the *Tetr* — and worked 40^y for a prominent miracle-cure temple at Canopus, Egypt. Details in Rawlins 1984A.

[5] Geometrically: half-Moon (linear terminator) occurs when the Moon is at a right angle in the slim Sun-Earth-Moon triangle.

[6] The correct mean value is $89°51'$, and the correct mean ratio is not 19 but close to 400. The sole purported surviving work by Aristarchos (Heath 1913 pp.353f) is on this subject, but I doubt its authenticity (regarding it as just an amateur's development of A's hypotheses), since much of it is based upon the writer's confusion of the word "μερος" (which means "part") with a sign of the zodiac ($30°$). Neugebauer 1975 pp.652 & 671 shows that ancient astronomers used "μερος" for 1/48th of a circle or $7°1/2$ — which is only a *quarter* of $30°$. If we believe the writer of the famous pseudo-Aristarchos analysis, the Moon is $2°$ wide (Heath 1913 p.353) and lunar eclipses can last 1/2 a day! (Heath 1913 p.353: "the breadth of the [earth's] shadow is [that] of two moons" — that is, $4°$, so that the Moon must move $6°$, at c.$1°/2$ per hour, to entirely mid-traverse the Earth's shadow.) But no serious astronomer could possibly have accepted such patently ludicrous propositions. (Archimedes, in the "Sand-Reckoner" p.223, directly attests that Aristarchos correctly made the solar diameter equal $1°/2$.) Since pseudo-Aristarchos' error is by a factor of 4, the treatise's otherwise-inexplicable confusions neatly evaporate upon our realization that the ancient pseudo-Aristarchos just mistakenly supposed that "μερος" was $30°$ instead of $7°1/2 = 30°/4$.

proves that the Sun is many times farther away than the Moon. Aristarchos is said to have made the distance ratio 19 (or perhaps: at least 19), since sec $87° \doteq 19$. [b] The Moon was well known in antiquity to be c.60 Earth-radii distant (*Almajest* 5.13); and the solar semidiameter was (angularly) c.$1°/4$ or $\pi/720$ radians. Thus, the Sun's radius in Earth-radii must obviously be about $(60{\cdot}19){\cdot}\pi/720 = 19\pi/12 \doteq 5$. Cubing this result to obtain an approximation to the Sun/Earth volume-ratio, we find[7] that it exceeds 100.

C2 Hist.sci archons seldom emphasize the curious fact that ancient geocentrists did not deny these conclusions — indeed, the ancients were quite aware that the Sun is many times larger than the Earth. Even the geocentrist bible (*Almajest* 5.16) makes the Sun's volume 170 times bigger than the Earth's!

C3 R.Newton, the modern pioneer of skepticism regarding Ptolemy's pretensions, asks a lethal common-sense question: how could the Sun be dominated by a body over 100 times smaller?! Does the tail wag the dog? We know no personal details about Ptolemy, but one has to wonder: was he smiling when he wrote (*Almajest* 5.16) that the solar tail is 170 times bigger than the terrestrial dog? [But could Ptolemy outsmile unique ultra-wag Eratosthenes — whose solar volume = 1/12 Earth's?! See *DIO 14* ‡1 eq.16. (Note added 2009.)]

D Inverts

D1 With respect to the common-sense principle now known as Occam's Razor, there is a flagrantly unacceptable feature of the Ptolemaic system: the inner and outer planets have different models. (Not so for the heliocentric[8] system.) In Ptolemy's scheme, each planet's model contains an annual motion: for the outer planets (Mars, Jupiter, & Saturn), the *epicycle* has period 1^y; for the inner planets (Mercury & Venus), the *deferent* has period 1^y. Why this bizarre model-inversion?

D2 From the heliocentric perspective: for both Ptolemaic models, the annual motion is simply the Earth's motion in geocentrist disguise. So why can't we have a consistent model? The answer is simple: if we tried imposing an outer planet model upon an inner planet (or vice-versa), then *the epicycle would be bigger than the deferent carrying it* — which would result in a cumbersome arrangement, with the Earth inside a hugely-swinging epicycle. (This would of course destroy Ptolemy's precious nested-spheres cosmology.)

D3 Indeed, if the inner planet model were imposed upon an outer planet, the epicycle's center would always be in the direction of the Sun (i.e., the planet would circle a point on the line connecting us and the Sun — just like the inner planets), which might provide onlookers the same dangerous whiff of heliocentrism that the inner planets' motion did (§B1). Thus, using inconsistent models (for inner & outer planets) was useful to those who wished to put over the geocentric system.

E Noneccentric Epicycles

E1 A feature of Ptolemy's astronomy that once seemed unexplainable (until Rawlins 1987): why are all his epicycles forced to be circular? The answer turns out to be elementary. We noted (§D1) that each Ptolemaic planet model has an annual motion; however, the reader may not hitherto have been aware of the precision involved: each planet's annual motion is not just roughly equal to the Sun's — in *Almajest* 9.4, these motions are (for all 5 planets) tabulated as equal to the Sun's, agreeing to a 50 billionth of a degree per day. Moreover, the mean longitude of the motion is also the Sun's (at any time). That is, for all five planets, both circular elements (of the annual part of each planet's orbit) are identical to the Sun's: ϵ (mean longitude at epoch) and n (mean motion).

[7] This is not quite the way Ptolemy figures it at *Almajest* 5.16, but it's quicker and gets a result at the same ordmag.

[8] For evidence that ancient heliocentrists even produced ephemerides (a point 1st understood by van der Waerden 1970) see ‡6 fn 36 & [despite a DR misjudgement] *DIO 11.2* ‡4 §G3.

E2 Ptolemy's insistence upon noneccentric epicycles seriously degraded the potential accuracy of his ephemerides, causing errors reaching 1° for Mars, 3° for Mercury (R.Newton 1977 pp.279, 323; Rawlins 1987 n.36). Thus, to pretend that his theories accorded with reality (often within $1'$ and in all cases to within $0°.1$), Ptolemy was driven to fake his "observations" (§E2). But, why did he bring all this trouble on himself? — what was the gain? For the answer to this question, I quote from Rawlins 1987 (pp.237-238, emph in original):

> Ptolemy's peculiar requirement that all epicycles be noneccentric is not as naïve as it may appear at first glance. The method in this seeming madness is obvious as soon as one tries to imagine eliminating the noneccentric feature: if both deferents and epicycles were eccentric, then all the outer planet epicycles and the inner planet deferents [the annual motion circles] would have eccentricities [e] and apogees [A] just equal to those of the solar orbit! It was already suspicious enough that these circles exhibited inexplicable fidelity to the Sun's mean-longitude-at-epoch [ϵ] *and* mean motion [n] . . . . If it were also publicly acknowledged that not just two but *all four planar elements* [ϵ, n, e, A] were (for the appropriate orbital circles of all five planets) identical to the Sun's — well, at that point, the heliocentrist heresy could probably no longer be contained. . . . Ptolemy systematically fabricated[9] perfectly accordant "observations" in order to promote the pretended reality of the noneccentric epicycles that constitute the prime geocentrist figleaves[10] which he and his religio-astrological cult employed to hide the falsity of the theologically preferable geocentric system. The attendant suppression of the heliocentric theory held back for over a millennium our realization of the true distances[11] of the planets, the data required for the discovery of Kepler's 3rd Law — itself the key revelation leading on to Newton's Law of Gravitation and the resultant flowering of mathematical physics.

E3 In addition to the 4 planar elements' identity with the solar orbit, accurate astronomical work would have found that the planets' annual-motion circles would also be parallel to the plane[12] of the solar orbit. So, for all 5 planets, 6 orbital elements would match the Sun's (30 elements, net). Extremely hard to explain away without heliocentricity.

F Parallax as Epicycle

F1 Despite the foregoing (or in innocence of it), Pedersen 1974 p.11 states that "there is no question that [the *Almajest*] was a greater scientific achievement than [Copernicus' 1543] *De revolutionibus*". Today, the 19th century discovery of stellar parallax (not Copernicus' book, 3 centuries earlier) is generally regarded as the clincher that finally & forever disproved geocentricity. In the esteemed *Proceedings of the Institute for the History of Science*, Derek Price (Yale Univ Hist.sci) & Francis Johnson have both stated that, in Copernicus' day, there was no empirical reason to prefer heliocentricity! Johnson even adds (Clagett 1962 p.220) the astonishing claim (forgetting J.Bradley's 18th century discovery of aberration) that until the F.Bessel-T.Henderson measurement of stellar parallax in the 1830s, geomobility

[9] R.Newton 1977 & Rawlins 1987.

[10] "Ptolemy's Ivy Leaf" (K.Locher *JHA 15.1*:32; 1984/2) has no relation to the present discussion.

[11] Also, according to geocentrist Ptolemy, the stars are just outside the orbit of Saturn. By contrast, heliocentrist Aristarchos placed them far, far beyond (as Archimedes reports: "Sand-Reckoner" p.222). The obvious reason: the invisibility of stellar parallax told Aristarchos that the effect is quite small — thus, the stars' distances must be enormous.

[12] See Rawlins 1987 n.38.

was "an open question in science".[13] But I will now exhibit[14] (§F3) the obvious falsity of one of the most durable and widely-accepted myths in scientific history, namely: the seemingly plausible notion that stellar parallax's discovery in the 1830s firmly established heliocentricity.

F2 To see the truth of the matter, let us start by supposing that Ptolemy had lived long enough for Bessel to face him with the reality of the stars' tiny annual loops: would Ptolemy have suddenly given up and converted to heliocentrism? (Just as easy a question: how often do lawyers convert each other in the courtroom?) The visible effect of parallax is merely a looping motion of period 1^y. Add this oscillation to the star's transverse "proper motion", and (as a little doodling will quickly show) the net motion is: a zig-zag-zig path — direct then retrograde then direct — that is, essentially *the very same path a planet describes*. How could this discovery possibly discomfit Ptolemy? — hell, he *lived* to alibi such effects. I have asked two 1990 audiences what he would have said to stellar parallax, and (within a few seconds) both[15] figured it out (though Hist.sci never has), namely: stellar epicycles.

F3 Quoting from DR's 1976 analysis (fn 14), one sees that Ptolemy himself purveyed the common misunderstanding that Price and Johnson share (§F1):

> Ptolemy asserts ([*Almajest* 9.1]) that the planets have no detectable parallax . . . — meaning, of course, *diurnal* parallax. But, in fact, the planets exhibit huge *annual* parallax [the planets' familiar retrograde loops] . . . . Indeed, Ptolemaic planetary astronomy can be seen as largely a design *for* converting the parallactic effect, of the Earth's annual revolution, into "epicycles" (deferents, for the inferior planets) allegedly inherent in the planets' own motion. . . . the hypothetical 19thcentury Ptolemy, confronted [via Bessel's stellar parallax data] with this familiar [annual] motion, would therefore have concluded, not for geomobility, but [instead for] a new Triumph of Ptolemaic astronomy: even the stars have our [*Almajest*]'s annual epicycles!

F4 Planetary parallax is as real as (essentially the same as!) stellar parallax — indeed, it even *looks* like it (§F2). We saw above (§E) that the noneccentricity of Ptolemy's epicycles was a figleaf (hiding Sun-planet element identities). But we now find that Ptolemy's epicycles were *themselves* figleaves, hiding the most crucial phenomenon of the helio-vs.-geo-centric debate: planetary parallax. I.e., a proof of heliocentricity which is just as powerful as stellar parallax (namely, planetary parallax: planets' retrograde loops) had always been grossly visible (requiring no telescope or heliometer) — even while geocentrists were denying that the Earth circuited the Sun . . . .

G Paradigm or Modern Cleanthes

G1 Thus, it is an utter misconception to suppose (with Hist.sci) that the long dominance of geocentricity was primarily based upon intellectual considerations (evidence or "paradigms"). When Aristarchos first broached the heliocentric theory publicly, he was not crushed by logic or lack of crucial experiments. He was simply threatened.

G2 From Plut *Mor* 923, we learn that Cleanthes (the leader of the Stoics) recommended "an action for impiety against Aristarchus the Samian on the ground that he was disturbing the hearth of the universe because he sought to save <the> phenomena by assuming that the heaven is at rest while the earth is revolving along the ecliptic and at the same time is rotating about its own axis."

G3 What killed ancient heliocentrism was not evidence. It was force. From the hemlockian fate of Socrates, we know what a charge of "impiety" led to. Had heliocentrists persisted, armed policemen attached to the prevailing theocratic dictatorship would have removed the offenders to prison — perhaps en route to execution. What has this brutal fact got to do with: mythical "decisive" new evidence (for which good-skeptical-scientists allegedly waited), "paradigms", "whiggism"[16] — and all the other highflown alibis & cult-fads that Hist.sci archons have for decades hauled out to try to pretend that there is something of genius in Ptolemy's geocentric contraption?

G4 Ptolemy's real genius was political. He made himself the advocate — the paid lawyer — for the dominant government view, which was effectively: popular realization that the Earth is not the universe's center could be corrupting to public morals. (Given the course of history since Copernicus: I won't take a firm position against that viewpoint. However, the truth and the beneficence of an idea are two separate issues.) The enormity of the gulf, that separates so many scientists from the currently fashionable Hist.sci center, is illustrated by a simple consideration here: obviously, scholars of principle ought to condemn (not alibi & laud) Ptolemy's convenient going-along with powerful false orthodoxy (hiding heliocentricity beneath the *ad hoc* layers of a gov't-certified figleaf salad). (To anyone among *DIO*'s scientist-readers who has behaved ethically, perhaps courageously, in the face of an unprincipled power-type: stop to consider how Hist.sci will record your respective careers.) What is it about certain Hist.sci archons that attracts them so magnetically to the seemingly-repellant task of glorifying a sell-out scholar? (Hist.sci's peculiar compulsion in this connection is especially incongruous since the Hist.sci field is so admirably bereft of careerists — and indeed is justly famous among scientists for its quintessential rectatude.)

G5 To investigate the self-evident ancient-modern parallel here a bit further: how have Hist.sci archons treated modern skeptics regarding Ptolemy's pretensions? [a] Flee debate for 20^y. [b] Alibi Ptolemy with the same[17] prejudiced intensity he exhibited when explaining away planetary parallax. [c] Slander dissenters in as vile a fashion as possible. (E.g., ‡1 §C7 & ‡3 §D.) [d] Apply totalitarian force: threaten and suppress[18] (& attack in politically safe forums).

Cleanthes lives.

[13] Price ("Contra-Copernicus") at Clagett 1962 pp.215-216: Ptolemy's *Almajest* "was at least original in many of its parts. The *magnum opus* of Copernicus does not have that distinction beyond its first few pages. . . . [and its central theory, heliocentrism] could not be proved or disproved by any observation available at that time. No wonder good scientists remained skeptical until the new & decisive evidence was forthcoming. . . . Copernicus made a fortunate philosophical guess without any observation to prove or disprove his ideas . . . his work as a mathematical astronomer was uninspired. . . . his book is conservative and a mere re-shuffled version of" Ptolemy's *Almajest*. Johnson (Clagett 1962 p.220): "The fact that should be emphasized and re-emphasized is that there were no means whereby the validity of the Copernican planetary system could be verified by observation until instruments were developed, nearly three centuries later, capable of measuring the parallax of the nearest fixed star [Henderson's work]. For that length of time the truth or falsity of the Copernican hypothesis had to remain an open question in science."

[14] The following demonstration (§F3), regarding Ptolemy's hypothetical incorporation of stellar parallax, was sent by DR to the 1976 IAU meeting at Grenoble via O Gingerich (Ptolemy's chief modern public relations man) — who answered it by simply refusing to read it there.

[15] In the 1st instance: my fertile freshman student Josh Renzi 1990/10/12. The 2nd: a bright, enthusiastic Amer Astron Soc audience at the 1990/10/22 AAS Planetary Sciences Division meeting, Charlottesville, VA.

[16] E.g., O Gingerich to DR 1979/12/10.

[17] Quoting from DR's 1990 abstract (*BullAAS 22.3*:1040) for this paper: If used with great care, Ptolemy's *Alm* is an invaluable sourcebook for our knowledge of ancient astronomy. But his famous patchwork celestial model's sole genuine claim to greatness is merely as a classic study in adamant unfalsifiability. The same might be said of the equally motley zoo of alibis for Ptolemy, conjured up by his modern team of admirably imaginative "historian" defense-lawyers.

[18] Details: ‡6 fn 15 & ‡5 fn 24.

References

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
Archimedes. *Works* c.260 BC. Ed: T.Heath, Cambridge U. 1897&1912.
M.Clagett, Ed. *Critical Problems in Hist Sci*, 2nd pr. 1962; Proc Inst Hist Sci 1957.
Thos.Heath 1913. *Aristarchus of Samos*, Oxford U.
O.Neugebauer 1957. *Exact Sciences in Antiquity*, 2nd ed, Brown U.
O.Neugebauer 1975. *History of Ancient Mathematical Astronomy* (*HAMA*), NYC.
R.Newton 1977. *Crime of Claudius Ptolemy*, Johns Hopkins U.
O.Pedersen 1974. *Survey of the Almajest*, Odense U.
Plutarch. *Moralia* c.100 AD. Eds: Babbitt, etc., LCL 1927-.
D.Rawlins 1984A. Queen's Quarterly *91*:969.
D.Rawlins 1987. American Journal of Physics *55*:235. [Note *DIO 11.2* §G & fnn 26-27.]
Tetrabiblos. Compiled Ptolemy c.160 AD. Ed: Frank Robbins, LCL 1940.
B.van der Waerden 1970. *heliozentrische System . . . griech, pers & ind Astron*, Zürich.
B.van der Waerden 1988. *Astronomie der Griechen*, Darmstadt.

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‡8 Royal Cometians

Reputability, Reform, & Higher Selfpublication

Texts for the Day

Donald Yeomans (Jet Propulsion Lab), closing an article cataloging some remarkably funny Dubious Achievement Awards related to Halley's Comet, offers a champagne toast (Yeomans 1983 p.10):

> Interspersed among the many important scientific results that are sure to come from the planned work on comet Halley, may the coming return also offer a wee bit of the lunacy and unabashed fun that has accompanied comet Halley's past apparitions.

John Bortle (W.R.Brooks Observatory) also asks of the 1986 Halley return: "What kinds of silliness will we see this time?" (Bortle 1985 p.110) — evidently expecting most of the folly to be generated by non-scientists. Yeomans' article presages otherwise, and his paper is as amusing as some of those reviewed below — with these crucial differences: [a] Yeomans' humor is intentional, [b] the unfortunates he writes of are long dead, while the menu of court-jester buffoonery set out in what follows here is entirely due to prominent astronomers still alive & powerful, some of them genuine contributors to our knowledge from time to time.

Thanks to the international efforts of numerous brilliant, hardworking, largely non-celebrity astronomers, the apparition of 1986 indeed brought us wondrous harvests of data and even a closeup view of the Comet's very nucleus. It also fulfilled Yeomans' & Bortle's wish for some zaniness, as the following will attest; though, whether the central (Royal Astronomical Society) act of zanity was funny or tragic, the reader must decide. I regard it as both.

A Cometose Populace

A1 I doubt that even 1% of the public saw Comet Halley outdoors during its 1985-1986 flyby.[1] But almost everyone heard about it — and was forthwith rigorously bored by a nonstop orgy of commercial promotion. A shame, since the unadorned event was magical — if a trifle stealthy.

A2 The cause of the Comet's aggravating visual elusiveness was not just faintness: it also seldom came near any other celestial object bright enough for nonastronomers to use for locating Halley with binoculars. And now to the secret that escaped more citizens than the comet itself, namely: some wellknown astronomers also had difficulty in locating Comet Halley, often misleading layfolk, a point amusingly illustrated by the hitherto unremarked though nationally televised misadventures of Carl Sagan (Cornell University and Hollywood — in some order or other) and Dr.Squareza[2] (president of a major university), both eminently

[1] But most of us saw Comet Halley's heart on television and then in magazines. Indeed, it's only fair that the worst apparition in 2 millennia was visited upon the only generation of terrestrials so far who could see the affair electronically. Any other arrangement would have given us 2 acquaintanceships and another era none.

[2] A pseudonym was substituted (for Squareza's name), after completion of this article, when Squareza's star plummetted (1990). Nonetheless, his overwhelmingly impressive vita includes contacts with: NAS, NRC, NASA, IAU, History of Science Soc., Cosmos Club, National Geographic, UNESCO, Kuwait Univ, Univ Colombo (Sri Lanka), US Armed Forces Inst, US Information Agency, arms dealer Adnan Khashoggi, Freedoms Foundation (Valley Forge Award); several other appearances on Koppel's *Nightline*, including hosting one of its lengthy "Town Meeting" shows; also a bank directorship, and on boards of directors of: Business Council International Understanding, & Linda Pollen Inst Medical Crisis Counseling. Why hadn't Squareza seen Halley? Probably busy with Very Important

Reputable establishmentarians, businessmen-salesmen, & aggressively loud opponents of (academically unfashionable) pseudoscience. As purported authorities on Comet Halley, these eminent personages were invited onto the 1985/12/5 edition of ABC-TV's *Nightline* by their friend, host Ted Koppel. At the conclusion of the show's half-hour, Koppel made the understandable error of asking both gentlemen the one question viewers most wanted to hear: where *in the outdoor sky* these days should the public look for The Comet? Sagan exhibited remarkable inventiveness in avoiding the question at length, but when Koppel finally put on a last-minute press, the spectacle got even funnier.

Koppel: Dr.Sagan, give those of us who want to take a look — give us a real quick crash course on how *and where* to look. [emph added]

Sagan: Well, the basic point is that the Comet is nothing as spectacular as it was in 1910 or will be in 2061 — and 2134 if you can hang on for that. That will be the best one. What you have to do is to get away from the air pollution and the light pollution of cities and look at a time when the Moon is new or has set. You also have to know what part of the sky it's in. It does not streak across the sky of course; it rises & sets with the stars. If you have a pair of binoculars — maybe 7x50s, something like that — that is absolutely all the instrumentation you need, although it is a naked eye object. You can see it without the binoculars; binoculars would help. [DR: Recall the one good line in the film *Nashville*? A d.j. muses aloud: ask a lawyer for the time, and a half hour later you'll know every detail of a watch's mechanics, but you still won't know what time it is.]

Koppel: We're down to 20 seconds. Where [is the Comet] right now? If I walked outside [in Washington] right now —

Squareza: "Southwest. Southwest above the horizon about $1^h 1/2$ to 2^h after sunset [i.e., about 6 to 6:30 PM], close to the constellation Aquarius, right above Jupiter."

Sagan: "That's right."

A3 Of course, anyone gullible enough to try following these impressively precise & authoritative directions, on that cold December night, would never find the Comet — not before being frozen as stiff as the entrails of an indoor astronomer, surprise-sandbagged on nationwide TV by an outdoor question like Koppel's. These instructions' entire value is their unambiguous demonstration that: [1] neither Sagan nor Squareza yet had any practical acquaintance with finding the comet in the real sky (though countless amateurs had been tracking it for weeks); and, much more important and telling, [2] both men were afraid to admit that they honestly just didn't know where Halley was, a comedy only enhanced by Sagan's that's-right bit of H.C.Andersenian pretense that he too had known all along where to see the Comet — now that Squareza had already confidently told him its location . . . . The most depressing aspect is the bottom line: at least one of these top educators (of youth & the larger public) risked faking knowledgeability simply because he didn't think he'd get caught.

A4 On 1985/12/5, Comet Halley was in central Pisces, and the nearest bright star was Algenib in the Great Square of Pegasus. At the time of day specified (§A2), the Comet was not in the south*west*, but rather was somewhat *east* of south. And it was not above Jupiter

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Phonecalls. (VIP Squareza is the sole astronomer quoted, hyperpuffing *JHA* Editor-for-Life's *Stellar Astronomy*, in EfL's self-published 1986/2 *JHA* full page ad for the book.) [Since learning of the disinterestedly impecunious American University board of trustees' almost irrepressible passion (described as "objectionable and indecent" by the *Wash Post*: 1990/11/10) to golden-parachute their departing friend Squareza with over $1,000,000, I've decided after all to supply sources for readers interested in the offbeat Squareza saga: former AU President and habitual strange phonecaller. The calls taped by police (1990) were made by him from his AU President's office phone. (See *Wash Post* 1990/4/27, *Chronicle of Higher Education* 1990/5/2; additional bio info: any recent *Who's Who in America*.) According to *Time* (1990/12/17), the board's largesse was finally scaled down (after public outcry), but a "compassionate" AU will next year restore Squareza to full professorship. Plus an AU telephone. (I'd like to see a little more compassion [a] to less well-connected phone-harrassers, and-or [b] to the victims of such calls, e.g., discouraging offenders in some more effective way than by swiftly returning Squareza — the most prominent example ever, among such offenders — at $70,000/year, to the university he advertised so . . . differently.)]

but roughly 1/2 a right angle to the left of it.

A5 For any question regarding the motion of Comet Halley, Koppel should have interviewed an unblowdried but truly knowledgeable dynamical scientist such as Yeomans of JPL — whose own canned appearance earlier on the same *Nightline* revealed him to be perfectly capable of communicating at the popular level. (And if *Nightline* didn't know any better, Sagan & co. should themselves have suggested this, without prompting. But the temptation of self-publicity can easily overwhelm the better self.)

A6 I would like to think that the foregoing account of *Nightline*'s literal disaster will encourage popular emcees to seek the advice of working scientists (not cocktail [or pot or pol] party royalty) when choosing figures to appear on their shows. To see the same Expert-Scientist faces again & again on Carson, Koppel, etc.: it betrays [an Olbermannesque] lack of originality and-or the effect of influence.

A7 *Nightline*'s Catastrophe-of-the-Reputables is particularly ironic in light of the fact that Sagan and Squareza have both been among the figurehead Fellows of an orthodox celebrity Committee, CSICOP (best pronounced "Sick Cop"), organized in 1975 to police[3] the excesses of the Disreputable pseudoscientist clan (astrologers *et ilk*): their fumblings, deceits, and above all *their detachment from empirical reality*.

A8 Incidentally, neither Sagan (despite his years of ostentatious liberal posing *vis-à-vis* Velikovsky's right-to-be-heard) nor Squareza offered a word of on-the-record comment regarding the most ghastly contretemps, ever, in Reputable Science's eternal conflict with pseudoscience, when [a] their very own CSICOP's biggest and most expensive pioneer experiment[4] backfired in 1977 (coming out in favor of the astrologer!) and [b] CSICOP then tried to cover up the fact: with statistical finagling initially, then censorship — finally reacting to attempts at open reporting via threats, background-snoopery, & whistleblower-ejection from CSICOP without specification of charges. (Again, no comment whatever from civil-righteous[5] Sagan, who was specifically informed by telegram of all of it. See Rawlins 1981S; preprints distributed nationally by publisher. This article's sudden unexpected appearance & circulation actually panicked brave CSICOP into calling off its scheduled 1981 annual pressconference at the very last minute. See also Pinch & Collins 1984. None of the US science periodicals that had previously covered CSICOP's activities ever reported a word on the scandal, so CSICOP's startlingly atypical shyness of reporters was successful here, as the science press cooperatively permitted the lying-lowlife atop CSICOP to slink away without the slightest public censure. What sort of lessons does such a spectacle teach?) At this crucial-experiment juncture, the upshot of the public silence of CSICOP Fellow Sagan and CSICOP Consultant Squareza was the effective destruction of CSICOP as a credible empirical-test opponent of witchdoctory, a lamentable waste, since such testing exploits the only inherent advantage science possesses[6] in a contest with irrationality.

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[3] Largely via its often enlightening if not always trustworthy journal, *Skeptical Inquirer* (abbrev: *SkInq*).

[4] A rashly conceived & rushly published challenge-experiment (fallaciously testing neoastrologer Gauquelin's nonexistent Mars Effect on a European sample not independently pre-checked), carried out in 1975-1977 by three instances of the same brand of Eminent Scholar encountered elsewhere here. Facts: [a] The astrologer won this test. [b] A strong anti-astrology outcome naturally ensued when proper design was introduced in a later (1978) Mars Effect experiment upon a US sample. [c] This 2nd test was entirely calculated by DR. (Paid for by CSICOP cheques to him. Note: DR deliberately had no rôle in choosing the sample.) I see that p.42 of a 1990/3 paper published at *QJRAS 31.1*:31 does not mention item [a] at all — and then seriously misreports items [b] & [c]. Regarding item [b], it is computationally demonstrated at p.28 of the very Rawlins paper (*Skinq 4.2*:26; 1979) cited by *QJRAS* as being inconclusive, that: the astrological claim under investigation was disconfirmed with a probability-strength of c.10000-to-1. As for item [c], the *QJRAS* 1990/3 paper p.42 cites an "investigation by Kurtz, Zelen, & Abell" plus an "analysis of the same data by Rawlins". As noted above (& clearly stated at p.23 of the very Kurtz-Zelen-Abell *Skinq* article cited by the *QJRAS* paper), DR did all the astronomical calculations for KZ&A. (DR also performed all worthwhile statistical tests on this sample prior to KZ&A, and the results are printed in his *Skinq* analysis.) For a detailed history of CSICOP's strange behavior in this affair, see "sTARBABY" (Rawlins 1981S).

[5] Credit to Luce-era *Time* magazinese.

[6] I.e., contact with truth & reality. Without this groundrock, conflicts become merely: one side's propaganda vs. another's. Which seems to be just fine with CSICOP's sort of scholar.

B The Awful Emptiness of Interaural Space

B1 On 1986/4/14, I attended a lecture at Johns Hopkins University given by Christopher Walker (Dep't. W. Asiatic Antiquities, British Museum) on Babylonian records of Halley's Comet. During his presentation, Walker referred to *the leading British expert on Comet Halley* as: David Hughes. Hughes' résumé: University of Sheffield Physics Dep't.; genuinely gifted writer and occasional *MonNotRAS*, *JHA*, & television expositor on Comet Halley; appointed co-investigator with the wonderful Giotto space mission to the Comet; sometime Councillor & Editor (*QJRAS*) and now (since 1988/3/11) a Vice-President of London's Royal Astronomical Society (RAS). (Hughes has also been Vice President of the even more entertaining Brit. Astr. Assoc.) If Walker's above-cited superlative assessment of Hughes' prominence is correct (and the foregoing list of credits is compelling evidence that British astronomy agrees with him), then Great Britain's notorious Brain Drain has gone kiloskulls beyond what anyone has heretofore realized.

B2 In mid-1980, Hughes was made sole Editor of the *QJRAS*. He soon distinguished himself there by improving the efficiency of the operation: saving postage by not replying to various contributors he was publishing or not publishing, letting the page proofs arrive suddenly as a surprise for the publishees — and just letting the other scholars dangle indefinitely. In some cases, he also improved refereeing, finding it took alot less time & bother (& RAS funds) just to accept slander about the author's person rather than going through the tedious formality of traditional refereeing: if Hughes declared a potential contributor Not Reputable (and Hughes possesses a razor-sharp eye for reliable classification: §E1-§E5), this was sufficient grounds for nonrefereeing, trashcanning, and total noncommunication,[7] including not replying to polite queries regarding papers' fates — as well as not even replying to inquiry regarding previous nonreplies! (RAS' own G.Darwin Lecturer, O Gingerich, wrote Hughes 1982/4/5 that he was "somewhat scandalized by the refereeing standards for the *QJRAS*".)[8] It is known (1983/10/21) to his admiring RAS Council that in one of the occasional cases where *QJRAS* refereeing of a paper occurred, Hughes secretly appointed, as its sole referee, the most committed public opponent of the author's viewpoint — an incident which triggered Council's explicit expression (1983/11/11: §C4) of complete confidence in his Editorship. Council's approval of Hughes' procedure has now been more clearly and grandly expressed by his 1988 Council-sponsored exaltation to RAS Vice-Presidency. (Upon learning of this event, DR concluded his two decades of association with the RAS.)

B3 A frequent contributor to the prestigious journal, *Nature*, Hughes has published there an amusing paper (Hughes 1976) attempting to identify & date the Star of Bethlehem. With the same purposes, he soon thereafter published a book (Hughes 1979) under the inspirational title: *The Star of Bethlehem, an Astronomer's Confirmation* — the subtitle evidently designed to harvest the reliably lucrative The-Bible-Was-Right market, in the fashion of the previous year's *God and the Astronomers* (produced by the Director of NASA's Goddard Institute for Space Studies, Jastrow 1978; see especially pp.14 & 116!).

B4 The hilarious positional astronomy in Hughes' Xmas Star book I have reviewed elsewhere (Rawlins 1984A p.977).[9] Some of the astronomy involved is so freshman-basic, that its rearrangement by a prominent astronomer may be unprecedented.

[7] Which, on one amusing 1981 occasion, entailed his virtually running out of the BAA meeting-room to avoid conversing with an amiable but déclassé scholar.

[8] All that upset OG was *QJRAS* publication of papers by his 2nd least favorite scholar, R.Newton. Shortly after this complaint, Hughes banned R.Newton from *QJRAS* and allied himself with OG & the *JHA* crowd (& began contributing his highly Reputable scholarship to *JHA*, elevating that extremely handsome journal's prestige in the manner shown here in §G2); so OG's opinion has doubtless since been altered — though Hughes' academic standards obviously have not.

[9] Curiously, this review was undertaken at Hughes' own insistence: in a letter of 1982/2/22, he criticised a skeptical Xmas Star manuscript of mine (précised at *idem*; full text in a future *DIO*) for not taking note of his works. Strangely enough, he has not since thanked me for taking his advice. I forgive him. See also the reviews (of Hughes 1979) written by David Clark (*Observatory 100*:82; 1980/6) and by Virginia Trimble (in an issue of *Archaeoastronomy* appearing at about the same time).

B5 Hughes sagely recognizes one of the major percs of being an editor: your journal can quick-disseminate your output when no other publication will. His recent amazing Halley's Comet paper (Hughes 1985, actually mailed out in 1986) was self-published in the very last issue of the *QJRAS* for which he was top Editor. This truly historic study of the geometry of all known Halley apparitions brought Hughes' natural cometic gifts to their fullest flower.

B6 As an elected RAS Fellow for a decade, I repeatedly — starting in early 1983 — suggested (details below: §C4) that the RAS separate itself from Hughes' original approaches to elementary astronomy & to the equally elementary rights of unrefereed *QJRAS* contributors. Whether there was any substance to the eventual seemingly[10] reform-minded 1983/11/11 Council decision establishing a new 3-man *QJRAS* Editorial Board (to supervise the journal), the reader may judge from the following items: [1] G.Cole, Hughes' now-reigning successor as *QJRAS* Top Editor, was on the Ed.Board threesome that expertly oversaw publication of Hughes' fateful *QJRAS* paper on Comet Halley (Hughes 1985, analysed below: §B-§E). [2] On 1988/3/11, Hughes was elevated to the office of Vice-President of the RAS, when, as usual, the entire slate nominated by the RAS Council was elected without a single exception. (All 14 candidates: ballots mailed out 1988/2/9. Democracy in action.) As ultra-Brit Col.Blimp so pithily put it (in a classic David Low cartoon, sent to Council at this time): "Gad, sir, reforms are all right as long as they don't change anything." [Original cartoon viewable at www.dioi.org/imm.htm#dgsb.]

B7 RAS Editor Hughes' 1985 paper is unique, in its own wonderful way, throughout the entire literature produced by professional astronomers in this millennium. (As for the previous millennium: see §C1.) The paper's title is: "The Position of Earth at Previous Apparitions of Halley's Comet", and its aim is to classify Halley apparitions (an idea taken from Bortle & Morris 1984, with acknowledgement: Hughes 1985 p.515), with the evident hope of explaining spatially the unusual faintness of the then-occurring 1986 appearance (roughly 2 magnitudes dimmer than any other on record). For all 31 encounters from 240 BC through 2061 AD, Hughes maps on a circle-diagram (Hughes 1985 p.514 Fig.1) a variable j which well characterizes apparitions, since j = Earth longitude minus Comet Halley longitude, at the moment of Comet perihelion (both longitudes heliocentric).

B8 Having completed all his computations and resulting charts, Hughes is then perplexed to find (in his Fig.1) dim 1986's j appearing in the same group (Hughes' "Class E") with the j of 66 AD, 374, 837, & 1759, all of these being excellent spectacles, most of them among the very best — especially that of 837, which was probably the most beautiful & inspiring Halley apparition that has ever or will ever occur.

B9 Bortle's brief but appreciative description rates this precious event the best comet display in recorded history. Due to the moving Comet-orbit node's chance proximity to Earth's orbit at that time, combined with Halley's happening to arrive there just about when the Earth was passing, the 837 AD approach was almost[11] perfect: the Comet only 5 million km away at closest approach, with a brilliance rivalling Venus', and (Bortle 1985 p.104): moving with "enormous velocity, crossing 60° of sky in 24 hours. . . . while the tail — which pointed from south to north when the comet was at its nearest — spanned most[12] of the vault of the heavens." The timing was seriously flawed in but one respect: all of humanity wasn't alive to see it. My view of envy is usually Mencken's.[13] But the 837 AD Halley display evokes longing and regret at missing it, in any astronomer of imagination.

[10] DR had already been through a Reform charade at RAS, when the written 1977/12/12 promise of then RAS Sec'y J.Shakeshaft (that the *QJRAS* would henceforth acknowledge all submissions) was regularly broken subsequently. I have been privately apprising Council of *QJRAS* strangeness for a decade, with the sole issue being: my consistent instruction in the elementary reality that private suasion is fruitless.

[11] Of course, one doesn't want the approach to be *too* perfect. The Tunguska reindeer who had the best view of the comet that hit Siberia in 1908 might have expressed some thoughts on a perfect encounter, had any survived it.

[12] Bortle (*loc cit*) quotes a Chinese record of 837/4/13, describing the tail as 120° long.

[13] "A Blind Spot", *The Vintage Mencken*, ed. A.Cooke, NYC 1956: "the fact that some . . . ass or other has been elected President . . . or appointed a professor at Harvard . . . is as meaningless to me as the latest piece of bogus news from eastern Europe."

B10 Since the j values of Hughes' five "Class E" apparitions are bunched together (in a span less than 45° wide), even though their visibility was wildly different, the paper concludes (Hughes 1985 p.519) that Class E is "rather a mixed bag".[14] The only mixed bag here is interaural, not interplanetary.

B11 The j value for 66 AD especially confounded Hughes' analysis. For, though that apparition was in fact a 1st magnitude visual spectacle, it looks (in Hughes' memorable Fig.1) distinctly *worse* than the inobtrusive 1986 visit: at Halley perihelion time, Earth is much closer to Halley aphelion (i.e., j is far nearer 180° in Fig.1 & Table 1) in 66 AD than in 1986. (The worst possible apparition's j would be near 180°. Just a trifle less.)

C The Doubly-Epochal Hughes Screwup

C1 The instant I saw this seeming paradox, I knew precisely the cause of it; and a (purely hypothetical) alert *QJRAS* referee would have had the same immediate response: Hughes has simply taken his Comet Halley longitudes from a source (Yeomans & Kiang 1981 p.643) using the ecliptic for epoch 1950.0, while taking his Earth longitudes from a source (Tuckerman 1962&64) using the ecliptic for epoch-of-date![15] Not a mixed bag, but mixed precessions. The only comparably cockeyed recipe, in the purportedly serious astronomical literature of the last 2000^y, is Ptolemy's mixing of nonprecessing solar orbit (*Almajest* 3) with precessing planet orbits (*Almajest* 9), but the visible effect was minuscule by comparison to Hughes' far superior canard.

C2 Thus, for each of the 30 Comet Halley apparitions examined by Hughes, the j value he displays[16] (Fig.1 & Table 1; also our Table A below) and uses for his analyses, is too low by an amount equal to the precession from its date to 1950.0. Since the 2 epochs can be almost 2200^y apart, the attendant absolute errors range as high as about 30° (240 BC). For each apparition (1986 back to −239), the correct j & the Hughes value (and their difference) are provided in Table A here (at the end of this section), where the correct results are properly given only to 1° precision since: [a] this precision is fully adequate for classification-purposes, and [b] the computed perihelion-times (upon which the entire classification-scheme is based) do not agree with observations better than similarly crude precision (ordmag 1^d: Yeomans & Kiang 1981 p.642 Table 5 middle column). The Hughes-minus-real differences[17] are also given to 1° precision.

C3 An incompetent scientist could not possibly become a leading officer of the RAS; thus, our explanation of the gross discrepancies listed in the last column of Table A is inescapable: Hughes has made the astonishingly clever & original discovery that: [a] the Comet moves in inertial space, while [b] the Earth moves on a Riemann surface[18] — a conformal remapping of the Earth's inertial-frame motion. Gratefully acknowledging that this represents Hughes' conception of a wholly novel type of celestial behavior, we will henceforth commemorate his immortal discovery with the apt title: the Hughes Transformation. And, noting the pseudo-helical[19] aspect of Hughes' newly revealed aethereal-torque

[14] Hughes goes on to propose that the position of the descending node is crucial, which it is for the close encounters. But this is virtually irrelevant to the cause of 1986 Halley dimness — and that should have been immediately obvious to an astronomer with even moderate gifts in spatial relations.

[15] For the respective adopted ecliptic-epochs, see Yeomans & Kiang 1981 pp.640&642 and Tuckerman 1962&64 1:3 n.1.

[16] Hughes' Fig.1 (& text for Class C at p.516) includes a 31st apparition (2061 AD) but no corresponding data are provided in his Table 1 or Figs.3-4.

[17] The occasional apparent discrepancy of 1° in Table A's last column (*vis-à-vis* the 2 middle columns' difference) is due to rounding.

[18] Hughes' ingenious conception of the Earth's orbital plane can be usefully approximated by a 26000-fold Riemann surface, corresponding to the function $w = z^{1-1/26000}$.

[19] A lesser scientist might see the situation as merely: the 1950.0 ecliptic and the (noninertial) ecliptic-of-date frames rotate (slowly in time) with respect to each other.

space-warp, we may conveniently abbreviate it as simply: the Hughes Screwup.[20]

C4 This is a VicePresident of (& longtime Editor of the house journal of) the venerable Royal Astronomical Society of London, whose Council (on which Hughes has sat & on which he retains numerous faithful friends & promoters) fortunately ignored a series of explicit DR warnings (e.g., 1983/2/9, 10/21, 12/19) regarding *QJRAS* Editor Hughes' demonstrated capacity for disaster. E.g., I wrote the RAS (2/9) that, given the potential for "tragic mistakes" appearing in the *QJRAS*, due to the Editor's incurable noninterest in normal refereeing, "I am advising you to replace David Hughes (of the R.A.S. Council . . .) as *QJRAS* Editor." (See also §B2.) Council responded with the following statement (1983/11/17 letter to DR, signed by the RAS' then-Sec'y — later Pres. — Rodney Davies): in its meeting of 1983/11/11, "Council expressed their full confidence in the Quarterly Journal editorship of Dr David Hughes". And a good thing: had it not been for RAS Pres. Davies' admirably impervious sponsorship, the *QJRAS* could not have presented to the world the invaluable Hughes Screwup.

Table A

Perih Date	Actual j	Hughes' j	Diff
+1986/02/09	195°	195°.53	+01°
+1910/04/20	265°	264°	−01°
+1835/11/16	110°	108°	−02°
+1759/03/13	230°	227°	−03°
+1682/09/15	052°	048°	−04°
+1607/10/27	094°	099°.55	+06°
+1531/08/26	043°	037°.54	−05°
+1456/06/09	329°	323°.27	−06°
+1378/11/10	120°	112°.72	−07°
+1301/10/25	105°	096°.22	−08°
+1222/09/28	078°	068°.83	−09°
+1145/04/18	282°	271°.35	−10°
+1066/03/20	254°	242°.69	−12°
+0989/09/05	057°	044°.80	−13°
+0912/07/18	011°	357°.26	−14°
+0837/02/28	236°	220°.99	−15°
+0760/05/20	316°	299°.97	−16°
+0684/10/02	087°	069°.92	−17°
+0607/03/15	252°	234°.09	−18°
+0530/09/27	082°	062°.76	−19°
+0451/06/28	354°	333°.83	−20°
+0374/02/16	227°	206°.08	−21°
+0295/04/20	289°	267°.16	−22°
+0218/05/17	316°	292°.85	−23°
+0141/03/22	263°	238°.56	−24°
+0066/01/25	208°	182°.87	−25°
−0011/10/10	100°	073°.51	−27°
−0086/08/06	036°	008°.24	−27°
−0163/11/12	134°	105°.93	−29°
−0239/05/25	327°	297°.15	−30°

[20] Hughes 1987 cites Hughes 1985 without correction; thus, we confirm that in his sage retrospective opinion, the proper means of computing the problem is via the Hughes Transformation.

D The Sun Never Rises on the British Umpire

D1 British refereeing procedures' remarkability is hardly restricted to any single incident or person. E.g., the RAS referee form has a portion for confidential remarks by referees. So, not only is the referee's identity confidential (an inverted, ascientific egregiosity in itself, though now docilely accepted as the norm in modern science journaldom) — but even his report may be secret and in various instances has been entirely so. The *J.Brit.Astr.Assoc.* says it does not send referee reports at all "a course forced upon us by the unreasonable behavior of many authors" (BAA Sec'y S.Dunlop, 1982/8/18). Heavens, why should anyone get unreasonable about British astronomy's streamlined starchamber refereeing procedures?

D2 Though I understand that not all RAS Councillors were entirely comfortable with the 1983/11/11 Council course of inaction (§C4), I shall nonetheless list here every person then on the RAS Council, so as to encourage any Councillor who wishes to go on record as having dissented (in whatever degree from the majority's masochistic inclinations) to do so by writing *DIO*. (I won't camp by the mailbox.) The RAS Council at the time (*QJRAS 24*:371): P.Charles, G.Cole,[21] Kenneth Creer, M.Edmunds, R.Fosbury, P.Fowler, D.Heggie, David Hughes, A.King, Pamela Rothwell, A.Roy, I.Williams.

E Classification Fiasco

E1 The learned RAS Council's laboriously considered decision has made it possible for us to be entertained here by the outré spectacle of an appointed, explicitly vouched-for (§C4) official of the Royal Astronomical Society (writing in the RAS' most widely read journal, a journal whose quality is triply fail-safe ensured through its governance by a special Council-appointed watchdog editorial troika: §B6) finding his critical j data by subtracting ecliptic-of-1950.0 longitudes (Comet) from ecliptic-of-date longitudes (Earth), both data impressively provided to $0^\circ.01$ precision — and all accomplished without the faintest awareness of the required precession correction: for 66 AD, merely *twenty six degrees*, an amount thousands of times larger than the precision displayed. Naturally, this spectacular gaffe guts the entire paper as it relates to classifying the then-imminent 1986 apparition — for which the article was published in the first place. (I.e., the various apparition classes are unreliably clustered[22] in Fig.1, rendering it impossible for Hughes to find the simple coherent key explaining 1986's dimshow — a solution to be presented below: §E6.)

E2 The episode is the sort of elementary debacle one customarily associates with a Historian of science or perhaps a lowgrade astrologer.[23] But I have never encountered a paper appearing in a supposedly Reputable astronomical periodical (and certainly not by a scholar who is of all things himself an internationally eminent arbiter of Reputability) which evidenced such pop-occultist-level innocence. (Indeed, it is only fair to add that no serious modern technical astrologer is ignorant of precession, though this hardly excuses the tropical majority's omitting it from horoscopes — unless they privately share my belief that astrological computations are irrelevant since all astrology is pure taurus anyway. See Rawlins 1984A pp.974-975.)

[21] Subsequent top *QJRAS* Editor (& see §B6).

[22] E.g., Hughes' tight Class A is neatly packed into only $16^\circ 1/2$ of the ecliptic in his Fig.1. But in the corrected Table A here, we see the same set of j values diffused over more than 40° — and, moreover, this (& the original) "Class A" space is polluted by the intrusion of -11's apparition, which Hughes' Fig.1 had put into his Class B. (His other misfiled j are cited in §E7.)

[23] For sunsign astrology (the sort that's in newspapers), "signs" are off for the same reason and by the same amount, for which folly astrologers have been incessantly and justly lampooned by centuries of professional astronomers. See, e.g., R.Culver & P.Ianna's informed & (deliberately) amusing *Gemini Syndrome* Tucson 1979 Chap.6.

E3 Understand that, to anyone with the slightest experience in positional astronomy (and this paper deals with little else), the very *first* thought upon encountering Hughes' Class E paradox would be: has precession been properly accounted for? Obviously, Royal Astronomical Society Vice President & Giotto co-investigator Hughes, politically prominent in British astronomical officialdom for over a decade, has no practical familiarity with such chapter one material.[24] This is further evident from his Fig.4 (Hughes 1985 p.518), an illustration presented with no indicated source, but actually based on Yeomans' ephemerides (also used by Bortle 1985, with credit at his p.98); Fig.4 maps Comet Halley's path in R.A. & Decl. for 21 of the 29 recorded pre-1986 apparitions (modernly skipping only 1835 & 1910, ephemerides not provided by Yeomans: Bortle 1985 p.98). The caption to Hughes' Fig.4 fails to inform the reader whether the 21 Comet tracks shown are Equator & Equinox (E&E) of 1950.0 or E&E-of-date.[25] (The former is the case, which severely limits the diagram's value for gauging terrestrial views of the ancient apparitions shown.)[26] Hughes is simply unaware that such things matter . . . . (Which is exactly why the classification-math of Hughes 1985 pulled off the incomparable §B8 achievement of putting history's best and worst Halley apparitions into the very same "Class E"!)

E4 Do they ever matter. The resulting error for the crucial 66 AD apparition (§B11) is virtually a zodiac sign, i.e., precession for 2 millennia — and is, incredibly, *identical to modern astrologers' most infamous and perpetually ridiculed expression of astronomical innocence* (fn 23). As noted above, Hughes' precessional pratfall here was about 26°. At 66 AD Halley perihelion, Earth was 208° ahead of the Comet, not $182^\circ.87$ (i.e., less than 3° from aphelion, which would make it by far the worst apparition of the lot) as on Hughes 1985 p.515 Table 1 (& mapped there in Fig.1 at p.514). And note that this table's data are largely[27] just sloppy interpolations from Tuckerman 1962&64, with, additionally, systematic ignoring of the fact that the Tuckerman dates are for 16^h UT, not 0^h ET, the time standard for Yeomans & Kiang 1981: in antiquity, the Earth-longitude difference is over 3/4 of a degree, which applies slightly against the 26° main (precessional) error for 66 AD, leaving a total j error nearer 25°. Incidentally, after writing the foregoing, my presumably complete amazement at Hughes' scrupulousness was then still further stretched, when I found that *all* of the required (nonHughes) transformations he ignores are explicitly & accurately noted on p.640 of his main source, Yeomans & Kiang 1981.

[24] For similar Hughesiana, see material cited at §B4.

[25] This is where good refereeing comes in. I well remember inadvertently omitting such information for a position datum in the very first paper I ever submitted to a professional journal. Right away, alert *P.A.S.P.* editor Kimball Hansen asked me to specify the E&E epoch.

[26] The same criticism applies to Bortle 1985, but E&E 1950.0 is clearly stated there. Some other small criticisms of this valuable & readable paper: [a] the brightest & most northerly part of the −163 return has occurred before the start of its table on p.99, [b] the greatest Halley near-approach to Earth is misdated on p.104 as 837/4/9 (actual date 837/4/11), and [c] throughout, negative years are wrongly equated to BC years (perhaps due to editorial alterations by the magazine), a calendaric matter which Hughes deals with correctly.

[27] Hughes 1985 p.513 notes no exceptions, but Tuckerman 1962&64 ends at 1649/12/31, so Hughes' Earth-longitudes for the apparitions of 1682, 1759, 1835, 1910 were computed in some uncited fashion and expressed only to $0^\circ.1$ precision (Table I: p.515). The computations are correct for 0 hrs (midnight), which is (unlike earlier Table I Earth-longitude data) consistent with Yeomans & Kiang 1981. Unfortunately, Hughes has some other problems hereabouts (even aside from the obvious fact that consistency of hour does not entail consistency of coordinate system). First, his 1986 Earth-longitude ($140^\circ.85$) is inconsistent with the perihelion time he gives: 1986/2/9.66. (For this time, one finds $140^\circ 37'$.) Perhaps Hughes used a different Yeomans perihelion time. Yeomans' 1983 *Comet Halley Handbook* p.1 makes it 1986/2/9.45175 or 11 AM. Hughes' Earth-longitude is correct for about 1986/2/9.9 (or 10 PM), so perhaps there was a half-day or factor-of-2 confusion here somewhere. In any case, there is no question that Hughes made a huge error for the 1607 apparition, since he failed to note that his main source for Earth-longitude data retained the Julian calendar even after 1582 (as stated at Tuckerman 1962&64 2:1), which is inconsistent by 10 days with Yeomans & Kiang 1981 (who state at p.642 that they follow normal convention: Gregorian dates after 1582). The error caused in Earth-longitude is almost exactly $+10^\circ$, that is: 1000 times the precision. Combined with Hughes' usual errors in precession ($-4^\circ 3/4$ here) and epoch-hour ($+2^\circ/3$ here), the net 1607 error in j is about $+6^\circ$: (Table A), which infects the 1607 data in both Fig.1 and Table I (but not Fig.4 which is entirely based on Yeomans' highly competent work). Below, we will encounter a much more fruitful 10 day Gregorian-Julian calendaric Hughesian mangling: §G.

E5 Once Hughes' enormous omission is corrected for, the 1986 Earth position in his Fig.1 is *thoroughly isolated* (the very result Hughes 1985 vainly sought), only 15° from Halley's aphelion: $j = 195°$. (The nearest competitor is the 66 AD position, over 13° more distant from the aphelion: $j = 208°$.) So 1986 is the sole member of a separate "Class F": F as in fiasco.

E6 Thus, the problem that so mystified Hughes is suddenly resolved into a simple principle (perhaps novel): if the Earth's longitude at Halley perihelion-time is within roughly 15°-20° of the Halley aphelion longitude (a span covering only about 1/10 of the zodiac), then the apparition will end up in Class Fiasco. As just noted: strangely, of history's 30 recorded encounters, there is only one[28] where this is the case, namely, the unfortunate instance of 1986. (However, things can be worse: indeed, if j were near 180°, the Comet would probably not even be noticed by an unsophisticated civilization.)

E7 In addition, a comparison of Table A to Hughes' Fig.1 or Table 1 will show that some discrepancies are so large that they have caused Hughes to put apparitions into the wrong class, according to his own classification-bounds: the j for -86 is about in Hughes' Class B not his Class C; 684 is nearer Class A than Class B; -11 is actually within Hughes' Class A not Class B; the great 837 apparition is within his Class D not Class E; and 295 is nearer Class C than Class D.

E8 Soon after the arrival (in my mailbox) of the *QJRAS* issue containing Hughes 1985, I wrote (1986/2/28, not in confidence) to a number of fellow scholars about this latest of Hughes' contributions to hysterical astronomy. For several years, I looked at each subsequent *QJRAS* but found no printed correction. What does this tell us? [1] No other of over 2000 RAS Fellows, all of whom receive the *QJRAS*, has read the Hughes paper? (Then why publish the *QJRAS*?) [2] They, presumably including the cream of British astronomy, have read it but have not understood the exceedingly simple astronomy any better than Hughes? [3] Some among them have noticed the Hughes Screwup but cannot write the RAS about it, having since been frozen by astonished incredulity or disabled by unremitting seizures of violent mirth? [4] Complaints have been received, but the *QJRAS* has been hesitant about printing them?

E9 It might seem that I should have sent a letter of correction directly to the *QJRAS*. However, given the quality and integrity of the top editorship of that journal, and given its record of frequent nonresponse, this course looked to be just a waste of time & postage — commodities Hughes is himself wisely parsimonious with, as is already clear from my earlier (§B2) admiration of his economies. In any case, if a correction is ever[29] made in *QJRAS*, I will be surprised if DR (who first revealed the full glory of the Hughes Screwup & solved the very problem Hughes poses) is permitted to write the note. (Similar case: ‡6 fn 15.) To test the point, I am sending a copy of this issue of DIO to the RAS, expressing here the request that a very brief, purely technical version (preferably written by *QJRAS*), of the foregoing correction and simple solution, be published in the *QJRAS* correspondence section (*with the corrected*[30] *Fig.1*, i.e., DR's Table A here, above), assented to by DR in writing, and including a reference (with address) to DIO's supplemental *J.HA*, for those *QJRAS* readers who wish full details.

[28] Rather less — but not significantly so — than the chance expectation of 30/10 = 3. See the comments of Yeomans & Kiang 1981 p.644, on pre-240 BC apparitions.

[29] The *QJRAS* may simply do nothing at all. Which will tell observers the obvious: that (in a case where embarrassment might attach to itself) it would rather print a false comet classification than a correct one. Since the paper was published a few years ago, the RAS may resort to a statute-of-limitations alibi. Comments: [a] There is no such statute in science. [b] The *JHA*'s Editor-for-Life, highly esteemed at RAS, expended about 1/4 of the 1987 *JHA*'s regular pages, while printing a patently fallacious & pathetically vain attack upon 2 publications of years past, one of which the *JHA* leadership had been seething about for over a decade.

[30] In addition to the novel spatial data (discussed elsewhere here) provided by Fig.1 of Hughes 1985, one also notes that the same figure has Halley brighter just before perihelion than after!

F Perihelion-Crossed Lovers & Horoscopic Inversion

F1 Mention of Halley perihelion reminds me how refreshingly little astrological garbage surfaced during the Halley rush of '86. That dearth is entirely an accident of astrologers' nonacquaintance with the history of the constellations (including even the sole asterism invented by their very own patron saint, C.Ptolemy). Said innocence protected them (until revelation here, at this safe temporal remove) from an odd little item that would surely have elated (the astrological wing of) astronomy's prostitutes, though all it illustrates is history's abundance of coincidences.

F2 Of all the places in the sky the Halley heliocentric perihelion could have ended up, it fell by chance into the tiny asterism Antinoüs, part of the constellation Aquila. I have related elsewhere (Rawlins 1984A) the sad tale of Antinoüs: the emperor Hadrian's boyfriend, who drowned in the Nile in 130 AD — but was commemorated in the sky when Ptolemy named (*Almajest* 7.5) the 6 most southern stars in Aquila after Antinoüs. (This followed Hadrian's visit to Ptolemy's temple: Rawlins 1984A p.973. Some 20th century star guides still exhibit this minor constellation, shrunk by now to merely the east end of its former self. But the IAU constellation list no longer recognizes Antinoüs; thus, the youth Hadrian sought so assiduously to immortalize seems — barring celestial affirmative-action — certain now to fade into oblivion, outside the realm of the classicists.)

F3 It is possible that there is some connection between Ptolemy's cooperation with Hadrian's desires and his own purely homosexual rules for pairing lovers (details in Rawlins 1977 p.69 & Rawlins 1984A p.974), rules which are now universally used by astrologers (innocent of their invert origin) to advise heterosexuals on forming love-matches. But I think it more likely that placing Antinoüs in the sky was merely symptomatic of Ptolemy's politically expedient pandering (e.g., astrology, geocentricity, & other popular superstition; see Rawlins 1984A & Rawlins 1987 — and here at §B3!), which is the single feature of his intellect that ensured him an immortality that will certainly outlive Antinoüs'.

F4 The *a priori* odds were well over 1 in 1000 against the Halley Comet perihelion being in Antinoüs. (There are $360^2/\pi = 41253$ square degrees in the sky. And modern Antinoüs covers only ordmag 10 of them.) Since comets are traditionally[31] held to be bad omens, one can imagine astrologers' glee at relating Halley's 1986 perihelion, in the sole homosexual constellation, to the fact that 1986 was the blackest year in the twentieth century for homosexuals, due to AIDS, which contracted mass hype at the same time the Comet did.

F5 I can picture the wisdom-of-the-ancient-astromancer gush: did not the whorey bores of yore reveal that comets are bringers of hideous plagues? Of course, *all* recent Halley heliocentric perihelions have been in Antinoüs: 1910,[32] 1835, etc. Also, the AIDS plague probably entered the US in 1979,[33] not 1986.

G In Which Toppe British Cometian Slays Fraudulent Frog First

G1 Though he lived over 85^y, Edmond Halley's only observations of his now-famous Comet occurred entirely within one span of about 3 weeks in 1682 — the same year he married. (A coincidence hardly of the malevolence comets are famous for.) But orthodox history has heretofore recorded that foreigners observed the 1682 return before Halley & other Britons.

[31] Though, see Christopher Marlowe ["Shakespeare"] *Henry VI Part 1*, Act 1, Scene 1, where comets are importuned to sweep away the evil stars connected to Henry V's death.

[32] Moore 1973 p.74 suggests that those veterans who think they remember Comet Halley actually saw another comet of 1910, since Halley "showed at its best from the southern hemisphere." However, according to Bortle 1985 p.110, Comet Halley reached its peak declination at about 20° northern declination, where it was ordmag 100 times brighter than at any 1910 position south of the celestial equator.

[33] Perhaps via Haiti. If so, then the 1979 culprit was not a dim comet but a brilliant President, who cleverly foresaw that a lax immigration policy would help assure his 1980 reelection . . . .

G2 Or so it seemed, until Britain's leading cometian turned his inimitable analytical mentality to this problem, during a paper (Hughes & Drummond 1984, on Halley's 1682 data) appearing in the world's most consciously prestigious astronomical-history periodical: Editor-for-Life (EfL) Michael Hoskin's extremely handsome *Journal for the History of Astronomy* (*JHA*). In this paper, Hughes announced (Hughes & Drummond 1984 pp.189-190) his epochal finding: prior astronomers (e.g., S.Vsekhsvyatskii; see also Bortle 1985 pp.107-109) are mistaken in asserting that the 1682 Comet Halley return was first observed by the French (in Paris) on 1682/8/26. Hughes correctly points out (*ibid*, p.189) that Greenwich astronomers observed the Comet on 1682/8/17. Since 8/17 is 9^d before 8/26, Hughes concludes that the British saw the Comet 9^d ahead of the French.

G3 Hughes also notes (*ibid*, pp.196 and 190) that Halley made the last British observation, on 1682/9/10, and that the last French observation was 1682/9/22 (same date in Bortle 1985 p.107).[34] That would seem to be 12^d later than the British. The mean of 9^d and 12^d is about 10^d.

G4 Paradox: why were the British observers about 10^d *better* than the French at the start of Comet Halley's 1682 apparition, while perversely being about 10^d *worse* than the French (a contrast Hughes does not draw attention to) at the apparition's end? (Anyone with an astronomical-geographical sense of spatial relations can see immediately that this is an absurdity and thus that the 2 nearly equal discrepancies must have some common unremarked source.)

G5 Obvious resolution: France (Catholic) adopted Pope Gregory XIII's superior calendar in 1582, while Britain (Protestant) did so only in 1752 (persisting with the Julian calendar until then). So in 1682 the French and British calendars differed by 10^d. And, after converting (to Julian) the French dates of observation of the Comet, we have: 8/16 and 9/12. Since British astronomers' time range of observation was (according to Hughes' own data, quoted above: §G2-§G3) 8/17 to 9/10, we see that the conventional account is correct: French astronomers saw the comet a little before their British counterparts at the start — and (slightly aided by France's more southerly latitude) saw it a bit later than the British at the end.

G6 I would have sent a correcting note on this to the extremely handsome *JHA* for publication. But, some years ago, I mailed the *JHA* a similar letter (regarding another *JHA* article's foulup), which the Editor-for-Life tried initially to ignore (his own subsequent written boast, incredibly: 1983/3/3 letter noted at ‡6 fn 15). When this proved impossible, EfL then angrily cut correspondence.[35] Therefore, I am unable to send the above correction to the Editor-for-Life (or to unresponsive author Hughes). Still, I'll go through the formality of imparting this *DIO* to some atop *JHA* officialdom, vainly expressing here a request for the printing of DR's (not Hughes') correction, namely: printing in *JHA* the exact *DIO* text given above, running from §G2 (starting at "In this paper") through §G5, including appended bibliographical information (required by the text's short citations), as well as provision of *DIO*'s name & address. The *JHA* Editor-for-Life has here: DR's published, unilateral, unconditional permission to print this correction *verbatim*, thus obviating any *JHA* concern regarding defilement by communication with DR. It will be entertaining to see how *JHA* excuses itself from publishing this brief material.

G7 The *JHA* calendaric messup is particularly peculiar because:

[1] Bernard Yallop of the grand Royal Greenwich Observatory seems to have taken an admirable amount of trouble & expert care to warn Hughes of just this 10^d calendaric difference[36] in another context in the very same paper (Hughes & Drummond 1984 p.196; see also p.189, 1st line: "O.S." [Old Style = Julian calendar]).

[2] The *JHA*'s own maternally-proprietary Editor-for-Life Michael Hoskin (Churchill College, Cambridge U) & Assoc.Ed. O Gingerich (Harvard) clearly regard themselves as authorities on the Gregorian calendar's adoption. EfL co-organized the 1982 conference (at the Vatican Observatory) celebrating its 400th anniversary (& co-edited the resulting published proceedings, Coyne, Hoskin, & Pedersen 1983), while OG published the world's largest astronomy magazine's celebratory history of the Gregorian calendar reform (*Sky&Telescope 64*:530-3). (If our ultimo Hist.sci experts can do anything right, that domain ought to include assisting mere astronomers with calendaric history. But: did either of *JHA*'s ruling editors actually read Hughes & Drummond 1984 before publishing it?)

G8 However, the *JHA* Editor-for-Life's attitude toward refereeing is legendary (‡1 §D4). Thus, the Editor-for-Life has evidently come to believe that high quality *JHA* refereeing is not crucial — since no critic of that extremely handsome journal will dare say anything publicly, no matter how hysterical *JHA* astronomy gets. Right as usual, Governor.[37]

G9 Small wonder that several world class scholars, all of whom have in the past had papers accepted at *JHA*, will no longer send manuscripts there.

H The Brightest Apparition: Halley Himself

H1 Halley had a sense of humor, as is evident even from some of his superficially staid published papers. So I expect he'd see the foregoing in the perspective of human variability. His own work is one of the pinnacles of the glorious British astronomical tradition, so let us conclude here with a remembrance of the circumstances & significance of his most felicitous gamble. I quote from a 1985 November article by one of my brightest & best friends (B.Rawlins 1985 p.7):

> Until Halley's announcement, it was generally[38] presumed that comets only appeared once and never returned. Knowing that (born in 1656) he likely would not live to see its fulfillment, Halley published the 1758 prediction as part of his 1705 *Synopsis of Cometary Astronomy*, a work largely given over to advancing the mathematical treatment of comets' orbits.
>
> The undeniable visual vindication of Halley's genius and daring indeed occurred in 1758, 16 years after his death (1742). On that year's Xmas[39] Day the comet was seen again on Earth — beginning one of its [brightest] apparitions of the 2nd millennium AD. The resighting[40] marked the first predicted return of the first comet subject to longterm prediction. How recently such powers have been the province of man is brought home by the realization that the 1986 return of Halley's Comet, though about the 30th on record, is only the 4th predicted one.

H2 Halley was one of the less gifted observers among Britains's Astronomers Royal; but he was an able, inventive, and bold theorist. The import of Halley's subsuming comets under the umbrella of Newton's gravitational mathematics cannot be overemphasized. Nothing

[34] Note typo: 1682/9/30 magnitude at Bortle 1985 p.109 should read 3.9.

[35] This tantrum occurred just before the erring *JHA* author courageously recanted. But EfL kept hiding from communication, which punitively killed an upcoming unrelated paper, Rawlins 1999, previously multiply-refereed & accepted in toto by *JHA*: ‡6 fn 15 & ‡1 fn 25.

[36] Recall that we found the same 10 day calendaric error in the 1607 j value in Hughes 1985: fn 27. One has to admire a prominent scholar whose ingenuity achieves a coherence of his separate confused papers into one gloriously seamless mass-cohughesion.

[37] Credit: Hedley Lamarr, *Blazing Saddles* (M.Brooks).

[38] A wise early dissenter from this conventional view was Seneca, in the 1st century AD. See Yeomans 1983 p.2.

[39] Ironic in that Halley was notoriously heterodox about religion. And: was Halley so chauvinistic as to plead his Englishness as part of his immortality, as appears in a now famous passage which first appeared in a posthumous work? (Often quoted, e.g., Yeomans & Kiang 1981 p.633.) The even-more-frequently-quoted alleged desire of Mark Twain (1835-1910) to die at Halley's 1910 return (Twain having been born in 1835, the previous appearance-year) was also posthumous thus comparably unverifiable: merely his biographer's recollection of a supposed 1909 Twain remark (A.Paine *Mark Twain: a Biography* 1912 p.1511; reprinted without source on a beautiful 36 cent aerogramme, released by USPS in 1985).

[40] By Johann Palitzsch. See *S&T 73.1*:4-5, 476 (1987/1) and *79.5*:548 (1990/5).

else in Newton's lifetime contributed so powerfully to scholarly realization that gravitation was universal. When 1st broached, the Newtonian conception was not alone in explaining the relatively tame orbits of the 6 known planets (one-directional, near-planar, near-circular, non-intersecting). But only it could also extend, without the slightest ad hoc amendment, to encompass even the wildly noncircular, tilted, overlapping orbits typical of comets. (See Roger Cotes' discussion, written while gravitation was still controversial, in his 1713 preface to the 2nd edition of Newton's *Principia*: Cajori 1934 pp.xxviii-xxx.)

H3 It is the Comet's critical role in the history of civilization that makes me especially glad to have seen Halley on 16 occasions (1985/11/17-1986/5/5), many of them with my wife Barbara and friends. (Including, on 1986/1/8, my old schoolmate & advisor, Baltimore attorney David Eaton and his daughter Caroline, then 5, who will be — as we cautiously told her — the only one of us left to see it return in 2061 AD, when she will be 80.) The Comet was not overwhelming visually. But, scientifically and historically, it certainly was.

References

Almajest. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
John Bortle 1985. Astronomy *13.10*:98.
John Bortle & Chas.Morris 1984. Sky&Tel *67*:9.
F.Cajori 1934, Ed. *Sir Isaac Newton's Mathematical Principles*, UCal.
G.Coyne, M.Hoskin, & O.Pedersen, Eds. *Proc . . . 400th Ann . . . Greg Cal*, VatCty 1983.
David Gregory 1726. *Elements of Physical & Geometrical Astronomy*, London.
Edmond Halley 1705. RoySocPhilTrans *24*:1882.
Edmond Halley 1726. rev. & transl. of Halley 1705; App. to Gregory 1726.
David Hughes 1976. Nature *264*:513.
David Hughes 1979. *Star of Bethlehem*, NYC.
David Hughes 1985. QJRAS *26*:513.
David Hughes 1987. Vistas in Astronomy *30*:145.
David Hughes & Andrew Drummond 1984. JHA *15*:189.
Robert Jastrow 1978. *God & the Astronomers*, NYC.
Karl Manitius 1912-3, Ed. *Handbuch der Astronomie* [*Almajest*], Leipzig.
Patrick Moore 1973. *Comets*, NYC.
T.Pinch & H.Collins 1984. Social Studies of Science *14*:521.
B.Rawlins 1985. Crab [Md.Library Assn.] *15.2*:1.
D.Rawlins 1977. Skeptical Inquirer *2.1*:62.
D.Rawlins 1981S. Fate *34.10*:67.
D.Rawlins 1984A. Queen's Quarterly *91*:969.
D.Rawlins 1987. American Journal of Physics *55*:235. [Note *DIO 11.2* §G & fnn 26-27.]
D.Rawlins 1999. DIO *9.1* ‡3. (Accepted JHA 1981, but suppressed by livid M.Hoskin.)
Colin Ronan 1969A. *Astronomers Royal*, NYC.
Colin Ronan 1969H. *Edmond Halley*, NYC.
Gerald Toomer 1984, Ed. *Ptolemy's Almagest*, NYC.
B.Tuckerman 1962&64. *Planetary, Lunar, & Solar Pos*, AmPhilosSocMem *54*&*56*.
Donald Yeomans 1983. Griffith Observer *47.4*:2.
D.Yeomans & T.Kiang 1981. MonNotRAS *197*:633.

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Retrospective — 2003/3/27

DIO was launched a dozen years ago, first handed out on 1991/1/14 at the American Astronomical Society meeting in Philadelphia.

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