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- Since 1991 inception, has gone without fee to leading scholars & libraries.
- Contributors include world authorities in their respective fields, experts at, e.g., Johns Hopkins University, Cal Tech, Cambridge University, University of London.
- Publisher & journal cited (1996 May 9) in *New York Times* p.1 analysis of his discovery of data exploding Richard Byrd's 1926 North Pole fraud. [*DIO* vol.4.] Full report co-published by University of Cambridge (2000) and *DIO* [vol.10], triggering *History Channel* 2000&2001 recognition of Amundsen's double pole-priority. New photographic proof ending Mt.McKinley fake [*DIO* vol.7]: cited basis of 1998/11/26 *New York Times* p.1 announcement. *Nature* 2000/11/16 cover article pyramid-orientation theory: *DIO*-corrected-recomputed, *Nature* 2001/8/16. Vindicating DR longtime Neptune-affair charges of planet-theft and file-theft: *Scientific American* 2004 December credits *DIO* [vols.2-9]. *DIO*-opposites mentality explored: *NYTimes* Science 2009/9/8 [nytimes.com/tierneylab].
- Journal is published primarily for universities' and scientific institutions' collections; among subscribers by request are libraries at: US Naval Observatory, Cal Tech, Cornell, Johns Hopkins, Oxford & Cambridge, Royal Astronomical Society, British Museum, Royal Observatory (Scotland), the Russian State Library, the International Centre for Theoretical Physics (Trieste), and the universities of Chicago, Toronto, London, Munich, Göttingen, Copenhagen, Stockholm, Tartu, Amsterdam, Liège, Ljubljana, Bologna, Canterbury (NZ).
- New findings on ancient heliocentrists, pre-Hipparchos precession, Mayan eclipse math, Columbus' landfall, Comet Halley apparitions, Peary's fictional Crocker Land.
- Entire *DIO* vol.3 devoted to 1st critical edition of Tycho's legendary 1004-star catalog.
- Investigations of science hoaxes of the –1st, +2nd, 16th, 19th, and 20th centuries.

Paul Forman (History of Physics, Smithsonian Institution): "*DIO* is delightful!"

E. Myles Standish (prime creator of the solar, lunar, & planetary ephemerides for the pre-eminent annual *Astronomical Almanac* of the US Naval Observatory & Royal Greenwich Observatory; recent Chair of American Astronomical Society's Division on Dynamical Astronomy): "a truly intriguing forum, dealing with a variety of subjects, presented often with [its] unique brand of humor, but always with strict adherence to a rigid code of scientific ethics. . . . [and] without pre-conceived biases [an] ambitious and valuable journal."

B. L. van der Waerden (world-renowned University of Zürich mathematician), on *DIO*'s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: "*marvellous*." (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)

Rob't Headland (Scott Polar Research Institute, Cambridge University): Byrd's 1926 latitude-exaggeration has long been suspected, but *DIO*'s 1996 find "has clinched it."

Hugh Thurston (MA, PhD mathematics, Cambridge University; author of highly acclaimed *Early Astronomy*, Springer-Verlag 1994): "*DIO* is fascinating. With . . . mathematical competence, . . . judicious historical perspective, [&] inductive ingenuity, . . . [*DIO*] has solved . . . problems in early astronomy that have resisted attack for centuries"

Annals of Science (1996 July), reviewing *DIO* vol.3 (Tycho star catalog): "a thorough work extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed excellent investigation".

British Society for the History of Mathematics (*Newsletter* 1993 Spring): "fearless [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth."

DIO

Table of Contents

DIO:	Page:
‡1 Pan-Babylonianism Redivivus? Ivy League Fundamentalism: by DAVID DICKS	4
‡2 Columbus's Landfall at Plana Keys: by KEITH PICKERING	14
‡3 Hipparchos' Sites, His Sph Trig, & R.Newton's Star Catalog Test: by RAWLINS	33
‡4 Casting Pearls Before Pyglets: a Cautionary Tale of Duffermuffs & Flatterfeet	48
‡5 Announcing DIO Edition of Tycho's Star Catalog: Gratis to Subscribing Libraries	50
Revised <i>Publisher's Statement</i> : Diversity, Self-Editing, Controversy, & Free Offprints	51

News Notes:

A. DIO 2.3 (1992) ‡8 §A9 wondered aloud at a decade of academic innocence of DR's simple physical solutions for the two highly discrepant extant ancient Earth-size estimates: Eratosthenes' 252000 stades (25200 naut mi) & Poseidonios' 180000 stades (18000 naut mi). (Frequently pseudo-explained away by manipulating the ancient stade's length, despite the final demolition of this avenue by David Dicks at pp.42-46 of his *Geographical Fragments of Hipparchus*, University of London, 1960. See DIO 2.3 ‡8 fn 9.) Jearl Walker immediately responded by sending DIO a photocopy of p.8 of his brandnew edition of the longtime standard college physics textbook, D. Halliday, R. Resnick, & J. Walker, *Fundamentals of Physics* 4th ed, NYC 1993. This develops DR's "double-sunset" solution (which, if accurately performed, will yield Poseidonios' 18000 naut mi, 20% low — a result severely influencing Columbus, who is the subject of article ‡2 in this DIO), and cites the DR 1979/2 *American Journal of Physics* paper announcing it. Rarely has a plea for public enlightenment been so promptly & competently satisfied!

B. DR has repeatedly invited the Neugebauer-Muffia to debate him, face-to-face. (See, e.g., DIO 2.1 ‡2 fn 24 & ‡3 fn 11.) Most recently, prior to & during the 1994/5/6-8 Dibner Inst (M.I.T.) Muffia-dominated conference on "Ancient Astronomy & Divination", DR re-issued the challenge — and even appeared personally at the conference to make tangible the suggested opportunity for arranging debate on the spot. (DR made available past copies of DIO, on the sample-literature table outside the meeting room. I am sorry to report that, at one point, the entire visible collection was stolen. This practice would presumably have continued, had it not swiftly become clear that publisher DR — on the basis of long experience with archons' attitudes & behavior towards criticism — had kept more than enough backup copies in reserve, to ensure the failure of this latest charming History-of-science demonstration of its commitment to open discourse: see also DIO 2.1 p.2. One is reminded of the late N.Simpson's deeply-slit throat: when power-drunks aim at silencing either a person or an idea, the intent is as plain as the coldbloodedness.) At the conference, DR handed out photocopies of DIO's 1994/4/26 letter (to *Isis*) which concluded: "At the proposed debate, Muffiosi will greatly outnumber skeptics (see DIO 2.1 ‡2 §H20). Well, that's OK by DR. Question: just how high must the odds be, before Muffia braves are willing to openly debate those they have never hesitated [e.g., DIO 1.1 ‡1 §C7 & ‡3 §D2-§D3] to slander in private?" (As of this DIO's press-time: still no takers.) Since the Muffia chose (again, as for 2 decades) to duck the challenge, I urge that scholars — who in future find themselves in the presence of a Muffioso who is (again, as for 2 decades) privately denigrating (e.g., DIO-J.HA 1.2 fn 11) the science-history competence of DR or Robert Newton — simply ask said termite a lethally elementary question (which suggests exactly how seriously this subterranean slime ought to be taken): whyever did you not make your statement above-ground, out in the daylight, in DR's presence, at the Dibner Institute conference, **under mutual cross-examination conditions**? Why, indeed?

DIO

Thrice-yearly DIO & its occasional *Journal for Hysterical Astronomy* are published by:

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DIO is primarily a journal of scientific history & principle. At present, a good deal of DIO copy is written by Dennis Rawlins (DR) and associates. However, high scholarship and-or original analytical writing (not necessarily scientific or historical), from any quarter or faction, will be gladly received and considered for publication. Each author has final editorial say over his own article. If refereeing occurs (only with author's explicit permission), the usual handsome-journal anonymity will not — unless in reverse. There are no page charges, and each author receives at least 50 free offprints.

The circumstance that most DIO articles are written by scholars of international repute need not discourage other potential authors, since one of DIO's purposes is the discovery & launching of fresh scholarly talent. Except for equity&charity reply-space material, submissions will be evaluated without regard to the writer's status or identity. We welcome papers which are too original, intelligent, and-or blunt for certain handsome journals. (Dissent & controversy are *per se* obviously no bar to consideration for DIO publication; but, please: spare us the creationist-level junk. I.e., non-Muffia cranks need not apply.)

Permission is hereby granted to other journals to reprint appropriately referenced excerpts from any issue, to date, of DIO or J.HA (edited, if desired, to these journals' stated standards), whether for purposes of enlightenment or criticism or both. Indeed, except for DIO vols.3&5, other journals may entirely republish DIO articles (preferably after open, nonanonymous refereeing). No condition is set except this single one: DIO's name, address, and phone number are to be printed adjacent to the published material and all comments thereon (then *or later*), along with the additional information that said commentary may well be (and, regarding comments on DR output, will certainly be) first replied to — if reply occurs at all — in DIO's pages, not the quoting journal's.

DIO invites communication of readers' comments, analyses, attacks, and-or advice. (Those who wish to be sure of continuing — or not continuing — on the mailing list should say so. It is hoped that our professorial readers will encourage their university libraries to request receipt of DIO: complete sets of back issues are available at no charge.) Written contributions are especially encouraged for the columns: Unpublished Letters, Referees Refereed, and regular Correspondence. (Comments should refer to DIO section-numbers instead of page-numbers.) Contributor-anonymity will be granted on request. Deftly or daftly crafted reports, on appropriate candidates for recognition in J.HA's pages, will of course also be considered for publication. (A subject's eminence may enhance J.HA publication-chances. The writer's won't.)

Free spirits will presumably be pleased (and certain archons will not be surprised) to learn that: at DIO, there is not the slightest fixed standard for writing style.

Potential contributors: send to the above address a *spare* photocopy of material (not to be returned) and phone DIO about 3 weeks later.

Each issue of DIO will be printed on paper which is certified acid-free. The ink isn't.

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‡5 **DIO Volume 3's 1004-Star Tycho Catalog**

Subscribing Libraries Receive Copies Gratis

DIO's entire 1993 output (*DIO* 3.1-3) was devoted to the first critical edition, ever, of Tycho Brahe's justly famous 1004-star catalog (which *DIO* calls "catalog D").

From our preface to this monumental work:

This *DIO* triple-issue represents the first formal critical edition of catalog D (epoch 1601.03), which has until now been the only great pretelescopic star catalog not thusly made available to modern scholars. Provided for the first time: a numbered listing of all 1004 stars' cat D positions (O), their real (C) positions (mean E&E 1601.03), as well as their O–C errors (in both ecliptical & equatorial frames). Our cat D establishes new standards for modern editions of antique star catalogs, including in particular: [a] Identification of every single one of the 1004 star-entries in cat D (Table 21). [b] Listing each star's (null dust&water) culmination-postextinction magnitude μ (also Table 21). [c] Spotlighting of all stars where $\mu > 6$ (Table 18). [d] Providing (Tables 21-23) the great-circle O–C errors for non-great-circle coordinates (longitude λ & right ascension α). [e] Computation of not only error standard deviations (Tables 5-17) but error medians (Tables 1-4). [f] Tabulated least-squares-fits (of constant & of 3-unknown-sinusoid) to catalog errors (Tables 9-12). [g] Individual investigation (by consultation of original field data) of every cat D equatorial position error exceeding a tenth of a great-circle degree (c.200 cases: §M). [h] Rigorous sph trig computation (from the original raw observational data) of all of Tycho's long-murky Final Fifty stars (1596-1597: Tables 19&20). [i] Weeding out stars that are nonexistent, hybrid, fishy, forced, fake,¹ and-or mere repeats (of earlier entries), in order to arrive at an accurate count of the number (965) of distinct outdoor stars Tycho recorded (§K4).

The catalog has been sent automatically, without charge, to those libraries that already receive *DIO*. Other libraries interested in adding the catalog to their collections are invited to contact us.

¹ [*DIO* 2.1 ‡4 established that ten stars in cat D were fabricated. And the subsequent (1994) discovery of an additional forced-data star [D971] (confirmed by DR's direct consultation of Tycho's original mss in the Danish Royal Library) brought the total of such cases to eleven, thereby reducing the number of Tycho-recorded stars from 966 (the [original 1992 edition] count at *DIO* 2.1 ‡4 §B2) to: 965.]

COMPETENCE HELD HOSTAGE

The History-of-Astronomy Journal Watch: #1 of a Series

[Do Not Miss Page 48]

JHA-Isis: THREE YEARS of Unretracted PageOne Mis-Arithmetic

In its 1991/5 issue, the extremely handsome *Journal for the History of Astronomy* published a LEAD [Muffia] article by falsely claiming — on the basis of [UNREFEREED] gradeschool & junior-high mismath (see *DIO-Journal for Hysterical Astronomy* 1.2 §C11, §G4, §G7, §I12, fn 63, & here at ‡4 §A) — that Hipparchos' 3 surviving solar-longitude trios cannot be satisfied by standard Greek-trig (eccentric model) orbits. And *Isis*' 1991/9 LEAD article (in its first University of Chicago issue) was an extension of the same fantasy.

Cleaning up after this mass-suicidal [demo of Hist.sci's refereeing standards], *DIO-J.Hysterical Astron.* 1.2-3 published all 3 "impossible" orbit-solutions, and this double-issue was mailed to the *JHA*, *Isis*, & the author¹ on 1993/12/31. [*Isis* cultishly ashcanned this] *DIO-J.HA* [Note our appreciation of *Isis*' later generous atonement: at *DIO* 14 [2008] Epilog [p.31].] But the esteemed *JHA* Editor-for-Life's reaction was more inspired: the journal was sent back to *DIO*, unopened, bearing His Lordship's inimitable scrawl: "RETURN TO SENDER" — with a pink 1994/1/31 sticker, on which the alleged reason for return is hand-checked "GONE AWAY". It would be churlishly unappreciative of *DIO* not to own that cowering Lord H has, at last, effected a solid contribution to science: establishing the reality of out-of-body teleportation.

The Journal for the History of Astronomy is co-edited by professors at Cambridge University (Hoskin) and Harvard University (Gingerich). Neither institution — nor the History of science Society [DR 2009 note: but see previous paragraph] — appears to have the slightest concern to check the level of integrity, scrupulousness, evenhandedness, or competence displayed by either of these History-of-science worthies. (See similarly at *DIO* 2.1 ‡3 fn 38.)

Muffiosi were 1st informed (written receipt) of the essentials of this mess on 1992/10/30. (Full details sent out 1993/12/31.) So the delay in coming clean has now assumed — and will continue to assume — highly impressive proportions. **We are waiting** to see how many more years will pass before the *JHA* admits what all investigating mathematicians have now mirthfully verified: that the journal the *JHA* loathes above all others has competently discovered and published² the very three orbit-solutions the *JHA* has prominently declared unfindable. Note that the last of these 3 solar orbits ("UH") is historically critical: the reference-orbit adopted by Hipparchos for locating the principal stars on which are based (see *DIO* 1.1 ‡6 §F-§G) the zodiacal longitudes preserved in his immortal (1025-object) star catalog, the oldest substantial star catalog we have (the sole such achievement surviving from antiquity): unmatched for the next 1500 years.

¹ [*DIO* note 2002/10/9 & 2005/3/14: For details of the history of the *JHA*'s eventual partial resolution of its obligations in connection with the matter under discussion here, see *DIO* 6 ‡3. Since publishing the above, *DIO* has come to admire much of [the author's] work. (Though, his *JHA* 33:15-20 paper was particularly disappointing. [See *DIO* 4.2 p.54 fn 2 & *DIO* 11.1 p.26].) Further on his best pioneering research [some of which has come to be as good as it gets — including an enormously appreciated discovery, justly displacing a DR misjudgement]: see *DIO* 1.2 [2001 printing] inside cover, *DIO* 9.1 inside cover, and ESPECIALLY *DIO* 11.2 cover story. For more reflections of our fondness for him and our admiring anticipation of his future great work, see www.dioi.org/pri.htm.]

² The 3 Greek orbits, which *JHA-Isis* 1991 frontpage papers declared impossible to find, have all been recovered by *DIO*. The 12 elements are printed at *DIO-J.Hysterical Astron.* 1.2-3 §G10, §K9, §M4 (& see fn 162 & fn 205).

‡1 Pan-Babylonianism Redivivus?

Fundamentalism¹ in Ivy League Garb

by David Dicks²

A The Integrity of Current History-of-Science Scholarship

A1 One of the few advantages of old age is the ability to take a more synoptic view of things than when one is actively engaged in the pursuance of a career. A certain detachedness can be cultivated by a retired academic; there is no longer any need to kowtow to the pernicious doctrine of ‘Publish or perish’ which seems to be the sole motivation of much of what now passes for scholarship (particularly in the U.S.A.), and, provided one is still sufficiently interested to keep up with developments in one’s chosen field of study (for which, in theory at any rate, there is now more time available), it should be possible to discern from a vista spanning, in my case, some 40 years, changes in the ways particular problems are envisaged and modifications in the methods used to approach them.

A2 It is gratifying to notice that, after my articles on Thales³ and the Pre-Socratics,⁴ there have been fewer attempts to foist anachronistic scientific knowledge on to famous names in the history of early Greek philosophy.⁵ It is also a source of some satisfaction that the views I adumbrated in my book *Early Greek Astronomy to Aristotle* [EGAA], Thames & Hudson, 1970 (e.g., pp.60-61, 89-90, & *passim*) on the course of development of Greek astronomy have now become so commonly accepted that (regrettably) they are paraphrased by many writers on ancient science without any acknowledgement of their source. For example, the main ideas in an article by B.R.Goldstein and A.C.Bowen, misleadingly entitled ‘A New View of Early Greek Astronomy’⁶ — namely, that it was the desire to measure time that triggered off the development of Greek mathematical astronomy, that planetary theory came late (probably astrological doctrines acted as an incentive for it), and the importance of Eudoxus for initiating the scientific stage as distinct from the pre-scientific stage of empirical observations — are all fully developed in EGAA, some 13 years earlier than this derivative article [published by History of Science Soc]. Yet there is not a single reference

¹ [See also *DIO* 1.1 ‡5 fn 12 and *DIO* 1.2 §E4 & fn 129.]

² Long recognized internationally for his expertise in Greek astronomy and geography, David Dicks retired in 1988 as Senior Lecturer in Greek at Royal Holloway & Bedford New College (London University). His academic career has encompassed professorial posts on 3 continents, including a Visiting Professorship at Princeton University and work at the Princeton Institute for Advanced Study. His writings have appeared in numerous prominent professional journals, and he has contributed several entries to the *Dictionary of Scientific Biography*. He is author of two books: *The Geographical Fragments of Hipparchus* (Athlone Press, London, 1960) and *Early Greek Astronomy to Aristotle* (Thames and Hudson, 1970, and Cornell University, 1970).

³ *Classical Quarterly* 9 (1959) 294-309.

⁴ *Journal of Hellenic Studies* 86 (1966) 26-40.

⁵ Although some old stagers continue to fight a losing battle, as witness the remarks of C.H.Kahn in *Science and Philosophy in Classical Greece*, ed. A.C.Bowen (Garland, New York, 1991), pp.2 & 8, who refers to an old controversy in the pages of the *Journal for Hellenic Studies*, but, not unnaturally, fails to mention *JHS* 92 (1972), 175-177, where I administer the *coup de grâce*.

⁶ *Isis* 74 (1983) 330-340.

B DIO-Style Flattery: Two-Dimensional Digits

B1 *DIO*’s new series, ‘Competence Held Hostage’ (debuting here at p.3), owes its inception & title to the locked-horns dynamic of the ongoing ancient-astronomy controversy.

B2 History-of-science archons are chilled by the ghastly realization that those occasional parts of DR’s damned scientific-history researches which they can follow, are turning out to be competent, contributory, even pioneering. (Several of Hist.sci’s own referee reports on DR papers are explicit about this.) Nonetheless, archons have for years effectively attempted to extort DR’s silence (about Hist.sci’s fear-driven censorial obsessions, among other mental limitations) by refusing *publicly* to acknowledge ANY value to DR output.⁷ In brief: able, seminal work has been imprisoned — and vital discovery-pearls’ recognition & development deliberately held hostage — just to protect certain (understandably) insecure Hist.sci archons’ shakily-propped-up images of dignified Authority. [Matt. 7.6.]

B3 An overview of this endless (*DIO* 2.3 ‡8 §D2) warfare suggests that we have here a case of mutual misprojection: [a] Archons apply to DR the same shun-starvation cajolery-techniques which have otherwise worked so unfailingly (note-in-passing: *what* does this say about academe?) when applied to their own fellow climbers. (Hist.sci volk cannot fathom why DR isn’t rushing to fire-sale his soul for the Privilege & Prestige of publication in sham editors’ handsome journals. See *DIO* 1.2 fn 9 & *DIO* 2.1 ‡3 fn 41.) [b] DR is equally blind. Since *DIO* openly admires valid scholarship — regardless (‡3 fn 20 & *DIO* 1.2 fn 16&174) of the source’s enmity and-or (*DIO* 1.1 ‡1 §C6, *DIO* 1.2 §I8) swinosity — DR is implicitly urging Hist.sci also to try impersonal fairness. Instead, snickering at *DIO*’s naïve adherence to their mythic gas about free discourse, worldly archons wonder if DR will ever attain Hist.sci wisdom, accepting that the sin of merely killing truth & its discoverers is trifling compared to Rebellion & Heresy, THE Cardinal Sins of Hist.sci, whose Law is: *Thou Shalt Not Criticize Archons or Their Sacred [Grant-Generating] Tenets*.

B4 Question: what exactly is the competence of the very Muffiosi who reflexively classify any dissenter outside their cult as Incompetent? — and indeed are typified by their tactic of *highlighting others’ supposed errors* as a basis for treating dissenters with slander and (‡1 §A3, *DIO* 1.2 fn 16&92) total ignoring of output. DR’s 1994/4/26 letter² to the Hist.sci Soc vainly challenged (above, p.2) the Muffia to debate & supplied the same 45-item table of Muffia muffs reproduced here at §A: “The [foregoing] 45 (yes **forty-five**) errors by Muffiosi (and Muffia-circle scholars & forums)³ have been pointed out serially since *DIO*’s inception, over 3 years ago. (Many are displayed in the satirelet, ‘Black Affidavit’: *DIO* 1.3 ‡10.) From those responsible for creating and-or promoting this impressively Reputable-looking collection of quasi-kwank⁴ literature, there has been: no response at all. Except the . . . attempted suppression of *DIO* itself.” (See *DIO* 2.1 p.2 *Info-Note*.)

B5 Those who push knowledge forward have always stood on the shoulders of giants. (*Isis* 24:107-109.) But, in History-of-science, they must also stand on the toes of pygmies.

¹ *DIO* 1.2 fn 173: “systematic noncitation . . . constitutes attempted murder of a scholar’s academic career.” The policy is caricatured (only slightly) at *ibid* §H2. Implicit real-political underlying logic noted at *DIO* 2.3 ‡6 §F4. (Similar case cited: *DIO* 1.2 fn 57.) Of course, given *DIO*’s irrepressibility, the inefficacy (more accurately, backfiring: *DIO* 1.2 fn 175) of Hist.sci’s shunning of *DIO*’s achievements is increasingly plain. (And increasingly clumsy: *DIO* 1.2 fn 58 & *DIO* 2.1 p.2 *Info-Note* & ‡2 fn 10.) But the blackballing’s many years of unrelieved institutional maintenance (*DIO* 1.1 ‡1 §A8) have ultimately served a useful purpose: revealing nakedly the real — the 100.00% careerist — face, behind Hist.sci’s public mask of openminded academic curiosity and integrity.

² HsS’s 1994/5/16 standard submit-a-formal-ms reply (contra *DIO* 1.2 fn 165), to *DIO*’s 4/26 letter, evaded the debate-challenge (by delay) & *no-commented* the 45-item list, despite emphatic 4/26 urging that the list be “REFEREED BY COMPETENT SCHOLARS — preferably by real scientists, not the same Hist.sci see-no-evils who’ve allowed the Ptolemy Controversy to fester for a quarter century. (Many of the muffs listed [here at §A] are so obvious that they will require but minutes to check out. Hist.sci archons should have done that a *long* time ago.)”

³ Sorry about printing §A in 7 pt type; but, for this (merely partial) compendium of Muffia muffs, spatial-density is an upshot of certain Muffiosi’s mental-density. Blaming our Muff-Catalog’s scrunched print on anything other than the Muffia’s own peculiar comic genius, is rather analogous to blaming prison-crowding on prison-architects — instead of on criminals’ committing so many crimes. [More Muffs: *DIO* 2.3 ‡8 §§C12-C13, *DIO* 6 ‡1 fn 1.]

⁴ See, e.g., *DIO* 1.1 ‡5 fn 12; and *DIO* 1.2-3 §E4, §G3, & §M7.

‡4 Casting Pearls Before Pyglets

Launching DIO's Competence-Held-Hostage Series

A Muffia Muff-Catalog: the Incompetence-Chargers' Competence

Exposure	Muffer	Sponsor	Muff
<i>DIO</i> 1.2 §R3	G.Toomer	Truesdell	Amazingly crude & mishandled eclipse-selection math.
<i>DIO</i> 1.3 ‡10	G.Toomer	<i>DSB</i>	Autumn Solstice!
<i>DIO</i> 2.1 ‡3 §C15	G.Toomer	<i>AIHS</i>	False explanation of <i>Alm</i> planet mean motions.
<i>DIO</i> 1.2 §I1	G.Toomer	Springer	Forces Greek word for "compiled" to mean what he wants: "computed".
<i>DIO</i> 1.2 §G2	G.Toomer	KramerFund	Cites Toomer 1973 as valid, despite paper's known scribal-error collapse.
<i>DIO</i> 1.1 ‡5 fn 7	N.Swerdlow	<i>Centaurus</i>	Repeatedly erring toward desired number: 19'32" → 19'31" → 19'30".
<i>DIO</i> 2.1 ‡2 §H28	N.Swerdlow	Hist.sciSoc	Bungled attack upon van der Waerden's math, sanity, & ethics.
<i>DIO</i> 1.1 ‡5 fn 20	N.Swerdlow	ΦBK	Alleges tiny near-solstitial motion ruins useful solstice-determination.
<i>DIO</i> 1.2 §E1	N.Swerdlow	ΦBK	Falsely (& trivially) suggests R.Newton hid <i>Almajest</i> -translation used.
<i>DIO</i> 1.1 ‡5 §A2	HamS'low	EfL&0	Misunderstands purpose (even title!) of book under review.
<i>DIO</i> 2.3 ‡8 §C8	N.Swerdlow	EfL&0	Copies Grasshoff misread of Newton 1977, but cites Newton 1977.
<i>DIO</i> 2.3 ‡8 fn 31	N.Swerdlow	EfL&0	Claims 14' waves (12' gt-circ) undetectable in Ancient Star Catalog.
<i>DIO</i> 2.3 ‡8 fn 31	N.Swerdlow	EfL&0	Unaware of required $\cos \beta$ weight-factor for gt-circ λ differentials.
<i>DIO</i> 2.3 ‡8 §C14	N.Swerdlow	EfL&0	Ignores 11' error wave due to attested false obliquity.
<i>DIO</i> 2.1 ‡3 fn 38	O.Pedersen	Odense U	Forced false arithmetic (for <i>Alm</i> planet mean motions).
<i>DIO</i> 1.2 fn 284	Neugebauer	BrownU	Misclaimed Ptolemy didn't believe his absurd lunar quadrature distance.
<i>DIO</i> 2.1 ‡3 fn 38	Neugebauer	Springer	Forced false arithmetic (for <i>Alm</i> planet mean motions).
<i>DIO</i> 1.2 fn 182	Neugebauer	Springer	Another forced math agreement.
<i>DIO</i> 1.2 fn 199	Neugebauer	Springer	Yet another forced math agreement.
<i>DIO</i> 2.1 ‡3 fn 18	R.Mercier	<i>AIHS</i>	Misclaims Ptolemy lacked value for sidereal year.
<i>DIO</i> 1.2 fn 126	Y.Maeyama	Pedersen	Confuses single-datum st.devs with mean's st.devs: off by factors up to 7.
<i>DIO</i> 1.3 ‡10	A.Jones	EfL&0	Winter Equinox!
<i>DIO</i> 1.2 §G4,G7	A.Jones	EfL&0	Innocently & falsely declared 3 solar trios unfittable by Greek-trig orbits.
<i>DIO</i> 1.2 §C11,G7	A.Jones	EfL&0	"Proved" last trio unfittable, though DR had already twice published fit.
<i>DIO</i> 1.2 §G9	A.Jones	EfL&0	Subtraction: $128^\circ - 65^\circ = 65^\circ!$
<i>DIO</i> 1.2 §G9	A.Jones	EfL&0	Sets $67d2/3 = 67^\circ 2/3$ (Velikovsky's 360d year: <i>Worlds in Collision</i> p.330).
<i>DIO</i> 1.2 §G2	A.Jones	Hist.sciSoc	Cites Toomer 1973 as valid, despite paper's known scribal-error collapse.
<i>DIO</i> 1.1 ‡8 §E1	D.Hughes	RoyAstrSoc	<i>The classic astrologer-pratfall.</i> [High precision. Lowlow accuracy!]
<i>DIO</i> 1.1 ‡8 §G5	D.Hughes	EfL	Due to own calendar-blunder, doubts French saw C.Halley first (1682).
<i>DIO</i> 1.2 fn 60	M.Hoskin	EfL	Ignorant of Hegel's 4/3-power proposal, translation omits heart of theory.
<i>DIO</i> 1.2 fn 135	G.Grasshoff	Springer	Numerous graphs' axes are inverted and-or distorted.
<i>DIO</i> 1.2 fn 149-50	G.Grasshoff	Springer	Entire book is chock full of typos.
<i>DIO</i> 1.2 §I3	G.Grasshoff	Springer	Key solar error-curve sign inverted.
<i>DIO</i> 1.2 §I5	G.Grasshoff	Springer	Highly irregular (& suspicious) reference-bibliographical practice.
<i>DIO</i> 1.2 fn 155	G.Grasshoff	Springer	Misclaims R.Newton uses $1^\circ/2$ arc graduation for Ptolemy's astrolabe.
<i>DIO</i> 1.2 §I6	G.Grasshoff	Springer	Data, 20' single-datum st.dev: 100'-amplitude wave = untestably small.
<i>DIO</i> 2.1 ‡3 fn 19	O.Gingerich	RoyAstrSoc	Insisted (over 3 warnings) <i>Alm</i> 9.3 Mars mean motion = <i>Alm</i> 10.9 ratio.
<i>DIO</i> 2.1 ‡3 fn 38	O.Gingerich	RoyAstrSoc	Misplaces Venus' apogee by 4000'.
<i>DIO</i> 2.3 ‡8 §C13	J.Evans	EfL&0	Tries pretending $8' \approx 29'$ (63° phase-diff = "not exactly" in phase).
<i>DIO</i> 1.2 fn 144-5	J.Evans	EfL&0	Signs onto Grasshoff spectacular solar error-curve sign-inversion.
<i>DIO</i> 2.1 ‡4 §H7	J.Evans	EfL&0	Unknowingly demands pretelescope Tycho took stars to 8th magnitude.
<i>DIO</i> 2.3 ‡8 fn 25	J.Evans	EfL&0	Inadvertently has 10th magnitude stars visible to naked-eye.
<i>DIO</i> 1.3 fn 288	J.Evans	EfL&0	Sneers at Ptolemy-doubters on basis of own parallax sign-error.
<i>DIO</i> 1.2 §E1	J.Britton	PrincetonInst	Falsely (without data) suggests R.Newton hid <i>Almajest</i> -translation used.
<i>DIO</i> 1.2 fn 170	J.Britton	PrincetonInst	Patently inaccurate perturbation expression.

[All muffers listed are still active, except the late O.Neugebauer (Princeton Institute). Abbrev for those cited here as "Sponsors": Hist.sciSoc = *Isis* (journal of History of science Society); EfL = Michael Hoskin, CambrU, Editor-for-Life of *J.Hist.Astron.*; 0 = Owen Gingerich, #2 Editor of same *JHA*; *DSB* = *Dictionary of Scientific Biography*; Truesdell = *Arch Hist Exact Sci* (Springer); *AIHS* = *Arch Int Hist Sci*; RoyAstrSoc = *Quarterly J Royal Astr Soc.*]

to *EGAA* in Goldstein & Bowen's work.⁷ It is said that imitation is the sincerest form of flattery, but in reputable scholarly circles such imitation at least refers to the original.

A3 There is a fine dividing line between failure to acknowledge one's indebtedness to earlier writings because the subject matter is so uncontentious that detailed reference to the original chapter and verse is otiose, and a similar failure caused by (at best) slipshod scholarship and ignorance and (at worst) malicious intent or deliberate discourtesy. Most reputable scholars are aware of this line and instinctively stay on the right side of it — others do not. A good example of what I mean is afforded by some passages in [Brown & Harvard professor] G.J.Toomer's 'Hipparchus and Babylonian Astronomy'.⁸ The last paragraph on p.360 and the first on p.361 merely restate in summary form the conclusions I had reached some 18 years earlier (see above: §A2); he even echoes my criticism (unprecedented at that time) of Schiaparelli's treatment of Eudoxus (cf. *EGAA* pp.179-180) but, by omitting to refer to this, conveys the impression that his pompous dismissal (*ipse dixit* 'the Master has spoken') of Schiaparelli's work⁹ is his own original insight. Similarly, his remarks on Hipparchus' rôle in the development of astrology¹⁰ carefully ignore my discussion of this very topic some 28 years earlier in *The Geographical Fragments of Hipparchus* [*GFH*] (Athlone Press, London, 1960), pp.11-14. It might have been thought that simple academic courtesy would have dictated at least a brief reference to these sources; but the school to which Toomer belongs¹¹ disdains such niceties and sedulously avoids any appreciation of scholarship outside its own narrow confines.¹²

B The Central Error of R.Newton, D.Rawlins, & Others

B1 There is, however, another development which I have noticed increasingly in writings on ancient science in the last few decades and which should be unreservedly condemned — namely, a refusal to credit the plain evidence of ancient texts (which, if not ignored, are wilfully misinterpreted) if this goes against the particular far-fetched theory being promulgated by the writer at the time; not only this, but the ancient writer himself is implicitly (or even explicitly) criticised if his remarks do not support the modern commentator's fantasies. Out of the numerous examples of this type of misrepresentation, I select a few of the more blatant. The arch exponent is, of course, R.R.Newton [Johns Hopkins University Applied Physics Laboratory] who in his tendentiously entitled book, *The Crime of Claudius Ptolemy*,¹³ by a mixture of an unhistorical approach, slipshod scholarship,¹⁴ and a method-

⁷ The fact that the diagrams on their p.335 bear a remarkable resemblance to mine in *EGAA* p.18 and *GFH* p.165 is no doubt coincidental

⁸ In *A Scientific Humanist: Studies in Memory of Abraham Sachs*, ed. E.Leichty, M.de J.Ellis & P.Gerardi (Philadelphia, 1988), pp.353-362.

⁹ *Ibid.* p.360 note 42.

¹⁰ *Ibid.* p.362.

¹¹ For the uninitiated, adherents to this school (irreverently named the 'Muffia' by D. Rawlins in his samizdat publications — see note 27 below) can be readily identified by the frequent appearance of the letters *HAMA* in their writings — this is an acronym for *History of Ancient Mathematical Astronomy*, 3 vols. 1975, the *chef d'oeuvre* of the late O. Neugebauer of Brown University, Providence, R.I. Neugebauer is to American historians of science in the second half of the twentieth century what G. Sarton was in the first half.

¹² As I have had occasion to remark before, in my review of D.Pingree's Teubner edition of Vettius Valens in *Classical Review* 39 (1989) p.24. [Similarly, see *J.Hysterical Astron.* 1.2 fn 148 & §J2, and *DIO-JHA* 1.3 ‡10, "Black Affidavit".]

¹³ Johns Hopkins Univ. Press, Baltimore, 1977.

¹⁴ As early as the Preface (p.xiii) we read, "In some cases, a topic has not seemed important enough to warrant the labor of locating a hard-to-get reference, and I have relied on secondary sources for these minor topics". Cf. p.42, "I have not seen an explicit reference to an ancient source which refers to the caravan method [for Eratosthenes' method — probably mythical (cf. J.Dutka in *Arch Hist Exact Sci* 46, 1993, p.58) — of estimating the distance from Syene to Alexandria], but I have not searched very hard"; p.136 [in a footnote], "I have not located this statement in the *Syntax* [Ptolemy], but I have not tried very hard to do so."

ologically unsound treatment of ancient observations,¹⁵ managed to convince himself (if few others) that Ptolemy in the *Almagest* (our primary source for ancient Greek astronomy) consistently lied in his presentation of the evidence and that all the observations which he says he made were, in fact, ‘fudged’ to suit his argument. In subsequent publications attempting to substantiate this same theme of Ptolemy’s fraudulence, he employs similar flawed techniques, and it is, perhaps, a measure of the mesmeric fascination of mathematics (as well as the gullibility of some modern writers on astronomy) that his erroneous views have been accorded more attention than they deserve.¹⁶

B2 However, Newton is by no means the only exponent of this art of misrepresentation; in fact, his severest critics reveal themselves as past masters at it. Swerdlow [University of Chicago], in an article¹⁷ that contains one or two sensible remarks¹⁸ amidst a plethora of baseless speculation, has no compunction in disbelieving what Ptolemy tells us about the development of solar theory in *Alm.* iii,1 and lunar theory in *Alm.* iv,2 because it does not fit in with Swerdlow’s (and many others’ — see below: §C1ff.) assumption that the Greek astronomers and especially Hipparchus took over most of the astronomical period relationships that they used from the Babylonians; when he can find no evidence for this unwarranted assumption — which itself arises from a conjecture of Kugler’s, over-enthusiastically taken up by [Yale’s] Aaboe and [Brown-Harvard’s] Toomer (see below: §C2) — Swerdlow is driven to complain (p.296 footnote 7), “Ptolemy appears to be unaware of the Babylonian origin, or even the pre-Hipparchan origin, of this parameter and of the other period relations in Hipparchus’ lunar theory”! He is apparently oblivious of the fact that this single sentence gives the game away completely, and shows that all his juggling with figures, and his belief that “Hipparchus need not have derived the value of the tropical year mentioned by Ptolemy, 365 1/4 – 1/300^d, directly from observation” (p.297), is a house of cards built on a complete disregard of what Hipparchus and Ptolemy actually say. Ironically enough, Swerdlow goes on to say, “. . . if Hipparchus had observations leading to one day in 300 years precisely, one would think that Ptolemy would have cited them”; but would Swerdlow have believed him? Hardly, to judge from the mode of ‘reasoning’ in this paper.¹⁹

B3 Even more perverse is his discussion of precession. Ptolemy first mentions this phenomenon, and ascribes its discovery to Hipparchus, in the long first chapter of the third book of the *Almagest* where he relies heavily on the latter’s results and cites (sometimes verbatim) extracts from his lost works, *On the Displacement of the Tropical and Equinoctial Points* (which, in all probability, was where Hipparchus first announced his discovery), *On the Length of the Year*, and *On Intercalary Months and Days*; but Ptolemy chooses to

¹⁵ It seems to me to be methodologically wrong to try to apply sophisticated mathematical techniques to the astronomical observations reported from antiquity. Such techniques are surely relevant only to modern-style observations carefully carried out with respect to possible sources of error, the observer’s personal equation, statistical probabilities, mean values derived from hundreds of observations, etc., etc. None of this (except the occasional reference to and elementary discussion of very obvious sources of error, such as the shifting of the alignment of instruments and the effects of refraction on horizon phenomena) can properly be imputed to ancient observations, and it is therefore futile to treat them mathematically as though they were results emanating from a modern observatory. It is rather like insisting on using microscopes, pipettes and sensitive chemical balances in the preparation of farmhouse cookery recipes — and about as sensible.

¹⁶ I am not alone in remarking this — cf. K. Okruhlik in *Proc. Philos. of Sci. Assoc.* 1 (1978) 80-81. Other relevant publications of Newton are *Ancient Planetary Observations and the Validity of Ephemeris Time* (Johns Hopkins Univ. Press, Baltimore, 1976), reviewed with proper condemnation by N.T. Hamilton & N.M. Swerdlow [Newton replies at *DIO* 1.1 †5] and by R. Mercier in *British Journal for the Hist. of Sci.* 12 (1979) 211-217, and *The Origins of Ptolemy’s Astronomical Parameters* (Centre for Archaeoastronomy, Technical Publication No.4, 1982), and *The Origins of Ptolemy’s Astronomical Tables* (Centre for Archaeoastronomy, Technical Publication No.5, 1985), reviewed (in too kindly a fashion) by J. Evans in *Journal for the History of Astronomy [JHA]* 24 (1993) 145-147.

¹⁷ *Archive for History of Exact Sciences* 21 (1980) 291-309.

¹⁸ E.g., p.293 note b, “Some of these derivations [by modern commentators discussing ancient period relationships] seem a bit round-about . . . and all seem more reliant upon a decimal calculator than upon the text of the *Almagest*.” Unfortunately, he does not seem to realise that this applies with equal force to his own work. [See *DIO* 1.1 †5 fn 7.]

¹⁹ See his unsatisfactory treatment (p.299) of the order in which Hipparchus’ treatises appeared — for a more satisfactory discussion see my *GFH* p.17.

References

- Almagest*. Compiled Ptolemy c.160 AD. Eds: Manitius 1912-3; Toomer 1984.
 J. Delambre 1817. *Histoire de l’Astronomie Ancienne*, Paris.
 David Dicks 1960. *Geographical Fragments of Hipparchus*, U.London.
 DSB = *Dictionary of Scientific Biography*, Ed: C. Gillispie, NYC.
 Aubrey Diller 1934. *Klio* 27:258.
 J. Evans 1987. *JHA* 18:155 & 233.
 J. Evans 1993. *JHA* 24:145.
GD = *Geographical Directory*. Ptolemy c.160 AD. B&J. Complete eds: Nobbe; S&G.
 Gerd Graßhoff 1990. *History of Ptolemy’s Star Catalogue*, NYC.
 Hipparchos. *Commentary on Aratos & Eudoxos* c.130 BC. Ed: Manitius, Leipzig 1894.
 Karl Manitius 1912-3, Ed. *Handbuch der Astronomie [Almagest]*, Leipzig.
 C. Müller 1855&1882. *Geographi Graeci Minores*, Paris.
 C. Müller 1883&1901. *Claudii Ptolemæi Geographia*, Paris. (Bks.1-5 of *GD*, plus maps.)
 O. Neugebauer 1975. *History of Ancient Mathematical Astronomy (HAMA)*, NYC.
 R. Newton 1974. *MonNotRAS* 169:331.
 R. Newton 1977. *Crime of Claudius Ptolemy*, Johns Hopkins U.
 C. Nobbe 1843-5. *Claudii Ptolemæi Geographia*, Leipzig. Repr 1966, p. A. Diller.
 PK = C. Peters & E. Knobel 1915. *Ptolemy’s Catalogue of Stars*, Carnegie Inst., Publ.#86.
 Pliny the Elder. *Natural History* 77 AD. Ed: H. Rackham, LCL 1938-62.
 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 1985G. *Vistas in Astronomy* 28:255.
 D. Rawlins 1987. *American Journal of Physics* 55:235.
 D. Rawlins 1991H. *DIO* 1.1 †6.
 D. Rawlins in-prep D. *DIO*. (Distributed 1982.)
 D. Rawlins 1999. *DIO* 9.1 †3. (Accepted *JHA* 1981, but suppressed by livid M. Hoskin.)
 M. Shevchenko 1990. *JHA* 21:187.
 S&G = A. Stückelberger & G. Graßhoff 2006. *Ptolemaios Handbuch Geographie*, U. Bern.
 Suda Lexicon. Compiled c.1000 AD. Ed: Ada Adler, Leipzig 1928-1938.
 Noel Swerdlow 1992. *JHA* 23:173.
 Gerald Toomer 1984, Ed. *Ptolemy’s Almagest*, NYC.
 H. Vogt 1925. *AstrNachr* 224:17.
 J. Włodarczyk 1990. *JHA* 21:283.

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[Note added 1996: In the larger context of the controversy over Ptolemy’s integrity (which has in late years lingered on only because his defenders understand so little science that they don’t know it’s over), the significance of the foregoing paper may be not be fully appreciated unless the reader realizes that the popular “pedagogical” apology (e.g., 0 Gingerich *Q. Jl Roy. Astr. Soc.* 21:253) for Ptolemy’s nonstellar fakery is irrelevant to excusing his sneak-theft-plagiarism of virtually the entire Ancient Star Catalog, of which very few stars were used in any of his *Almagest* computational examples. (For details on this and related matters, see, e.g., *DIO* 2.3 †8 §§C2, C22-C23, C31-33, & fn 22. In truth, most of these “examples” were intended not to instruct his readers but rather to fool them into accepting that his astronomical models were precisely & universally accurate because they were founded, by rigorous mathematics, upon outdoor observations. And most of these “observations” were also pretenses.) The bottom line here: no honest pedagogue would resort to the slide&hide ploy to hide a massive plagiarism: a *thousand* stars.]

ensured that the sea-horizon was the most southerly possible from coastal⁴⁸ Rhodos. Note that Rawlins 1982C found the odds slightly higher for southern than northern Rhodos as Hipparchos' location, though the northern part of the island was not statistically ruled out by the 1982 analysis.

G3 There is but one legend that survives regarding Hipparchos' personal life: it has him astonishing a king with a bit of weather astrology. (See Dicks 1960 pp.48-49: *Fragm.C.*) This does not sound like a fellow who lived in remote woodlands. So, once we have the center of Hipparchos' operations (where his high-quality observations were made) near latitude $\phi = 36^{\circ}08'N$, it is not difficult to find his longitude, since anything but the vicinity of Lindos ($\phi = 36^{\circ}05'N$, longitude $28^{\circ}05'E$ of Greenwich) would put him far from high civilization. His 19 accurate declinations⁴⁹ reveal that he observed them in $E = -131 \pm 5^y$ (Rawlins in-prep D), and that his $\Delta\phi = 0' \pm 1'$. (See *idem* & §F1. Confirmed: fn 6.) Thus, we may say that his main observatory's latitude $\phi = 36^{\circ}08'N \pm 1'$ — which is an unexpectedly & gratifyingly precise probable solution to this ancient mystery. As remarked at §F3: since Lindos' $\phi = 36^{\circ}05'N$ (just 3' less),⁵⁰ it is indicated that Hipparchos worked in the hills just NW of the city. (The 371 m hill mentioned in §F3 is at $36^{\circ}08'N$, $28^{\circ}03'E$.) Perhaps the observatory was adjacent to (or part of) a local ruler's estate.

G4 As noted by Neugebauer 1975 (p.275 n.11), the Keskindo inscription — evidently from near Hipparchos' century (Neugebauer 1975 pp.698-705) — reveals that other astrologers were working on the island of Rhodos. Keskindo is a west suburb of Lindos — just 3 nmi south (& a little west) of the hill suggested above as a possible site of Hipparchos' observatory. The finding that he observed almost nextdoor⁵¹ to Keskindo suggests that several astronomer-astrologers worked in the Lindos region — with a cohesion which we can now only guess at. It is possible that the southern portion of the Catalog was observed by an astronomer at the south end of the island who was part of a team effort (§C6) to cover the sky, presumably supervised by Hipparchos.

G5 Perhaps it is too much to hope that fragments of (or inscriptions from) Hipparchos' legendary observatory might someday be recovered. In any case, one hopes that the foregoing will assist in greatly narrowing the range of search.

⁴⁸ Terrain over 200 m high is available near C.Prassonesi, which could assist observations by slightly reducing extinction & by producing a horizon dip of nearly a half-degree. Rhodos' tallest mountain, 1215 m-high Mt.Atabyron, at $36^{\circ}12'N$, would offer a sea-horizon or *mag* 10' more southerly, but the difference would hardly be worth the trouble of building & supplying an observatory at such a remote site.

⁴⁹ Sample described at §F2. Results (for E & $\Delta\phi$) based upon same sort of 2-unknown least-squares analyses as those of §F9. Adding the 10 fractional δ from Hipparchos *Comm* (producing a 29 star sample, net), we have instead $E = -138 \pm 7^y$ & $\phi = 36^{\circ}06' \pm 2'N$. Adding Arcturus to these (30 stars in all) produces: $E = -134 \pm 7^y$ & $\phi = 36^{\circ}07' \pm 2'N$. (For just our ten fractional Hipparchos *Comm* stars: $E = -159 \pm 19^y$ & $\phi = 36^{\circ}02' \pm 4'N$.) However, using the Hipparchos *Comm* stars here alters the 19 star result only slightly and brings less reliable data into the problem. (DR's prime statistical rule: a small clean sample is preferable to a big dirty one.)

⁵⁰ An ancient geographer of c.100 BC, using the Eratosthenes-Hipparchos scale (700 stades/degree), placed Lindos 4500 stades north of Alexandria (Müller 1855&1882 2:479). All ancients knew that the distance from Alexandria to Rhodos was under 4000 stades, thus the 4500 stades figure is likely to be an error for 3500 stades — which is precisely 5°. Eratosthenes put Alexandria at $\phi = 31^{\circ}04'N$ — evidently rounded by Hipparchos to $\phi = 31^{\circ}05'N$ (fn 44) — so we may take the 5° difference as indicating that some ancients (contra fn 34) placed Lindos at $\phi = 36^{\circ}04'N$ or $36^{\circ}05'N$, which is within a mile of the truth. I note that the same geographer also reports (Müller 1855&1882 2:479) the Tanais (Don River) klima, where $M = 17$ hrs, to be 18056 stades north of Alexandria. Well, using Eratosthenes' obliquity (*Almagest* 1.12: $\epsilon = 23^{\circ}51'20''$) in the formula of fn 34, we find $\phi = 54^{\circ}00'18''N$. (Toomer 1984 p.87 n.56 gets precisely the same result.) At 700 stades/degree, this is 37803 stades and Alexandria's $\phi = 31^{\circ}04'N = 21747$ stades. The difference is 16056 stades, which disagrees with the text (18056 st) by precisely 2000 st. Thus, if we again (as for the Alexandria-Lindos latitude difference) suppose there to be an ancient error (or discrepant convention) in the thousands-place, the passage has been restored. (Some uncertainty in the latter case's thousands-place is discussed in the notes of Müller *loc cit.*)

⁵¹ It is of course possible that Hipparchos worked in the region of Lardo ($36^{\circ}05'.4'N$, $28^{\circ}02'E$) or Keskindo ($36^{\circ}04'.7'N$, $28^{\circ}01'E$), the 2 towns Neugebauer 1975 p.698 n.1 mentions in connection with the inscription. (The inscription's numbers are not related to what we now know of Hipparchos' work.) Given (via least-squares: §G3) that Hipparchos' adopted ϕ ($36^{\circ}08'$ or $36^{\circ}07'1/2$, perhaps interchangeably: §F3) was high by $0'.2 \pm 1'.2$, the inscription-site latitude's 95%-confidence statistical incompatibility with it is too borderline for safe exclusion.

postpone detailed discussion of precession until that part of his work that deals with the fixed stars²⁰ because, as he explains, the prior establishment of solar and lunar theory is indispensable for a proper study of the fixed stars, and such theory depends fundamentally on establishing an accurate value for the length of the tropical year, which forms the main topic for *Alm.* iii,1. Ptolemy's account is perfectly clear and logical — the only slight complication is that some observations of Spica (α Vir) and of lunar eclipses made by Hipparchus relative to the equinoctial points obviously played a dual rôle in helping to estimate both the length of the year and the rate of precession, which leads to a certain amount of repetition in *Alm.* iii,1 and *Alm.* vii,1-3 where Ptolemy treats precession at some length. He tells us that Hipparchus was led to his discovery by noting that, according to the observations made by Timocharis in Alexandria (about 150 years earlier), the position of Spica was estimated to be 8° from the autumnal equinoctial point, while his own observations gave a figure of 6° ; and Ptolemy specifically tells us that Hipparchus found the same difference with other fixed stars, a difference that Ptolemy was able to confirm by comparing his own observations with those of Hipparchus — he even mentions the instrument he used (the armillary astrolabe, described in *Alm.* v,1).²¹ Then in *Alm.* vii,3 further observations by Timocharis, Hipparchus, Menelaus, Agrippa, and Ptolemy himself are cited, from which a final figure of 1° in 100 years for the precessional movement (envisioned as a very slow rotation of the sphere of the fixed stars) is deduced.

B4 Now, what does Swerdlow make of all this? Very little, it seems. So obsessed is he with trying to 'prove' his absurdly speculative thesis that Hipparchus derived his rate of precession from prior knowledge (based, of course, on Babylonian sources) of the difference between the tropical and sidereal year and not (as we are plainly told) by comparing fixed star observations made by Timocharis and himself, that he can actually say (p.301), "It is **generally supposed** that Hipparchus derived his estimate of the precession from comparisons of observations of the fixed stars by Timocharis and himself separated by an interval of about 150 years. Indeed, **something of the sort may have played a role** in his qualitative recognition of precession and the distinction of the sidereal and tropical year . . ." [my emphases]. This travesty of interpretation is Swerdlow's apparent reaction to Ptolemy's clear and straightforward account!! One is entitled to ask what is the point of reading the ancient writers at all, if their explicit testimony is to be disregarded in favour of the far-fetched speculations of modern commentators?²²

B5 Swerdlow goes on (p.305) to refer to Neugebauer's discussion of various Babylonian "years" in *HAMA*, 528-529 and quotes his mention of one parameter "attributed by Vettius Valens (2nd century) to the Babylonians, **although this parameter is not directly supported by any surviving cuneiform source**" [my emphasis]. Precisely! What Swerdlow significantly fails to quote is Neugebauer's remark on p.529, "All these 'years' are certainly to be taken as sidereal years, even if their derivation, from a modern viewpoint, would make them 'tropical' or 'anomalistic' years. [See *DIO* 1.1 ‡5 fn 8.] Our sources leave no doubt that **such distinctions lie outside the framework of Babylonian astronomy**" [my emphasis]. Even Swerdlow feels obliged to say (*loc.cit.*), "Much of this reconstruction is obviously speculative"; such a statement of the obvious renders nugatory his optimistic claim on p.306, "The results of this study may be instructive . . ."

²⁰ *Alm.* vii,1-3.

²¹ Cf. my paper, 'Ancient Astronomical Instruments' in *J.Brit. Astron. Assoc.* 64 (1954) 77-85.

²² It is possible that acquaintance with the ancient texts only through translations plays a part in such misinterpretations. Certainly, Swerdlow thrice (pp.300, 306, & 307) quotes Toomer's erroneous translation of the Greek $\upsilon\pi\omicron\nu\epsilon\nu\eta\omicron\kappa\omicron\varsigma \phi\alpha\iota\nu\epsilon\tau\alpha$ as "seems to have suspected"; but this is to confuse the use of $\phi\alpha\iota\nu\omicron\mu\alpha$ with the infinitive and its use with the participle — a distinction that I used to try to hammer home in my Beginners Greek classes! If Ptolemy had meant "seems to have suspected" he would have written $\upsilon\pi\omicron\nu\epsilon\nu\eta\omicron\kappa\omicron\varsigma$ and *not* $\upsilon\pi\omicron\nu\epsilon\nu\eta\omicron\kappa\omicron\varsigma$ which means "clearly [or plainly or evidently or obviously] has suspected". Manitius' [1912-1913 Teubner *Almagest*] translation (Bd.II, p.15, line 10), "Das ist offenbar auch die mit Vorbehalt hingestellte Annahme Hipparchus gewesen" ["This is evidently also Hipparchus' opinion put forward with reservation"], is much more accurate; although even he 'nods' on occasion (see *JHS* 103, 1983, p.137, 'A Mistranslation in Manitius').

C The Muffia’s Babylonian-Origin Idée Fixe

C1 Such wholesale disregard and distortion of Ptolemy’s evidence has, unfortunately, proved irresistible for other members of the ‘Muffia’, who have vied with each other in selecting other parts of the Greek evidence to traduce in favour of their own misguided pre-conceptions. Thus, that well-known double act in American ‘scholarship’, Goldstein and Bowen, in their inimitable, long-winded and pompous style,²³ claim to “demonstrate that many of Hipparchus’ reports and criticisms concerning Eudoxus and Aratus are, on occasion, anachronistic and even polemical” and “propose that Hipparchus does not describe faithfully the state of astronomy before his time but ‘modernized’ it, thereby providing Ptolemy with an astronomical history that was in some respects inaccurate and distorted”.²⁴ One shudders to think what an “inaccurate and distorted” picture future historians of science might receive from papers such as this, which dresses up total misconceptions in pseudo-scientific language (à la R.R.Newton), demonstrates ignorance of the relevant scholarly literature (as in §1, which, e.g., fails to mention my *GFH* which deals with just the topics under discussion — cf. pp.174-175), and labours mightily to “show that the division of the day into equinoctial hours or into 360° of time was unknown in Greece at the time of Eudoxus” (cf. §§3-4), a statement of the obvious that nobody with any competence in the field would waste time on. At least, however, Bowen & Goldstein do not (yet — no doubt it will come . . .) lay claim to be clairvoyant about Hipparchus’ methods, which is what Y.Maeyama does in an article²⁵ that not only follows the modern fashion of subjecting ancient data to inappropriately sophisticated mathematical techniques (on the illegitimacy of this see note 15 above) — and, naturally, praises Newton (p.307, “In his excellent study on fractions of degrees Newton has shown . . . This would agree with Newton’s highly interesting study on fractions of degrees”) — but also actually claims to know just how Hipparchus worked (p.305, “Hipparchus left writings on only specific problems, *driven to their solutions by new discoveries which often visited him all of a sudden*”) [my emphasis]!! A claim that is about as justified as his assumption “that there must have been abundant accurate observations of the fixed stars made at least at the epochs –300 ~ –250 in Alexandria. They must have disappeared in the fires which frequently raged there” (p.302). The cavalier manner in which ancient evidence is treated by modern commentators is well illustrated by this last remark; Maeyama is forced into making it because of his perverse refusal to accept Hipparchus’ low opinion (reported by Ptolemy in *Alm.* vii,3) of Timocharis’ stellar observations, a refusal which itself is a direct result of the application of inappropriate mathematical methods to the ancient data. One is tempted to quote Scott’s “Oh, what a tangled web we weave . . .”! It would be tedious to cite all the recent effusions of commentators intent on (a) disbelieving and denigrating as much of the ancient sources as they can, and (b) insisting on the Babylonian origin of most of Greek astronomy [note by DR: it should be stated that the present work was independently completed & circulated before the author saw the DR & Thurston anti-(b) analyses cited below here, in the bracketted portion of fn 37]; but one particularly glaring example must be given. This is a paper by A.Jones²⁶ [University of Toronto & board of *Archive for History of Exact Sciences*], which as well as exemplifying both (a) and (b) above, invents the brand new concept of a “winter equinox” (p.119),²⁷ and contains the truly incredible sentence (p.122), “Ptolemy’s organization of the theories of the Sun, Moon, and planets into an apparently rigorous logical progress from which every trace of Babylonian methodology was ruthlessly expunged, must be seen as a radical reform of

²³ Commented on even by fellow ‘Muffia’ member O.Gingerich [Harvard History of Science Department] in *JHA* 22 (1991) 186-187.

²⁴ *Proc. Amer. Philos. Assoc.* vol.135 no.2 (1991), p.235.

²⁵ *Centaurus* 27 (1984) 280-310. [See *J.Hysterical Astron.* 1.2 fn 126.]

²⁶ *Journal for the History of Astronomy* 22 (1991) 101-125. [*DIO* 1.2-3 is devoted almost entirely to analysis of this bizarre *JHA* frontpage paper’s math follies, which include several highschool-level foulups. See above at p.3. See also R.Newton at *DIO-JHA* 1.2 §F3.]

²⁷ On this, see Rawlins in *DIO*, vol.2, no.3 (1992 October), p.102ff. [Also *J.Hysterical Astron.* 1.2 §B4.]

Table 3: Ancient Observers’ Epochs & Geographical Latitudes

Observer	Epoch E	$\pm\sigma_E$	Latitude ϕ	$\pm\sigma_\phi$
Timocharis	–295	$\pm 11^y$	31° 14’N	$\pm 3'$
Aristyllos	–257	$\pm 10^y$	31° 14’N	$\pm 3'$
Hipparchos	–131	$\pm 05^y$	36° 08’N	$\pm 1'$
Anonymous	159	$\pm 08^y$	31° 11’N	$\pm 2'$

(6 stars): $\Delta\phi = +1' \pm 3'$. Anonymous (the 12 nonsuspect stars): $\Delta\phi = +4' \pm 2'$. (Utterly incompatible with the $\Delta\phi = -14'$ of Ptolemy, who falsely claimed to have observed these stars: see fn 6.) In each of the 3 cases, the sign of the $\Delta\phi$ solution is consistent with the difference between the observer’s adopted ϕ and the real Alexandria ϕ (Museum 31° 12’N, Lighthouse 31° 13’N). Including Hipparchos (§G3), the star-declination-based solutions for epoch⁴⁵ E & latitude ϕ (both actual, not adopted values) are as set forth in Table 3.

G Hipparchos’ Sites

G1 For centuries, astronomers have wondered where exactly on Rhodos lie the remains of the great central observatory of Hipparchos, the legendary⁴⁶ “father of astronomy”. (The remains of Hipparchos himself almost certainly lie at the same site.) Now, at last, we have some probable answers.

G2 From the foregoing, we find that the southern stars of the Ancient Star Catalog were observed with an inferior⁴⁷ transit instrument at Cape Prassonesi, the southern tip of the island of Rhodos. The site is reasonable for deep-south observations, since: [a] it permitted a more unobstructed view of the southern horizon than did any other readily-accessible part of Rhodos Island, and [b] observing from the most southerly latitude on the island

⁴⁵ The deduced epochs E of Table 3 are consistent ($\pm\sigma_E$) with the start of the following (19^y) cycles of Meton’s famous calendar (starting epoch = –431 Summer Solstice): Timocharis, 8th Metonic cycle (–298); Aristyllos, 10th Metonic cycle (–260); Hipparchos, 17th Metonic cycle (Egyptian calendar Thoth 1 = –127/9/24: *DIO* 1.1 §6 eq.28 & §D8); Anonymous, 32nd Metonic cycle (+158). (If the *Almagest* 7.3 nonsuspect 12 stars’ epoch E is assumed to be 137 AD — the same epoch which the 1025-star Catalog was ineptly faked to agree with — then one is tempted to drop conspicuously-discrepant α Ori from the sample. However, our analyses treat E as an unknown — one of two. If we omit α Ori, leaving an 11-star sample, then least-squares analysis produces: $E = 152 \pm 8^y$ & $\Delta\phi = 3' \pm 2'$. But including α Ori only trivially increases our calculated standard deviations, while gratifyingly producing a 12-star median error which is fully 1’ lower than the 11-star median error. The E & $\Delta\phi$ based on the unfiltered 12-star sample were adopted for Table 3. Thus, as in fn 5, we base our work on Ptolemy’s own sample-splitting.) If the last date is correct, then the *Almagest* was completed c.160 AD (ordmag a decade later than now generally believed): during the reign of Marcus Aurelius (1st regnal year’s Thoth 1 = 160/7/14), which is in fact the epoch assigned to Ptolemy in the *Suda* vol.1 part 4 (1935) p.254 entry 3033. (I was initially inclined to a contingent crude redating of the *GD* to c.170 AD because, at the time of the *Almagest*’s final rendition, the *GD* — largely a cumbersome collection of thousands of places’ longitudes & latitudes — was evidently just at the pre-planning stage: see the observant remarks of Toomer 1984 p.130 n.109. However, the *GD*’s sloppy dependence upon prior authors — especially Marinus of Tyre — hints at oft-indiscriminate high-speed borrowing: see Rawlins 1985G §10. So the *GD*’s compilation could have taken alot less than 10^y. Thus, I’ll let stand my earlier rough estimated *GD* date: c.160 AD.) Note that the very idea of stellar-epoch would be meaningless for Timocharis & Aristyllos if stellar precession was then-unknown, as most scholars now accept. (Rawlins 1999 produces evidence that precession was known to Timocharis’ & Aristyllos’ contemporary, Aristarchos of Samos.)

⁴⁶ That Hipparchos’ rôle in ancient astronomy has been overestimated is something that Muffiosi & DR can agree upon in general, whatever the disagreement on particulars. For Hipparchos’ debts, see *DIO* 1.1 §6 (Rawlins 1991H) §E5, *DIO* 1.3 §§N8, O1, R14, S1, fnn 224, 226, 235, 253, 288. For his credits, see both articles, *passim*, especially *DIO* 1.3 §S2.

⁴⁷ In the southern part of the Catalog, there is a correlation between [a] inaccuracy of data, and [b] frequency of whole-degree Z . No surprise (§E4).

(15'), 3 (20'), 4 (24'), 6 (30'), 3 (36'), 1 (40'), 5 (45'), 2 (48').

F5 Second, we may use the fact that 3 other ancient astronomers — all of whom probably observed in Alexandria ($\phi = 31^\circ 12'N$) — also left us declination data. In these cases, we know at the outset (within a few arcmin) the degree-fraction for ϕ . So, we may use this knowledge, as a check, to see if the same test (which we just used here for Hipparchos) is consistent with reasonable ϕ for the other 3 ancient observers. Keep in mind that the only possible accurate Alexandria ϕ which follow ancient rounding convention are: $31^\circ 10'N$, $31^\circ 12'N$, and $31^\circ 15'N$. Each ϕ entails giveaway nulls: $12' & 15'$ ($31^\circ 10'N$), $10' & 15'$ ($31^\circ 12'N$), and $12' & 20'$ ($31^\circ 15'N$).

F6 The most obvious case is Timocharis (c.300 BC: Rawlins 1982G p.263), who is directly attested (*Almajest* 7.3) as having observed in Alexandria. He left us 12 declinations, which come to us via Ptolemy (*idem*) through Hipparchos. If he used the correct value, $\phi = 31^\circ 12'N$, then we would expect null δ cells for $10'$, $15'$, & $45'$. And we indeed find null cells at $10'$ & $15'$, though a single star³⁹ possesses the disallowed $45'$ fractional ending.

F7 From Aristyllos, a 260 BC⁴⁰ follower of Timocharis, we have only 6 data (*Almajest* 7.3). It is generally presumed that he too observed in Alexandria. His results are consistent with his having used $\phi = 31^\circ 15'$ — which would require null δ cells at $12'$, $20'$, & $48'$. All 3 of these cells are in fact empty. Indeed, all the δ data he left us are rounded to the nearest 1/4 degree.⁴¹

F8 Finally, we turn to the Anonymous from whom Ptolemy lifted the declinations he presents as his own in *Almajest* 7.3. As in the case of Aristyllos, it seems likely that he adopted $\phi = 31^\circ 15'N$ — since this implies the same null δ cells ($12'$, $20'$, & $48'$), and they are indeed again found to be null.

F9 Some comments. Firstly, in each of the foregoing cases, were the δ observations exactly accurate & unrounded (including the effect of refraction, which the ancients didn't correct for), some of the δ cells here required (by rounding & choice of ϕ) to be null would instead be filled. (Three stars each for Hipparchos⁴² and Timocharis.) This is further evidence for the effect of rounding, which is the basis of the foregoing conclusions from nulls. Secondly, there has long been a controversy regarding the reality of the largely inaccurate six declinations which Ptolemy uses (at *Almajest* 7.3) to prove precession. (Were they faked? — or just conveniently selected?) The fact that not one of these suspicious stars breaks the null-cell requirement of §F8 suggests that perhaps they are real. (Rawlins in-prep D tentatively took this position about a decade ago. DR remains agnostic on the point, but wishes to note that this latest evidence is somewhat⁴³ in favor of Ptolemy.) Thirdly, least-squares analyses (Rawlins in-prep D) of the three Alexandrian observers' data (including refraction) have produced estimates of each observer's error $\Delta\phi$ in his adopted latitude (ϕ). All these $\Delta\phi$ are quite small⁴⁴ and are roughly in agreement with the foregoing. Timocharis (11 stars, dropping spurious Arcturus): $\Delta\phi = -2' \pm 3'$. Aristyllos

³⁹ For $\phi = 31^\circ 12'N$, a null δ cell is required at $45'$, but this is filled by Aldebaran, whose $\delta = 8^\circ 3/4$. However, given [a] the 3rd hand nature of the Timocharis δ data, & [b] the oversized Aldebaran δ residual ($-12'$), one may hypothesize that the Aldebaran δ is affected by an ancient scribal error. (Possibly, a highly accurate North Polar Distance $\theta = 81^\circ 1/15$ was later inadvertently miswritten as $\theta = 81^\circ 15'$. Similar errors: *DIO* 1.3 §O3, [*GD* 8.3.4 XZ mss.]) Note: no other Alexandrian ϕ (§F5) produces a distribution that fits better than that for $\phi = 31^\circ 12'N$.

⁴⁰ Rawlins 1982G p.263 (fn 17). (Based upon Rawlins in-prep D; see here at §F9.) So Aristyllos' correct declination-deduced date was prominently published by DR some years before Y.Maeyama's 1984 paper (*Centaurus* 27:280), which is unfailingly cited by Hist.sci in this connection, despite the paper's exceedingly odd statistical treatment of data. (See *J.Hysterical Astron* 1.2 fn 126.)

⁴¹ This point has long since led DR to reject (see *J.HA* 1.2 fn 53) the common assertion that degrees did not exist in 3rd century BC Greek science. The current paper's findings (nulls in the fractional distributions of Timocharis' & Aristyllos' declinations) add yet more support for this conclusion.

⁴² See fn 33.

⁴³ The probability is about 35% that all 6 stars would accidentally miss the 3 null-expectation cells. Not statistically significant, but: at least it's better than 50%.

⁴⁴ By contrast, inaccurate ϕ values for Alexandria are: Eratosthenes' $31^\circ 04'N$ (Rawlins 1982G eq.10) and Hipparchos' $31^\circ 05'N$. Strabo 2.5.39 (Hipparchan data): 25400 stades (fn 34) minus 3640 st = 21760 st = $31^\circ 05'N$.

the science." Shades of Newton's conspiracy theories!!

C2 Probably the most enthusiastic jumper on the Babylonian bandwagon is G.J.Toomer who, in a chapter entitled 'Hipparchus and Babylonian Astronomy' in a volume commemorating A.Sachs,²⁸ shows himself to be so obsessed with the idea that Hipparchus used various Babylonian values for period relationships of the moon and planets that not only does he not believe Ptolemy's clear account in *Alm.* iv,2,²⁹ but he actually invents (p.359) an entirely imaginary visit of Hipparchus to Babylon, which by the end of the chapter he has deluded himself into stating as fact!! No doubt 'evidence' for this mythical trip will be adduced in future publications (cf. note 32 below). Both in this chapter and in an earlier paper³⁰ Toomer makes great play with F.X.Kugler's work, *Die Babylonische Mondrechnung* (Freiburg im Breisgau, 1900).³¹ In this book Kugler examines a cuneiform tablet (no.272 in his classification, no.122 in Neugebauer's *Astronomical Cuneiform Tablets* [ACT], 3 vols., 1955), which he describes (pp.9-10) as a "Rechnungstabelle des Kidinnu" and which lists new moons for the years 208 to 210 of the Seleucid Era = -103 to -100 or 104 to 101 B.C. (the epoch of the Seleucid Era being 311 B.C.);³² the tablet comprises 18 columns of numbers, and by analysing 12 of these (the remaining 6 "were explained by Schaumberger in 1935" according to Neugebauer)³³ Kugler was able to show that they were based on System B so-called of Babylonian lunar theory.³⁴ Not only this, but they imply a series of astronomical parameters for mean lunar motions which are fundamental in Babylonian astronomy and which turn out to be exactly the same as those known to Hipparchus. Seizing on these coincidences (which are not all that surprising, given that both Babylonian and Greek astronomers, although using entirely different methods, were examining the same phenomena — 'the moon belongs to everyone', as a once popular song put it), Kugler leapt to the totally unwarranted conclusion that Hipparchus had simply taken his values for the various period relationships from Babylonian sources to which Kugler assigns the priority of discovery in each case. Thus he refuses to accept Ptolemy's account in *Alm.* iv,2 where it is clearly explained how Hipparchus obtained his results by comparing his own observations of eclipses with earlier ones.

C3 Now Toomer talks of "The enormous significance of this discovery", "the misleading impression which one derives from the *Almagest*",³⁵ "this truly astonishing revelation" and "how neglected it has been among classical scholars",³⁶ all in connexion with Kugler's work. Perhaps it is worth spelling out a few of the reasons why some of us have not been able to accord Kugler's conjectures the same uncritical acclaim that Toomer displays. To start with there is the date of the tablet; despite Toomer's clumsy attempt to backdate it (see

²⁸ See note 8 above.

²⁹ And it is a clear account despite the "inconsistencies" that A.Aaboe (in *Centaurus* 4, 1955, pp.122-125 — followed by Toomer, p.99 of note 30 below) claims to find in it. Of these "inconsistencies" the first is trivial (a discrepancy of 11 in the 4th sexagesimal place for the value of the mean synodic month, which amounts to less than 1/12th of a second!), and the second can readily be explained as the result of a Ptolemaic correction for the effect of precession (supposed to be 1° in 100 years) for the period between the early Babylonian eclipses used by Hipparchus to compare with his own observations and the dates of the latter. In fact, Aaboe himself suggests this perfectly feasible explanation, but perversely takes it as additional proof that Hipparchus, as well as making use of Babylonian observations for purposes of comparison (which nobody is going to deny — see below: §C2), knew and used the Babylonian figure for the sidereal year, which Aaboe describes (p.123) as "perhaps the most fundamental parameter in Babylonian astronomy", but which "is nowhere attested in the *Almagest*" [my emphasis].

³⁰ 'Hipparchus' Empirical Basis for his Lunar Mean Motions', *Centaurus* 24 (1980), pp.97-109.

³¹ The conjectures in Kugler's book were taken up equally uncritically in two publications by F.Cumont, in *Neue Jahrbücher für das Klassische Altertum* (Leipzig), 14, 1911, pp.1-10, and in *Florilegium ou Recueil de travaux d'érudition dédiés à M. le Marquis Melchior de Vogüé* (Paris) 1909, pp.160-165.

³² Incredibly, Toomer (in the Sachs volume p.354 — see above, note 8) dates this text as "computed for the years 185 to 188 of the Seleucid Era in Babylon", i.e. 126 to 123 B.C. — this presumably in order to bring it within the possible span of Hipparchus' lifetime (see my *GFH*, p.2ff.) and so buttress his own arguments.

³³ ACT, vol.1, p.145.

³⁴ Treated by Neugebauer in *HAMA*, II B, p.474ff., and elsewhere in this eccentrically organised work.

³⁵ Cf. note 30 above, pp.99 & 100.

³⁶ Cf. note 8 above p.354.

note 32 above), tablet 272 (Kugler) or 122 (*ACT*) is dated by both Kugler and Neugebauer to 104 B.C. at the earliest, i.e., some 20 years later than Hipparchus' probable life-span (see my *GFH* pp.2-3). So, as far as the date is concerned, the Babylonian scribe is more likely to have been copying Hipparchus than the other way round.³⁷ It is all very well for Toomer to say (p.98 in note 30 above) "Although he could not show that the tablets from which the above relationships were extracted predate Hipparchus, Kugler rightly concluded that the priority belonged to the Babylonians (as has been amply confirmed by subsequent investigations of the cuneiform material)", but not a single reference is given to substantiate this claim, and even in his later chapter in the Sachs volume (see note 8 above) all Toomer can do is to reiterate his misguided belief and weave a web of implausible inferences, still without adducing any hard evidence.

C4 Then there is the matter of the actual format of this type of Babylonian tablet; it consists entirely of lists of numbers, and detailed knowledge of the working of System B is necessary before these numbers can be interpreted in any meaningful way. Kugler himself, in a work³⁸ published 10 years after his *Die Babylonische Mondrechnung* in which he repeats the claims made in the earlier book about Greek borrowing from the Babylonians and the latter's priority, nevertheless introduces one welcome note of caution: on p.121 he says, "Zunächst hat man sich vor einer irrigen Vorstellung zu hüten. Die obigen Werte sind in den babylonischen Tafeln nicht etwa als einfache Beobachtungsergebnisse aufgeführt; sie sind vielmehr mit einer Reihe von andern Gröszen zu einem höchst sinnreichen Rechenmechanismus verbunden. So greifen in der Tafel SH.272 die numerischen Elemente von nicht weniger als 18 Kolumnen wie Räder einer Maschine ineinander." ["First of all one has to be on one's guard against an erroneous representation. The above values are not presented as simple observational results; rather they are connected with a series of other magnitudes to a highly ingenious calculating machine. Thus in tablet SH.272 the numerical elements are distributed into no fewer than 18 columns like wheels of a machine."] Quite obviously, without a comprehensive knowledge of how the 'Machine' works no one could derive the period relationships simply from the numbers in the columns. Yet even Kugler agrees that Hipparchus and Ptolemy did not know the **details** of the Babylonian System B³⁹ — but in that case, how could they possibly derive the exact periods which they are supposed to have copied? Kugler's very shaky reasoning in these pages not only involves denying Ptolemy's explicit testimony, but is logically inconsistent in itself. Equally suspect is Kugler's claim (*op.cit.* pp.85-86) that the Babylonians were the first to discover the inequality of the astronomical seasons, a claim he insists on despite saying (p.86) "Es ist angesichts dieser Verhältnisse allerdings recht merkwürdig, dass **keine dieser babylonischen Jahrespunktbestimmungen auf uns gekommen ist**" ["It is in the face of these circumstances surely quite remarkable that **none of these Babylonian determinations of the seasonal points has come down to us**" — my emphasis], and actually agreeing that Hipparchus and Ptolemy relied on the Greek observations of Euctemon and Meton despite their inaccuracy, and not on Babylonian ones. These and other infelicities⁴⁰ afford good reason to doubt the wisdom of Toomer's apostolic fervour in promoting Kugler's conjectures.

³⁷ See Rawlins, *Vistas in Astronomy* 28 (1985) p.256, for other instances of transmission from Greek sources to Babylonian. [See also *DIO* 1.1 †6 §A7 & §B10-§B11, *J.Hysterical Astron.* 1.2 §E1-§G4 & fn 73, *DIO* 1.3 fn 266, and Hugh Thurston *Early Astronomy* (Springer, NYC, 1994) pp.123 & 128.]

³⁸ *Im Bannkreis Abels*, 1910.

³⁹ *Die Babylonische Mondrechnung*, pp.52-53. There is, in fact, no good evidence whatsoever that Hipparchus and Ptolemy knew any more about Babylonian astronomy than the occasional borrowing of some eclipse and planetary observations; certainly there is no hint in the *Almagest* or elsewhere that the Greeks knew anything about the structure of Babylonian lunar theory.

⁴⁰ E.g., his suggestion (p.51) that Hipparchus was not astronomically active before the mid 2nd century B.C. (on this, see my *GFH*, p.2) or that (pp.103-104) the Babylonians were aware of the phenomenon of precession (categorically denied by Neugebauer in *HAMA*, pp.369 & 543 note 13, who, incidentally, throws doubt on another of Kugler's cherished beliefs, namely that the author of System B was Kidinnu — in *HAMA*, p.611 we read "It is not at all evident that the colophons in question mean that Kidinnu is the architect of System B").

& at 15', just as we find³³ in §F2. However, a 10' ending for ϕ would produce a null δ cell at 48', which is in fact filled (see §F2). And 09' is too unrounded for Hipparchos. So, since Hipparchos is known to have used eighths of degrees (07' 1/2, evidently interchangeably with 08': see *DIO* 1.3 fn 251), the natural conclusion is that his main observatory's adopted ϕ ended in 08'. On the island of Rhodos (where Hipparchos observed: *Almagest* 5.3&5, 6.5), this has to be 36°08'N. Going clockwise around Rhodos Island, its 4 major cities were:³⁴ Kamiros ($\phi = 36^\circ 20'N$), Ielysos ($\phi = 36^\circ 24'N$), Rhodos city ($\phi = 36^\circ 26'N$), & Lindos ($\phi = 36^\circ 05'N$). Thus, our result ($\phi = 36^\circ 08'N$) unambiguously identifies Hipparchos' city as Lindos. According to the Army Map Service³⁵ there is a hill 371 m high, just (3 1/2 nmi) NW of Lindos, at $\phi = 36^\circ 08'N$, near the ancient town of Kalathos (modern Calato).

F4 There are several simple ways to confirm our finding for Lindos. First, we turn to Hipparchos *Comm* (his sole surviving work), where — though many stars' positions are given crudely (to whole degrees) — 10 declinations³⁶ show fractions (R.Newton 1974 p.339). Again, we find null³⁷ δ cells for 12' and for 15'. Adding (to our previous sample) these 10 stars from Hipparchos *Comm*, we have (dropping³⁸ the 00', 10', & 50' cells, as in fn 37) a Hipparchan set of 24 stars, the actual fractions-distribution of which is: 0 (12'), 0

into the 20' cell (for δ), we must add the associated probabilities (§F2): 1/24 + 1/10 = 17/120. The full δ expected distribution is generated in the same fashion (displaying nulls at 12' & 15'): 17/120 (00'), 1/6 (10'), 0 (12'), 0 (15'), 17/120 (20'), 1/15 (24'), 19/120 (30'), 1/20 (36'), 1/20 (40'), 1/12 (45'), 3/40 (48'), 1/15 (50').

³³ It is worth noting that, had these 20 data been observed exactly correctly (& never rounded), 15% of them would have ended up in the 15' cell. See §F9.

³⁴ *GD* 5.3.34 (Nobbe 1843-5 2:16, Müller 1883&1901 p.837, or E.Stevenson 1932 ed. p.114) gives for Rhodos Island (latitude & longitude E. of the [Cape Verde] Islands): Panos Akra (35° 11/12 & 58°; Nobbe 2:16 has 58° 1/3), Kamiros (35° 1/4 & 58° 1/3), Lindos (36° & 58° 2/3), Ielysos (36° & 58° 1/3). As revealed in Rawlins 1985G (pp.261f), ancients commonly derived their geographical manuals' ϕ lists from klimata data for longest day *M*. Presuming 35° 15'N (grossly erroneous) is a scribal error for 36° 15'N, we find that all four *GD* Rhodos ϕ are mere calculations from $M = 14h1/2$ ($M/2 = 108^\circ 3/4$), using $\tan\phi = -\cos(M/2)/\tan\epsilon$, where $\epsilon = 23^\circ 51'$ (Eratosthenes), $\epsilon = 23^\circ 11/12$ (early Hipparchos), and $\epsilon = 23^\circ 2/3$ (late Hipparchos). The 3 calculations produce, respectively (rounding to the nearest 1/12th of a degree, as the *GD* always does): $\phi = 36^\circ 00'$, $\phi = 35^\circ 55'$, & $\phi = 36^\circ 15'$. (Note that Strabo attests $\phi = 25400$ stades = 36° 1/4 for the Hipparchos Rhodos klima. See Diller 1934 and Rawlins 1985G.)

³⁵ A.M.S. M506 Balkans 1:250,000, "Scarpanto-Rhodos" Sheet G18, (1948). (Originally compiled in Britain: Royal Engineers, 1944.) The Army Map Service is now the US "Defense Mapping Agency".

³⁶ There are 12 fractional declinations in Hipparchos *Comm*, but two of these stars also appear in *Almagest* 7.3.

³⁷ Except for 36°08'N and the 35°58'-36°01'N interval, all other possible Rhodos ϕ entail nulls in δ -fraction cells which are in fact filled. However, $\phi = 36^\circ 08'N$ is nearer a major city than 36°00'N, and Hipparchos was a man of the world (§G3). (South of Lindos, the east coast of Rhodos swings sharply to the west; so, any site near 36°00'N would be about 10 nmi from the nearest city, Lindos. By contrast, at 36°08'N, Hipparchos would be in the city's north suburbs.) Making a meaningful statistical choice between 36°00'N and 36°08'N is best accomplished by the following logic: before applying the null-cell test, it was already known that 36°00'N would not be testable (since no null cells at all can result from a 00' shift of *Z*'s original fraction-ending via eq. 1). However, Hipparchos' finest known precision for geographical latitudes is 1°/12, same as Ptolemy's standard *GD* precision. (E.g., $\phi = 23^\circ 55'$ for Elephantine Island at *GD* 4.5.70 is probably from Hipparchos, since it equals his first adopted obliquity: fn 18.) Thus, 36°00'N would have been Hipparchos' formal ϕ — if he believed that his latitude was in the range 36°00'N \pm 2' (5 possible whole-arcmin endings out of 60). However, the *a priori* odds are but 5/60 = 1/12 that such an ending is true; in the other 11/12 of a large sample of such cases, the null-cells-test filter will reveal the genuine value(s) that ϕ may take. And there is further evidence against $\phi = 36^\circ 00'N$, namely, the comparison of expected distribution (for 36°00'N) vs. observed distribution (§F4 & fn 38), for the cells from 12' through 48'. The 00', 10', & 50' cell-counts were of course ruined by rounding in the *Comm*. Thus, we subtract their sum probabilities from unity and divide into 10 to find the true probable total of *Comm* stars to add onto the previous sample of 19. This patchwork total is 34.79 for $\phi = 36^\circ 00'N$. For $\phi = 36^\circ 08'N$, it's 35.00. For $\phi = 35^\circ 58'N$, it's 33.47. So the expected cell-counts are, for $\phi = 36^\circ 08'N$: 0 (12'), 0 (15'), 4.96 (20'), 2.33 (24'), 5.54 (30'), 1.75 (36'), 1.75 (40'), 2.92 (45'), 2.63 (48'). For $\phi = 36^\circ 00'N$: 1.42 (12'), 2.27 (15'), 2.55 (20'), 2.83 (24'), 3.40 (30'), 2.83 (36'), 2.55 (40'), 2.27 (45'), 1.42 (48'). For $\phi = 35^\circ 58'N$: 2.20 (12'), 0 (15'), 3.85 (20'), 1.38 (24'), 3.30 (30'), 3.99 (36'), 1.24 (40'), 3.58 (45'), 3.30 (48'). Vs. the distribution of §F4, we have χ -square: 4.9 ($\phi = 36^\circ 08'N$), 10.7 ($\phi = 36^\circ 00'N$), 10.9 ($\phi = 35^\circ 58'N$). The associated probabilities *P* are, respectively: $P = 0.77$ ($\phi = 36^\circ 08'N$), $P = 0.22$ ($\phi = 36^\circ 00'N$), $P = 0.21$ ($\phi = 35^\circ 58'N$). The last solution ($\phi = 35^\circ 58'N$) survives the null-filter test — but it's more precise than any of hundreds of surviving Hipparchan expressions for angles. Regardless, let us note that all 3 solutions resulting from our analysis are in the range 35°58'N-36°08'N, and are thus: [i] in the southern part of Rhodos Island, & [ii] have Lindos as their nearest city.

³⁸ Of the original 19 Hipparchos declinations, 5 were in the now-dropped cells; thus, adding ten *Comm* stars to the remaining 14 stars yields 24 stars in all.

options),²⁴ and by 2 (number of rough north bounds for transit data),²⁵ still: the odds are hundreds-to-1 against the findings here being due to chance.

F Locating Hipparchos' Main Observatory: Lindos

F1 Having used the foregoing logic to locate the latitude ϕ of the observer of the southern part of the Catalog, DR next decided to see if one could draw revealing information from Hipparchos' surviving explicit declination data. These δ (the error of whose mean is virtually null: fn 51 & Rawlins 1982G n.17) are of far higher quality than the star Catalog's λ & β . Thus, they presumably represent work done at his main observatory, not a perhaps-temporary southern outpost.

F2 There are extant 20 high-quality Hipparchan values of δ : 18 stars in *Almagest* 7.3; also Polaris in *GD* 1.7.4 and Schedar in Strabo.²⁶ Dropping the spurious²⁷ value for Arcturus, we have 19 data. There are 12 permissible degree-fraction cells, since degree-fifths are used for ancient declinations. The cell-counts²⁸ are: 3 (00'), 2 (10'), 0 (12'), 0 (15'), 1 (20'), 3 (24') 1 (30'), 3 (36'), 1 (40'), 3 (45'), 2 (48'), 0 (50'). This may be compared to the expected frequency²⁹ distribution for a random set of data: 1/6 (00'), 1/10 (10'), 1/24 (12'), 1/15 (15'), 3/40 (20'), 1/12 (24'), 1/10 (30'), 1/12 (36'), 3/40 (40'), 1/15 (45'), 1/24 (48'), 1/10 (50').

F3 On 1993/4/8, I realized that the information for locating Hipparchos has lain before our eyes for two millennia — right there in the δ -fraction distribution of §F2. The key clue is the pair³⁰ of nulls at $\delta = 12'$ and $15'$. These would be expected if, when Z observations were converted to δ data (by eq. 1), the automatic data-contraction,³¹ occurring around Z whole-degree readings, was carried into the δ fractions. With this thought in mind, it was easy to see that if ϕ ended in $08'$ - $10'$, then null δ -fraction cells would have to occur³² at $12'$

²⁴ The most popular fractions are the 8 cited at §B3. Using fifths as well can bring the total to 12 (§F2). If we add in eighths (§F3) and tenths & twelfths (*DIO* 1.1 ‡6 §D9), this brings the top possible total of allowable Hipparchan degree-fractions to 24.

²⁵ The rough northern boundary for the southern transit data is, *a priori*, not likely to be outside the region where $45^\circ < Z < 60^\circ$. So, given that boundaries extending over 8° (§E7) all produce $\nu > 4$, one must note that the number of 8° regions covering this 15° range in Z is merely about 2. Dividing high odds by this amount does not seriously degrade the large unlikelihood that our results here are the product of chance.

²⁶ From Strabo 2.5.41, we have α Cas's North Polar Distance $\theta = 31700$ stades or $45^\circ 1/4$, so declination $\delta = 44^\circ 3/4$.

²⁷ Hipparchos' Arcturus $\delta = 31^\circ$ (*Almagest* 7.3 & Hipparchos *Comm* 1.8.16), though the star's real $\delta = 31^\circ 17'$ at Hipparchos' epoch, $-127/9/24$ (Rawlins 1991H eq.28 & §§F4-F5). The $-17'$ error appears to be based on Hipparchos' false Alexandria latitude ϕ plus his sign confusion for the star's Alexandria zenith distance Z . Strabo 2.5.38 (part of his summary of Hipparchos' geography) states that Arcturus transitted slightly *south* of the zenith (positive Z : see §E3), though the truth is that it transitted about $5'$ north ($Z = -5'$) of Alexandria's zenith at Hipparchos' -127 epoch. Since Hipparchos took Alexandria's latitude to be $\phi = 31^\circ 05'N$ (fn 44), this theory perfectly explains his curiously false Arcturus δ : taking Hipparchos' $Z = +5'$, then, by eq. 1, $\delta = 31^\circ 05' - 5' = 31^\circ 00'$. Arcturus is dropped from the sample for all main Hipparchos (& Timocharis) analyses here. (Regardless, inclusion of its whole-degree ending would upset none of this paper's conclusions. See, e.g., fn 49.) I note the provocative coincidence that this worst declination of the *Almagest* 7.3 set happens to have by far the highest declinational proper motion.

²⁸ Throughout this part of the analysis, it is important to note that (for reasons of analytic consistency) all degree-fractions for negative δ are subtracted from $60'$ before being entered into a δ distribution.

²⁹ See the chance distribution (without degree-fifths) given at §B3.

³⁰ Note that the $50'$ null is accidental by the $36^\circ 08'N$ hypothesis. But this is not a problem since: [a] only 1 hit is expected in this cell for a 19 star distribution, & [b] had the actual 19 declinations of the sample been observed exactly correctly (no rounding), none would have ended up in the $50'$ cell.

³¹ I.e., due to ancient rounding convention (§B3), all data between $55' & 05'$ are sucked into one cell: that for $00'$.

³² Take the (Hipparchos) case of ϕ ending in $08'$. By eq. 1, those Z ending in $00'$ would produce δ ending in $08'$, which rounds to $10'$. Those Z ending in $50'$ would produce δ ending in $18'$, which rounds to $20'$. (I.e., $12'$ & $15'$ endings are impossible.) Those Z ending in $48'$ would produce δ ending in $20'$ — adding more to the $20'$ cell. Those Z ending in $45'$ would produce δ ending in $23'$, which rounds to $24'$. Continuing to proceed similarly, one may produce an expected δ distribution exhibiting both nulls as well as shifted and/or merged probabilities from the Z distribution. E.g., since the $48'$ & $50'$ cells for Z were (via eq. 1 & the $08'$ ending of ϕ , as just noted) merged

D Fits of Pan-Babylonianism

D1 The title of this paper refers to a phenomenon in German scholarship of the early years of this century, well described by Neugebauer:⁴¹ “The main thesis of this school was built on wild theories about the great age of Babylonian astronomy, combined with an alleged Babylonian ‘Weltanschauung’ based on a parallelism between ‘macrocosm and microcosm’. There was no phenomenon in classical cosmology, religion, literature which was not traced back to this hypothetical cosmic philosophy of the Babylonians. **A supreme disregard for textual evidence**, wide use of secondary sources and antiquated translations, combined with a preconceived chronology of Babylonian civilization, created a fantastic picture which exercised (and still exercises) a great influence on the literature concerning Babylonia” [my emphasis]. Ironically enough, Neugebauer goes on to praise Kugler's *Im Bannkreis Babels* for demonstrating the absurdities of this ‘Pan-Babylonianism’ (by collecting “17 pages of striking parallels between the history of Louis IX of France and Gilgamesh, showing that Louis IX was actually a Babylonian solar hero”), and suggests that his “example should be studied by every historian because it demonstrates far beyond its original purpose **how easy it is to fit a large body of evidence into whatever theory one has decided upon**” [my emphasis]. It seems to me that, at least partially in the restricted field of the history of ancient astronomy, ‘Pan-Babylonianism’ is in danger of being revived, and that certainly the two tendencies I have emphasized above are still very much in evidence. My paper is an attempt (probably foredoomed to failure, given the entrenched position of the American establishment⁴² in the history of science) to protest against these tendencies and to enter a plea for a more balanced view of the relationship between Greek and Babylonian astronomy. Just because some parameters turn out to be the same in both (or, to put it more accurately, parameters that appear in Greek astronomy can be derived by modern methods from the Babylonian data preserved on selected cuneiform tablets), we are *not* justified in simply assuming that the Greeks copied the Babylonians (or *vice versa*), particularly as the textual evidence of the *Almagest* specifically tells us how the results were actually obtained. Over 20 years ago in my *EGAA* (pp.165-175) I discussed at some length possible borrowings by Greek astronomers from Babylonian sources and “the difficulties inherent in the utilization of Babylonian material” (p.171), and came to the conclusion that analysis of the evidence showed that “astronomical knowledge developed independently in accordance with the different aims of the Babylonian and Greek astronomers” (p.175). Given the totally different structure of the two systems (Greek astronomy, right from Pre-Socratic times, exhibited an essentially geometrical approach to an overall picture of an ordered cosmos, whereas the Babylonian astronomers used strictly arithmetical means to manipulate the sequence of the phenomena they were interested in — mostly horizon phenomena [see *DIO-JHA* 1.2 §E3 & §G3] such as first and last

⁴¹ *The Exact Sciences in Antiquity*, 2nd ed., 1957, p.138.

⁴² Unfortunately, Toomer is a very influential figure in this, and any idea he cares to float is eagerly taken up by other members (either singly or in pairs — see above: §C1) — even his erroneous ones. Thus, in a paper in *Centaurus* 18 (1973) 6-28, he attempts to prove that Hipparchus' chord table was different from Ptolemy's by being based on a circle of radius $3438'$ ($= 57^\circ.3$), as found in Indian sine tables, instead of $3600'$ ($= 60^\circ$); his argumentation is largely circular (assume what you want to prove, and then use it to ‘prove’ your assumption), the figures do not support his thesis without some wildly speculative assumptions [see *DIO* 1.3 eqs.19, 20, 23, 24 & especially §P2], and the comparison with Indian sources is far from convincing. Not only this, but Toomer himself, some ten years later in his translation of the *Almagest* p.215 note 75, actually repudiates his earlier suggestion by saying, “These calculations not only vindicate Hipparchus' computational abilities, but cast doubt on my claim that he was operating with a chord table with base $R = 3438'$ ” [my emphasis]. Yet Neugebauer (in *HAMA* pp.299-300) accepts Toomer's fallacious thesis *in toto*, and such is the prestige of this unwieldy work (see the rave reviews when it first appeared, by Aaboe in *Isis* 69, 1978, by Chandrasekhar and Swerdlow in *Bull. Amer. Math. Soc.* 84, 1978, and by Hartner in *JHA* 9, 1978 — a slightly more judicious appraisal is given by Mercier in *Centaurus* 22, 1978) that it is likely that this error will continue to be repeated by future historians of science. [See *DIO* 1.3 §P4.] In the same way, I fear that Toomer's uncritical acceptance of the false notion of wholesale and unacknowledged Greek copying of period relationships from the Babylonians is destined for similar repetition — as will be, no doubt, his invention of a Babylonian trip by Hipparchus.

visibilities of the moon and planets, but also including eclipses — to enable accurate predictions to be made for astrological and calendaric purposes), and in the light of further research, I see no reason to change this assessment. It does not seem at all strange to me that the two systems should arrive at identical results as regards lunar periods [e.g., *HAMA* p.310 eqs.2-4], independently and perhaps even at the same time (although on the available evidence I should give priority to the Greeks); rather, it would be far stranger if they differed to any significant degree, if one assumes an equal level of competence in the practitioners of both, working on exactly the same phenomena.⁴³ Of course, there was *some* borrowing from Babylonian astronomy. Ptolemy mentions Babylon, the Babylonians or the Chaldeans (synonymous with the Babylonians for Greek writers) about twenty times in the course of the *Almagest*, including the passage in *Alm.* iv,2 where he explains how Hipparchus arrived at his lunar periods by comparing his own observations with Chaldean ones; Ptolemy also uses three lunar eclipses of 721 and 720 B.C. in his calculations concerning the first lunar anomaly (*Alm.* iv,6), others of 491, 502 (*Alm.* iv,9), 383, 382 (*Alm.* iv,11), 621, 523 (*Alm.* v,14), and a few planetary observations (of Mercury in 237 and 245, *Alm.* ix,9, and Saturn in 229, *Alm.* xi,7), all from Babylonian sources.⁴⁴ The paucity of Babylonian planetary observations that might be of use to Hipparchus and Ptolemy is particularly noteworthy; even the three cited above are all from the Seleucid period (and Ptolemy is well aware of their shortcomings — cf. *Alm.* ix,2), and there are none at all for Venus, Mars, and Jupiter. The reason for this is that the planetary phenomena that interested the Babylonian astronomers were mostly horizon phenomena, which, as Neugebauer remarks,⁴⁵ were “least suited for [*sic*] Ptolemy’s needs and furthermore subject to the greatest observational inaccuracy”. The eclipse observations, too, that Hipparchus and Ptolemy borrowed from the Babylonians, obviously had to be selected with great care (*Alm.* iv,9), owing to the difficulties of accurate time measurement in ancient astronomy; Ptolemy himself draws attention to this as it affects Greek astronomy at the beginning of *Alm.* v,14, and a recent paper by Stephenson and Fatoohi⁴⁶ emphasizes the inaccuracy of Babylonian time measurement — according to this (p.266), “the mean discrepancy between measured and computed time-intervals is some 12 deg or almost 50 [time]minutes”, and they go on to remark (p.267), “Typical errors of at least half an hour in measuring intervals of no more than six hours represents a poor performance by any reasonable standards.”

D2 Other Greek borrowings from Babylonian astronomy are of a more general nature: the use of the sexagesimal system itself is a certain example of such borrowing, and Herodotus may be right when he says that the Greeks learned about the gnomon and the division of the day into hours from the Babylonians;⁴⁷ but the idea (espoused by Kugler and many others) that the Greeks derived their stellar nomenclature (especially of the zodiacal

⁴³ I have never understood the aversion of scholars to recognizing coincidences; instead they go to enormous lengths to fashion hypothetical connexions in disparate systems on the flimsiest of ‘evidence’. Thus, because he finds the ratio 3:2 for the longest and shortest daylight playing a rôle in Babylonian, Indian, and Chinese astronomy, Kugler (in *Im Bannkreis Babels* pp.119-120) jumps to the conclusion “wenn wir nicht ein Spiel des blinden Zufalls bzw. menschlicher Willkür annehmen wollen” [“if we do not want to assume an example of blind chance or else human arbitrariness” — my emphasis] that “der Einfluss der babylonischen Astronomie bis nach China hinüber und bis ins Pandschab hinabgedrungen ist” [“the influence of Babylonian astronomy penetrated right to China and into the Punjab”] — which seems to me much more unlikely than simple coincidence.

⁴⁴ Because of Ptolemy’s words in *Alm.* iv,2, and because he expressly states that Hipparchus also made use of the lunar eclipses in 502 (*Alm.* iv,9), 383 and 382 (*Alm.* iv,11), it is generally accepted that Hipparchus too had at his disposal all the Babylonian observations that Ptolemy mentions. [Ptolemy cites no Babylonian observations subsequent to Hipparchus.]

⁴⁵ *HAMA* p.145.

⁴⁶ ‘Lunar Eclipse Times Recorded in Babylonian History’, by F.R. Stephenson and L.J. Fatoohi, *JHA* 24 pt.4 (1993), pp.255-267. [*DIO* 1.3 fn 223 estimates the root-mean-square error of the —382-381 Babylonian eclipse trio (*Alm.* 4.11) as about a half hour. (*Idem*: the rms error of the —200-199 Greek eclipse trio is ordmag 10^m.) All of which creates an obvious problem for 1994 Muffia speculation that 5th century BC Babylonians possessed a highly accurate lunar theory. (See also *DIO* 1.2 fn 81&87.)]

[Advice added 1997: Consult the perceptive conclusion of J.Steele & F.Stephenson at *JHA* 28:119 (1997) p.130.]

⁴⁷ Herod. ii, 109 — see my discussion in *EGAA*, pp.165-166.

was largely observed at the south tip of the island, Cape Prassonesi, which is at latitude $\phi = 35^{\circ}53'N$ (longitude $27^{\circ}46'E$ of Greenwich), a figure evidently set at $35^{\circ}5/6$ by the observer. (Hipparchos and other ancient astronomers normally expressed their parameters in conveniently rounded fractions. See fn 37.) Given the large and varying systematic errors affecting this part of the Star Catalog, it is reasonable to suppose that the chosen transit instrument was portable and was not scrupulously maintained & checked for proper orientation. This sloppiness is probably related (fn 47) to the outsize number of whole-degree Z that the observer recorded.

E5 For random observations of Z , we would expect the fraction-cells to exhibit the same frequency distribution²¹ already set out in §B3: 53 (00'), 40 (10'), 26 (15'), 40 (20'), 53 (30'), 40 (40'), 26 (45'), 40 (50'). Instead, for $\phi = 35^{\circ}50'N$, the Z data, reconstructed by the math of §E1 & §E3, results in: 75 (00'), 39 (10'), 27 (15'), 30 (20'), 57 (30'), 37 (40'), 23 (45'), 29 (50'). For the whole-degree entry, standard-deviation $\sigma = \sqrt{Npq} = 6.64$, since probability $p = 1/6$, $q = 1 - p = 5/6$, & $N = 317$ stars; $n_o = 75$ hits is 24%, same as the entire unprocessed (original Hipparchos) Catalog’s λ whole-degree frequency. This is far above the random-profile expected number, $Np = 53$ (more exactly: $Np = 317/6 = 52\ 5/6$), 17%. Indeed, the normalized deviation $\nu = (n_o - Np)/\sigma = (75 - 317/6)/6.64 = 3\ 1/3$ — which corresponds to odds of more than 1000-to-1 against the whole-degree total $n_o = 75$ having occurred merely due to chance.

E6 DR later discovered (1993/4/11) that there is an even more refined correlation between [a] size (southerliness) of Z , and [b] the percentage of whole-degree-cell Z . Examining the 317 southern stars in cumulative stages (starting at the horizon), one finds that the statistical significance, of the south-sky whole-degree Z excess, peaks at about $\delta = -18^{\circ}$ or $Z = 54^{\circ}$. To be precise: in the southern portion of the Catalog, of the 209 stars which are south of $\delta = -18^{\circ}$, 58 stars’ Z fall²² into the whole-degree cell,²³ where the expected number would be $209/6 = 34\ 5/6$. Since $\sigma = \sqrt{Npq} = 5.39$ then $\nu = (58 - 209/6)/5.39 = 4.30$ — thus, odds of about 60,000-to-1.

E7 Examining the number of whole-degree Z , from the horizon up to anywhere between $Z = 49^{\circ}$ and $Z = 57^{\circ}$: one finds that ν exceeds 4 — which corresponds to odds of over 15000-to-1. When evaluating the significance of such odds, one must of course take into account the range of option-choices (obliquity ϵ , latitude ϕ degree-fractions, transit-data north-bound) that went into the hypothesized scenario. But even if one divides by 2 (ϵ options) and by 10 (ordmag the number of common ancient-rounded ϕ degree-fraction

²¹ Actually, as we will see later here (§F1), Z observations were probably written more finely than celestial latitudes β (since ancients normally used fifths of degrees for equatorial coordinates: *Almagest* 7.3). But this will have no effect at all upon the count in the crucial Z whole-degree-cell.

²² The full 209 star distribution: 58 (00'), 24 (10'), 18 (15'), 15 (20'), 35 (30'), 27 (40'), 17 (45'), 15 (50').

²³ These 58 transformations, from Catalog (λ & β) to α & Z , are the reverse of reality. Checking the real transformations, from observed Z & α to Catalog λ & β , one finds that (primarily because most β cell-ranges are smaller than the whole-degree cell’s) some of the 58 stars do not succeed. However, since the latter part of the slide&hide hypothesis merges 15' longitude fractional-endings with 20's, and 45's with 40's, this makes feasible the transformations for, e.g., PK728 & PK859. Also, the Cataloger tended to avoid 45' fractional endings (a point noted at R.Newton 1977 p.248 & Graßhoff 1990 p.85 with respect to β — and confirmed in the current analysis as regards α); this habit assists a few more whole-degree- Z places to yield the Catalog position — e.g., PK790 & PK842. Permitting α to be expressed to the most basic standard ancient precision (§B3), as well as degree-fifths & (corresponding to half-timemins) degree-eighths, we can specify 54 stars with integral Z (00' ending) which could have transformed to the Catalog’s ecliptical coordinates (*Almagest* 8.1). (If we allow degree-tenths, 57 stars.) One more if PK964 is added (fn 20). Accounting for the smaller cell-ranges noted above, we would expect to lose about 5 stars when reverse transformations are checked (we are implicitly assuming precise, infallible Hipparchan sph trig computers, which is unrealistic); and this is roughly what has been found. So the high odds (against chance producing our results) persist, though this check is actually superfluous, since the analyses of §E5-§E6 have already statistically established the correlation between the Catalog’s southern part and Z whole-degree fractions for $\phi = 35^{\circ}50'N$. The strength of the excess in our Z whole-degree-cell can be underscored by noting that its members comprise 58 out of 209 data = 28% (vs. 17% expected: §E5 or §F2), similar to the (unprocessed) Hipparchos 00' rates for north & zodiac λ ($(95 + 94)/(359 + 344) = 27\%$) or north β ($108/359 = 30\%$).

following test: [a] convert (by sph trig) all southern stars, from their ecliptical Catalog positions, into the hypothesized original equatorial positions (rt asc α & decl δ), and [b] then (choosing an assumed value ϕ for the observer's adopted observatory latitude ϕ) recover the observed zenith distances Z , and [c] finally, in that set of data, look for an empirical excess (§A1) population in the whole-degree Z cell (i.e., the reconstructed Z that fall into the range $55'$ to $05'$).

E2 Since Hipparchos had used 2 different obliquities¹⁸ during his career, both were tried. It would transpire that the earlier value, $\epsilon = 23^\circ 55'$, was the one used by Hipparchos' mathematicians for the sph trig equatorial-to-ecliptical transformations (the reverse of the sph trig transformations of the modern ecliptical-to-equatorial reconstruction performed here at §E1) required to produce the southern part of the Catalog from the original transit-instrument observations underlying the published data.

E3 It is wellknown¹⁹ that the ancients determined celestial objects' declinations δ just as modern astronomers do: observe the upper-transit zenith distance Z (positive to the south), and then subtract it from the adopted latitude ϕ of one's observatory. Simple arithmetic:

$$\begin{aligned} \delta &= \phi - Z & (1) \\ \text{— which may be rewritten:} & & \\ Z &= \phi - \delta & (2) \end{aligned}$$

Thus, once the declinations δ are regenerated (sph trig of §E1) out of the Catalog's ecliptical data, we need only subtract them from an assumed latitude ϕ , to find the fractions-distribution of the raw Z data which are here hypothesized to underlie the southern part of the Catalog.

E4 Almost immediately (1993/3/23), DR found that, if the observer's ϕ ended in $50'$, there is a spectacular excess of stars falling into the Z whole-degree cell.²⁰ Since Hipparchos worked on the island of Rhodos, the natural suggestion is that the southern part of the Catalog

¹⁸ The earlier of Hipparchos' two long-adopted obliquities was $\epsilon = 23^\circ 55'$ (fn 34); later, Hipparchos switched to the much more accurate obliquity, $\epsilon = 23^\circ 40'$. Details in Rawlins 1982C and *DIO* 1.1 ‡6 fn 21, *DIO* 1.2 fn 104, *DIO* 3 fn 70. (Explicit ancient attestation of $23^\circ 55'$ cited at Rawlins 1985G fn 6.)

¹⁹ *Almagest* 1.12, 5.12-13. A byproduct of the current paper is verification that the procedure Ptolemy reports was indeed standard for ancient scientists. This is the basis of a DR argument (fn 6) that Ptolemy's declinations were not his own. (See also §F9.)

²⁰ As an example, we examine the star PK805, θ Eri. (The significance of this star's huge errors in position & magnitude are intelligently analysed by Graßhoff 1990 pp.170-171. PK805's rating as 1st magnitude — mindlessly copied by Claudius Indoor Ptolemy — was obviously due to an early confusion with Achernar.) Observed $Z = 84^\circ$ (so using $\phi = 35^\circ 56'$ in eq. 1 yields $\delta = -48^\circ 10'$), and observed $\alpha = 27^\circ$. Thus, for $\epsilon = 23^\circ 11/12$ (fn 18 & fn 34), sph trig calculation-transformation from equatorial to ecliptical coordinates produces: $\lambda = 357^\circ 33'.9$ & $\beta = -53^\circ 30'.1$ — which round to $\lambda = \text{Psc } 27^\circ 1/2$ & $\beta = -53^\circ 1/2$; Ptolemy added $2^\circ 2/3$ to λ , leaving: $\lambda = \text{Ari } 0^\circ 1/6$. These are precisely the coordinates we find in the Star Catalog (*Almagest* 8.1; Manitius 1912-3 2:57 and Toomer 1984 p.386).

Another example is perhaps afforded by PK964, a star listed in the Catalog as $m = 3$, though no star of such brightness exists near the given place. (PK964 has been noted as anomalous for over 1000 years: PK p.112.) DR proposes that PK964 may actually be a bungled version of δ Cen (real -126.3 coordinates: $\alpha = 157^\circ 06'$, $\delta = -39^\circ 08'$), a star already listed (uncontroversially: PK p.118) as PK960. Assume observed values (rather typically-rounded) for δ Cen: $\alpha = 10\text{h}7/12$, error $\Delta\alpha = +6^{\text{m}}.6$ at Catalog epoch ($-127/9/24$, fn 27); and $Z = 75^\circ$, error $\Delta Z = +3'$ (-126.3 transit at C.Prassonesi, $\phi = 35^\circ 53'$). Thus, $\alpha = 158^\circ 3/4$, while eq. 1 gave $\delta = 39^\circ 1/6$. Transformation (again, $\epsilon = 23^\circ 11/12$) gave: $\lambda = 179^\circ 56'$ & $\beta = -43^\circ 44'$, which rounded to $\lambda = 180^\circ$ & $\beta = -43^\circ 3/4$. (Note that the other Hipparchos Catalog listing for δ Cen, PK960, does give precisely $\lambda = 180^\circ$: PK p.94.) Hipparchos, originally expressing (or computing) the λ in "steps" of 15° each (Neugebauer 1975 pp.302, 669f, 1049), correctly put λ at the start of the 13th step. But, there was later a unit-mis-step during conversion of steps into degrees for the Catalog: $\lambda = 13 \cdot 15^\circ = 195^\circ$. The Hipparchos Catalog position of PK964 was indeed (PK p.94): $\lambda = 195^\circ$ & $\beta = -43^\circ 3/4$. And PK964 is the right magnitude (Catalog $m = 3$) for δ Cen: pre-extinction $m = 2.60$; -126.3 C.Prassonesi culmination null-dust post-extinction $\mu = 3.12$. (One might possibly argue that our $\lambda = 180^\circ$ version of PK964 is μ Vel, the brightest seemingly-omitted star in the Catalog: $m = 2.69$, $\mu = 3.19$. Real -126.3 coordinates: $\alpha = 140^\circ 05'$, $\delta = -39^\circ 05'$. This star's -126.3 C.Prassonesi-transit $Z = 74^\circ 55'$ is in fact close to 75° ; but, since the star's α is nowhere near $158^\circ 3/4$, one must then propose the accidental occurrence of two large Hipparchos-calculation errors, not the δ Cen hypothesis' single slip. [But see retractive conclusion at *DIO* 4.3 ‡14.] Suggestions implicit in the speculation that PK964 = δ Cen: [a] At least some star-observations were doubled. (Similar occasional doublings in Ptolemy's geographical records are noted by Rawlins 1985G §§8&10 & fn 6.) [b] The count at §E5 may be $n_o = 76$.

constellations) from the Babylonians is almost certainly wrong — rather, recognition of the zodiac is yet another example of parallel but independent, and perhaps coincidental, development in the two astronomies.⁴⁸ What this paper is concerned to stress is that these borrowings **do not include** detailed knowledge of the various lunar periods derivable from Babylonian cuneiform tablets, as so many recent commentators assume; before this myth becomes established fact, a *caveat* should be entered about the shaky grounds on which it is being constructed, and the dubious 'reasoning' that seeks to justify it. It took the Great War of 1914-1918 to put an end to the first outbreak of Pan-Babylonianism — let us hope that another war is not required to stop the present irruption!

D3 Finally, I should like to draw attention to some wise words by W.R.Knorr,⁴⁹ which should be taken to heart by all historians of ancient science (p.163): "These three examples from the study of Euclid turn about a common methodological recommendation — that the historian of mathematics should give priority to the critical examination of the texts before undertaking a wider exploration of their philosophical and mathematical ramifications. This may sound too obvious to warrant special comment. But the combination of fragmentary evidence with a subject area readily associable with modern fields of mathematics and philosophy has made the study of ancient mathematics **an arena for ambitious interpretation, where reconstruction overwhelms textual criticism**. The result has been a striking use of intentionalist terminology in **accounts so heavily dependent on the critics' special predispositions (mathematical or philosophical), that the ancient authors could hardly have actually intended what is claimed for them.**" Substitute 'astronomy' for 'mathematics' and 'astronomical' for 'mathematical', and this describes the situation in the field of ancient astronomy with great accuracy — particularly the words I have emphasized.⁵⁰

Publisher's Note:

Except for several bracketted informational references to *DIO* & *J.Hysterical Astron.*, inserted by DR at the author's suggestion and (as also the DR-inserted subtitle, section-titles, author-bio, & institutional identifications of scholars) approved by him at the proof-stage, the foregoing text is effectively identical to that offered by the author (1994/1/5) to the Editor-for-Life of the *Journal for the History of Astronomy*. The EFL, typically, **refused even to referee the paper**. (Further details of this incident will appear in *DIO* 4.2.) I.e., if Muffia control of apt history-of-science journaldum were still as inescapably ubiquitous as formerly, back in the medieval Era BD (Before *DIO*), then Dicks' revealing information & critical insights — and his right to publish them — might have been lost forever.

⁴⁸ Cf. *EGAA*, pp.163-165.

⁴⁹ In a chapter entitled 'What Euclid Meant: On the Use of Evidence in Studying Ancient Mathematics', pp.119-163 of the volume cited in note 5 above.

⁵⁰ I would commend these especially to the attention of Toomer, who, in his translation of the *Almagest* (Duckworth, 1984) on p.176 note 10, with breath-taking arrogance dismisses Ptolemy's account in *Alm.* iv,2 as "not historically accurate", and prefers instead his own wildly speculative views.

Table 1: Columbus Landfall Theory Scorecard

#	Columbus's Description	Pl	Sa	Co	Wa	Ca	Eg	Gr
1	Saw light 1492/10/11, 10 PM (LAT)	3	1	3	1	3	1	1
2	Island is well watered	3	2	2	3	3	1	3
3	Large laguna in the middle	2	1	3	2	1	3	3
4	Went NNE along I . . .	3	0	3	3	2	3	2
5	. . . to other, E part	3	3	3	2	3	3	1
6	Island is surrounded by a reef	3	3	3	1	3	2	3
7	Large harbor between reef & island	2	2	2	3	3	0	3
8	Fortifiable peninsula	0	1	1	1	3	1	2
9	Saw many islands from I . . .	2	2	2	1	2	3	3
10	. . . and headed for largest	3	3	1	3	0	0	3
11	From I to II is 5-to-7 leagues	2	3	2	3	1	2	3
12	N-S coast of II faces I	3	1	0	1	2	1	3
13	N-S coast of II is 5 leagues long	3	3	0	0	0	1	3
14	E-W coast of II is 10+ leagues long	2	2	0	0	2	2	2
15	Can see III from II	1	1	3	3	0	3	0
16	From II to III is 8 or 9 leagues . . .	3	3	1	1	1	3	0
17	. . . almost E-W	3	3	3	3	3	3	1
18	Coast of III runs NNW-SSE	3	3	3	3	2	3	0
19	Harbor 2 leagues from end of III . . .	3	3	0	0	3	3	3
20	. . . with two mouths	1	1	3	3	1	1	1
21	NW of harbor, part of III runs E-W	3	3	0	0	1	3	1
22	Coast of III is 20+ leagues	3	3	3	3	2	3	0
23	Six hours sailing from III to IV	3	3	3	3	0	0	0
24	N end of IV lies E of III	2	2	3	3	1	0	1
25	Coast of IV runs W . . .	0	0	0	0	2	2	1
26	. . . 12 leagues to C.Hermoso	0	0	2	2	3	3	3
27	N end of IV has many pools	2	2	2	2	1	1	1
28	WSW, C.Isleo to C.Verde fix	2	2	2	2	0	0	0
29	C.Verde fix to Ragged is 23 leagues	3	3	3	3	0	0	0
30	W by S, C.Verde fix to Ragged	3	3	3	3	1	0	0
31	From IV to I is 8 leagues	1	1	0	0	3	0	0
Total Score:		70	63	59	58	52	51	47
Average Score:		2.3	2.0	1.9	1.9	1.7	1.6	1.5

In this Scorecard, abbreviations are adopted for obvious space reasons. The scoring system (from 0 to 3): 0 = does not fit the log (Columbus's *Diario*), requires assumption of transcription error; 1 = poor fit to the log, requires unusual interpretation; 2 = reasonable fit with the log; 3 = perfect fit with the log. (Further details below at §D2.) Islands (in order of discovery): I = San Salvador, II = Santa Maria, III = Fernandina, IV = Isabela. (This paper's ultimate identifications: Plana = I = San Salvador, Acklins-Crooked = II = Santa Maria, Long = III = Fernandina, Fortune = IV = Isabela.) The seven vying theories (regarding the identity of I = San Salvador), which are being scored in the above Table 1, are: Pl = Plana, Sa = Samana, Co = Conception, Wa = Watlings, Ca = Caicos, Eg = Egg, Gr = Grand Turk.

Note: Figures 1-8, referred to throughout the text, will be found below at pp.25-28. These eight illustrations are based upon modern maps and-or *Diario* descriptions of the areas in question.

$\chi^2 = 13$. Either way, the discrepancy is statistically significant for $df = 5$.

C5 The upshot: using the RN slide&hide hypothesis, we find for the northern and southern stars, compatibility between the longitudes and latitudes. The exception is the zodiac. However, it was proposed years ago by DR on completely independent grounds that the zodiac longitudes and latitudes are not from the same set of observations. (See Rawlins 1982C pp.369-371.)

C6 Follow-up: χ^2 tests¹⁵ show that all three longitude profiles (north, zodiac, south) are statistically incompatible with each other, and all three latitude profiles are likewise, except that the discrepancy is too weak to be statistically significant for zodiac vs. south ($\chi^2 = 11$ for 7 df, so $P = 1/6$). However, we already found (§B5) that the latter is consistent with randomness while the former is not. The reasonable conclusion is, then, that all 3 portions of the Catalog were observed under different conditions; thus, the north, zodiac, and south portions of the Catalog represent independent samples, perhaps taken by 3 members of the same Rhodos team of astronomers.

D Randomness: Hipparchos' Possession of Sph Trig

D1 The explanation for randomness' domination (§B5) of the southern portion of the Catalog has been around for years. It is simply: these stars were mostly observed with a transit instrument (the sort described at *Almajest* 1.12), not the armillary astrolabe (*Almajest* 5.1) that was used for the majority of the Catalog's stars. Rawlins 1991H fn 25 already suggested¹⁶ this theory for a few patches of southern stars.

D2 The reason this will produce random Catalog fractions is that, when sph trig is used to transform equatorial coordinates to ecliptical coordinates, the resulting fractions will not be subject to eyeballing's natural tendency (§A1) to produce whole-degree measurements. (This proclivity is the basis of the RN fractions-frequency profile for the non-south sections of the Catalog.) Thus, randomness in the southern part of the Catalog's ecliptical coordinate-fractions provides hitherto-unperceived evidence for the existence¹⁷ of sph trig in the 2nd century BC.

D3 This is also evidence against the long-attractive theory (e.g., Graßhoff 1990 pp.182, 190-191) that Hipparchos might have used a globe as an analog-calculator (equatorial-to-ecliptical transformation in the present case) — perhaps in preference to computing his Catalog's ecliptical coordinates via sph trig. Use of a globe would entail eyeballing the ecliptical coordinates that went directly into the Catalog; and this would produce a notable excess of whole-degree ecliptical data, an excess which (as we saw above at §B5, §C2, §C3) is not found in the southern part of the Catalog.

E Hipparchos' Southern-Outpost Observatory Located

E1 The theory that the southern stars were originally observed & recorded in equatorial coordinates could be expected to have certain consequences. So, DR decided upon the

¹⁵ The longitude χ^2 (df = 5) are: 11.5 (north-zodiac), 17 (north-south), & 15 (zodiac-south). The latitude χ^2 (df = 7): 17 (north-zodiac), 27 (north-south), & 10.8 (zodiac-south). The last is not significant ($P > 0.15$).

¹⁶ DR first proposed this theory in private correspondence not later than 1987/12/20. The spark that launched his suspicion: there are huge group-errors in some southern constellations which appear to be impossible without presuming equatorial errors. Does this paper's finding (southern portion of Ancient Star Catalog originally taken in equatorial coordinates) partly vindicate the Muffia opinions cited at *DIO* 1.2 §11? DR's view: it is by now obvious (especially given the non-Ptolemaic $\epsilon = 23^\circ 11/12$ of §§E2-E7) that virtually every star in the Catalog came from Hipparchos (*Almajest* 7.1; rare exceptions: *DIO* 2.3 ‡8 fn 20) to Ptolemy, already rendered in ecliptical coordinates, no matter whether originally observed via armillary astrolabe or transit circle. (See also the *Almajest* 7.3 passage noted at *DIO* 1.2 §11.) Thus, resemblance of Muffia opinion and the present paper's results is but partial.

¹⁷ Therefore, this paper will hopefully dampen the absurd longtime Muffia passion for denying that sph trig existed in Hipparchos' time. (See Rawlins 1984A p.982 and *DIO* 2.1 ‡3 §A2 & fn 3.) DR first realized the randomness of the fractional endings in the southern section of the Catalog (& explored the randomness-equatorial link) on 1993/3/12.

necessitated Ptolemy's **knowing destruction of data**. For that substantial fraction of the Catalog whose longitudes were expressed with 15' or 45' endings, he fudged them (by 5') & thus hid-merged them with other data — with the result that when we now try to reconstruct Hipparchos' longitudes, we cannot know for sure which stars (of those with 0' or 20' endings in Ptolemy's rendition) have been altered by 5'. Again: understand that we are talking about the deliberate & clandestine annihilation — forever — of some of the scientific data in a classic, legendary scientific opus. All this, in order to hide one's own theft of another scientist's greatest work: the Hipparchos star catalog, which Pliny 2.95 justly refers to as "a legacy to all mankind". Question. If Historians-of-science do not regard data-destruction & data-theft as scientific crime, then: what ARE they willing to call criminal?¹³

C2 Once the latitude distribution's consistency with randomness is realized, we apply the "hide" part of the foregoing slide&hide technique to the 40'-slide-displaced longitude distribution of §C1 (random); i.e., we merge the 25' & 55' counts with the 20' & 00' counts, respectively. The result is the following predicted profile of probabilities: 5/24 (00'), 1/6 (10'), 5/24 (20'), 1/8 (30'), 1/6 (40'), 1/8 (50'). In short, this is the expectation-profile if RN's hypothesis is correct. For 317 southern stars, the expected numbers would be: 66 (00'), 53 (10'), 66 (20'), 40 (30'), 53 (40'), 40 (50'). The actual numbers of stars in each cell are (Table 1): 79 (00'), 66 (10'), 55 (20'), 34 (30'), 51 (40'), 32 (50'). The resulting $\chi^2 = 10$, which is not a significant discord ($P = 1/13$) for $df = 5$. (The same computation for the zodiacal & northern Catalog stars will produce $\chi^2 = 38$ and 29, respectively, both grossly significant.) Incidentally, if we jettison RN's slide&hide hypothesis and simply test the southern star fractions profile for straight randomness (which would theoretically produce equal numbers of stars for each fraction: 53 stars), we get $\chi^2 = 31$ for $df = 5$ — which is almost excessively significant (probability $P = \text{ordmag } 1/100,000$). Thus, the southern longitude fractions are wildly incompatible with straight randomness, though they are compatible with a random distribution, after application (to it) of the Ptolemy slide&hide process discovered by RN. (Virtual fractions-randomness in the southern part of the Catalog was established above: §B5.)

C3 Another test: slide by 40' the actual (not random-theoretical) latitude-fractions profile (§B5 or Table 2) and then hide-merge the 25' and 55' entries (§C2), in order to predict the longitude profile. This transformation results in the following expected distribution for the southern stars: 72 (00'), 55 (10'), 55 (20'), 40 (30'), 52 (40'), 43 (50'). Comparing to the actual distribution (§C2 or Table 1), we find¹⁴ $\chi^2 = 3$. So the discord is not remotely significant. (For $df = 5$, $P > 2/3$.) Thus, the southern stars — which the extremely handsome *JHA* had adduced to tear down its RN-satan by splitting the Catalog sample into finer portions (lower statistical significance) — have ironically provided instead a lovely vindication of his slide&hide thesis: showing that RN's theory is correct for the Catalog not only in-the-large but in-the-fine.

C4 We may apply the same empirical-expectation-profile test to zodiacal and northern portions of the Catalog, as well. For the northern stars, the actual latitude-fractions profile is (Table 2): 108 (00'), 29 (10'), 33 (15'), 39 (20'), 75 (30'), 36 (40'), 10 (45'), 29 (50'). The slide&hide process transforms this into an expected longitude-fractions profile: 72 (00'), 75 (10'), 46 (20'), 29 (30'), 108 (40'), 29 (50'). The actual longitude numbers (Table 1): 62 (00'), 61 (10'), 67 (20'), 29 (30'), 95 (40'), 45 (50'). Which yields $\chi^2 = 10$; thus, for $df = 5$, the discrepancy is not statistically significant. For the zodiacal stars, the latitude profile is (Table 2): 68 (00'), 30 (10'), 28 (15'), 33 (20'), 82 (30'), 49 (40'), 20 (45'), 39 (50'). The slide&hide process transforms this into an expected longitude-fractions profile: 61 (00'), 82 (10'), 69 (20'), 39 (30'), 68 (40'), 30 (50'). The actual longitude profile (Table 1): 81 (00'), 52 (10'), 58 (20'), 36 (30'), 94 (40'), 23 (50'), and $\chi^2 = 16$ or (without Tau informata)

¹³ For an ironic answer, see *DIO* 2.1 ‡3 §C10 (& fn 32).

¹⁴ The formula for finding χ^2 for several samples is provided at R.Burington & D.May *Handbook of Probability & Statistics with Tables* 1970 ed, p.234. Włodarczyk 1990 p.292 gives a much neater version of this formula for the special case of two samples. (Proving the latter expression from the former is a fun student exercise.)

‡2 Columbus's Plana Landfall

Evidence for the Plana Cays as Columbus's San Salvador

by Keith A. Pickering¹

A Introduction

A1 In recent years, the majority of those scholars active in the robust Columbus-landfall controversy have enthusiastically pointed out how their respective theories are a better match with the *Diario* (the log of Columbus's first voyage to the New World) than the Watlings Island theory supported by many historians, most notably Samuel Eliot Morison.² Among the theorists are Arne Molander³ with Egg Island, Joseph Judge⁴ with Samana Cay, Pieter Verhoog with Caicos, Dr. Stephen Mitchell with Conception, Robert H. Power with Grand Turk, and Ramon Didiez Burgos with the Plana Cays. All these theories have, as their purpose, the determination of the location of San Salvador [Columbus's 1st "landfall" — i.e., sighting of land]; and, as their method, the tracing of Columbus's track to and-or among San Salvador & its neighboring islands. All tracks proposed to date have inconsistencies (often serious ones) with the distances, directions, and descriptions provided in the *Diario*. The *Diario* itself is an abstract of Columbus's centuries-lost original log, prepared by (Fray) Bartolomé de Las Casas in the early 16th century; this situation has given rise to a hive of speculations on possible transcription errors, often conveniently located just where they will do a particular theory the most good.

A2 In this paper, I will propose a new track which has fewer inconsistencies and greater fidelity than any proposed thus far; and I will — in an equitable⁵ Columbus Landfall Theory Scorecard (Table 1, p.14) — compare this track to other currently popular tracks, with an eye toward rating the various tracks vs. the descriptions in the *Diario*; and I will show how the *Diario* is far more internally consistent and error-free than many now suppose.

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² Samuel Eliot Morison, *Admiral of the Ocean Sea*, 1942; Little, Brown, & Co., Boston.

³ My knowledgeable friend Arne Molander (whose criticisms have contributed materially to the improvement of this paper) disagrees strongly with many of the views expressed here. Those who wish to hear his side of the ongoing landfall debate may contact him at 19131 Roman Way, Gaithersburg, MD 20879 (telephone 301-948-7341), whence Arne frequently circulates a useful *Columbus Landfall Round Robin*, featuring a spectrum of viewpoints. The *Round Robin*'s printed list of Contributors includes Joe Judge (Senior Editor emeritus, National Geographic), Jim Kelley, & the present writer. The list of Observers includes Charles Burroughs (Washington Area Explorers Club), Dennis Rawlins (*DIO*), Bradley Schaefer (Yale University, Physics Department), & John Russell-Wood (Johns Hopkins University, History Department). Microfilm copies of much of the *Round Robin* correspondence are available from David Henige, Memorial Library, 728 State Str., Madison, WI 53706 (phone 608-262-6397; fax 608-265-2754). [Note by DR: Correspondence-fallout from the present paper is at least as likely to appear in the *Round Robin* as in *DIO*. In fact, some has already appeared in the *Round Robin* — because a version of this paper (subsequently revised) has been circulating privately since 1992.]

⁴ [Note by DR: much of the computer work for Judge's important *National Geographic* studies, on the Columbus landfall question, was supervised by my late friend, the Minnesota scientist-explorer Robert Lillestrand, of Control Data Corp. (Lillestrand was also the scientist primarily responsible for the 1968-1969 precise determination of the northernmost point of land in the world, Kaffeklubben Island, which had been first reached by the premier US arctic explorer, Robert Peary, in 1900 May.)]

⁵ See §D2.

B The Transatlantic Track

B1 There are two ways to trace the track to San Salvador: forward from the Canaries, across the Atlantic; and backward from Cuba. The transatlantic track has been traced at least four times in the 20th century: first by John W. McElroy,⁶ in support of Morison's work; then in 1986 by Luis Marden,⁷ in support of Judge's work. Marden accounted for leeway and drift, which McElroy had not, moving the end-of-track south to the vicinity of Samana Cay; but a year later, Richardson and Goldsmith,⁸ using a more precise accounting for leeway and drift, moved the end of track north again to the vicinity of Watlings. All of these analyses were based on an isogonic chart for epoch 1500 which was drawn by Willem Van Bemmelen in 1899.⁹ Van Bemmelen's study was based on extremely sparse data, however, and its validity is highly dubious (as Van Bemmelen himself was careful to point out). We know for example that archeomagnetic studies are not precise enough to support this kind of application; the most recent work available¹⁰ indicates that the position of the north geomagnetic pole (NGP) for epoch 1500 can be placed only within a radius of some 400 nautical miles (nmi) at the 95% confidence level. This means that the standard deviation of direction for a magnetic vector at a mean point on the Earth's surface is about 8 degrees; which in turn implies that a single standard deviation for the transatlantic track is about 6 latitude degrees, north or south of the mean end-of-track.¹¹ In 1991, Goldsmith and Richardson tried again, creating their own isogons (based on the known landfall of Columbus's second voyage in 1493) and using a shorter length league; this placed the end-of-track in the vicinity of Grand Turk Island.¹²

B2 It is clear from this evidence that regardless of what mean end-of-track one chooses, the uncertainties are at present so large that there is no island in the Bahamas which can be eliminated from consideration on the basis of the transatlantic track.

C The Backward Track

C1 The transatlantic track being unhelpful (without, e.g., more accurate isogons), we are left with the backward track from Cuba. Columbus spent time at four islands in the Bahamas, which he called San Salvador, Santa Maria de la Concepción, Fernandina, and Isabela; the native names are recorded only for the first and fourth, Guanahani and Saometo, respectively. (It is common for Columbus scholars to refer to these four islands, respectively, by Roman numerals I through IV. See abbreviation-key attached to Table 1.) After leaving island IV, Columbus stopped at what he called the *Islas Arenas*, which are today known as the Ragged Islands.

C2 The Ragged Islands constitute the first step in the backward track from Cuba; Ragged is the only Bahama landfall accepted by all (or nearly all) Columbus scholars, and included

⁶ John W. McElroy, "The Ocean Navigation of Columbus on His First Voyage", *The American Neptune*, 1 (1941), pp.209-240.

⁷ Luis Marden, "The First Landfall of Columbus", *National Geographic*, November 1986, pp.572-577.

⁸ Philip L. Richardson and Roger A. Goldsmith, "The Columbus Landfall: Voyage Track Corrected for Winds and Currents", *Oceanus*, 30 (1987), pp.3-10.

⁹ W. Van Bemmelen, "Isogonen-karten für die Epochen 1500, etc.", Appendix to "Die Abweichung der Magnetnadel", a supplement to *Observations of the Royal Magnetical and Meteorological Observatory at Batavia, XXI* (1899) Batavia. Marden did not quote the reference directly. He apparently used the isogonic lines as reproduced in McElroy's 1941 article, as Van Bemmelen did not continue his isogons past 60° W and McElroy extrapolated them.

¹⁰ Ronald T. Merrill and Michael W. McElhinney, *The Earth's Magnetic Field*, 1983; Academic Press, London and New York, pp.84-86 & 99-101.

¹¹ Merrill & McElhinney 1983. This study (among others) also suggests that the 15th century was a period of rapid movement of the NGP; thus, even if we had a better understanding of the position of the NGP at epoch 1500, its position merely a decade earlier might have been nontrivially different. (Curiously, our knowledge is so fuzzy in this connection, that Columbus's reports may contribute as much to it, as it can tell us about Columbus's reports.)

¹² Roger A. Goldsmith and Philip L. Richardson, "Numerical Simulations of Columbus' Atlantic Crossings", *WHOI-92-14*, February 1992.

Table 1: Fractional Endings: Star Catalog Longitudes

Region	Totals	00'	10'	20'	30'	40'	50'
North	359	62	61	67	29	95	45
Zodiac	344	81	52	58	36	94	23
South	317	79	66	55	34	51	32
Sums	1020	222	179	180	99	240	100

Table 2: Fractional Endings: Star Catalog Latitudes

Region	Totals	00'	10'	15'	20'	30'	40'	45'	50'
North	359	108	29	33	39	75	36	10	29
Zodiac	349	68	30	28	33	82	49	20	39
South	317	52	43	33	39	55	38	17	40
Sums	1025	228	102	94	111	212	123	47	108

(30'), 38 (40'), 17 (45'), 40 (50'). The expected random profile — already given at §B3 — is strikingly similar. Comparing the two distributions statistically, we find $\chi^2 = 5$ for $df = 7$, not a significant discrepancy. (Probability $P > 50\%$.) By contrast, the same test (also $df = 7$) for the latitude-fractions (Table 2) of the zodiacal and northern stars (vs. the random profile of §B3) yields, respectively, $\chi^2 = 22$ (probability $P = 1/300$) and 70 ($P =$ forget-it) — highly significant in both cases. So only the southern section of the Catalog reveals a random profile. Later, below (§D), we will see that there is a simple, revealing explanation for why the southern stars' latitudes exhibit random fractions. However, before coming to this, I wish to confirm the randomness hypothesis by examining the southern stars' longitudes.

C Ptolemy's Slide&Hide Sleight: Final Confirmation of R.Newton

C1 RN was the first to discover how Ptolemy had solved a potentially-embarrassing difficulty implicit in his method of stealing the Catalog. Our standard eight fraction-endings' frequencies are listed in §B3; when Ptolemy's theft (§A1) slid each of these fractions upward by 40', the resulting fractions had a displaced set of frequency rates: 1/8 (00'), 1/6 (10'), 1/8 (20'), 1/12 (25'), 1/8 (30'), 1/6 (40'), 1/8 (50'), 1/12 (55'). Obviously, the 25' & 55' fractions jarred with the other fractions (especially with no 15' or 45' fractions at all resulting from the 40' shift!), and so — if left unaltered — would have revealed Ptolemy's plagiarism of the Catalog. Thus, he hid his trail by changing all 25' fractions to 20' and all 55' fractions to 00'. *Sneakeeey*. (Note: This RN hypothesis¹² was thus the first to explain the virtual lack of 15' & 45' fractions in the Catalog longitudes, a deficit which — before RN — had previously seemed particularly odd, since the Catalog latitudes had plenty of these fractions.) For brevity, we will call the full (2 stage) Ptolemy plagiarism-procedure: "slide&hide". Two comments: [a] This is deliberate, conscious fraud. [b] It

agreement with PK & Manitius 1912-3 2:50.) Of the 1028 stars in the Catalog, 3 redundant stars are here dropped: PK147 (= PK96), PK230 (= PK400), PK670 (= PK1011). (Which leaves a total of 1025 separate stars. This correct count first appeared at Rawlins 1982C p.359.) Of the 349 zodiacal longitudes, 5 exhibit 15' endings — thus they are not included in Table 1. (See *DIO* 2.3 ‡8 fn 20.) No other Catalog longitudes have either 15' or 45' endings.

¹² R.Newton 1977 pp.250-254. RN concludes this discussion with the comment: "Ptolemy would surely be startled if he could know how much we can learn about his fabrication simply from studying the fractions in the star catalogue."

B Randomness Unperceived: JHA Wetdream-Comes-Nightmare

B1 As Thurston has scrupulously remarked (in his 1993/1/10 letter), the fact that the southern part of the Catalog does not exhibit the usual dominance of 40's had already been pointed out by Shevchenko 1990 p.195 and Włodarczyk 1990 pp.290-291. Unlike Thurston, both of these contributors to the extremely handsome *Journal for the History of Astronomy* attempted to use this discrepancy to attack the credibility of the *JHA*-hated RN's entire Catalog analysis. We are about to discover the very reverse of this *JHA* wetdream, once we observe a remarkably elementary point which our *JHA* authors overlooked.

B2 RN's critics appear to have forgotten that the whole *basis* for RN's expected distribution was simply: the latitude-fractions' frequency-profile with 40' added. So, for any *portion* of the Catalog where there is no unusual statistical excess of latitude 00's, there need be no excess of longitude 40's. When we examine the southern part of the Catalog, this is precisely what we find. Thus, the nonexistence of an excess of 40's (for the southern stars) has — with delicious perversity — provided complete vindication for RN, instead of the longed-for refutation which our judicious *JHA* so fervently desired when it spotlighted the southern 40' nonexcess. (See §C3.)

B3 Both to understand this point and to see why it has not been previously discovered, we must first examine the peculiar ancient convention for writing fractions of degrees, which was accomplished via “unit fractions”, i.e., inverse integers. (E.g., 15' was written as 1/4 degree, and 45' or 3/4 of a degree was expressed as 1/2 + 1/4.) The Catalog latitudes were entirely expressed in whole degrees, halves, thirds, quarters, sixths. The result is that all the Catalog latitude fractions are (if we express them in arcmin): 00', 10', 15', 20', 30', 40', 45', 50'. However, note that these eight possible fractions *do not cover equal empirical ranges*. Taking midpoints (between these values) as bounds, the eight corresponding ranges' upper bounds are, respectively, 05', 12' 1/2, 17' 1/2, 25', 35', 42' 1/2, 47' 1/2, 55'; thus, the size of the eight ranges are, respectively, 10', 7' 1/2, 5', 7' 1/2, 10', 7' 1/2, 5', 7' 1/2. Dividing by 60', we have the expected frequency⁹ of each fraction in a random case: 1/6 (00'), 1/8 (10'), 1/12 (15'), 1/8 (20'), 1/6 (30'), 1/8 (40'), 1/12 (45'), 1/8 (50'). For the 317 southern stars, the predicted (random case)¹⁰ distribution would thus be: 53 (00'), 40 (10'), 26 (15'), 40 (20'), 53 (30'), 40 (40'), 26 (45'), 40 (50').

B4 Previous researchers, glancing at the latitude-fractions distribution for the Catalog's southern stars (Table 2), noticed that the 00' & 30' fractions were slightly more numerous than other fractions and so naturally assumed that the Catalog's southern latitudes followed the RN mean profile for the whole Catalog. (Thus the seeming mystery of why the southern longitudes didn't do so.) But, what has been previously overlooked is that a *completely random* set of star observations would also display (§B3 & §B5) a modest excess of 00' & 30' fractions — merely because of the large 10' ranges (55' to 05' & 25' to 35', respectively) associated with these two fractions, a result of nothing more than the range-inequalities (§B3) inherent in the ancients' fashion of using unit fractions for degree-division. This situation is in contrast to the mean total 00' plus 30' excess (of the entire Catalog) found by RN, which for 00' was clearly over&above the random-profile frequency of §B3, and was due to a common observer's bias (§A1) toward rounding data to whole degrees.

B5 To attack the question of the fractions in the Catalog's 317 southern stars, we start with a straight χ^2 test upon the observed southern latitudes vs. the random-case profile of §B3. The observed¹¹ profile is (see Table 2): 52 (00'), 43 (10'), 33 (15'), 39 (20'), 55

⁹ I note that these probabilities were earlier computed by R.Newton 1977 p.247. Incidentally, there is little evidence that 30' Hipparchos Catalog endings were more likely than the random probability (1/6). True (§A4), as seen in Table 2, for zodiac β , 23% have 30' endings, which is well (3.4 σ) above chance; however, zodiac β is a separate sample from the rest (§C5), presumably older (see Rawlins 1982C p.369& *DIO-JHA* 1.2 fn 152, 1992/12 bracket). Possibly zodiac β were observed with an astrolabe graduated in half-degrees. See Neugebauer 1975 p.699.

¹⁰ Compare to the distribution if degree-fifths are allowed: §F2.

¹¹ To avoid needless disagreements with the Muffia, I have here throughout rigorously adopted the fractions given in the scrupulous rendition by Toomer 1984. (His misprinted fraction for PK575's latitude I have taken to be 1/6, in

on the track of nearly every theory. The reason for this wide acceptance is easy to see: the Ragged Islands are in the right direction from Cuba; they are at the right distance from Cuba; and they match Columbus's description of the *Islas Arena* perfectly — seven or eight islands in north-south¹³ line. The Ragged Islands pass what I call the “3-D test”: right distance, right direction, right description. Any island along the track that passes this test can be considered as close to being proven as it is possible to get.

C3 The next step back from the Ragged Islands is to Columbus's *Isabela*, island IV. But here we seem to be stymied almost before we get started: Columbus tells us the direction he sails (west-southwest) when leaving island IV, but he omits the distance he makes during the day of October 24. However, at dusk Columbus reports that he is 7 leagues southwest of “Cape Verde” (which is “in the western part of the southern part”¹⁴ of island III). This establishes the so-called Cape Verde fix, and allows us to plot his movements from island III instead of island IV. During that night, he makes only two leagues because of bad weather, then makes 5 more leagues the following morning, continuing WSW. At 9 a.m., he turns straight west and makes 11 leagues, at which time he sees the Ragged Islands lying 5 leagues ahead of him.

C4 We now have enough information to compute the distance & direction (backward) from Columbus's Ragged Islands landfall to Cape Verde on island III, using a simple summation of vectors: from island III, we go seven leagues southeast to the Cape Verde fix, then follow the fleet seven leagues WSW, and another 16 leagues west. The summed vector is 19.1 leagues long at a direction 23.5 degrees north of east, which is quite close to east-northeast. We can now plot this on a map, if we know how long a league is. The shortest league mentioned by modern scholars is the 16,240 foot Geometric League (GL) advanced by James E. Kelley,¹⁵ while the longest commonly used is the 19,400 foot Portuguese Maritime League (PML) used by Morison and others. We will use these figures as outside limits in our plot. In Figure 1, I have plotted a box which includes all points lying 23.5 degrees N of E from (i.e., nearly ENE of) the Ragged Islands, at any distance which is between 19.1 GL and 19.1 PML. If we believe Columbus, Cape Verde on island III must lie inside this box.

C5 Only Long Island lies inside the box. Columbus tells us that Cape Verde is in the southern part of island III, and only the southern part of Long Island lies inside the box, which is good confirmation. But, just to be absolutely sure, there is one more test to apply: does Long Island match the description Columbus gives of island III? Does Long Island have a coast at least 20 leagues long? Does Long Island have a coast that runs NNW? Does part of the coast run east-west? The answer is Yes to all these questions. Long Island matches the description in the *Diario*, it is in the right direction from the previous landfall, and it is at the right distance from the previous landfall; Long Island passes the 3-D test. This runs the Egg Island theory aground, which has Andros as island III (Andros matches the description, but is in the wrong direction and is hopelessly too far away). Both the Caicos and Grand Turk theories have Acklins as Island III, but Acklins has the wrong distance, wrong direction, **and** wrong description, as it has no NNW coast.

C6 Having determined the identity of island III, we now work back to island II, using the same technique and applying the same criteria. The distance from Long Island to the western cape of island II is 8 or 9 leagues, and the direction is east — or “almost east-west”. In Figure 2, I have plotted 8 GL and 9 PML as the limits of the search box, and have run the box eastward along the coast of Long Island. The box includes part of Rum Cay, but not

¹³ Throughout this paper's tracings of Columbus's inter-island route, we will implicitly identify his compass headings with true ones — i.e., we are presuming effectively zero compass variation in this region in 1492. For transatlantic tracking, the zero-variation assumption would be disastrous; but the errors it introduces over short Bahamian distances are trifling.

¹⁴ Oliver Dunn and James E. Kelley, Jr. *The Diario of Christopher Columbus's First Voyage to America, 1492-1493*, 1989: University of Oklahoma Press, Norman (OK) and London, p.113.

¹⁵ James E. Kelley, Jr. “In the Wake of Columbus on a Portolan Chart”, *Terrae Incognitae*, 15 (1983), pp.102-107.

the western cape. Since there may be uncertainties in Columbus's distance estimates,¹⁶ we cannot positively eliminate Rum Cay with this evidence; but the western cape of Crooked Island is within the box, which should lead us to prefer Crooked. Just to make sure, we will apply our final test. Columbus describes island II as having a north-south coast five leagues long and an east-west coast of ten leagues or more. In Figure 3, I have superimposed an F-shaped device over a map of Rum Cay, and over the Acklins-Crooked Island group at the same scale; the F shows a 10-by-5-league coastline in both GL and PML. Rum Cay is only a third (or less) the size of island II; while it is clear that if the north coasts of Crooked and Acklins are considered a continuous coastline, we have a nearly perfect fit. As Gustavus V. Fox has pointed out,¹⁷ the Acklins-Crooked group is the only coast in the central Bahamas that matches this description so perfectly. Acklins-Crooked therefore passes the 3-D test: right distance, right direction, right description.

C7 Rum Cay is in the right direction, but is at somewhat the wrong distance and does not even remotely match the size described in the *Diario*. Mitchell argues¹⁸ that the *Diario* distances are transcription errors of leagues for miles: that Columbus wrote the length of these coastlines as five miles and ten miles, the "miles" being mistranslated into "leagues" by Las Casas. However, we have two witnesses who both saw the Admiral's own copy of the log and reported on its contents: Las Casas, who transcribed the *Diario*, and the Admiral's son Fernando Colon, who wrote a biography of his famous father. Fernando's biography records many details of the first voyage lacking in the *Diario*, showing that Fernando was relying on the original log (not Las Casas's rendition); and Fernando's biography also records the length of these coastlines as five leagues and ten leagues. Thus the suggestion of transcription error by Las Casas is untenable in this instance.

C8 The final step in the backward track is from Acklins to island I, Columbus's San Salvador. There are two estimates given in the *Diario* for the distance from San Salvador to island II: when setting sail on the 14th, Columbus estimates five leagues, but when arriving the next day, he revises this to seven. And the direction is clear: there is only one place in the *Diario* in which Columbus mentions the direction to San Salvador from any other point. The fleet was detained by tides on the morning of the 15th, and arrives at island II around noon. Columbus describes this island as having a coast running north and south for five leagues, and a coast running east and west for more than ten leagues. But here we arrive at a vital passage that is unaccountably missing from nearly every article and book written about the 1st landfall: for Columbus also says, in that very same sentence, that the coast which runs north-south **faces San Salvador**. For anyone searching for the location of San Salvador, this is arguably the single most important description in the *Diario*, because this is the only place in the *Diario* that records the direction to San Salvador from any other point: due east of that five-league-long north-south coast.

C9 Returning to our backward track, we plot 5 GL and 7 PML, working east from the east coast of Acklins. As seen in Figure 4, only the Plana Cays lie within the plot box. Only Plana is at the right distance and in the right direction to be San Salvador. The western end

¹⁶ Kelley 1983 pp.94-97. Kelley's paper brilliantly demonstrates that Columbus's distance-estimates along the north coast of Cuba were inflated because of the half-knot current he was working against. It was these same inflated estimates which earlier led Morison to postulate that Columbus used an ultra-short "land league" when measuring coastlines.

¹⁷ Gustavus V. Fox, "An Attempt to Solve the Problem of the First Landing Place of Columbus in the New World", *Report of the Superintendent of the U. S. Coast and Geodetic Survey* (Appendix No. 18, June 1880). Washington: Government Printing Office, 1882, p.47.

¹⁸ Steven W. Mitchell, "Columbus's Track from San Salvador to Cuba: a New Conception", unpublished lecture notes from U. S. Naval Institute seminar of 24 April 1992, Annapolis. It is true that, at several points in Las Casas's transcription, he originally wrote "leagues" and then crossed this out, substituting "miles". (Columbus's "mile" was considerably smaller than our modern 1852 meter nautical mile.) It seems to me that these false starts simply reinforce the idea that all measurements in the original log were in leagues, and Las Casas was converting to miles as he went along. Since the possibility of transcription error is enhanced when the copyist must pause for such a calculation, the fact that all such instances occur during the recording of miles suggests that miles (not leagues) were the result of these calculations.

at least to me) I decided to check for myself. . . . the southern constellations: RN effect absent. . . . [As to whether I think it] likely that Ptolemy added 2°40' to most of the [Catalog longitudes] the answer would [be] yes; in fact, not so much likely as almost certain. . . . If you had asked "Do you believe that Ptolemy added 2°40' to the longitudes of all (or almost all) of the stars in the catalog?" the answer would have been *no*. Reason: the strong argument for this suggestion, namely the [RN] arc-minute distribution [excess of 40' fractions], does not apply to the whole catalog. . . . the origin of the southern part of the catalog is a complete mystery. . . . Presumably it was not observed by Ptolemy, unless he corrected his error⁶ in the latitude of Alexandria.

A4 A few weeks later (3/20), Thurston added:

Two odd facts may be significant in some way. The northern constellations have a huge preponderance of zero endings for latitudes. . . . The zodiac latitudes have substantially more halves than zeroes. . . . [However, Ptolemy at *Almajest* 5.1] said that his [astrolabe rings'] scales were graduated in fractions of a degree as small as practical.⁷ This cannot mean whole degrees nor, unless we interpret it extraordinarily liberally, half-degrees. So if the scales were graduated in whole or half degrees here is another bit of evidence that Ptolemy was not telling the truth. . . . Finally, a quotation from Bishop Berkeley that you might think applies to the muffia: "I observed how unaccountable it was, that men so easy to confute should yet be so difficult to convince."

A5 It will be seen that the paper which follows here grew out of Thurston's queries (§A1 & §A3) on the southern part of the Catalog. After completing the core of the math research, DR wrote Thurston (1993/5/5):

Thanks for your letters of 1993/2/25 & 3/20, which were nicer than I deserved. . . . I am just finishing up the computer work for a *DIO* paper (whose underlying new finds commenced 1993/3/9-10), the upshot of which may interest you. An oddity: though one of the main programs is rather long . . . and occasionally intense, nonetheless, its key discovery (1993/4/8) is so simple that a [gradeschooler] should be able to follow it. Even a Muffioso might. Might.

As noted, the paper in question is the same one you are reading — which owes its very inception to Thurston's frequently fruitful skepticism.⁸

⁶ This refers to *DIO* 2.3 ‡8 §C22 item [f] (Rawlins 1987 p.236 item 2): the discovery (Rawlins 1982C p.367) that the Catalog's indicated latitude error $\Delta\phi$ is incompatible with Ptolemy's $\Delta\phi = -14'$. (Cataloger's $\Delta\phi = 0' \pm 1'$: *idem* Table V, zodiac stars, column *y*. See also §G3 & fn 19 here.) It should be noted that there is a reverse incompatibility for Ulugh Beg's star catalog: though UB estimated his latitude correctly, the meridian ring of his astrolabe was mis-set ordmag 0°.1 high. (As accurately confirmed in 1992 by K.Krisicunas. See *BullAAAS* 24.4 & *JHA* 24.4:269. I disagree with the explanation given for the discrepancy.) This is less suspicious than the case of Ptolemy, whose defenders must assume that, though he had the wrong latitude for his nonexistent Alexandria observatory, he accidentally happened to set his equally nonexistent (fn 7) astrolabe's meridian ring right smack on the correct latitude. (Suspicion independently confirmed at §F9 via star declinations.)

⁷ DR comment (expanding upon Thurston's observations): Can one imagine an astronomer describing *his own* astrolabe in such a vague fashion? *An instrument on which he has personally measured the λ & β of 1000+ stars?* (Would not an observer, so intimate with the instrument, specify precisely that its long-familiar-to-him rings were graduated in quarter-degrees or sixth-degrees or whatever? Doesn't he even *know* which it is?) Such considerations long ago revealed the phoniness, of Ptolemy's pretensions, to the perceptive (& highly competent) astronomer Delambre 1817 1:xxvii.

⁸ See Thurston's erudite survey of the history of yearlength measurements, which the *Griffith Observer* deserves commendation for running as the lead article in its 1993/6 issue. (I would add only 2 small observations: [a] Ancients preferred solstices to equinoxes for estimating the year's length. And they were wise — at least in time — to do so. See R.Newton 1977 pp.81-82, *DIO* 1.1 ‡5 fn 20, & *DIO* 1.3 §K4. [b] The *GO*'s diagram at the top of p.5 makes the Winter Solstice noon zenith distance of the Sun in ancient Alexandria equal to 55°.2 instead of the correct value, 54°.9. See Rawlins 1982G pp.261 & 264.)

Summary

From the Ancient Star Catalog and Hipparchos' stellar declinations, we discover his Rhodos Island astronomical observing sites' precise locations, unknown for 2000^y. The main observatory, where his high-precision star declinations were measured, was within a mile or two of his adopted latitude $\phi = 36^\circ 08' N$ — perhaps upon a hill in Lindos' northwest suburbs. The less accurate southern portion of his famous ecliptical Ancient Star Catalog was observed by crude transit instrument, at Rhodos Island's southern tip ($\phi = 35^\circ 53' N$), which he took to be at latitude $\phi = 35^\circ 50' N$.

A Thurston's Skepticism

A1 On 1993/1/10, Hugh Thurston (Dep't Math, University of British Columbia) wrote *DIO* a letter which would shortly prove fateful for historians of the immortal ancient astronomer, Hipparchos of Nicaea. We recall that one of the two⁴ most compelling arguments for Hipparchos' authorship of the Ancient Star Catalog is the finding by R.Newton (RN) that, while the Catalog's latitude fraction-endings exhibit the usual empirical excess of 00's (due to naïve observers' natural tendency to round data) and 30's (§B4), the longitudes' fractions show a different pattern: 40's outnumber 00's, and 10's outnumber 30's — just as one would expect if (as 1st publicly charged by Tycho in 1598, on other grounds) Ptolemy had stolen Hipparchos' star catalog by merely updating it for precession: adding 2°40' onto all the longitudes, while leaving the latitudes unchanged. Commenting on this famous RN fractions argument (R.Newton 1977 pp.245f; summary: *DIO* 2.3 ‡8 §C7), Thurston said:

Many thanks for *DIO* [2.3]; I've been wondering how you'd react to Swerdlow. . . . I tested the [RN fractional] distribution for myself. (Yes, I'm a skeptic.) For the northern constellations the RN distribution showed up well; for the zodiac, weakly;⁵ in the south, not at all.

A2 DR responded (1/28):

Does "skeptical" mean that you don't believe it likely that Ptolemy just added 2°40' onto most of the Catalog stars' longitudes to get the places he reports as [his own] "observations"? If so, I'm surprised — but will want to hear your reasoning anyway, and will even publish it if you wish.

A3 Thurston replied 2/25:

A skeptic is someone who does not take everything on trust but likes to check things for himself or herself. Aren't you a skeptic? Anyway, when I read RN's account of the arc-minute endings (way back before he was well known,

⁴ For the other, see *DIO* 2.3 ‡8 §§C10-C15.

⁵ There have been a number of attempts to split the Catalog's zodiacal stars, in order to show that only part of this sample exhibits the RN distribution. Shevchenko 1990 p.194 suggests that only Sgr through Gem (7 constellations) do so, while Cnc through Sco (5 constellations) don't. A friend of mine suggests that the split ought to be: the 6 north constellations (Ari through Vir) don't, while the southern ones (Lib through Psc) do. The catch with such schemes is that the deviation from the RN distribution is so weak that, when one takes into account all the ways one may split the sample, the full-contextual probability is not statistically significant. (The fundamental sample-splitting underlying the current paper's start is merely: north, zodiac, south. But these are Ptolemy's divisions, not DR's. See similar approach at fn 45.) Also, while it is true that, for the north half of the zodiac, 00's outnumber 40's (contra the RN distribution), this result is due to the eleven Tau informata stars, which are so peculiar (the first 9 are all 00's, an anomaly noted by Włodarczyk 1990 p.294) that even the Shevchenko 1990 attack on RN throws them out. Using the numbers of Shevchenko 1990 (Table 4, p.193, which contains some small errors, and a *JHA* typo of 11 where 1 is meant for the Psc 50's): we have more 40's than 00's AND (the most dramatic contrast, noted at *DIO* 2.3 ‡8 §C22 item [e]) far, far more 10's than 30's. And these RN-profile patterns hold both for the northern and southern half of the zodiac. Note that, though including the Tau informata will (for the northern zodiac) permit the 00's to outnumber the 40's, this will have null effect upon the enormous 10' vs. 30' contrast.

of the box bisects West Plana, but given the accuracy of his estimates, this is unimportant. The actual distance from his anchorage off the western point of Plana to Northeast Point on Acklins is 4.6 to 5.6 leagues, depending on the league one chooses. As a final test, we check the description. Does Plana have a coast that runs north-northeast? Does Plana have a laguna, reef, and harbor? Yes to all questions; Plana passes the 3-D test. Samana Cay is at the right distance from Acklins, but it is in the wrong direction and does not match the description, since it has no north-northeast coastline. Only Plana is in the right direction, at the right distance, and matches the description. Only Plana can be San Salvador.

C10 Since Columbus had previously stated his intention to sail southwest when leaving San Salvador, this implies that Columbus changed his mind. As we shall see, Columbus was unsure of his destination on the following day, so this makes perfect sense. When we have such a case as this — when Columbus's before-the-fact intention disagrees with his after-the-fact description — the preferred interpretation should **always** be the after-the-fact description. This is especially true when, as in the present instance, Columbus shows doubt in the interim.

C11 Also note that the *Diario* does not say that the entire east coast of island II is five leagues long. The *Diario* addresses only the length of the coast that faces San Salvador. South of Creek Point, the coast of Acklins turns southwest and no longer faces Plana. According to the *Diario*, the coast of island II facing San Salvador must have two independent qualities: [a] it must run north-south; and [b] it must be five leagues long. Of those islands that are possible first-landfall sites, only Plana and Grand Turk pass both of these tests. Samana passes neither test; ditto Watlings; ditto Conception; ditto Egg. It is astonishing to find that this critical clue has been ignored by almost every landfall theorist of the 20th century, including (but not limited to) Morison, Judge, Molander, and Mitchell; it is distressing to find that most theorists not only do not attempt to solve the problem, in most cases they fail even to inform their readers that the discrepancy exists.

C12 Fox was one of the few landfall theorists to deal with this problem; in his original paper advocating the Samana landfall, he attempted to dispose of this inconsistency.¹⁹ Fox relied on R. H. Major's suggestion that what Columbus really meant was that the north-south coast faces in the direction that Columbus arrived at Acklins while sailing from San Salvador. This is one of the few times that the Samana theory dips into what David Henige²⁰ derisively calls Presumably-Columbus-Meant-To-Say thinking — an infinitely flexible form of logic capable of explaining any discrepancy. Fox further assumed that Columbus was so poor a sailor that he could not compensate for tidal currents, another highly debatable point.

D Keeping Score

D1 Many of the descriptions of San Salvador in the *Diario* are too vague to be of much use in comparing the various theories against each other. For example, Columbus describes San Salvador as "green" and "flat", but compared to his previous landfall in the Canaries, any island in the Bahamas could be considered green and flat.

D2 Question: can the strengths and weaknesses of the many landfall theories be objectively evaluated? The prospects for such a delicate enterprise will be enhanced if we write down each important description in the *Diario* and then compare the several competing theories with each description, assigning a score or ranking to each. I have done this in Table 1, printed at the start of this article (p.14): the Columbus Landfall Theory Scorecard. In 1986, Arne Molander²¹ performed a similar numerical evaluation, when comparing the

¹⁹ Fox 1880, pp.47-48.

²⁰ David P. Henige, "Samuel Eliot Morison as Translator and Interpreter of Columbus's *diario de a bordo*", *Terrae Incognitae*, 20 (1988), p.85.

²¹ Arne Molander, "Egg Island is the Landfall of Columbus", *San Salvador Conference* (1st: 1986; College of the Finger Lakes, Bahamian Field Station), pp.161-169.

Watlings and Egg Island theories. The current evaluation is different in that: [a] it includes seven theories, and [b] the clues are not ranked as to weight. Also, there are quite a number of differences in which clues are included and which are excluded. I have concentrated on quantitative descriptions in the *Diario*: distances, directions, and existence of fixed features; and generally excluded those clues for which only circumstantial evidence is available for evaluation. For example, Molander gives Watlings the edge on cultivation of cotton; I have excluded this clue, on the grounds that the best current evidence is very spotty, regarding the distribution of native cotton in the pre-Columbian era.²² Also, I have assigned a score to each clue for each theory, not just to that theory which is superior. These scores are necessarily subjective, and therefore open to debate. I can only state that I have made every effort to be as objective and fair as possible, and have tended to give theories the benefit of the doubt whenever possible.

D3 To win the debate, a theory must show its clear superiority over every other competing theory, and through the entire track from San Salvador to the commonly accepted later Ragged Islands landfall. As the Scorecard shows, the only theory that can do this is Plana. In order to find out why, let us again trace Columbus's track, this time going forward from San Salvador, evaluating the various theories with respect to the *Diario*.

E San Salvador as Columbus Saw It

E1 One of the most famous parts of Columbus's account is his sighting of land on the night of October 11 in the form of a dim light in the west. This sighting has generated controversy among Columbus scholars, primarily because the sighting took place at 10 p.m., four hours before land was actually sighted — at 2 a.m. on the 12th (by Rodrigo de Triana of the *Pinta*, which announced the historic moment via pre-arranged signal). At the speed the fleet was travelling that night, Columbus must have been about 35 or 40 nautical miles from San Salvador: an impossible distance to see such a light, in spite of attempts to prove otherwise. Some islands have an advantage in this regard, however, since it is possible that the fleet passed close by another island earlier in the night, from which the light was seen. Proposed landfalls which have such an island to the east rate a 3 on the Scorecard; those which do not are forced to rely on Morison's explanation that Columbus's eyes were playing tricks on him. Morison's explanation seems unlikely, however, since at least two other persons saw the light at the same time; but in the interests of mercy, we will evaluate these landfall-theories with a 1, instead of a 0. [After spotting land at 2 AM of Oct.12, Columbus held position, a few miles to the east of San Salvador, until dawn. It is a historian's privilege to empathize with the discoverer's ecstatic frame of mind in these first post-uncertainty hours, during which he could at last savor the realization that the visionary success he so deserved had actually come true. One doubts he spent any of these hours in sleep: indeed, his diary entries for Oct.11&12 run together. Finally, as the grand morn of Oct.12 broke, his party — spying a flock of curious natives on the beach — went ashore.]

E2 The important question of San Salvador's size poses a knotty problem, since the *Diario* does not give the size as a quantified measurement; and because two other sources (Las Casas's *Historia de las Indias* and Ferdinand Columbus's biography of his father) each describe the size of the island as 15 leagues. Morison has speculated that the 15 leagues actually refers to the size of island II, and somehow was attached to San Salvador by mistake. [A mistake which might be related to Columbus's or a follower's desire to magnify his find.] I nonetheless prefer a quite distinct speculation: that the 15 league value refers to the size of Cat Island, which is about that size, and which is identified as "Guanahani" or "San Salvador" on many 16th and 17th century maps. Under this assumption, the name Guanahani (or a similar name: native names are notorious for near-homonyms) applies to

²² Richard Rose, "Lucayan Lifeways at the Time of Columbus", *San Salvador Conference* (1st: 1986; College of the Finger Lakes, Bahamian Field Station), pp.328-329.

‡3 Hipparchos' Rhodos Observatories Located: Lindos & Cape Prassonesi

Spherical Trig's Existence in the 2nd Century BC

Claudius Ptolemy's Stellar Sneak-Thievery Established: R. Newton's Star Catalog Theory Utterly Vindicated

by Rawlins¹

The more *outré* . . . [evidence] is, the more carefully it deserves to be examined; and the very point which appears to complicate a case is, when duly considered and scientifically handled, the one which is most likely to elucidate it.²

Dedication

To Noel Michelson Swerdlow (*JHA* Advisory Editor and History-of-science MacArthur Fellow), who couldn't have better timed his prominent *JHA* "moratorium"-proposal (Swerdlow 1992 p.182),³ which suggests that, since research on the Ancient Star Catalog is (NMS believes) getting nowhere in discovering the Catalog's authorship, scholars should cease wasting further labor on such a barren mine.

¹ Rawlins is: Impossible. [The succinct & infallible judgement of no less than the dear *J.Hist.Astr.*'s dearer Editor-for-Life. See *DIO-J.Hyster.Astr* 1.2 §§B1, C2, & G6.]

² A.C.Doyle *Hound of the Baskervilles* last chapter. See also Doyle's *Study in Scarlet* Chap.7.

³ Swerdlow's precious *JHA* paper is critiqued at *DIO* 2.3 ‡8 §C. One is similarly grateful for Evans 1993, which appeared at the very time the present paper's discoveries were being accomplished. (Evans 1993 arrived at the JHU Library only 2 days after DR's 1993/3/17 finding of the §C2&§C3 evidence which confirmed RN's slide&hide explanation of the Star Catalog — hitherto attacked by *JHA* authors, including Evans 1987.) Considering that it appeared in the *JHA*, Evans 1993 is downright friendly, merely (warming the *JHA* putative editorship's putative heart by) accusing RN of "ahistorical" work. (Evans 1993 at least credits RN with stimulating some current investigations. See *DIO* 2.3 ‡6 §E1 & P.Huber at *DIO* 2.1. So: why's there still no symposium debate of these matters? — this despite DR's various challenges, including material distributed to Hist.sci worthies preceding & even during the Muffia's 1994/5/6-8 Dibner Institute conference at M.I.T.) Evans 1993 concludes by croc-tearfully regretting that RN "squandered his considerable talent". In the context of the [Muffia] community, one is tempted say no more than: at least RN had talent to squander. But the more relevant point is that, as always, the Muffia cannot admit that the RN-DR axis has made any substantial discoveries in ancient astronomy. Such behavior is not scholarly. It's religious. One used to suppose that theocratic dictatorships had withered away since the demise of Austria's Franz-Josef & Russia's Nicky 2. But, luckily, the extremely handsome *JHA* has clone-resurrected these pious caesars' spirits, in order to thrill us with a sociological counterpart to *Jurassic Park* — as gov't-fed, idea-eating behemoths lightfootedly (*DIO* 1.1 ‡5 fn 12 & *DIO* 2.1 p.2 *Info-Note*) roam academe, permitting no heresy to escape their eye and tooth.

Because the Indians, which he had taken in the first island of Guanahani or San Salvador, told him and indicated through signs that the island of Saometo [IV], which had been left behind, was larger³⁸ than Fernandina [III], and that they should return to it (and they must have done this in order to get closer to their land, from where he had taken them), the Admiral decided to turn around toward the east

H6 Two things are clear from this: first, that Saometo had been left behind during the trip from island I to island III; and, second, that returning to Saometo would bring the natives of Guanahani closer to their home. Perez has speculated that Las Casas had access to Columbus's map at the time he wrote this, which would account for these geographical concepts which are not immediately obvious in the *Diario*. In any case, this startling new evidence provides another heavy burden for all theories save Plana and Samana.

I Conclusions

I1 Regardless of which theory one supports, the important point is this: any theory, to be viable, must at least approximately fit the internally consistent cycle of distances among the four islands. Any theory that cannot is in trouble. Of all landfall theories that have been advanced, only two, Plana & Samana, are even remotely consistent with this internal cycle.

I2 And of these two, Plana must be preferred for three reasons. First: Columbus saw a light on the night of October 11 at 10 o'clock. From Plana, the island of Mayaguana lies about 40 miles east; the light he saw was on Mayaguana, somewhat to the west of south. [Note: since "landfall" refers to sighting, then, technically, this paper is actually suggesting that Mayaguana might have been Columbus's first landfall. Or: lightfall?] From Samana there is no such island to the east, and thus nowhere³⁹ for that light to be.

I3 Second, Plana has a coast that runs north-northeast, as Columbus describes the coast of San Salvador. Samana has no such coastline.

I4 Finally, and most importantly, Plana agrees with Columbus's description of island II (§F6) that "the face which is toward San Salvador runs North-South": the single most important description (§C11) in the *Diario* fits Plana perfectly, but it does not fit Samana, nor any other possible landfall site.

I5 In contrast, there is hardly one description in the *Diario* that can be decisively said to fit Samana better than Plana. The objection that Plana is two islands is based not on any firm evidence in the *Diario*, but rather on a presumption (contra §E9), in the supposed absence of such evidence. The biggest strength of the Samana theory is the track after San Salvador, which Plana shares; and the biggest weakness of the Samana theory is the description of San Salvador itself, which Plana wins easily. Clearly, Plana is by far the best fit to the descriptions in the *Diario*, and sets a new standard — by which all other theories must henceforth be judged.

³⁸ [If so, and if the Plana track is true, then "Saometo" refers not only to Fortune but to Crooked and perhaps even Acklins, in combination. (If Columbus, too, sometimes mentally combined nearly-contiguous Fortune & Crooked, this helps ease the III-to-IV directional problem of §F12: Figure 6 vs. Figure 8. Note also §§F11&F13: net sailing direction not due eastward.)]

³⁹ Why did Columbus not see Mayaguana but later did notice Plana? Assuming the Plana track, we note: [a] Plana's distance at discovery was less than that of Mayaguana when the light was reported. [b] The Moon rose (i.e., apparent terminator on sealevel horizon) at 10:30 PM, Local Apparent Time — thus, the well-risen moon's light upon Plana was stronger and more direct than the oblique rising-moon's light falling upon Mayaguana earlier. [No irresistible alternate ocular theories explain the 10 PM light; but, to try our best, we check out the sky: The 2nd magnitude star Ras Alhague set (azimuth $A = 284^\circ = 14$ degrees north of due west) at 9:51 PM LAT (sealevel); at 9:30 PM, apparent $h = 4^\circ 27'$, $A = 282^\circ$, zero-aerosol post-extinction magnitude $\mu = 3.6$; at 9:40 PM, $h = 2^\circ .3$, $A = 283^\circ$, $\mu = 4.3$; at 9:45 PM, $h = 1^\circ 1/4$, $A = 284^\circ$, $\mu = 5.0$. (The h cited here are sealevel: unenhanced by 7' dip, for height 15 m.) Morison (fn 2) 1:297 is understandably skeptical: for the claimed light-sighting, Columbus "demanded and obtained the annuity of 10,000 maravedis promised by the Sovereigns to the man who first sighted land."]

more than one Bahamian island. Cat Island itself has not been seriously advocated as the site of the first landfall since the *Diario* became widely known in the 19th century, because of its serious inconsistencies with the inter-island track. The *Diario* itself is of no particular help, with Las Casas describing San Salvador three times as an "isleta" ["small island"], and later quoting Columbus calling it an "isla", and a "bien grande"²³ one at that. Yet it was apparently small enough for Columbus to have explored substantially all of it by a small boat in a few hours. What we are left with, then, is little if any solid contemporaneous evidence on the size of San Salvador. For this reason, I have excluded the question of size from the Scorecard.

E3 Plana, Egg, and Caicos are multiple islands; the other contenders are single islands. It is true that Columbus nowhere says that there is more than one island at San Salvador. Is it equally true that Columbus nowhere says that there is only one island at San Salvador. Columbus gives San Salvador only one name, yet it is common practice among navigators and cartographers to give one name to more than one island: Midway is an example, Andros (in the Bahamas) is an example. And Plana itself is an example: five hundred years later, still only one official name for these two closely spaced islands. Columbus's use of "la isla" is no help, since if San Salvador is multiple, the singular could simply denote that particular island to which Columbus is referring at the time. We are therefore left with Columbus's sole description of the San Salvador coastline, in a single sentence: "I . . . went north-northeast along the island in order to see what there was in the other part, which was eastern part, which it has."²⁴ The final codicil *que habia*, as Molander has pointed out, is almost universally omitted from translation; yet it is important because every island has an east coast, but San Salvador has another part, which is the eastern part. And in order to reach this eastern part, it is necessary to go north-northeast along the island. Plana fulfills these requirements of the *Diario* perfectly well.

E4 Columbus explored San Salvador on the 14th, using both the boat from the ship, and the launches from the caravels. Much has been made of this exploration by some small island advocates, principally Judge,²⁵ who noted that at Watlings the trip to Graham's harbor, proposed by Morison, is too long to row. However, we know from the *Diario* entry of October 24 that the ship's boat carried a sail; and we have good reason to suspect that the caravels' launches did, too. On January 1, the launch made a round trip of 28 nmi (Navidad-Amiga-Navidad) between midnight and vespers, to collect rhubarb. Sunset (hence compline) occurred at about 5:30 on January 1, so vespers was at about 4:30. Allowing an hour ashore (for the rhubarb) leaves 15.5 hours of travel time from a midnight departure, at an average speed of about 2 knots. This implies that the launch was being sailed, not rowed. But regardless of whether one assumes rowing or sailing, the January 1 trip makes it quite clear that the boat trip of October 14, for both Watlings and Plana, is within the capabilities of the launch, in terms of both speed and endurance. I am assuming about 9 hours of boat travel on the 14th, which seems reasonable considering the niggling distance made by the ships before nightfall. Since all proposed boat trips fall within the parameters of possibility, I have evaluated all theories equally by excluding this clue from the Scorecard.

E5 Columbus's description of going NNE along San Salvador gives a huge headache to Samana Cay supporters, since there is no comfortable way to make this description match Samana. The entire island of Samana lies east and west; and since the ends are sharp points, there is no north-northeast coast at all. Judge supposes that Columbus may have spent a few minutes on a north-northeast course during five or six hours of rowing east and west along the south coast of Samana. And when he got back to his ship, he then wrote in his log that his most notable direction of travel was north-northwest, and conveniently ignores

²³ The correct English translation of *bien grande* is much in dispute. For what it's worth, I prefer "good sized" — which nicely reflects the ambiguity of the Spanish.

²⁴ Following Dunn & Kelley 1989, p.73; with the final three words added by this author.

²⁵ Joseph Judge, "Columbus's First Landfall in the New World", *National Geographic*, 170 (November 1986), pp.589-590.

the remaining six hours. Frankly, this scenario is completely unconvincing. I gave Samana a 0 on this point in the Scorecard.

E6 Grand Turk has the opposite problem: the entire island lies north-south, so while there is a little bit of NNE coast, the island has no appreciable “eastern part”, it has only northern and southern parts. I gave Grand Turk a 1 on this point.

E7 At Watlings Island it is possible to go north-northeast along the coast, which rates a 3 on that point. However, if you go north-northeast along Watlings, you do not get to the eastern part of the island; you get to the northern part of the island. Watlings is somewhat wider than Grand Turk, though, so I gave it a 2 on the “eastern part”.

E8 Look at a map of Plana, however, and Columbus's words require no explanation because the meaning is so self-evident. Columbus was anchored off the southwest point of West Plana (the logical anchorage in the prevailing NE trades). He wanted to go to “the other part, which was the eastern part”; and, to get there, he was required to go north-northeast along the island. Egg Island, and Conception to a somewhat lesser extent, also follow this coastline pattern. I gave 3's to each.

E9 By the way, this sentence is the first of two instances where Columbus refers to the duality of San Salvador. Columbus uses the word “part” often in the *Diario* to describe islands having eastern parts, northern parts, southern parts, and so on. But this is the only place in the Bahamas where Columbus describes an island as having an “other” part: other, implying separateness. And when Columbus arrives at this “other part”, how does he describe it? He describes it as “that island”. Not “the island”, not “this island”, but “that island”, again implying separateness. Columbus says that he was afraid to go ashore, because “that island” was completely surrounded by a reef; and East Plana is in fact completely surrounded by a reef, just as Columbus describes.

E10 Of course reefs are ubiquitous in the Bahamas, but the reef described by Columbus must entirely surround the island, which is a more difficult requirement. Most of the proposed islands fare pretty well, but the reef at Watlings clearly does not encircle the island. Further, the reef must be offshore, since the large harbor is between the reef and the island; this requirement foils the Egg Island theory, whose “reef” is an onshore coral barrier.

E11 One of the enduring controversies surrounding the identity of San Salvador is the correct translation of the word *laguna*. Columbus says that San Salvador has a very large *laguna* in the middle, which has been variously rendered as “lake” and “lagoon”. The modern *Diccionario* of the Royal Spanish Academy defines *laguna* as “A natural deposit of water, generally fresh and commonly of smaller dimension than a lake.” The Harper-Collins Spanish dictionary agrees, translating the term as “pool”. But these are modern usages; do they conform to 15th century usage? And, specifically, does the word *laguna* imply either fresh or salt water, and does it imply any kind of size?

E12 Other than the case at hand, there are eight times when *laguna* or *lagunas* is used in the *Diario*. In one case, Columbus uses the phrase *laguna de mar* (“pool of the sea”) to describe a saltwater feature, even though it is obvious from the context that the feature is part of the sea. It is therefore reasonable to suppose that if the *laguna* on San Salvador was part of the sea, or an arm of the sea, Columbus would have used the phrase *laguna de mar* to describe it. The other seven usages of *laguna* all take place at the northern end of Isabela, and it is clear that the *lagunas* there are fresh, since Columbus fills his water casks from them. Also, Columbus describes these *lagunas* as being “grande”, yet they are small enough to walk around and shallow enough to wade into. Therefore, Columbus appears to be using *laguna* in the same way as the modern dictionary: to describe a feature which is both fresh (or at least unconnected to the sea) and fairly small. The *laguna* on West Plana, at three-fourths of a mile long, fills both requirements perfectly. But the *Diario* also says that the *laguna* is in the middle of the island. The *laguna* on West Plana is not in the middle when viewed from above, but is in the middle of the coast when viewed from the vantage point of Columbus's anchorage. I have been generous in the scoring of this point to allow room for alternate interpretations.

G6 With these corrections, the log reads that from Cabo del Isleo, the northern point of Isabela, the coast runs southwest two leagues to Cabo Hermoso, and Cabo del Laguna lies nearby at the southwestern end of the island. This description matches Fortune Island, island IV of the Plana-Samana track.

H The Cycle of Distances

H1 One of the arguments against the Plana-Samana track is that Columbus would have had to return to a place which he had visited before (since Crooked lies quite close to Fortune); and since there is no explicit mention of Isabela (island IV) being close to Santa Maria (island II), the implication is that the two islands are far apart. This idea is countered by another beautiful internal consistency in the *Diario*, as shown in Figure 7. Since the distance from III to II is 8 or 9 leagues “almost east-west” (§C6), and the distance east along II is “ten or more” leagues, and the east-to-west distance from island II (Santa Maria) to island I (San Salvador) is 5 to 7 leagues, this means that the total E-W distance from I to III is 23+ to 26+ leagues. Since we previously determined from internal descriptions that the distance from III to IV is 17 leagues or less, we now have an important result:

Distance *z*, from Isabela (IV) to San Salvador (I), is 6+ to 9+ leagues E-W, or more.

H2 This result is critical because on November 20, while sailing north of Cuba, Columbus tells us the distance from Isabela (IV) to San Salvador (I): 8 leagues.

H3 Bingo again! We now have a beautiful and internally consistent cycle of distances among these four islands. To restate the case, the *Diario* gives us two different ways of figuring the total east-west distance from island I (San Salvador) to island III (Fernandina): one way via island II and another way via island IV. Both of these measurements are substantially consistent with each other. But this internal consistency of the *Diario* breaks down if we assume that any one of these distances is in miles instead of leagues; so the Conception theory, which measures coastlines in miles, founders. And this internal consistency of the *Diario* also breaks down if we assume that Columbus was using two different lengths for a league; so the Watlings theory, which measures in two different leagues, also founders. There are only two theories left: Plana and Samana.

H4 The actual distance from Fortune, the Plana-Samana island IV, is 11 to 13 leagues in each case (Plana or Samana), depending on the exact length taken for a league. For the Plana-Samana track we must assume that Columbus did not actually see the distance from San Salvador to Isabela, but instead calculated it, perhaps in a manner similar to the way I have done in Figure 7; and further, that his computation was somewhat in error. This explanation is not entirely satisfactory, and I have given it a 1 on the Scorecard. Nevertheless, this distance is much closer for Plana and Samana (an error of 3 to 5 leagues) than for any other theory save Caicos. At Watlings and Conception, the distance is a huge 25 to 30 leagues. Mitchell accounts for this distance by supposing that the entire southern half of Long Island somehow slipped from Columbus's mind as he wrote this entry³⁵ — an idea so strained that the theory could sink on that basis alone.

H5 In addition to this internal inconsistency, the Columbus-couldn't-have-returned argument has been demolished by a recent discovery. Alex Perez³⁶ has noticed a remarkable description in Las Casas' *Historia de las Indias*:³⁷ the relevant passage takes place just prior to Columbus leaving Fernandina — in search of Saometo, the native name for island IV.

³⁵ Mitchell 1992.

³⁶ Alejandro R. Perez, “Did Las Casas Have Columbus's Map?”, August 20, 1992. Letter to Molander's *Columbus Landfall Round Robin* (fn 3).

³⁷ The *Historia* was the major work of Las Casas, in preparation for which he copied and paraphrased the *Diario*. Since the relevant sections of the *Historia* are clearly drawn from the *Diario*, landfall-dispute historians have mostly ignored the *Historia*, rightly preferring the more original source. However, the *Diario* was not the only source that Las Casas drew upon in preparation of the *Historia*.

the southern part of island III, and the Egg Island theory must here put Cape Verde in the eastern part of the northern part of island III. In other words, one must assume the existence of three separate transcription errors in the same sentence in order for the theory to fit the *Diario*.

G The Inconsistencies of Isabela

G1 Twice our Plana theory rates a 0 on the Scorecard regarding Columbus's description of island IV, Isabela, called Saometo by the natives. When Columbus arrives at the northern tip of island IV, Cabo del Isleo, he notes that the coast from there runs west [which sounds *prima facie* unlikely if there *is* a north tip] for 12 leagues, to a cape called Cabo Hermoso. Here it proves impossible to take the *Diario* at face value. Consider the following: [a] The north point of island IV is on a west-to-east course from island III, or maybe (as suggested by the sailing directions from III to IV: §F11) rather south of east; [b] Columbus reached this point in less than 6 hours, sailing approximately east-southeast (*idem*), suggesting that the actual III-to-IV distance is most likely in the range of 10 to 12 leagues (which is indeed about the distance from Long to Fortune: Figure 8); [c] from the north point of Isabela, the coast runs west for 12 leagues. So it seems that the coast that runs west from Isabela must intersect or almost intersect Fernandina, yet it took 3 hours of sailing even to see the island.

G2 Clearly there is an error here, and there have been many attempts to explain it. Oliver Dunn and James E. Kelley have suggested³² that *gueste* (west) is a mistranslation of *sueste* (southeast), which supports the Watlings-Conception identification of Crooked Island as island IV.

G3 There is further reason to believe that the coast cannot actually run west from Cabo del Isleo to Cabo Hermoso. First, if the coast actually runs west, how is it that Columbus, on an easterly course, arrives at island IV at Cabo del Isleo without first coming to Cabo Hermoso? Second, if the coast runs west from Cabo del Isleo, how is it that when Columbus departs from Cabo del Isleo on the 24th, he sails WSW, and yet does not run aground on that coast? Yet Columbus says that Cabo Hermoso is in the western part of the island.

G4 This is one of the few times when the *Diario* shows an internal inconsistency, and the best way to resolve it is to suppose that the coast runs southwest, not west, from Cabo del Isleo to Cabo Hermoso. This solves the problems of the approach and the departure, while allowing Cabo Hermoso to remain in the western part of the island. The idea is reinforced by an interesting observation: on the evening of the 19th Columbus anchors for the night at Cabo Hermoso, yet on the following morning, the 20th, he weighs anchor from another cape, Cabo del Laguna. Dunn and Kelley have pointed out that the best explanation of this is the supposition that Cabo Hermoso and Cabo del Laguna lie very close to each other;³³ and Cabo del Laguna, Columbus tells us, is at the southwestern end of Isabela. We therefore require a transcription error in the *Diario*, not to make the *Diario* match the Plana theory, but instead to make the *Diario* match itself. The coast from Cabo del Isleo runs southwest, not west, to Cabo Hermoso.

G5 Plana also rates a 0 on the Scorecard in the distance from Cabo del Isleo to Cabo Hermoso, given by Columbus as 12 leagues. Here, for the first and only time in this paper, we require a numerical transcription error to make the *Diario* match the theory. Perez³⁴ suggests that *doze* (twelve) is a mistranscription for *dos* (two); this fits if you believe that Isabela is the modern Fortune Island, since the south coast of Crooked extends west from the north point of Fortune for about this distance. In other words, a *single letter* of the original log was miscopied or illegible to a transcriber.

³² Dunn & Kelley 1989, p.99.

³³ Dunn & Kelley 1989, p.103.

³⁴ Alejandro R. Perez, *Columbus's First Landfall in America and the Hidden Clues in his Journal*, 1987: ABBE Publishers, Washington, D.C., p.68.

E13 One of the most vexing clues for any landfall theory is the peninsula that Columbus describes as being a good place for a fort. No candidate island has such a peninsula at the present time. Proponents of Watlings, Samana, Egg, and Conception point to islands separated from their main island by shallows, which are supposed to have been sand-filled in 1492 and since washed away by storms. The Plana theory proposes the reverse process: the far end of the peninsula has become attached to the main island by sand deposition in the intervening centuries, forming an inland lagoon. There are currently two such lagoons at Plana which may have been the site of such deposition, one at the east end of East Plana. Each lagoon has two isthmuses which, if either were eliminated, would make the required peninsula. Core samplings could determine if any of the four isthmuses were a recent feature. Since the Plana theory is open to criticism on this point, I have given it a 0 on the scoresheet, pending definite geological evidence. It is interesting to note in this context that Judge's original peninsula candidate, on the southern shore of Samana, has been proven a recent feature of non-cemented sediments. It therefore seems possible that the feature seen by Columbus could have been entirely sand-based, and could have entirely washed away in the intervening time, without leaving any clue for modern scholars.

F Through the Islands

F1 After exploring San Salvador, Columbus returns to his ship, and there follows one of the most intriguing passages in the *Diario*. "I . . . set sail and saw so many islands that I did not know how to decide which one to go to first . . . Finally, I decided to go to the largest, and so I am doing. It is about five leagues distant from this island of San Salvador, and the others, some more and some less."²⁶

F2 First, it seems strange that Columbus did not know how to decide which way to go, since the previous day he had stated his intention to sail south or southwest to find the source of the natives' gold. And how far would Columbus have had to sail from San Salvador in order to see another island five leagues away? The answer is, no distance at all. Columbus could have seen this second island, at a distance of five leagues, anytime while at anchor during the previous two days (assuming an average height for a Bahamian island).

F3 These apparent inconsistencies are resolved when we realize that Columbus's ships possibly did not have ratlines. Sending a man into the masthead was therefore a difficult operation; the masthead was not a normal watch-standing position, and no one was sent up the mast without a specific reason such as searching for a landfall. It seems likely to me that there just wasn't anyone in the masthead between landfall on the morning of the 12th and setting sail again on the 14th. Setting sail provides a good opportunity to put a man in the mast, because a lookout can ride up on the yardarm as it is hoisted into place. When Columbus set sail that afternoon, he sent a man up — and while still only a few dozen meters offshore, he had a major shock. He discovered to his surprise that San Salvador was part of a huge and previously unimagined archipelago. Upon questioning his native guides, he found that there were more than a hundred islands in these waters.

F4 That is why Columbus did not know how to decide which direction to take. Eventually, he decided not upon the southernmost nor the southwesternmost island, nor on the island closest to the gold; he decided upon the **largest**. In other words, faced with this discovery, he temporarily put aside the idea of gold, and sailed instead to find new lands for his sovereign.

F5 Columbus's description of his dilemma on the 14th certainly is interesting and useful, but nowhere that day does Columbus tell us the direction in which his destination island lies. This turns out to be a minor point, however, because after arriving the next day, he neatly fills in the gap.

²⁶ Dunn & Kelley 1989, p.77.

F6 After arriving at island II on the 15th, Columbus says, "I found that the face which is in the direction of San Salvador runs north-south and that in it there were five leagues; and the other, which I followed, runs east-west."²⁷ If the north-south coast faces San Salvador, and the east-west coast by implication does not face San Salvador, it is clear that San Salvador can lie in only one direction: due east of that five-league-long north-south coast. Only Plana and Grand Turk receive full marks on this point. Ramon Didiez Burgos, the first to propose Plana as San Salvador, has Columbus's anchorage on the night of the 15th on the north shore of Crooked.²⁸ I prefer Judge's route around Bird Rock to Landrail Point (see Figure 3), in view of the wind shift to the southeast which threatened the anchorage.

F7 Columbus sails from island II, which he names Santa Maria, to an even larger island III that he calls Fernandina. He estimates the distance between the two islands at nine leagues at first, then revises it to eight leagues "almost east-west". He says on the 16th that this coastline "may" be more than 28 leagues, but on the next day, after actually exploring it, he says that he saw only 20 leagues, and "it did not end there". The east coast of Long is 22 leagues, using James E. Kelley's 2.67 nmi league. The Grand Turk theory requires us to believe that Columbus sailed from Mayaguana to Acklins, across some 40 miles of open ocean, and **mistook it for a coastline**.²⁹ This is the kind of explanation that rates a 0 on the Scorecard.

F8 One of the objections to the Plana-Samana route is that Columbus says that island III is visible to him while he is still at island II. This seems to be in contradiction to his later assertions that the distance between the two islands is 8 or 9 leagues. However, this is not necessarily the case. If Columbus anchored along Landrail Point on the night of the 15th, he would have had to sail around the western end of Crooked Island. Since the caravels were faster ships than the Santa Maria, it is possible that one of them went far enough into the Crooked Island Passage to make out the island. Or, Columbus might have learned of the island from the Indians: at one point, he says he has an "indication" (*amuera*) of the island's presence.

F9 Didiez Burgos shows Columbus circumnavigating Long Island, which seems particularly unconvincing to me; again, Judge's track along the east coast of Long is far superior. Columbus describes a harbor 2 leagues from the end of island III. The harbor has two narrow entrances separated by an *isleo*, or small island: a description that fits Little Harbor (Figure 2) passably well, although Little Harbor has two small islands in the entrance. Fox identified Burrows Harbor, slightly farther south, as this harbor. Fox's harbor has two mouths, but it is only about half the correct distance from the end of the island. In my mind, either of these harbors is a better fit than the Newton Cay harbor at the northern tip of Long, which is the candidate of the Watlings and Conception theories; this harbor is far too close to the northern end of the island. The Egg Island theory has this harbor at Conch Sound on Andros, which has only one wide opening. Caicos-advocates use Lovely Bay on Acklins, which has many wide openings instead of two narrow ones; and the Grand Turk theory uses Abraham's Bay on Mayaguana, which has two openings but no *isleo*. Columbus sails NW from this harbor "as far as the coast that runs E-W", which is another problem for the Watlings-Conception theories, since there is no such E-W coast, nor any coast at all, north of Newton Cay.

²⁷ Dunn & Kelley 1989, p.77.

²⁸ Ramon J. Didiez Burgos, *Guanahani y Mayaguain*, 1974: Editoria Cultural Dominicana, Santo Domingo, p.171.

²⁹ Robert H. Power, "The Discovery of Columbus's Island Passage to Cuba", *Terrae Incognitae*, 15 (1983), pp.165-167.

F10 On the evening of the 17th, the Fleet runs into foul weather. Columbus heads for the southern cape of the island to anchor; as the wind was northwest, this cape would have afforded shelter. He spends the 18th continuing around the island when the weather allows; he anchors when it does not, mentioning that "I did not go ashore". In moderate winds, Columbus would have reached South End on Long Island about midday, and spent the afternoon (or part of it) beating northward a few miles along the west coast of the island. This is slow, wind-in-your-face sailing, and Columbus had only a few daylight hours before he was forced to give up and return to the only available anchorage, near South End. The Egg Island theory requires us to believe that Columbus spent this entire day out of sight of land, sailing at top speed through the treacherous shoals between Andros and Long, anchoring in the Jumentos Cays. In a similar fashion, the Caicos theory also requires Columbus to sail away from Fernandina on the 18th, in direct contradiction of the *Diario*. The reason: there are only 6 hours of sailing from island III, Fernandina, to island IV, Isabela, and these theories require extra time to make the distance. No mere transcription error can save these theories here; one must instead assume that Columbus just did not know what he was talking about when he wrote the entry of the 18th. This is again the kind of discrepancy that rates a 0 on the Scorecard.

F11 Columbus sets sail the next morning, spreading the fleet from ESE to SSE; after three hours, he makes his landfall at Isabela, island IV. Nowhere does Columbus tell us the distance between Isabela (IV) and Fernandina (III). But let us make some reasonable assumptions: the maximum speed in the transatlantic crossing was 3 leagues per hour, thus, Columbus could have been no more than 9 leagues from island III when he made his Isabela (IV) landfall. Next, if we take the shortest reasonable length for the league (2.67 nautical miles) and the highest reasonable height for a Bahamian island (50 meters), then — using 15 m as the height of the ship's observer — we conclude³⁰ that island IV could have been as much as 8.6 leagues distant at landfall. So the maximum distance from island III to island IV is 17.6 leagues, although it could well be less than this.

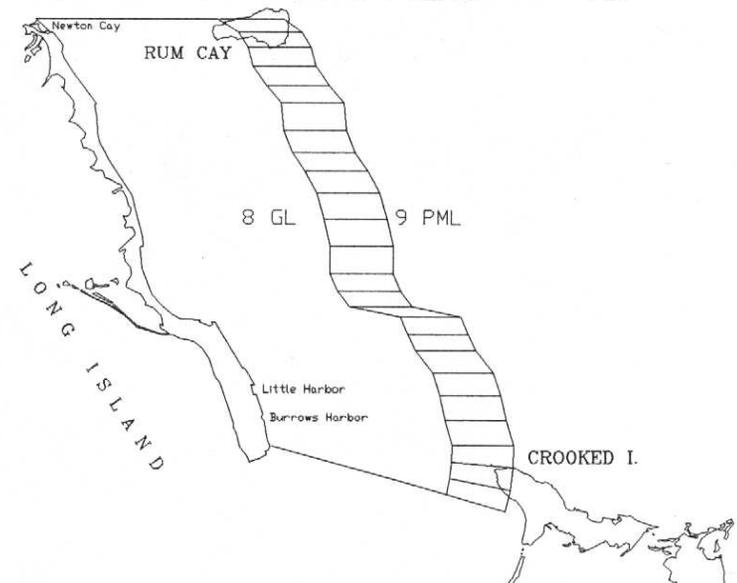
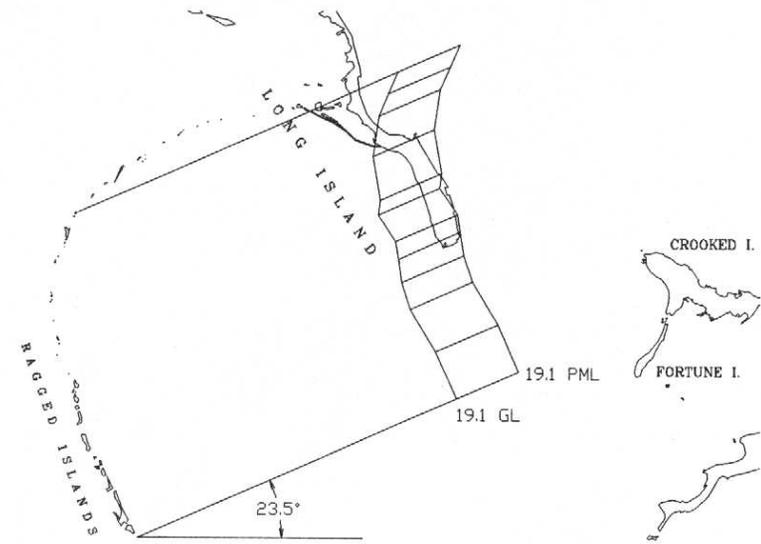
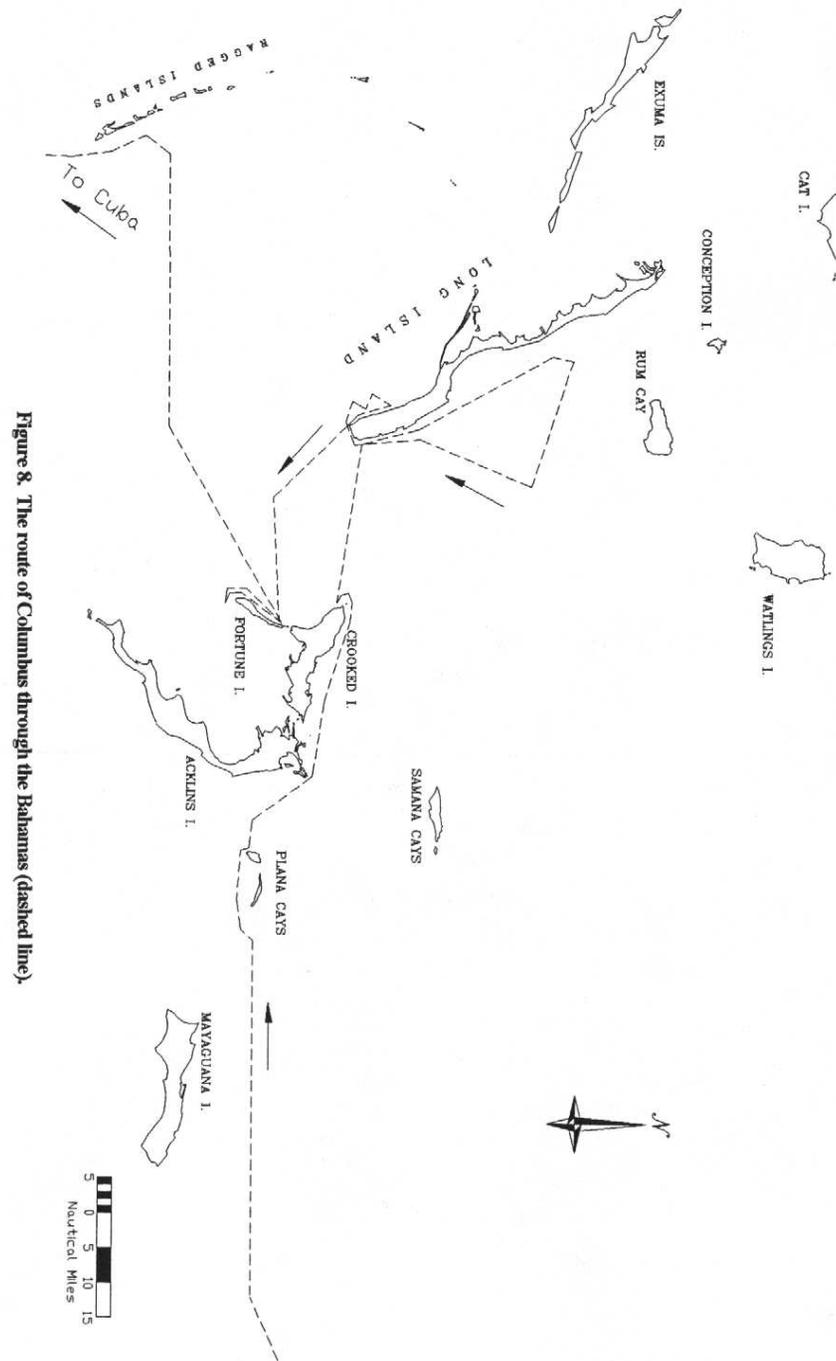
F12 Now, Columbus mentions that the northern end of Isabela (island IV) is on an E-W line from Fernandina (island III); there is reason to doubt that this is precisely true, but let us assume for the moment that it is. When Columbus departs Isabela (IV) on the 24th, he sails WSW from its northern point, for an unspecified distance, until he sees Cape Verde, the southern point of Fernandina (III), lying 7 leagues to the NW (see Figure 6). All right now, trigonometry³¹ fans: if the angle *A* in Figure 6 is 22.5 degrees, and angle *B* is 45 degrees, and side *Z* is 7 leagues, what is the length of side *X*? The answer: 16.9 leagues.

F13 Striking. We have now calculated the distance from island III to island IV in two completely different ways (§F11 & §F12), and have arrived at answers that agree within less than a league: 16.9 leagues & 17.6 leagues. I must again (as at §F11) point out that this distance could be less than the mean result, 17 leagues (indeed, by our Plana track, it is c.10 leagues, Long-to-Fortune: §G1 [b] or Figure 8), since Columbus sailed from III towards IV on a SE & then E course, for under 6 hours, and likely he was making less than 3 leagues per hour. But it could not be greater than 17 leagues, since this would require Columbus to have sailed NE from III, in contradiction to the *Diario*, and at speeds greater than 3 leagues per hour, which is most unlikely.

F14 It is this beautiful internal consistency of the *Diario* that does in the Caicos theory, the Grand Turk theory, and the Egg Island theory. Each of these theories requires far more distance from island III to island IV than the *Diario* allows. The Egg Island theory assumes that the 7 leagues to Cape Verde is a transcription error for 70 leagues. And indeed, if one assumes that Columbus was estimating the direction from a 16-point compass instead of a 32-point, a portion of northeastern Andros just does meet this criterion. However, this does not help much, since Columbus also says that Cape Verde is in the western part of

³⁰ Computation: $2.1[50^{1/2} + 15^{1/2}]/2.67 = 23$ nmi, or 8.6 leagues.

³¹ Whether computed by plane or spherical trigonometry, the results (for such a small spherical triangle) naturally agree almost perfectly.



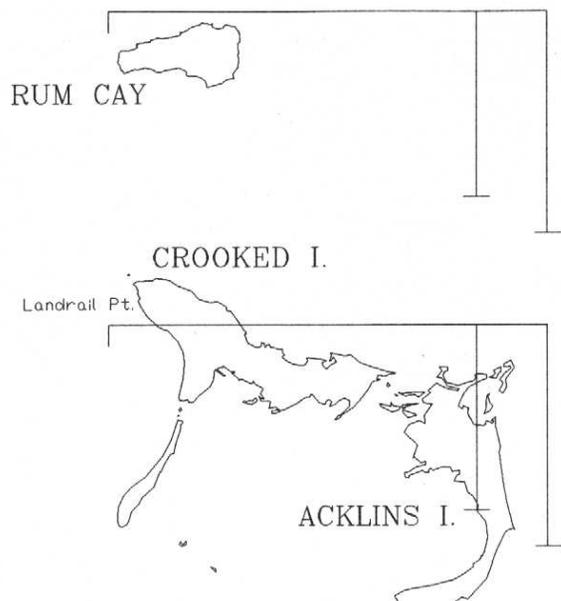


Figure 3. Island II candidates Rum Cay and the Crooked-Acklins group at the same scale. The F-shaped device indicates the size of ten league and five league coasts, using Geometric Leagues and Portuguese Maritime Leagues. See §C6.

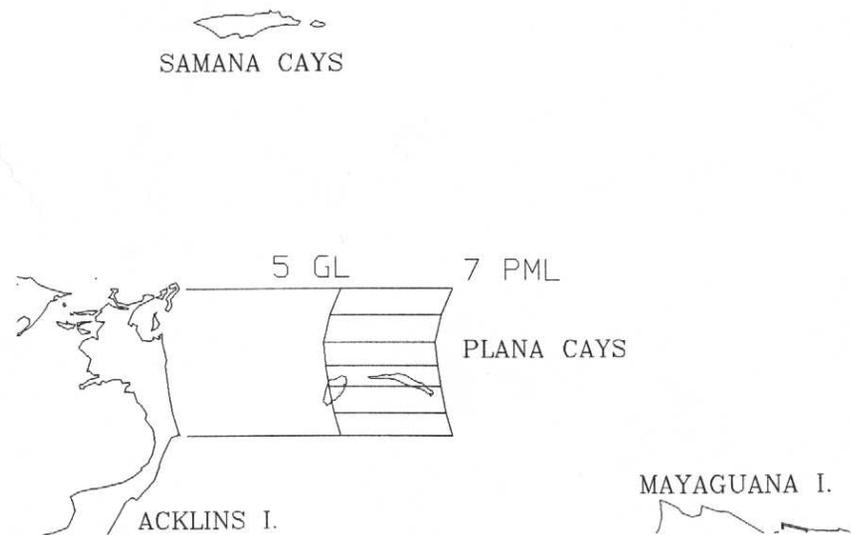


Figure 4. The plot box shows all points between five and seven leagues east of the north-south running coast of Acklins (island II). Island I should lie in or near the plot box. See §C9.

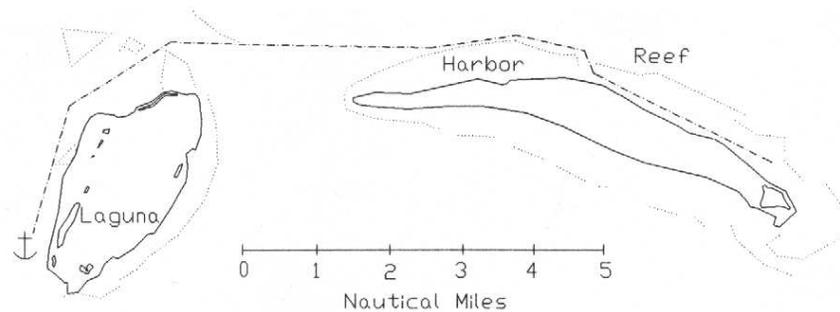


Figure 5. The Plana Cays (island I), showing one possible route for the boat exploration of October 14. See §E4.

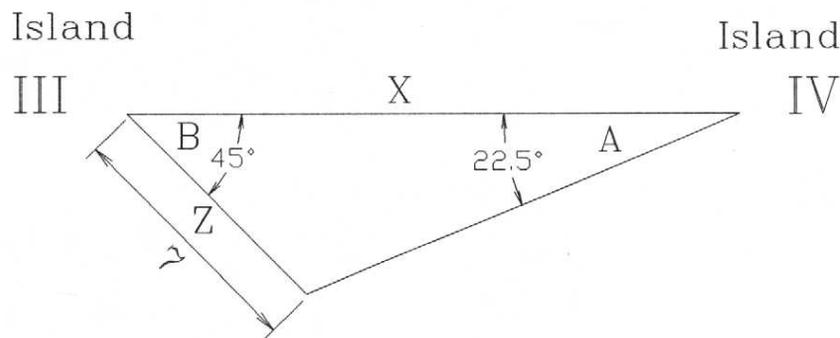


Figure 6. Columbus left island IV on a WSW course, reaching a point seven leagues SE of Cape Verde on island III. If island IV is directly east of Cape Verde, distance X must be 17 leagues. See §F12.

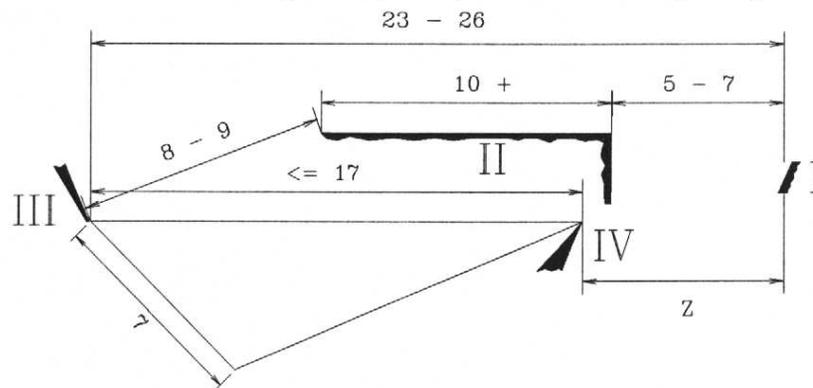


Figure 7. The relative positions of the four islands, based solely on the descriptions in Columbus's log. The distance from island I to island III can be measured both via island II and via island IV. See §H1.