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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.”
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### News Notes:

DIO had quite a 1996 . . .

[a] On 1996/S9, our skepticism of the Byrd 1926/5/9 North Pole claim (DIO 4.3 §11 & §12) made the front pages of the world’s newspapers, starting with the New York Times p.1 story (citing DIO) expertly written by John Wilford. This triggered general recognition (on all three national television networks’ evening news) of the North Pole 1926 priority of the Amundsen-Ellsworth-Nobile expedition. (Amundsen & 1926 companion Wisting were 1st to the S.Pole as well: 1911.) Considering some of our past comments on the NTimes, DIO feels humbly and admiringly obliged to take explicit note of the fact that the Science Dep’t of the NTimes took the world lead in correcting the paper’s own 1926/5/10 headline.

[b] Keith Pickering has given invaluable editorial and technical assistance in maintaining the quality of DIO. So we are especially delighted to learn that his DIO 4.1 §2 analysis of Columbus’ landfall (Keith’s first academic publication) has led on to his next article’s appearance in the journal of England’s Royal Institute of Navigation.

[c] Hugh Thurston, another valued member of the staunch band of DIO Untouchables, recently received the welcome news that his Springer-Verlag 1994 book, Early Astronomy, has been such a success both with reviewers (e.g., Royal Society’s D.King-Hele in Nature) and the public, that it is going into paperback.

[d] Our shocking discovery (DIO 1.1 §6), that the yearlength appearing on the Babylonian tablet BM 55555 (Astronomical Cuneiform Text #210, c.100 BC) was computed from famous Greek solstice observations, is now so widely accepted2 (the Neugebauer-Muffia2 being increasingly-isolated, lockstep-lockjaw holdouts) that it has caused BM 55555 to be exhibited at the British Museum explicitly on this basis.

[e] Annals of Science 33.4-823 (1996 July) congratulatorily praised3 the “excellent” and “thorough” achievement of DIO’s pioneering edition of Tycho’s Star Catalog.

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1 DIO 6 §1 fn 137.
2 Whose Princetitude-backed math-follies shine at §5 §B, DIO 4.1 §4, & DIO 6 §1 fn 1-10.
3 But the larger of the review’s 2 paragraphs was mostly odd sniping: [i] Calling our use of 9 pt type for main text “appalling”. (Guess what typesize the review was printed in?!) [ii] Pseudo-lordly horror at Catalog comments on DR’s “obviously unhappy relationship with the established world of science history”, (Too bad Muffiosi are unhappy; perhaps they will someday knowjoy, by sharing DR’s jocular view of their pretensions: e.g., Hist.sci’s Catalog’s offensive criticisms evaluated. Sense of proportion: DIO’s Catalog took a total of about one cheery page (DIO 3 §83 & fn 97) to counter a several-ways goofy-fallacious (and unavoidably-relevant) sixty-four-page 1987 J.Hist.Astr. attack on DR. (Where was DR supposed to respond?) He’s banned from the very JHA that issued this brave megas assault. And the dithering Hist.sci. Soc. is deaf&dumb to suggestions that matters must be refereed: DIO 4.2 §7 §B29 & B44.) For further math-funmies in JHA’s 1987 disaster, see DIO 1.1 §6 fn 15, DIO 1.3 fn 288, DIO 2.1 §4 §H7, & DIO 2.3 §8 §C13. (Partial catalog of dozens more tenet-getting Muffia muffs: DIO 4.1 §4 §A. With the lone [and valued] exception of A.Jones [JHA 1995 May], none of those scholars has retracted. All simply hide.) I.e., Hist.sci. archons see purple at the original Mighty response, while maintaining silence on the offensive’s technical blunders. (DIO 1.2 §7: in Hist.sci, “double norms are the single norm.”) But, have some pity: our poor rich Hist.sci. archons labor in the depressing shadow of the more tenet-gutting Mufa muffs: DIO 4.1 §4 §A. With the lone [and valued] exception of A.Jones [JHA 1995 May], none of those scholars has retracted. All simply hide.) I.e., Hist.sci. archons see purple at DIO’s Mitey response, while maintaining total silence on the original Mighty JHA-offensive’s technical blunders. (DIO 1.2 §7: in Hist.sci, “double norms are the single norm.”) But, have some pity: our poor rich Hist.sci. archons labor in the depressing shadow of the more tenet-gutting Mufa muffs: DIO 4.1 §4 §A. With the lone [and valued] exception of A.Jones [JHA 1995 May], none of those scholars has retracted. All simply hide.) I.e., Hist.sci. archons see purple at DIO’s Mitey response, while maintaining total silence on the original Mighty JHA-offensive’s technical blunders. (DIO 1.2 §7: in Hist.sci, “double norms are the single norm.”)

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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, the usual handsome-journal anonymity will not, unless in reverse. No page charges. Each author receives 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-junk level junk. I.e., non-Muffia cranks need not apply.)

Other journals may reprint excerpts (edited or no) from any issue of DIO to date, whether for enlightenment or criticism or both. Indeed, excepting 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 & 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. (And we urge informants to snitch on lurking typos: they get rubbed out at reprinting-time.) Written contributions are especially encouraged for the columns: Unpublished Letters, References Referenced, and regular Correspondence. Contributor-anonymity granted on request. Defly or daily crafted reports, on appropriate candidates for recognition in J.HA’s pages, will of course also be considered for publication.

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.

Contributors should send (expendable photocopies of) papers to one of the following:

Robert Headland [polar research & exploration], Scott Polar Research Institute, University of Cambridge, Lensfield Road, Cambridge, England CB2 1ER.

Charles Kowal [celestial discovery, asteroids], Johns Hopkins University Applied Physics Laboratory, Johns Hopkins Road, Laurel, MD 20707.

Keith Pickering [navigation, exploration, computers, photography, science ethics], Analysts International Corp, 8200 Normandale Blvd, Suite 400, Minneapolis MN 55437.

E. Myles Standish [positional & dynamical astronomy], Jet Propulsion Laboratory 301-150, Cal Tech, 4800 Oak Grove Drive, Pasadena, CA 91109-8099.


Christopher B. F. Walker [Mesopotamian astronomy], Dep’t of Western Asiatic Antiquities, British Museum, Great Russell Street, London WC1B 3DG, UK.


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Science-History’s Dark Ages Get Darker

The Passing of B. L. van der Waerden

A Mathematician’s Appreciation

by Hugh Thurston

Bartel L. van der Waerden was best known to the world as a mathematician, though perhaps he was better known to readers of DIO as a historian of astronomy.

His mathematical fame rests not so much on his teaching and research as on his textbooks, and one textbook in particular: Moderne Algebre, which appeared at the end of the second world war. It was clearly in advance of any other algebra text, and students of algebra were known to learn German solely in order to use it. It was soon translated in English as Modern Algebra, and became so influential that university syllabuses were changed to cover precisely the material in the book, whereupon the book changed its title to simply Algebra.

Already by his late thirties, van der Waerden was publishing pioneering papers on the early history of astronomy, a highly specialized field to which he brought considerable mathematical skill and a gift for clear explanation. He acquired a broad and deep knowledge of Greek, Babylonian and Indian astronomy. He was noted for his modesty and his openness.

While van der Waerden was in Baltimore, he took out Sternkunde und Sterndienst in Babel Part 2 (F. X. Kugler, S.J., 1914) from Gilman Hall, where the Johns Hopkins Library then was. (This was less than a mile from where I was going to elementary school at the time. When I worked at the library in the summer of 1953, it was still in the same building.) The signature and the date (1947/12/30) on the library card (which still resides in the volume) are in a gentleman’s fountain pen, like his letters. His is the only name on the card. I may have been the very next person to take it out. Four decades after. (When library cards were no longer used.) As a pure scholar (not a politician), he did not even know who the then-President of JHU was (1988/12/20 letter to DR) — but his knowledge of math & history was wonderful into his 80s, as all those who benefited from his generous advice will recall with gratitude. Dedicated readers of DIO are well aware that the very name of this journal is partly due to him (DIO 1.1 §1 §2) — which is apt, since DIO aims at encouraging unbiased fairness and original adventurousness, qualities he unaffectedly exemplified. The passing of such a good person is all the greater a loss to our hopes of balanced & capable modern reconstruction of ancient astronomical history, given the heightened current state of that field — locked (without the slightest hope of internal reform) in the grasping grasp of the History-of-science church. One is reminded of a better-known Dark Ages — and van der Waerden’s heartfelt lament at its 6th century AD onset, as Greek mathematics “dies like a snuffed candle”. This, from the last page of his famous Science Awakening Part I (1963 ed. p.291), which has been in my library since I bought it in Harvard Square, 1/3 of a century ago.

The paper van der Waerden here cites (his favorite among DR’s MSS, the preface to which he read aloud before our wives during our last visit with him: 1987/5/20) is the same paper, “Ancient Planet Tables’ Long-Cycle Ancestries”, which, were it not for DIO’s existence, could (see DIO 4.3 §15 §1) have been submerged for another 2000 yrs. [Now finally published in 2002: DIO 11.2 §4 (revised&retitled in 2003).]

Fabricated Transit Data by Abram Robertson

by E. Myles Standish

Astronomy

Jet Propulsion Laboratory

California Institute of Technology

Robertson’s Data

A1 Pages from the 1811-1812 observing books of Abram Robertson, then director of Oxford University’s Radcliffe Observatory, were copied and sent to me by C. A. Murray (1991) of the Royal Greenwich Observatory with permission from the Royal Astronomical Society’s Manuscripts Division. The observations are transit-times recorded with a five-wire telesopic instrument at Oxford, where Robertson’s predecessor, Rev. Thos. Hornsby, had previously established an excellent observing record (including what is now our best set of early post-discovery observations of Uranus).

A2 All the data from two consecutive pages (covering about three weeks) of Robertson’s 1811 January transit-times are listed here in the left-hand section of Table 1 (next two pages), transcribed exactly as they appear in the notebook, with superscripts added for clarity. (A few obvious, harmless scribal errors are put in italics.) Each row represents the data (ideally for all 5 wires) of a transiting object. The timeminutes and timeseconds are generally listed for all wires; the hours are listed for the middle wire only. Sometimes the timings are listed to an integral number of seconds of time (“xx”), sometimes they are listed to a half second of time (“xx.5”) or a tenth second of time (“xx.x”); and occasionally the second wire is listed to one-quarter or three-quarters of a second of time (“xx.25” or “xx.75”).

Astronomers have used transit circles at least since Timocharis (Alexandria, early 3rd century BC), to take advantage of the fact that spherical astronomy becomes simple arithmetic on the meridian. The objects Robertson observed in transit were mostly stars (predominantly bright ones), but also included solar & lunar limbs, and planet-centers.
July. But Ellenberger's behavior & tactics are no different now than they were between 1977 & 1983 when he learned the rules of engagement under L. Greenberg's tutelage and was golden. Only the objects of his fulmination have changed: instead of V's critics and alternate catastrophists,33 his targets are his former colleagues. Now he is a pariah, exposing the hypocrisy manifested by those who fail to follow the same standards of scholarship to which they hold their critics accountable34 and who obdurately give Velikovsky's hypotheses & intuition priority over physical evidence & the laws of nature. Witness, e.g., Velikovskians' hard-core reactions to ice-core evidence against Worlds in Collision.35

[3] In an effort to “de-program” Velikovsky cultists, Ellenberger distributes informative memorandums & postcards, intended to alter mind-sets — efforts that are not appreciated by old guard opinion-formers who dominate Velikovsky publications & conferences.

C13 Epilogue by DR. After the Portland conference (1994), Sherrimer invited a 2-stage exchange,36 to begin a widening-down37 of Skeptic’s involvement in the Velikovsky debate, on which DIO also publish nothing further.38 We are, significantly, giving Ellenberger our last word on the matter. Primary reason: counter-balancing the Portland ban. (NB: the entire public success of the Velikovsky movement has been based upon its being seen as a victim of censorship. So: where does its own 1994 Portland behavior leave its credibility?)

But we share with Skeptic an implicit awareness: the Velikovsky debate — such as it was39 — is long since over,40 even in half-serious scholarly circles. And the skeptics have won.

Greenberg protested to NL that Davis had “departed the world of reality never to return.”


38 Those who wish to hear the Velikovsky side of these issues (and—or learn of errors and omissions that may have occurred here from human fallibility) are encouraged to consult the several pro-Velikovsky publications cited above — or to contact the cultists themselves at 800-230-9347. Addresses: Kronia Communications, PO Box 5215, Arola, OR 97006. Aeon, 601 Hayward, Ames, IA 50014. Chas. Ginethal (718-897-2403), c/o 65-35 108th St, Suite D15, Forest Hills, NY 11375.

39 Post-carnage-mop-up historians who wish to enjoy Ellenberger-as-Letterman may contact him for his handy one-page “Top Ten [or is it Eleven?] Reasons Why Velikovsky Is Wrong About Worlds in Collision” — plus a fuller version of the present article, including physical evaluations of V’s theories (discussions which are mostly outside the realm of DIO).

40 See §4 fn 21. DIO 4.3 114 asks an oft-overlooked question: “if even the most logically & evidently one-sided controversies are . . . indeﬁnitely irrepressible, then — why investigate anything?”
C10 By the time Ellenberger was informed he would be *poisona non grata*, after leaving (1994/11/20) a message on superVelikovsky29 Dave Talbott’s answering machine, he already had a non-refundable airline ticket; so he went to the conference, socialized in the hotel’s public areas discreetly selling various “Velikovsky’s right!” paraphernalia,30 delivered pre-prints of an invited paper by Slabinski31 that was not published before the meeting (as promised), and sold anonymously both his Macmillan first printing of *Worlds in Collision* (with dust jacket) and other collectible Velikovsky publications at the conference book table. Rose chastized former *Kronos* staffers seeing fraternizing with Ellenberger. Talbott made clear Skeptic could have any reporter in the world, except Ellenberger, when editor/publisher Michael Sherman tried to get Ellen Myles Standish. Talbott was so intent on barring Ellenberger that he made the ludicrous threat in a telephone conversation to have Ellenberger ejected from the hotel if he tried to claim his reservation!

C11 While the ostensible reason for barring Ellenberger was his alleged potential disruptive ness, a more likely contributing cause was the animosity between true believers and a turncoat. The aftermath of the 1994 Nov meeting was hashed out on talk.origins in early December. Copies of the major postludes * plus “The Annotated Rose” (see fn 21), “Dénouement” (see fn 24), and “Magnetism, Dynamos, & Neptune” (see fn 35), are available from either of the addresses at the head of this letter. (Or telephone: 314-773-0329.)

C12 Esprit d’Escalier. Some background to Ellenberger’s rôle in the Velikovsky cult:  
[1] In late 1978, when Marcello Truzzi (E. Michigan University sociologist) was organizing the “Dialogue on Velikovsky” for his admirably open journal, *Zetetic Scholar*, Ellenberger accepted the invitation to participate, despite being aware that *Kronos* staff were boycotting it. When the “Dialogue” appeared in mid-1979, Ellenberger was on the *Kronos* staff and felt strongly that a rebuttal to the critics was in order despite Senior Editor Rose’s desire that *Kronos* continue to ignore Truzzi’s project. Disobeying Rose’s injunction, Ellenberger submitted a rebuttal. When it appeared, it merited Rose’s compliments. Ellenberger’s services to *Kronos* led to his being rapidly promoted and named “Sr. Ed. & Exec. Sec.’,” the only dual-titled staffer, in mid-1981. Later that year he was awarded the Macmillan first printing mentioned above (C10). This perk, and more, in spite of many initiatives and memos to staff that conflicted with the Editor-in-Chief’s need for control. Ellenberger resigned from *Kronos* in 1986 December and terminated his duties as back-issue order-filler in 1987 November, a holdover task he had continued at the request of the Editor-in-Chief.

[2] Now, as a Velikovsky disbeliever, Ellenberger has been transformed from hero to exile. In private, his former colleagues consider him “a barbarian unfit to be in polite academic society,”22 as ex-Ed.-in-Chief L. Greenberg wrote a mutual correspondent in 1991

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29. Talbott organized Kronia Communications in 1987 and was publisher of *Pensée*, 1972-1974, author of *The Saturn Myth* (N.Y.City 1980), and founding editor of *Aeon*, 1987-1991. His *idee fixe*, following a hint by Velikovsky, is that, during the “Golden Age” ruled by Kronos-Saturn, a seasonless Earth orbited the Sun in close proximity to Saturn, which loomed immobile over the N. Pole: rich entertainment for those who give priority to lethal falsification vs. ambiguous confirmation, and to the laws of physics vs. interpretations of mythic imagery, whose meaning is arguable to say the least.

30. Velikovsky gave Ellenberger permission to market “Velikovsky’s right!” t-shirts in 1979 June.


32. The feeling is mutual. When Greenberg published an ad hominem reaction (*Aeon* 3:2.82-88) to Ellenberger’s memoir in *Aeon* 3.1 (see fn 24), part of Ellenberger’s response was a 1993/6/15 postcard, whose closing read: “With a mongoose’s respect for a cobra.” (D. J. Smith [1958], polite unbarbarian Greenberg kissed off Ellenberger as “a disaffected zealot who long ago drifted beyond the pale of rational objection.” Further with respect to “polite academic society”, when R. Davis, Emer. Prof. of English, Columb Univ, in *The New Leader* (1977), penned V’s *Peoples of the Sea*, mirrorless
B A Startling Symmetry

B1 For the initial analysis, I wondered if it would be sufficient to use only the reading from the central wire, since, after all, the average of the times for the 2nd and 4th wires should be approximately equal to the middle wire time, and similarly for the 1st and 5th wires. Upon testing this hypothesis, I found that, Lo and Behold: these averages were not approximately equal; instead, for a great majority of the stars, they were exactly equal!

B2 The right-hand section of Table 1 shows (in successive columns): the 1st & 5th wires' sum ("1+5"), the 2nd & 4th wires' sum ("2+4"), & the center wire doubled ("2 Mid"). All sums are expressed modulo 60 and are printed to the same precision with which the individual wires were recorded by Robertson. An asterisk indicates those few cases where the three sums are not all exactly equal. Note also that in every case where the 2nd wire is listed as "xx.25" or "xx.75", the 4th wire is also listed as "xx.25" or "xx.75".

C Explaining the Mystery: Fabrication Established

C1 Can it possibly be that Robertson’s observations are accurate below the level of 0.1 timeseconds? The answer is no. I have modelled Robertson’s observations using a more detailed analysis which determined corrections to each individual star position and to each night's clock readings. After taking account of all of these factors, I found that the mean error of a single observation was more than a half a timesec. Such inaccuracy is not remarkable for observations of that era, though it should be noted that the mean error of Lalande’s earlier star-transit data (published in 1801) is less than half Robertson’s.

C2 Is there another explanation for the artificial symmetry of Robertson’s data? Occasionally, astronomers produce predictions of observations: using present knowledge, they predict the future result of some observation. (Galileo did this with the satellites of Jupiter in order to demonstrate the ability to predict the satellites’ positions in advance.) However, in Robertson’s notebook there appear notes in the right-hand margin: “High wind” and “small flying clouds during the time of these observations”; also, instances are noted where it is written, “After this observation I put the Clock forward 2’”. Clearly, this was not a prediction notebook.

C3 Is there any other possible explanation for the remarkable agreement? Yes, sadly, there is. The observations were not honestly recorded; they were fabricated. For what reason, I don’t know.

C6 Actually, Rose and Ellenberger broke off relations in 1983 when Rose refused to concede that the omissions and self-serving misinformation in Velikovsky’s Stargazers and Gravediggers were material and important. In 1990, Rose refused an invitation to debate Ellenberger on the Greenland ice cores as a crucial test of Worlds in Collision (see fn 35) at Milton Zysman’s “Reconsidering Velikovsky” Conference in Toronto. Ellenberger’s antagonism with Ginenthal began in 1984 when the latter’s letters to Kronos were sent to the former for reply and Ginenthal tenaciously resisted any scientific explanation that contradicted pro-Velikovsky dogma.

C7 At Haliburton, Rose was upset by Ellenberger’s stream of pregnant questions from the audience, following Rose’s rebuff of a simple request for a clarification while remarking sotto voce: “I do not take questions from that source.” Irving Wolfe, Prof. of English (Univ. of Montreal) and arch-relativist, as attendee, tried to quell Ellenberger’s interrogatories, contrary to the meeting’s established format which encouraged audience participation. This was in distinct contrast to Wolfe’s posture as moderator in 1992 when he allowed Ginenthal to lead two audience rebellions — i.e., bullying by outnumbering — against keynote speaker Ellenberger’s explanation of the bearing of the Pioneer and Magellan missions’ results upon Velikovsky’s claim that Venus is young.

C8 Thus, Ellenberger, who had never truly disrupted a meeting, was banned from Portland, while Ginenthal, who actually had previously (C7) been a disruptive influence, was on the program — demanding Ellenberger be barred. Ginenthal had boycotted the 1994 Haliburton meeting as a protest against Ellenberger’s attending.

C9 Since the organizers for Portland did not believe Ellenberger would actually attend, they did not take seriously his several expressions of intent that ended posts on talk.origins in August, September, and October and, therefore, did not communicate the seriousness of their concern for barring him. Not even when he volunteered to replace astrophysicist Victor Slabinski, who had declined his invitation in late September. This concern, which had been apparent since June when Carlinsky talked with Ellenberger, was subject to jokes at Haliburton. Under those circumstances, by late October, Ellenberger had arranged to attend as a reporter for Skeptic magazine, *SkInq* not having been interested.

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25 So perturbed was Rose that he blurted out a reference to such world-class scholars as P. Huber and R. Parker collectively as “the jerks”. (But, in fairness, one notes that Rose is not in the least perturbed at ending up simultaneously maintaining two contrary historical chronologies!)


27 Previously, in 1993 June, Ellenberger had been barred as a contributor to *Aeon*, as a condition of L. Greenberg joining the staff, at the same time he was told the publication of his memoir had been cancelled. The cancellation was a surprise since the last word from *Aeon* concerned the need to polish up the section “Legacies”. Ironically, D. Patten, author of several fundamentalist books invoking interplanetary collisions *à la* Velikovsky, was also on the program at Portland, despite having been barred from *Aeon* in 1991.

28 As, too, C. Whelton, who joined Rose and Ginenthal in the Portland ban, but at the last minute could not attend. Velikovsky’s daughter Ruth V. Sharon had also conditioned her promised attendance upon barring Ellenberger, but she was a no-show, too.
C Hysterical Velikovskians Flee Own Frankenstein-Mongoose!

To: DIO
From: Ellenberger, 3929A Utah Street, St. Louis, MO 63116 c.leroy@rocketmail.com

C1 It may merit a very sweet ironic smile that turncoat and apostate Leroy Ellenberger, until 1983 one of Velikovsky's most active defenders,19 and since considered by some his "most unrelenting critic",20 was barred from a Velikovsky-retrospective meeting in Portland, OR, 1994 November 25-27, co-sponsored by Kronia Communications and the equally Velikovskian organ Aeon. The meeting, "Velikovsky, Ancient Myth, & Modern Science", was actively promoted on USEnet's talk.origins newsgroup as open to the public. The ban of Ellenberger was stipulated by at least two speakers, Charles Ginenthal21 and Prof. Lynn Rose (Philosophy, SUNY Buffalo). Rose is — rather ironically in the present context — author of "The Censorship of Velikovsky's Interdisciplinary Synthesis".3

C2 The organizers established a veneer of intellectual respectability by including on the program, as "call girls",22 Dr. Victor Clube (Physics, Oxford), co-author of The Cosmic Winter (see fn 33); Dr. Henry Bauer (Science Studies, Virginia Polytechnic Inst & State Univ), author of the highly-acclaimed (though not by Velikovsky partisans) Beyond Velikovsky (Urbana 1984); and astronomer Dr. Tom Van Flandern (author of numerous able professional papers in mathematical & observational astronomy, and now publisher of the Meta Research Bulletin)23 — all of whom are friends of Ellenberger — as well as several academic Velikovskian camp-followers, including sociologist Gunnar Heinsohn (Univ. of Bremen), classicist William Mullen (Bard College), and anthropologist Roger Wescott (prof. emeritus, Drew University).

C3 Van Flandern was so upset upon learning of the ban on November 21 that he initiated a conference call with the organizers and the censoring speakers; but they would not relent. The organizers decided that letting Ellenberger audit the sessions by an audio feed to his hotel room violated the spirit, if not the letter, of the ban.

C4 The ostensible reason for barring Ellenberger was his threat as a disruptive influence (after an incident at Haliburton, Ontario, during the previous August's annual meeting of the Canadian Velikovsky study group), which might interfere with the videotaping of the Portland proceedings for a documentary. However, the organizers' concern over the possibility of Ellenberger's attending was apparent before August (in June), when Reichian watch-dog Joel Carlnskey imparted what he had learned during a May visit in Portland with one of the organizers of the upcoming November meeting.


D1 It is statistically self-evident that Table 1 cannot be purely the result of real empirical observations. Assuming that some of the Robertson wire-data are genuine, he evidently until 1983 one of Velikovsky's most active defenders,3 was barred from a Velikovsky-retrospective meeting in Portland, OR, 1994 November 25-27, co-sponsored by Kronia Communications and the equally Velikovskian organ Aeon. The meeting, "Velikovsky, Ancient Myth, & Modern Science", was actively promoted on USEnet's talk.origins newsgroup as open to the public.

D2 It may merit a very sweet ironic smile that turncoat and apostate Leroy Ellenberger, until 1983 one of Velikovsky's most active defenders,3 was barred from a Velikovsky-retrospective meeting in Portland, OR, 1994 November 25-27, co-sponsored by Kronia Communications and the equally Velikovskian organ Aeon. The meeting, "Velikovsky, Ancient Myth, & Modern Science", was actively promoted on USEnet's talk.origins newsgroup as open to the public.

D3 By chaining such elementary means, it would be possible also to create manufacture the other three. For that star, this would force all three sums (right-hand side of Table 1) to be equal — and force all four inter-wire intervals to be equal. So this explanation will work for numerous data here, as we will see below at §F.

E1 Standish notes (§B2) that all the 1st/4 and 3rd/4 endings occur strictly for the 2nd and 4th wires. A similarly peculiar pattern: of the ten suspect stars where all the wire-times are alternately integral and half-integral, nine (90%) have w2&4 or w3&5 when data are being interpolated instead of observed. Assuming the central wire was observed for most 1811 stars, the asymmetries suggest (but do not prove) application of eq. 1 to a pair of non-adjacent, usually integral wire-data (w1&3 or w3&5). However, these 15 stars reflect but a minority of the whole sample. So it may be that Robertson customarily fabricated by extrapolation (eq. 2) — that is, if he built a star's five-wire record from merely two wire-data (§D3), the two wires were usually adjacent. (E.g., w2&3 or w3&4 or such.) One possible motive for such selectivity will soon (§E5) become evident.

E2 Perhaps we can better understand Robertson's methods in this late stage of his life, by comparing them to his early years in the study of the Venerian phenomena. In his 1936 study of the Venerian observational data, Robertson found that in 1933 the star Vesta was missing one observation, and the wire-times for the remaining four wires were as follows: w1 = 0.40, w2 = 0.41, w3 = 0.42, w4 = 0.43. Robertson interpolated the missing observation using eq. 1 to a pair of non-adjacent, usually integral wire-data (w1&3 or w3&5). However, these 15 stars reflect but a minority of the whole sample. So it may be that Robertson customarily fabricated by extrapolation (eq. 2) — that is, if he built a star's five-wire record from merely two wire-data (§D3), the two wires were usually adjacent. (E.g., w2&3 or w3&4 or such.) One possible motive for such selectivity will soon (§E5) become evident.

E3 The final rendition is so full that, for 95 of the 96 stars of Table 1, Robertson provides times for all five wires. The sole exceptions occur on 1811/11/11, when: [a] Only w5 was recorded for that evening's 1st star (Hamal), and the hour & minute were missed, so the record is not entered in Table 1. [b] For the star 78 Tau (miscalled 79 Tau), only w1, w3, & w5 were recorded.

E4 A possible alternate theory: Robertson usually took all 5 wire-times, but the original data showed so much random scatter that he later "tidied up" the record — and way overfaked it. For circumstances in favor of this theory, see §E8 & fn 27. Also: there is no overlap of any of the stars' 5-wire time-sets. (I.e., if the observer intended to take only w2&4 for a star, then he might go on to another star right away — and thus might inadvertently create temporal overlap of the two stars' eventual 5-wire sets of data.) But no such overlaps appear in Table 1. However, there are other explanations for this.

E5 But there is a suggestion of at-least-temporary use of an interval-list (§F) in the fact that, in Table 1, two of the five cases of quarter-timesec intervals are for the same star: β Tau (1811/11/1 & 1/17) — and the interval is identical: 25¹/4. (By contrast, the correct interval by eq. 6 is 25¹/2.) Another explanation for the β Tau data: only 3 wire-times were observed (see also §F10), say, w1&3 on 11/1 and w3 on 1/17 (all integral) — and the other 7 data were then fabricated via eqs. 1&2, adopting the 1/17 interval for 1/1/17.
E2  There are (see Table 1) more than 20 perfect-symmetry cases involving wire-times ending in timesec-tenths. Now, if Robertson were using eq. 1 as often as eq. 2 we would expect a sizable fraction of these cases to create data ending in timesec-tenths. Yet no one does so — a highly unlikely coincidence. This again (as with §E1) is consistent with preferential use of adjacent wire-data — which obviates the need for eq. 1. (Possible alternate explanation: §F.)

E3  However, we recall that §E1’s 15 cases suggested that eq. 1 was used on occasion. So why is there a complete lack (§E2) of endings in timesec-tenths (which would occur for about half of all tenth-timesec-precision data when eq. 1 was used)? The answer, of course, is that such claimed precision would be incredible on its face. (See §E5.)

E4  In exploring this matter, we first note that even for the dozen asterisked cases (where all three sums aren’t equal), most of the stars show equality for two of the three sums. (See right-hand side of Table 1.) This points the way to a few realizations about the asterisked stars: [a] Some of the inequalities may just be from scribal or arithmetical errors. [b] Some may be stars for which 4 or 3 wires (not just two) were observed, so that only 1 or 2 wire-data (not three) needed to be faked to flesh out the apparent record.

E5  For the 2nd star of Table 1 (where none of the 3 wire-sums are exactly equal), we may wonder how likely it is that $w = 5^m39'.4$ would agreeably interpolate with interpolation (from eq. 1) within 0'.05. Ignoring hours, we have

$$\frac{(w+1)+w3}{2} = \frac{5^m16'+6^m02'.9}{2} = 5^m39'.45$$

(3)

And the same star’s $w4 = 6^m26'.5$ also agrees with interpolation to the same amazing precision; proceeding analogously to eq. 1 or eq. 3:

$$\frac{(w3+w)+5}{2} = \frac{6^m02'.9+6^m50'.0}{2} = 6^m26'.45$$

(4)

These coincidences both occur in a data-set whose standard deviation is an ordmag larger (§C1). It is more reasonable to suppose that $w1&w3&3 = w$ were observed and $w2&w4$ fabricated therefrom via eq. 1 — but both results had to be rounded⁶ (to timesec-tenths) when the computed figures exhibited the ridiculous precision of 0'.05. Perhaps such uncomfortable experiences nudged Robertson to prefer adjacent wire-pair data (§F5) whenever tenth-timesec data — thereby avoiding the halving process (eq. 1) that caused the need for rounding.

E6  Curiously, one of the weirdest instances (where it appears that 1/20th timesec was shaved off data) occurs for an unasterisked case. The 1811/11/13 solar limb wire-time sets are both preternaturally symmetric: for both limbs, the 3 wire-sums are identical (thus the lack of asterisk in Table 1) — this despite the fact that (in both cases) the intervals are not quite uniform! Again, the most likely explanation: this is a case in which (at least) $w2&w4$ were created by interpolation (as in eqs. 3 & 4) — but the former was then diminished by 1/20th of a timesec, while the latter was identically enhanced. (Indeed, for the 2nd limb data, it appears that, instead of writing the endings as “xx.75” & “xx.25”, Robertson simply rounded⁶ to “xx.7” & “xx.3”.) He thereby neatly ensured that, for both limbs, all 3 wire-sums would be identical. Thus, computation of the mean transit time ($w$ = eq. 5) here was child’s-play⁹ — circularity would require that $w1$ was just equal to $w3$.

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⁶When Ptolemy indoor-calculated his allegedly-outdoor 1025-star catalog, he confronted a similar overprecision problem. His sly solution (discovered by the genius of the late Robert Newton) is explained at DIO 4.1 §3 |C1 and independently confirmed in detail elsewhere in the same article.

⁷Note that the former-vs-latter situation is the same for both limbs’ data.

⁸By 1812, Robertson was recording “xx.7” for virtually all cases where he formerly would have written “xx.75”.

⁹Which was the prime intent of these fabrications. See §G5.

B4  Both §B3 Princetitute fouldups achieve the delicious distinction of exceeding the size of the entire measuring stick for the problem! (Reminiscent of other bloops by Ptolemy & his equally honest defenders: see DIO 1.3 fn 288.) The Princetitute’s repeatedly botched and sales-quickly delivered (DIO 4.3 §15 |F1) effusions on ancient astronomy are perversely over-rated in academe primarily because no journal (other than DIO) will criticize Princetitute-Hist-sci super-archonial behavior: pathetically amateurish science, plus censorial attitudes that do an artistically faithful imitation of raw fear. (Only at absolute-zero pinpricklessness can “arrogant gasbags”¹⁵ survive. So, an uncritical environment has to exist.) I.e., Hist-sci won’t criticize archonial misbehavior, restricting (p. 2 fn 3) criticism instead for him who does. (See, e.g., DIO 1.2 fn 30 & fn 96.) Thus, discussion of suppression is itself suppressed.

B5  My 1987/3 Amer J Physics paper (n.24) noted that the Almajest 9.10 celestial description puts the −264/11/15 Mercury longitude at c.213°.1 (ident or HAMA p.166), disagreeing (by over 10°) with the stated longitude (213°1/3), but closely accordant with the Canobic Inscription orbit. So I proposed: when founding the prior CanInscr, Ptolemy performed his usual pseudo-orbit-establishment math (identical to Almajest 9.10 except: using CanInscr orbit elements). Later, when he adopted the different elements of the Almajest theory, he had to wrench this longitude up to 213°1/3 so that when he repeated the same pseudo-proof (Almajest 9.10: intimately dependent upon these now-mutated elements), it accorded with exactly the same mean motion already announced in the CanInscr.

B6  But I only recently (1997/1/22-23) noticed a stark and — precise — independent trace of this procedure: in the other longitude (−264/11/19) given at Alm 9.10, Ptolemy places Mercury at longitude 213°6, which does not match (even within 10°!) the Almajest Mercury orbit — the same orbit which DR’s AJP paper suggested ten years ago (on quite independent evidence: §B5) was behind this entire Almajest 9.10 math-charge. In exact sciences, dishonesty often (§1 |G2) leaves a slimy trail.¹⁷ So we should be all the more grateful for the rare true giants of ancient astronomy, one of whom will be the subject of a reconstructive appreciation in an imminent DIO.

¹⁵Disguised 1997/1/15 appraisal, by a well-known veteran observer of academ pretense.

¹⁶More precisely, the original −264/11/15 longitude survived: this Mercury “satellite” is not used in Ptolemy’s Alm 9.10 math, thus, it wasn’t worth laboriously recomputing. I.e., Ptolemy lazily left the 2nd datum (11/19) as it was; but, noting that his new value for the 1st longitude (11/15) was now merely 1°4/2 (not 1°2, as previously) west of the 11/19 longitude, he merely took a moment to alter his report (of 4th differential longitude-motion), to make it agree. Sloppy. (For another instance of such precisely revealing Ptolemaic sleuth, see DIO 1.1 §5 |H5.) Resulting hybrid gap: 213°37” (CanInscr for −264/11/15) vs. 213°48” (Almajest for −264/11/15) for −264/11/15.25. Which was indeed half a moonwidth, as Ptolemy reports (hitherto inexplicably) in his Almajest 9.10 discussion of these data. Note: if we do not accept some such hypothesis, we must believe that this already doubtful-suspect (notoriously discrepant [Toomer loc cit] and altered-orbit-ensnared) observation-pair report had a 4°-motion-error (1°4) that just-so-happened to match the difference between the CanInscr and Almajest theories here. This, when there is no question that Ptolemy kept his Mercury mean motion, allegedly math-based upon the −264/11/15 longitude (pseudo-proof at Almajest 9.10) — despite alterations (prior CanInscr vs. later Almajest) of the underlying orbital parameters of this math, which required a 1°4 alteration in the −264/11/15 longitude in order to ensure that his math would still promote exactly (and I mean exactly) the same mean motion in both works. (Above, §B5.) See discussion of this alteration at Rawlins Amer J Physics 55:235 (1987) p.236 |item 85 |n.24. For the great mathematician van der Waerden’s delighted appreciation of the finding of vanishing on the Ptolemy debate, see DIO 1.1 §6 fn 37. The Princetitute-Muffala’s typically honorable reply, to this thoroughly refereed & very prominently published lethal proof of Ptolemy’s Mercury hoax? Ten years of silence. While profitably peddling Ptolemy to academia as a brilliant and highly ethical scientist.

¹⁷As slick as a trail as any is Ptolemy’s Mars orbital eccentricity of 0.10000! — allegedly (Almajest 10.7-10) based on observations, but so overeat it’s actually funny. (See R.Newton Crime of C.Ptolemy Johns Hopkins Univ. 1977 pp.319-320, or Rawlins Amer J Physics 55:235 n.25.) For a crashingly clear proof of Ptolemy’s fraudulence here, see mathematician H.Thurston at DIO 4.2 |6.
B Covering Up the Covering Up of Princetitute Amateurishness

B1 Our “Black Affidavit” (DIO 1.3 §10) noted that we have exposed several botched (even faked) calculations in ancient-astronomy researches emanating from the Princeton Institute-Mufa. Though these results are known11 to Mufa & Princetitute personages, not one of the errors (see also DIO 6 §1 [H4]12 has ever been acknowledged. Standard archival integrity. The following will add yet further material to that which the Princetitute will simply tuck under its increasingly Himalayan rug. Similarly, its courageous Mufa’s desire for rational discourse may be gauged by its habit of attempting to hide from DR the date & location of all its snug little get-togethers [e.g. its 1994 Dibner Inst. symposium].

DIO requests that we be informed of future ancient astronomy conferences.

B2 From a DR letter to R.Newton (1985/9/12), part of our astonished monitoring of the perpetual Hist.sci sales-prestige that the Almajest is a marvel of accuracy for its time.

There is a [Mercury] station of −264/11/16 observed on 11/15 & 11/19 by the Dionysians . . . accidentally preserved . . . [at Almajest 9.10]. To Neugebauer’s credit, he [recognizes the station] [HAMA, p.166-167; also Toomer’s 1984 Almajest, p.464 n.99], though he does not remark the revealing fact that Pтолемей (who regards stations as worthless, Almajest 9.2) hasn’t any idea of why these data were [valued] by the Dionysians. ((Stations) provide the empirical basis [DIO 2.1 §3 fn 17] of the Almajest mean motion of Mercury . . . ) Aaboe’s 1980 Centaurus, p.27, similarly protects the reader from understanding Pтолемеев ignorance in this fundamental connection.

B3 Neugebauer’s diagram (HAMA, p.1254, Fig.152) is useful but misleading. He remarks (p.167) that the data’s agreement with Tuckerman is “almost perfect” if we shift them for precession error. Not so . . . [For Mercury’s net geocentric motion in the ↓4], Pтолемеев has . . . an error by a factor of over two [1/4 claimed, vs. 0°.6 actual] — huge by the measuring unit (lunar diameter) specified. N says (p.166) that pt.10 on Fig.152 corresponds to Mercury’s place on −264/11/14, “one day before Pтолемеев’s first observation [and point] No.11, for Nov.19, coincides with the second observation.” These points are taken directly from Tuckerman (who uses ↓5 intervals for Mercury). But N has forgotten13 that . . . Tuckerman’s places are all for . . . 6 PM Alexandria [a rather unconventional hour for observing Mercury when it’s west of the Sun!] Thus, pt.10 is ↓2/2 (not ↓1) before 11/15.25, and pt.11 is ↓2/2 (not 0) after 11.19.25. . . if we follow N’s error and compare evening positions, the position shift in the ↓4 interval grows to [0°.9], which disagrees even worse with [Pтолемеев’s ↓2/4. The discrepancy here is ↓2/3 than Pтолемеев’s measuring unit — namely, the lunar diameter.]14 Finally, the sharp point (extending Mercury’s alleged path nearly a full degree to the right of pt.10) at the station on N’s Fig.152 is [pure Princetitute] imagination. Mercury only went about ↓2/4 [15] beyond pt.10 before station [at 122°-43°].

E7 As for §E4’s proposal that some asterisked data are slips: suggestive instances are not hard to find. E.g., for the 3rd star in Table 1, \( t = 7^	ext{th} 07' \) looks like a tens-place miswrite (or miscalculation) for \( t = 7^	ext{th} 07' \). And, for the 2nd star of 1811/11, \( w = 11^	ext{th} 10'34' \) may be another slip. (The correct mean of \( w \& w5 \) is 11°10′14″.)

E8 An independent bit of evidence of data-wrenching: on the handwritten record, the three 1811/11 entries involving 1/4 timesec precision are plainly peculiar. The “.25” and “.75” are visibly scrunched (the figures smaller than normal), in five out of the six renderings. It is obvious to the eye that each of these endings was jammed-into the appropriate space only after the integer portion of the data had been entered. (See §F4 & fn.4.) The handwriting looks like Robertson’s.

E9 Speaking of the entire 1811 January record: it is remarkable that such a data-set ever got into the record of an eminent observer. Could any astronomer have expected the data of the 4th star of 1811/1/27 to be believed? (All five of its wire-times end in eight-tenths of a timesec.) And, though the frequency of fabrication declines after 1811 January, we find just as incredible a 5-wire set of data atop the record for 1812/4/18.10 all five ends in nine-tenths of a timesec. And two stars later, all five times end in seven-tenths of a timesec. (Same for the last star of 1812/4/21.)

E10 The 1812 Spring records include a column explicitly reducing all wire-times to a middle-wire mean time, \( w_m \). If all five wire-times are taken, then

\[
\begin{align*}
\text{E11} & \quad \text{But, for an asymptotic set, it is not so simple. An example is the two-limb solar record for 1812/4/20. For the 1st limb, } u \& w_2 \text{ were not recorded, and } w_3 = 152^\text{m}49^\text{s}, \quad w_4 = [53]^\text{m}11.5, \quad w_5 = 53^\text{m}33.5. \quad \text{For the 2nd limb, } w_1 = 54^\text{m}15^\text{s}, \quad w_2 = [54]^\text{m}37.5, \quad w_3 = 155^\text{m}0^\text{s}, \quad w_4 = [55]^\text{m}22.5, \quad w_5 = 56^\text{m}45^\text{s}. \quad \text{(The precisely-uniform-interval 2nd limb data look fleshe dout by the same fabrication-approach as the 1811 January data.) Reconstructing: Robertson, using a 22.5 interval (from 2nd limb data or eq. 6), reduces the 1st limb’s } u \& w_5 \text{ to the middle wire (i.e., subtracts 22.5 from } w_4 \text{ & } 45^\text{s} \text{ from } w_5), \text{ and averages these to find } w_m = 52^\text{m}48.3. \quad \text{(All the data in his record are rounded to hundreds of arcsec.) This he averages with the 2nd limb } w_m, \quad 55^\text{m}, \text{ to find solar-center } w_m = 53^\text{m}54.415, \text{ which (rounding as usual) he writes as } 53^\text{m}54.41. \text{ This is an at least partly-proper set. But the whole procedure illustrates how his awareness of intervals was used in arriving at such means. (Robertson used the means to check his sidereal clock’s rate.)}
\end{align*}
\]

F One Wire-Time Per Star? — Using a List of Wire-Intervals

F1 Though empirical extrapolation-interpolation is a possible solution to the suspicious Robertson data, there is another simple explanation that can also account for many of the fabrications (e.g., §G4), namely: Robertson had at hand (or in memory, at least in part) a list providing the mean inter-wire time-intervals \( t \) for bright stars — or, simply a table13 providing \( t \) for, say, every degree of \( \delta \). Since the equatorial wire-interval \( t_\delta \) was about 12°22', he could compute any star’s wire-interval \( t_\delta \) from its declination \( \delta \) via the simple equation:

\[
\begin{align*}
\text{F2} & \quad t = t_\delta / \cos \delta
\end{align*}
\]
Thus, he could easily (in just a few seconds) flesh out a full five-wire display for any familiar star, after taking but a single wire-time: merely by forming integral multiples of \( t \) and adding them to (or subtracting them from) the sole real wire-time. (In the long term, this technique would not be so simple for non-stellar objects, whose \( \delta \) — and thus \( t \) — will not in general stay effectively-fixed for years^13 on end.) We will (by contrast to §D2) call this: non-empirical extrapolation. (Meaning that the interval \( t \) is computed or assumed^14 not observed.) In this connection, one notices that (in the Oxford 1811 record), some bright stars' wire-time-intervals \( t \) are frequently identical from night to night. E.g., \( \alpha \) Tau 23^15, \( \beta \) Tau 25^15, \( \alpha \) Lyra 28^15, \( \beta \) Gem 25^15, \( \alpha \) Gem 26^15, \( \delta \) Per 32^15 or 32'14, \( \alpha \) CMa 23^15, \( \alpha \) CMa 22'31 or 31'2. (The data for Castor & Pollux exhibit particularly uniform spacing in 1811. And, though their data for 1812/5/3 are irregular, the only value for \( w \) computed there is slightly erroneous, due to quick&dirty use of the same^16 old 25^15 Pollux interval: see §F9.)

Below (§G14), we will see large-scale evidence against the one-wire-time theory. Other, smaller problems with it follow here.

On 1811/1/11, \( \alpha \) Tau and \( \beta \) Tau were expressed to 1/4 precision (\( t = 22^3/4 \) or 25's/4, resp), though the real \( t \) in each case was actually near-integral (23^16 or 25^16, resp). Thus, the 1811/1/11 record suggests empirical interpolation (eq. 1), not the use of §F2's list. Note that this is the very night where we find physical evidence of fudging: §E8.

And use of a list seems unlikely to explain the case^17 of \( \alpha \) Aur (magn 3.0), where we find at least three variants for \( t \): 30' (1811/1/21 & 1/29, etc), 30'1/4 (1811/1/20), and 30'1/2 (1811/2/13). (Since \( \delta = 43^3/4^16 \), the real \( t \) was about 30'1/2.) There is a provocative implication here: in a five-wire set, a variation of 1/2 in \( t \) will entail a discrepancy of 2' in \( w \) at most. Such an error is too large for real observations in the 1811 season. (The least ambiguous indication is that \( \epsilon \) Aur could not have been on the list of major-star hypotheses at §F.) But it could easily happen to someone estimating (to crude fractions of timesec) a single interval of adjacent \( w \) (see §E5) — and then fabricating all the other wire-times. This appears to have repeatedly occurred for \( \epsilon \) Aur. (At §G, we will find confirmation of the suspicion that such fabrication was indeed a regular occurrence in this record.)

In early 1811, the interval \( t \) for Jupiter’s center^18 was 22'.9. Either the Fabricator kept this figure at his side, or, on 1811/2/1, he opportunistically used the \( t \) already adopted for the 1811/1/15 Jupiter record.

Indeed, the very same day (1811/2/1), he also used the same \( t = 22^3/4 \), for all eight intervals of his 5-wire observations of both limbs of the Sun (which happened to be near Jupiter’s declination). Similar artificiality in solar observations appears in the data (all 5-wire sets, for both limbs: see Table 1) for 1811/1/16, 18, & 19, where all twenty-four intervals are identical at \( t = 23^3/5 \). (There are no solar data for 1/17.)

The 188/5/8 observation of \( \alpha \) Gem is a curious hybrid,^19 based on two wire-times: instead of interpolating, Robertson (or whoever) simply extrapolated (using his standard 26' interval for \( \alpha \) Gem) from each of the two observed \( w \). Result: \( w_1 = 23^3/14^3/5 \), \( w_2 = 23^3/14^3/5 \). But days are another matter. Especially for the outer planets: see §F6.

A clear example at §F10.

There is a 1' arithmetic slip for \( w_5 \) on 1811/1/29.

The 5-wire Pollux record of 1812/2/13 is in perfect accord with the same \( t = 25' \), despite “High Wind.” The Gemini twins' regularity may have a partly empirical cause: eq. 6 yields Pollux 25'.0, Castor 26'.0.

A less flagrant example is 137 Tau: constant interval 22'.2 on 1811/1/18, vs. 22'.8 on 1811/2/22. Small difference — but large implication: \( t \) was not based on a pre-listed interval for this star.

On 1811/1/15 and 2/1, all four intervals \( t \) for Jupiter’s center are 22'.9. Since Jupiter’s width is several time-secs, this is quite an implicit precision-claim!

The \( \beta \) Gem data of 1811/1/14 are probably a similar set.


6Hist Céleste pp.217-229 (1796/3/15-4/15). Note that analysis of this flock of Uranus observations could either confirm or disconfirm the suggestion that the still-persistent Lalande 1795 Neptune residual is due (in nontrivial part) to the effect of planetary nonpunctuality.

7The original mss of the Histoire Céleste are in the Paris Observatory archives: (A.)C.5 bis, G14). We will see large-scale evidence against the one-wire-time theory. Other, smaller problems with it follow here.

At vol.33, p.303. atop the 1800/10/25 start of sweep#2, a hitherto-unpublished note in M.L.Lalande’s hand: “This project has been conceived in fructidor year 8 [1800/8/19-9/17] by le franc¸e. [Lalande] & Delambre” (ibid). The original mss were ofcially presented to the National Institute on 1800/11/2. (See ibid.) However, hope melted (after the 1800/10/25 start) the followup­survey­that­never­matured. . . .

8Hist Céleste pp.570-576. See this paper for details.

9See mss vol.36. When Lalande recorded the 3rd wire of Gemma at 21:59 Paris Mean Time, Neptune was a few timesecs from transiting the 1st wire, had he immediately shifted the telescope through a net he spent 10 years scrupulously weaving.
contacted many years ago (while RGO archivist Philip Laurie was alive) by leading British officialdom, as to the whereabouts of these mss (which he was the last to use) he simply did not reply! I know from two sources that he has, now that Laurie is dead (since 1983), begun claiming that the texts out of the letters he published from the missing file (years after its disappearance) were in notes given him by Laurie. I therefore immediately proposed (to both sources) that he be requested to produce these alleged notes, in Laurie’s hand. (I would write him myself, but he will not answer my communications.) We now await the next chapter of this ongoing tale.

A8 I wish to add that virtually all British astronomical officials are innocent of the circumstances of the Neptune file’s disappearance — and are (as if this needed to be said) involved in no conspiracy. They are as dismayed as DR at the continuing unavailability of a crucial documentary record of one the grandest chapters in the history of astronomy.

A9 It is a pleasure to reveal here an unexpected credit to the RGO in the Neptune affair. In 1847, Harvard’s Prof. B. Peirce besmirched (by a misbegotten public attack) the mathematical legitimacy of the eternally-glorious predictive discovery of the 8th planet. (Since Harvard-trained physicist DR’s detailed 1970 laying of this matter to rest [Mon. Not. Roy. astr. Soc. 147:177], Peirce’s case is now taken seriously only by those less fortunate than ourselves mathematically, e.g., Harvard’s Prof. 0. Gingerich at Sci Amer 1996 Sept p.181.) As noted at DIO 2.3 [9 in 5, John C. Adams eventually (1876) published a learned discussion of the fallacy in Peirce’s reasoning. But who first discerned the error privately? Answer: the very party who (in popular mythology) has been most frequently & ignorantly abused as an idiot in celestial mechanics — no other than Astronomer Royal George Airy! A10 The following letter was found in the same file as the above Christie note.

To: J. C. Adams, Esq. 1847/4/29
From: G. B. Airy, Royal Observatory Greenwich

… I was astonished to see Prof. [Peirce]’s remarks about the equation depending on \( n - 2n^2 \) [i.e., the Uranus-Neptune 2-1 resonance]. Such are necessarily of long period, or, even if they constrain the orbits to exact commensurability, they do not produce sensible disturbances in one revolution.

A11 Having for years (e.g., DIO 2.3 [9]) defended Airy from the uncritically repeated charge that he was (Scientific American 1963/3) a “school-bright, hapless donkey” & “unusually conceited”, I am gratified to find such positive proof of his intuitive expertise in the Neptune context. As for “conceit”: well, the reason we have explicit evidence of his initial overskepticism towards the solubility of the Neptune problem (his 1834/11/23 letter to Thos. Hussey) is that Airy himself published it in 1846, along with the wry remark that, “It will be readily understood that I do not quote this letter as a testimony to my own sagacity”. A final remembrance of Airy as a decent human being (far from the popular image of unfailing machine): though he called James Challis (history’s chief fall-guy for sudden in 1846-1847) out of the desire to keep Neptune from being called “Leverrier” — as the Paris Observatory was then urging. [Note by DR.] I know this from practical experience in such problems. If the enormous 2-1 perturbation is completely ignored in the theories of the motion of Uranus and Neptune, and the disturbed elements are then re-determined on this basis, the resulting theory will track either planet were dealing with, when they solved the Uranus mystery.

3The same letter also shows early motion in the process whereby British almanacs eventually (under Adams’ direction, I believe) ceased calling Uranus “The Georgian” — a reconsideration born (of a sudden in 1846-1847) out of the desire to keep Neptune from being called “Leverrier” — as the Paris Observatory was then urging.

2This is one of a number of 1812 cases where unrecorded w3 would be identical to recorded w4.

3“High wind” is noted beside the 1811/2/16 solar data.

21“High wind” is noted beside the 1811/2/16 solar data.

22See §F2.

23Grabbing off previous data is also evident at fn 10.

24See also §D & §E.

25Another point in Oxford’s favor: Robertson did not publish his fabrications.

26I.e., Ptolemy pretended that his “observations” proved his theories, when in truth the observations were computed from the theory. See, e.g., R.Newton Crime of Claudius Ptolemy (Johns Hopkins Univ 1977) or here at §5 §B5. For modern mass-pretense, see above at News Notes fn 2.
the other wire-times to be calculated by false extrapolation, whose error of course ballooned to 2' or more for the last two wires. In real observations, such errors are ludicrously unlikely to occur for two consecutive wires.

G4 As for Alcyone (mag = 2.9), it was observed 1811/1/28 & 2/1. On both occasions, the fabricator faked most of the wire-times, using $t = 23'$ — perhaps borrowing the interval of Alcyone’s fellow-Bullstar, Aldebaran. However, Aldebaran’s $\delta$ was 16°07', while Alcyone’s $\delta$ was 23°31', so (by eq. 6) Alcyone's actual $t = 24'$. Therefore, both these Alcyone records contain perfectly systematic errors in $w_5 - w_1$, amounting to four timesec. On each night the observer could have recorded only one wire-time (say, $w_3$) and later fabricated the other four wire-times (using $t = 24'$). Thus, the consistent falsity of the Alcyone data is neatly explained by the 5-wires-from-1 hypothesis of $\xi$F. Though, 5-from-2 (via eq. 2) works as well (assuming Alcyone $t = 24'$ was a 2-wire empirical accident one night, copied therefrom the other 1-wire record). But, regardless of the precise method of indoor invention, the critical point here is that, when two consecutive recordings of Alcyone both involve rigidly uniform systematic errors that entail 4' errors in $w_5 - w_1$, then: we know to a certainty that most of these data are fabricated.

G5 The bottom line here appears to be pretty elementary: whoever doctored the Oxford transit data realized that, the fewer wires he was actually using in his computations (and-or the more symmetric his wire-time data became via fudging or fabrication), the less time & computational labor would be required to [a] observe them, and [b] to reduce them — all while [c] leaving a busy-looking data-record. So he leaned in the direction of streamlining, neatness, and simplicity.

G6 The Robertson record as we now have it is a copy of prior raw-data records. (Which heaves open the possibility that he was not the fabricator. However, a lot of suspect data appear to be in his hand, and the pages are all signed by him, as observatory-director. So he — at the very least — bears the responsibility for lending his name & Oxford's to patently incredible data-sets: $\xi$E9.)

G7 Realization of this non-primary nature of the record led me momentarily into a merciful hope of explaining the fabrications as part of an innocent calculational checking-scheme, carried out to help ensure correct reduction. However, sobriety soon set in: that theory cannot explain why all but ordmag 1% of the Table 1 stars displayed all 5 wires' times. There must have been plenty of cases where two symmetric wire-times were obtained ($w_2$&$w_4$ or $w_1$&$w_5$): in these instances, checking one’s math would not require filling out a full 5-wire record. (This was done for show, presumably to fool employers.)

G8 I.e. there is no way around Standish’s conclusion that the record is at least a heavily doctored one. Indeed, in such a suspicious context, the fact that the extant record is but a copy raises the question of what the original looked like: Was it sparse? Or full, but as-yet unsmoothed?

G9 Regardless, the party (or parties) responsible kept up the pretense for many months. His methods were as various as opportunistic, e.g., $\xi$D2, $\xi$F, and fn 23. But the purpose

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24 In the 1811/2/13 record, Robertson accidentally skipped the 5-wire record for Capella and wrote down the 5-wire record for $\beta$ Tau before realizing his omission. He then scratched out the $\beta$ Tau data and wrote Capella's on the next line, and $\beta$ Tau's on the line following. Such a sequence could not have happened were the record being made in real time. (The same slip occurs in the 1811/1/29 record, for $\varepsilon$ Aur [temporarily skipped] and 125 Tau [first entry scratched].)

25 Standish has wondered if this transit work was funded on a per-star basis. Whether or not Robertson was paid (rigidly so), the general theory seems reasonable. Also, if an underling was doing the actual observing, payment per full-wire-set could help explain the creation of this odd record. If Robertson was the paddler ($\xi$E8, he was probably doing other work simultaneously and was understandably bored with transit observing. I.e., he should have delegated it (as Flamsteed sometimes did, and [DIO 2.3 ff fn 1] Lalonde did entirely) — & later checked output. But all these are feeble excuses. The immortal theorist Bessel did lots of dull transit work, yet the drone-nature of it did not lead him to fake data.

A 5 Unpublished Letters

A1 I’ve recently found (Cambridge, 1996/9/20) a letter by Airy’s successor as Astronomer Royal (from 1881, after J.C. Adams refused the post), Wm. Christie, exhibiting the same penchant. (My thanks to St. Johns College archivist Elizabeth Quarnby Lawrence for assistance with my exam of the file containing this find: Adams ms Box 17.)

A2 I quote the entire letter (merely adding an occasional comma for clarity):

To: [Dr. Donald] MacAlister

1893/4/24

From: W. H. M. Christie, Royal Observatory, Greenwich, London, S. E.

A3 Before sending you the copies of the letters you asked for, I submitted them to Mr. W. Airy and enclosed is a copy of the letter he has written to me after going carefully through the whole correspondence. There are some other letters besides those to [Cambridge’s Adam] Sedgwick which, I think, should not be published without some excisions — those of Leverrier in particular.

A4 As I am to a certain extent responsible in the matter, would you mind letting me see what you propose to publish, when the time comes? Leverrier’s letters seem to me to require rather delicate handling, as he was evidently very angry with [John] Herschel when he wrote, but you will, I have no doubt, judge discreetly as to what should be published.

A5 The $\xi$A6 contemplation of censoring Airy-Sedgwick letters should be of particular interest to our readers, since $\xi$DIO 2.3 19 $\xi$A6 specifically stated in 1992 that the remains of the Airy-Sedgwick 1846 Neptune correspondence indicated to DR that it had been protectively censored. $\xi$DIO's full 1992 comment: “This is part of a series of Neptune ms disappearances suggesting systematic suppression of documents, a situation encouraging some otherwise unthinkable speculations.” As for modern bans: 150th anniversary pieces appeared (1996/9) in $\xi$ST (Patr. Moore), $\xi$Astronomy (Sheehan&amp;Baum), & $\xi$Sci. Amer (0 Gingerich); all omitted the trille that the key RGO file walked in the 1960s ($\xi$DIO 4.2 10). $\xi$Astronomy’s lawyer’esque 1996/7/29 plea: its article was on the 19th’ not the 20th’ century!

A6 In pleasant contrast to such discouraging patterns: I wish to credit the Brit Astron Assoc for being unafraid — indeed proud — to welcome heresy: the BAA invited DR to give a 1996/9/21 lecture before its annual National Astronomy Week meeting (Birmingham) on his long-unorthodox view of the Neptune scandal. (The 150th anniversary of the discovery was 1996/9/23. Both the BAA & the audience were more than fair. I.e., I was not tarred&amp;feathered — not even after asking why $\xi$England was celebrating National Astronomy Week from 1996/9/23 to 9/30, this being the 150th anniversary of precisely the week [1846/9/23-30] during which England was the only nation in northern Europe that did NOT know where Neptune was.) Heavy post-lecture feedback reflected evaporation of old myths; e.g., DR had just dropped an unequivocal bomb: Brit-hero Adams’ final Neptune solution (Hyp X: $\xi$DIO 2.3 19 $\xi$E4, E8, F3, Tables 1&2) wasn’t on any Berlin Starchart.

A7 While in England (1996/9/19, after being taken out of earshot of anyone else), I was privately briefed by an insider (who prefers anonymity) regarding the odd behavior of the chief suspect (a former high RGO official and confidante of the then-Astronomer Royal) in the disappearance of the Royal Greenwich Observatory’s Neptune file: when
between observed and assumed E or W altitudes $h$ to find orientation, or monitor the rate of ascent or descent. The altitude $h_0$ of the sun on the prime vertical (E or W) is given by:

$$\sin h_0 = \sin \delta / \sin \phi$$  

(7)

Ephemeris tables provide the precise declination $\delta$, and a noon reading gives latitude $\phi$. With a log-trig table (and a page or two from each table will suffice), it is easy to compute predict $h_0$, so one simply watches by sextant until the sun has attained that value. The sun is then due east or west. The precision is much superior to the N-S method ($\S$E1-E2). Ignoring small variations in $\delta$ and in the equation of time, the ascent-rate $d\theta/dt$ is just:

$$d\theta/dt = h = \cos \phi \sin A$$

(8)

where $A$ = azimuth; so, near the prime vertical, the sun’s rate of ascent $h_0$ is virtually:

$$h_0 = \cos \phi$$  

(9)

In our earlier example ($\S$E1), $\phi = 87^\circ$N, so $h_0 = 1/19$ — i.e., ‘1° of altitude change will correspond to 19° of time or azimuth instead of 7°. The precision is improved by a factor 22 of about 20. Near the poles, the eq. 7 method is not very latitude-sensitive either, since $\sin \phi$ is effectively constant (at unity) near the North Pole.

E6 The conclusion must be that at best the $\S$A1 memo is an uncertain later reconstruction from memory, not a record of actual observations for navigation, nor a description of superior methods. Whether this should influence the evaluation of Peary’s claim to have reached the North Pole must depend on weighing the total evidence.

References

which triggered the *Wash Post* 1993/6/1 article which itself caused the skeptical 1993/6/11 story in *Science* (Amner Assoc Adv Science). [Note added 1997/3/10: NGS’ Pole myth has since evaporated in the scientific community. See also Rawlins 1992 fn 2 & DIO 2.3 $\S$8 fn 11.) More hits on it will soon risk SPCAr-wrath at deadhorse-abuse. NGS greeted Bryce (fn 22) with standard-slowbleed p.c.: the-controversy-will-continue. DR (1991/8/13 Wash Post, emph added) on the same mantra: “Needless . . . . [NGS] should . . . . have Admiral Peary’s claim and the [1989-1990 Nav Fnd Rpt] evaluated by the National Academy of Sciences, just as papers are routinely refereed every day in US science.

I am willing to abide by the Academy’s evaluation. Is National Geographic?” Silence . . . . Finis.]

22 This ratio (the factor by which E-W sights are superior to N-S ones for steering) should probably be doubled ($\S$E3), since culmination-time would (in 1909 field practice) be determined by equal-altitudes. (Claiming better eyesight [than 1°] will only increase the ratio, which is about proportional to the inverse square root of the acuity proposed.) [Note added 1997/3/3: R.Bryce’s invaluable new book *Cook&Peary . . . .* (1997) produces at p.420 another Peary memo on steering. Written for the mathematician whom Peary hid at home (before producing his “data”), it shows that, in 1909 Oct, Peary didn’t yet know if he could trust the very steering method (sextant-gauged upper culmination) NavFou says ($\S$E1) Peary confidently used 6 months earlier to effect his miraculously-aimed Pole-in-one. Naturally, he never publicly claimed using such an inferior method. (So this was just another passing shade in Peary’s chameleonic spectrum of pathetically-transparent-afterthought stabs at explaining his steering. Other hues: [a] $\S$1A1; [b] Rawlins 1991 $C6$: $c$ ibid $\S$C2&D7 vs. Peary 1910 p.211.)

appears to be common: doing less work while pretending to more. G10 However, though the foregoing several examinations of fabrication show that the 1811 January record contains dishonest elements, they also imply ($\S$D3) that: [a] At least a substantial fraction of the data are real. [b] Fudging or smoothing did not ($\S$G13-G14) result in huge disagreement with real positions.

G11 A passing comment on “fudging”: fudging real data may be less reprehensible than fabricating data outright. (Though, there is unambiguous evidence of the latter recourse here: see $\S$15 & $\S$G.) But in one sense the two crimes are the same; after all, if one is forking a datum, then: *what value is it being made equal?* A fabricated one. (Or, in other arenas, a plagiarized one.) We occasionally need to be explicitly reminded of the common truth implicit in such cases.

G12 An ancient error-theory lesson. The latter point in $\S$G10 reminds one of the case of the 2nd most remote transit observer known to us, Aristyllos, a conscientious and able astronomer, who observed (presumably in Alexandria) c.260 BC30 and who (perhaps out of caution at his data’s seemingly meaningless slight inconstancy) rounded up 30. The precision is improved by a factor 31 of about 20. The precision is improved by a factor of about 20. Near the poles, the eq. 7 method is not very latitude-sensitive either, since $\sin \phi$ is effectively constant (at unity) near the North Pole. Other arenas: a plagiarized common truth implicit in such cases.

G13 Returning to point [b] in $\S$G10, we have a little mitigation of Robertson’s misbehavior: his fabrications are unlike the very many of Ptolemy or the very few of Tycho, in that the fudging is not betrayed by large discrepancies between his results and Timocharis’ data (c.300 BC: 32 no- of Blunder). The precision is improved by a factor of about 20. Near the poles, the eq. 7 method is not very latitude-sensitive either, since $\sin \phi$ is effectively constant (at unity) near the North Pole. Upshot: Aristyllos (whose accuracy was perhaps the ancients’ best — *DIO 1.2 fn 126*) is the only empirical astronomer all of whose extant data are correct. Which sounds like a compliment — until one realizes the ironic consequence of the very perfectionism which caused both the accuracy and the smoothing: he lowered the ultimate value of his hard-earned data (inadvertently degrading the precise accuracy of their mean and its standard deviation), by over-rounding them so conservatively.

G14 Though the case of Alcylene ($\S$G4) suggests (without proving) that the fabricator used the 5-wire-times-out-of-1-wire-time method ($\S$F) on occasion, the previous point ($\S$G13) poses a difficulty (even aside from fn 17) with proposing it as a common method for all the 1811 stars, namely: there seem to be approximately zero Oxford stars that are consistent with there generally having been multiple real wire-times per star. The precision is much superior to the N-S method ($\S$E6).

G15 Thus — though [a] padding is awful science, and [b] the stars’ mean accuracy is not impressive35 — still, these transit data are (in a technical sense) not entirely valueless.

G16 On the other hand: given the availability of other observatories’ entirely real raw transit data from the same era, one may doubt whether anyone today would wish to use the fudge-neatened Robertson transit material.

G17 Bottom line: there’s no patient that doctoring kills deader than empirical data.
2 Hipparchus and Spherical Trigonometry

by Curtis Wilson

A Hipparchus’ Trigonometric Equation on the Sphere

That spherical trigonometry was developed or used by Hipparchus has occasionally been claimed. According to Neugebauer, however, the solution of spherical triangles became possible only with the discovery of two theorems by Menelaus (first century A.D.). The sole evidence adduced to the contrary that I am aware of is Hipparchus’ alleged use of a formula to determine latitude from length of longest day:

\[
\tan \phi = \frac{\cos M}{\tan \varepsilon}
\]

where \(\phi\) is latitude, \(M\) is the length of the longest day converted to angle at 15° to the hour, and \(\varepsilon\) is the obliquity of the ecliptic.

B The Analemma Alternative

B1 However, the Greek equivalent to formula 1 is derivable, without use of spherical trigonometry, by analemma methods. That Hipparchus used such methods seems very likely indeed. In his Aratus Commentary, he claims to have derived “by rigorous methods” (διὰ τὸν γραμμὸν) the arc above the horizon of a star of declination 27°20′, for latitude \(\phi = 36°.4\). The problem in effect uses formula 1 backwards, with declination 27°20′ replacing the obliquity \(\varepsilon\), and \(M\) as the unknown. Hipparchus’ result was 224;15′; a present-day hand calculator gives 224;07′.

B2 To derive the Greek equivalent of (1), we use the analemma construction shown in Figure 1, which is adapted from Neugebauer’s Part I Fig.284. \(\text{OU}\) is the trace of the horizon plane, \(\text{OM}\) the trace of the equator, \(\varepsilon\) the obliquity of the ecliptic, \(\phi\) the latitude. The half circle \(VST\) represents half the Sun’s path on the day of the summer solstice, with

\[\Delta A = \arccos\left(\frac{180° - 1'}{180°}\right) = 6°\]

Including differential refraction (based on DIO 2.1 fn 17), we need 1'.3 true altitude change at the horizon to produce 1' of change in apparent altitude, so eq. 5 must be altered to:

\[\Delta A = \arccos\left(\frac{180° - 1' - 3'}{180°}\right) = 7° = 28\arcmin\]

Thus, for nearly an hour (1/2 hour either side of the “touch”), the sun’s altitude \(h\) will vary by only 1', the discernment of which (even over a smooth sea horizon) would be close to the limit of human vision. (As one gets nearer the Pole, this time becomes greater yet.)

E4 Of course, one could do better with a sextant, but no claim is made that this was used in connection with the \(\Sigma A\) note, and the expedition records contain no such sextant observations. It is therefore a fair conclusion that the remarks in the Peary \(\Sigma A\) memo do not relate to instrumental observations.

E5 Incidentally, a much better and simpler way to navigate is by transverse (E-W) observations via sextant. To estimate latitude \(\phi\), one may merely measure the difference

\[\Delta A \text{ is here about inversely proportional to the colatitude’s square root. I.e., none of the navigational methods fantasized (by Peary’s vanishing band of defenders) are successful in salvaging his claim because } all \text{ break down when closing in on the Pole — which is, after all, slightly relevant to the process of getting there. (Navigators are urged to enjoy Nav Fnd Rpt p.58, which attempts to dance around — actually to invert — this self-evident truth.) See Rawlins 1973 p.114.}\]

21 As regards lead-sledger Bartlett (fn 18): he lacked a sextant when in the lead, since Peary’s bringing-up-the-rear party carried the only sextant. (Which wasn’t used before 3:22 noon: Peary 1910 p.248.) An overview reveals that modern defenses of Peary’s steering uniformly slide past several obvious items. [a] Peary himself never explained it (even when under attack on the point: fn 2), a fact with exceedingly obvious implications. (And that is why his defenders have spent hundreds of pages attempting to invent methods for him — uniformly ignoring his suppression of his only diary statement on steering: fn 2.) [b] Again (fn 4, Rawlins 1973 p.143, 1992 fn 50 conclusion), where are the written records of Peary’s alleged solar-based corrections of compass? (Notably: no such data in the \(\Sigma A\) memo!) [c] It is redolent of remote-fringe scholarship to propose that a highly capable explorer, carrying both sextant and theodolite on his sledge (Peary 1910 p.288 note) — which he used to steer all his previous trips — would for the first time in his career suddenly decide to forego such swift, accurate, tried&true methods and instead zero in on the Pole by eyeballing slow and erratic horizon phenomena. This is apology, not history. (For an extremely simple, nonconspiratorial explanation of how Peary was forced into claiming no sextant steering data, see Rawlins 1973 pp.114, 144, 149-150: briefly summarized here at fn 2.) [d] And if, as all apologists claim, Peary was right to eschew transverse observations (en-route) in 1909, then, why did he use precisely such data (Nav Fnd Rpt pp.221-222) at the “Pole” camp, 1909/07/6 AM (70°W time, 6:40 AST)? (Another way of expressing this key difficulty with Peary’s 1909 navigational story: why take the best \([\Sigma E5]:\text{factor of } 20\)] type of steering observations only after arrival at the point one was steering for?!! See Rawlins 1973 pp.114 & 149.)

It is not required that transverse sextant shots be on the prime vertical in order to steer by them. (But \(\Sigma E5’\)’s method makes the navigational math easier.) In any case, some sort of transverse sextant observations were the standard method Peary and other explorers used for steering at a pole. Despite this, the Navigation Foundation (hired consultant to National Geographic, which used to boast it established its international renown by certifying Peary’s claim: Rawlins 1973 p.190) prominently asserted that, since Amundsen hit the South Pole (1911) without transverse sextant solar shots for longitude, then Peary could have done likewise. (See National Geographic Magazine 1990 Jan p.47, and Nav Fnd Rpt pp.61-62.) But then Ted Heckathorn discovered proof that Amundsen of course had used standard sextant observations for aiming at the S.Pole. See Rawlins 1992, the DIO 2.2 paper

[Note by DR.] Curtis Wilson (St. John’s College, P.O.Box 2800, Annapolis, MD 21404) is rightly respected as one of the world’s leading experts on Enlightenment-period mathematical astronomy. He is co-Editor of the General History of Astronomy (the majority of whose 4 Editors are not admirers of DR). The present contribution marks the 1st submission to (and, of course, publication by) DIO of a pure-Neugebauer-Mufa-viewpoint article. As ever (DIO 4.2 [7 §B43]), we encourage the submission of others. [Paper printed essentially as received. Headers & bio supplied by DR.]
be about on Peary’s horizon. (That is, the difference between the entries in the middle row of Table 2 is 87°10’ − 86°53’ = 17’ which is about equal to the solar semidiameter.) The sun’s bottom would spend over three hours beneath the horizon, while covering a range of azimuth of about 50°. Not very helpful to a navigator.

D7 Peary’s diary for 1909/3/27 makes no claim that he set his compass by such lax means, nor does it state that the sun touched the horizon, merely noting: “Sun did not set last night.”

E Imaginary vs. Real Navigation

E1 A navigator with good eyesight might try locating lower culmination by repeated altitude fixes over more than one half hour, in a manner similar to the one discussed (for upper culmination) in the Navigation Foundation Report pp.55f. But, for latitude \( \phi = 87°N, 180° \) from the Pole, if the sun’s disk just disappears (at lower culmination) in the north (upper limb at horizon), then the azimuths \( A_t \) of the two points at which the 32°-wide solar disk is touching the horizon (lower limb at horizon) are 35° on either side of the touching-point! For the polar regions, a crude calculation of \( A_t \) will be of sufficient accuracy:

\[
\cos A_t = (180° - 32°)/180°
\]

An exact equation is:

\[
\cos A_t = \sin \delta \sec \phi \sec h - \tan \phi \tan h
\]

Eq. 4 (for \( \delta = 1°58’ \), refraction 46’) yields the same result as eq. 3, namely, \( A_t = 35° \). So, as noted, this method leaves an aiming slack of about \( \pm 35° \).

E2 And attempting to estimate the midpoint by eyeball-gauging sunset & sunrise — as Molett 1989 proposes — runs into the same type of difficulty as we examined at §B4: the sun skims the horizon at such a gradual angle that it is a practical impossibility to determine when it “sets” or “rises.”

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18 A dedicated apologist may wish to argue that Bob Bartlett, not Peary, was the observer of the alleged solar horizon-touching. (Though, Bartlett is not mentioned in the §A1 note.) Bartlett’s trail-breaking party went ahead of Peary’s main party (which was immobile at 0°) late in the day on 1909/3/26. (It is clear from Peary’s diary that Bartlett arrived at 87°05’N [or less: fn 17] around the middle of 1909/3/27. [Not earlier than 10:30 AM, and probably later.] Thus, at the previous midnight, he cannot have been anywhere near 87°09’N, the horizon-touching latitude of Table 2.) Assuming Bartlett was a few miles north of 86°53’N will very slightly ease the 16’ discrepancy (Table 2) for 1909/3/27 0° — but not nearly enough to get rid of the main problem: the solar lower limb on the horizon not at true north but at two points some hours and many degrees apart. Also, if Bartlett is assumed in motion, then adding his 2 knots (2/ hr, the mean claimed sledging speed of the expedition) to the 1/ hr declination-increase of the Sun makes a total of 3/ hr linear motion superposed upon the virtually-quadratic lower culmination phenomenon — thereby throwing off (to the left) the position of solar lower culmination by about triple the previous estimate (fn 15): roughly 4°. (Thus, an observer sledging northward after 3/27 would have seen lower culmination about a quarter hour before local apparent midnight. Bartlett was traveling at midnight at this time. And Peary says he was, too, as he approached his alleged N.Pole camp.) Just one more indication that this entire approach is a somewhat imprecise aid to the determination of true north. I conclude with a compact approximate formula for the error \( E \) in solar-culmination-directed steering, where \( E \) is the distance in nmi leftward (midnight) or rightward (noon) of the North Pole one is seeking, if aiming toward (midnight) or opposite (noon) the position of observed culmination (latitude cancels out of the problem if it is expressed thusly): \( E = 9 \sin \epsilon \cos \alpha + 11v/3 \), where \( v \) = traveler’s sunward velocity in knots, \( \epsilon \) = obliquity of ecliptic, \( \alpha \) = solar right ascension. (It is assumed that one is in the Arctic, and the tiny ellipticity of the Earth’s orbit is ignored.) At midnight in early Spring, this becomes nearly \( E = 3.6(1 + v) \); so, for Peary’s claimed speed (\( v = 2 \) knots), \( E \) is over 10 nmi to the left of the Pole.

C Wilson Hipparchus & Sph Trigonometry 1997 Feb DIO 7.1 ¶2

S the point where the Sun sets; this half circle, originally at right angles to the plane of the diagram, has been swung about TV through 90° so as to lie in the plane of the figure. The arc ST therefore represents half the longest day, or M/2, and the angle \( \angle n \) shown in the diagram is half the excess of M/2 over 90°. Then

\[
VT = 2rd = \text{crd}(180° - 2\epsilon)
\]

and

\[
UR = \frac{r_2 \text{crd}2n}{2}
\]

so that

\[
\frac{UR}{RO} = \frac{\text{crd}2\phi}{\text{crd}(180° - 2\phi)} = \frac{r_2 \text{crd}2n}{\text{crd}2\epsilon}
\]

Given that half the chord of the double angle is equal to the sine, and \( \sin n = -\cos(90° + n) \), then (5) transforms into (1).

C DIO Position Not Justified

If, then, Hipparchus’ use of (5) is the basis for the claim that he had spherical trigonometry at his command, the claim is unwarrented.

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Figure 1. Rendition by KP & DR.
3 Hipparchos at Lindos: a Modest Confirmation

by DR

A Hipparchos’ Adopted Latitude

A1 While preparing the preceding article (§2) for publication, I became curious about the modest disagreement regarding Hipparchos’ calculation of the time $M$ which the chosen star (ν Boo) — of declination $\delta = 27\,^{\circ}\,1/3$ — spends above the true horizon: the $M$ values computed by Neugebauer (224°06’; Neugebauer 1975 p.302) and Wilson (224°07’; §2 §B1) do not agree with that cited by Hipparchos, whose report is more precise than is usual for Hipparchos Comm phenomena. Stellar $\delta$ is effectively given to 2° precision, and $M$ is evidently being expressed to the nearest timemin.2 $M = 15 - 1/20$ hours = 14°57’ = 224°1/4 (Hipparchos Comm 2.2.26; pp.150-151).4

A2 In Rawlins 1994L, we found that Hipparchos’ assumed latitude $\phi = 36°08’$ (Lindos vicinity) for calculating declinations from zenith distance observations. If we try that value (instead of the generally accepted round figure $\phi = 36°$) in §2 eq. 1, then we find $M = 224°21’$ which rounds to 14°57’ or 15 – 1/20 hours, as reported (§A1). By contrast, if we use $\phi = 36°$ in the calculation, the rounded result5 is $M = 14°56°00’$ or 15 – 1/15 hours, not Hipparchos’ stated value.

B Excluding 36’ N

B1 Next, we instead invert the problem and (via §2 eq. 1) simply seek $\phi$ from the attested Hipparchos values (§A1) for $M$ & $\delta$.

36’ it was not possible for an observer to see the solar disk intersect the northern horizon on both dates cited in the §A1 note. No adjustment of dates or refraction constants can change that essential fact.

D4 Let us take a closer look. At 0:00 LAT of 1909/3/26, solar declination $\delta = +1°57’$. With refraction 46’ (fn 16) and solar semidiameter 16’, we may easily find the latitude $\phi$ (along the 70°W meridian) where an observer could see the midnight sun’s bottom (lower limb) touch13 the horizon:

$$\phi = 90°00’ - 1°57’ - 46’ + 16’ = 87°33’N$$

(2)

D5 Table 2 supplies declination data at 0:00 LAT for March 26, 27, & 28, as well as (with 46’ of refraction)16 the latitudes17 (for those same times): touching and claimed.

D6 From Table 2, it is clear that for 0° on 1909/3/26 & 3/28, the sun does not come anywhere near a touch — the entire disk goes well below the horizon on the former date, and well above it on the latter. At 0° on 1909/3/27, the sun’s center (not lower limb) would

15 Due to declination-variation, the Sun’s lower culmination here (c.87°1/4 N) occurred, for a fixed observer, about 5° before local apparent midnight. (A moment which, by chance, was almost exactly local mean midnight.) Thus, lower culmination was over 1° to the left of true north — and such a systematic error (intrinsic to the horizon-touch aiming-notion) will grow rapidly as one approaches the Pole (and will be larger yet if the observer is moving northward while detecting the touch: fn 18) as will the already disbelievably-large random uncertainties indicated elsewhere here.

16 During these days, Peary’s diary makes the temperature about ~30°F, for which DIO refraction (fn 11) is 46°.1 — and we again (idem) round to whole arcmin.

17 Based on accounts of the expedition, as condensed in the valuable chart of adulatory biographer Wm. H. Hobbs (Peary NYC 1936 p.344 opp). The 87°38’ N latitude is directly based upon R. Marvin’s 1909/3/25 sextant sight (made about 1/2 a day before 3/26 00:00 LAT). The following days’ figures were gotten by adding, to Marvin’s figure, the Peary diary’s often over-optimistic (see also Kane & Hayes) dead-reckoning march-estimates, 15 nmi & 12 nmi, respectively. (The 1909/3/28 camp’s latitude was estimated as 87°12’N on p.262 of Peary 1910 and as 87°15’N at ibid p.338.) The next sextant sight (1909/4/1) showed that the expedition was 15 nmi south of where its exaggerated dead-reckoning estimates had placed it. The discrepancy was (diary & Peary 1910 p.268) blamed on wind. The diary dead-reckoning figures in nmi for the 5 marches between 1909/3/26&3/31 are: 15, 12, 12, 20, & 23 — total 82 nmi, vs. 67 nmi = difference of sextant sights (87°45’N – 86°38’N). Even accepting the shaky Bartlett sextant-sight at face value, this indicates a dead-reckoning exaggeration-factor of 82/67 or about 1.22; dividing that factor into the figures claimed for the 1st two marches and adding to 86°38’N, we find that 86°50’N & 87°00’N are more likely than Table 2’s dead-reckoning-based estimates (86°53’N & 87°05’N) for Peary’s actual respective 1909/3/26&27 latitudes. Such dreamy overestimates as 20 nmi & 23 nmi are accepted as real by Molett 1989 p.144, without noting the 1.22-factor discrepancy. The 20+ nmi/march claims continued from there to the “Pole” camp (1909/4/6-7), during the allegedly-high-speed-though-unfortunately-not-verified final dash, where the trail was now hewn by Henson not Bartlett, even though Peary’s 1906 diary scoffed at the former’s drive (Rawlins 1991 §D4). Peary’s opinion had not improved in 1909, when the 42-yr-old Henson was 3 more years past his exploring prime; see Peary 1910 p.240 and diary 1909/3/22 (similar to 5/23): “Henson still in his igloo as usual.”

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1See K.Pickering at DIO 2.1 §2 [F10].

2The star’s $\delta$ ends in 1/3, which means that pre-rounded $\delta$ was between 27°17’1/2 and 27°22’2. (Hipparchos used degree-fifths for declinations: Rawlins 1994L §§F2&F4.) However, $\delta$’s precision does not affect the ancient calculation which is the subject of this paper — since it just used $\delta = 27°1\,1/3$.\footnote{Exact result: $M = 14°56°27’$.}

3The hour-stars of Hipparchos Comm 3.5 are sometimes expressed to 30ths or 20ths of hours — a one-timemin discrimination.

4Neugebauer 1975 p.302 n.10 correctly reports that Manitius confuses hour-fraction with timemin: Hipparchos Comm pp.151 & 298. Neugebauer 1975 p.166 n.3 suggests just the same type of scribal slip by an ancient. Note that precisely this sort of error turned out to be the ancient source of the slight discrepancy (in Almajest 4.11) of Hipparchos’ assumed mean distance (of the Moon) for his eclipse-trio B analysis vs. that assumed for his trio A analysis. (See Rawlins 1991W §G3.)

5The hour-stars of Hipparchos Comm 3.5 are sometimes expressed to 30ths or 20ths of hours — a one-timemin discrimination.

6Due to declination-variation, the Sun’s lower culmination here (c.87°1/4 N) occurred, for a fixed observer, about 5° before local apparent midnight. (A moment which, by chance, was almost exactly local mean midnight.) Thus, lower culmination was over 1° to the left of true north — and such a systematic error (intrinsic to the horizon-touch aiming-notion) will grow rapidly as one approaches the Pole (and will be larger yet if the observer is moving northward while detecting the touch: fn 18) as will the already disbelievably-large random uncertainties indicated elsewhere here.

7Based on accounts of the expedition, as condensed in the valuable chart of adulatory biographer Wm. H. Hobbs (Peary NYC 1936 p.344 opp). The 87°38’ N latitude is directly based upon R. Marvin’s 1909/3/25 sextant sight (made about 1/2 a day before 3/26 00:00 LAT) of Hipparchos’ assumed mean distance (of the Moon) for his

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Table 2: Peary’s Position vs. Where the Sun Touches the Horizon

<table>
<thead>
<tr>
<th>Date</th>
<th>Decl</th>
<th>Touching Latitude</th>
<th>Peary Latitude</th>
</tr>
</thead>
<tbody>
<tr>
<td>1909/3/26</td>
<td>1°57’</td>
<td>87°33’N</td>
<td>86°38’N</td>
</tr>
<tr>
<td>1909/3/27</td>
<td>2°20’</td>
<td>87°10’N</td>
<td>86°53’N</td>
</tr>
<tr>
<td>1909/3/28</td>
<td>2°44’</td>
<td>86°46’N</td>
<td>87°05’N</td>
</tr>
</tbody>
</table>
Table 1: Solar Lower-Limb-Touch Horizon Azimuths Along 70°W

<table>
<thead>
<tr>
<th>Date</th>
<th>Rise</th>
<th>Set</th>
</tr>
</thead>
<tbody>
<tr>
<td>latd</td>
<td>LAT Azimuth</td>
<td>latd LAT Azimuth</td>
</tr>
<tr>
<td>1909 Mar 21</td>
<td>85°26'N</td>
<td>5:30 +82°4</td>
</tr>
<tr>
<td>1909 Mar 22</td>
<td>85°43'N</td>
<td>5:07 +76°6</td>
</tr>
</tbody>
</table>

24°/day (or 1°/hour). Table 1 provides the solar azimuth at the horizon (i.e., rising or setting) at lower-limb-touch on the two equinox days mentioned, together with Peary’s claimed latitude (latd) and the local apparent time (LAT) when each event occurred. (For −40°F temperature, the compact formulas of DIO 2.1 §3 fn 17 find 48° of refraction at the horizon, and that value is adopted for the discussions of this section, including Table 1.)

C2 The azimuth changes are so rapid just from morning to night that anyone wanting to adjust his compass via solar azimuth can certainly not be (as in Peary’s §A1 note) blithely cavalier about which day to do it on.

C3 Moreover, for both dates (March 21 & 22), when the Sun was actually “due E. & W.” (§A1), it was not rising or setting — to the contrary, every part of its disk was above the horizon by an amount exceeding that disk’s diameter! In fact, as we see from Table 1, no part of the Sun contacted the horizon within 7° of “due E. & W.” on either date.

D Horizon-Touching

D1 So much for the “accurate [equinox] check on compasses”. Now to examine the matter of the Sun allegedly just touching13 the northern horizon on March 26 and March 27. This obviously refers to the time when the Sun’s lower limb is coincident with the horizon.13

D2 Peary’s description (§A1) of the Sun “touching” the horizon on March 26 and 27 is particularly noteworthy, since it requires that between those two dates he suddenly ceased his life’s obsession poleward march and — at double his usual daily speed — raced southward 24 nautical miles (mi). (I.e., solar declination increased 24° during the 24°.)

D3 The difficulty satirized by §D2 is this: the Peary expedition was claiming (e.g., Peary diary 1909 March 22) about 12 nmi (12°) per day, while the Sun’s declination was increasing almost 24° per day; thus the Sun’s midnight altitude above the northern horizon was increasing in notches of about 36° every day — an amount which is more than double the solar semidiameter (16°). The rapidity of the phenomenon therefore makes it unlikely a priori that a neat “touch” will occur. And, since the Sun’s entire 32° diameter is less than 24°/day (or 1°/hour), Table 1’s sunset latitudes are found at Peary 1910 pp.338&352, respectively, while the sunrise latitudes are interpolations from these data and the Peary diary’s travel schedule.

10 Table 1’s sunset latitudes are found at Peary 1910 pp.338&352, respectively, while the sunrise latitudes are interpolations from these data and the Peary diary’s travel schedule.

11 Peary’s diary entries for this time indicate a mean temperature of about −40°, for which, the DIO formulas yield refraction 48°.0. Subtraction of solar parallax makes rscp a bit less, but we will round to whole arcmin, given the uncertainty (fn 8) of near-horizon refraction.

12 As elsewhere here, apologists may be temporarily tempted to try accenting the unevenness of the real rather than ideal horizon. But a moment’s reflection will reveal that this factor brings much more harm than aid to the cause of defending Peary’s note. (The theoretical horizon is simply the great-circle locus of points 90° from the zenith. For a person of normal height, dip would put a sea horizon at 90°02′ from the zenith, a trifling adjustment which is in any case wiped out by the roughness [and comparable height] of an ice-horizon.)

13 However, assuming upper limb does not salvage the §A1 memo’s credibility.

14 More exactly: the mean daily variation of solar declination during the days under discussion here (March 26-28) was about 23°1/2.

B2 The result: φ = 36°05′. (Which is the real value — as well as the anciently-known value — for the latitude of Lindos: §C.) Taking M′, the precision as (timedimensions, we check solutions for M between 14°56′1/2 & 14°57′1/2, finding that this constrains φ to the range:

\[ 36°00′22″ < φ < 36°09′09″ \]  

— which does not include the usually-presumed Hipparchos latitude φ = 36°N.

B3 Moreover, the statistical analyses of Nadal & Brunet 1984 (see, e.g., their Table 5) concluded that the latitude used in Hipparchos’ calculations was not equal to 36°N but was a little higher.

C Lindos Re-Indicated

Thus, the foregoing provides a bit of confirmation of recent novel evidence (§A2) indicating that Hipparchos’ main observatory was in the vicinity of Lindos (φ = 36°05′N: §B2) — on Rhodos,8 the Mediterranean island he is known to have worked at.

References

O.Neugebauer 1975. History of Ancient Mathematical Astronomy (HAMA), NYC.
D.Rawlins 1994L. DIO 4.1 §3.

Note added 2016:

In 2012, at DIO 20 3 §A3, we caught on at last to what had been right in front of us for years: Hipparchos’ eclipse calculations and his klimata mutually confirm two historically important discoveries:

[1] Hipparchos’ mechanical calculational ability was unerring, and
[2] his era’s trig tables were accurate to 1°.

These realizations make the foregoing “modest” exploratory paper a good deal less modest. They mathematically confirm our finding by a completely independent induction (Rawlins 1994L or above at fn 8) that in his calculations Hipparchos used 36°08′ for the geographical latitude of his observatory, which was very near Lindos.

8Rawlins 1994L fn 50.
7At first, it may look as if the left bound in eq. 1 can be rounded to 36°; however, one must realize that 36°00′22″ is not a calculational output, but is instead simply the lowest input that will keep computed M′ within the bounds established in §B2.
8Rawlins 1994L §F3 showed only that Hipparchos’ adopted value for his main observatory’s latitude ended in precisely 08′. However, from the Catalog’s antarctic circle, Rawlins 1982C (eq.14) had already showed that Hipparchos’ Ancient Star Catalog was observed at about north latitude 36°2±0′.4. Combining this information with the fact that Hipparchos’ declinations indicate an observatory-placement error of 0′±1′ in latitude, Rawlins 1994L §G3 concluded that his central observatory was at 36°08′N±01′; near Lindos — probably just north of it.
On the Navigation of Polar Explorer Robert Peary
by Hanne Dalgas Christiansen

A Peary’s Curious Memo

A1 Considering the still-simmering discussion of how close Robert E. Peary came to the North Pole in 1909, and the scant evidence of his navigational methods, a revealing note (found among his papers) seems to have hitherto received too little attention. [Though, Peary-defender Wm. Molett 1989 p.142 calls this “probably the most important document in all the Peary archives as pertains to his navigation”. Compare to fn 2.] It is a memo, in Peary’s distinctive hand (Peary 1909 records, official US National Archives microfilm, frame #0267), which reads:

The sun setting due E. & W. Mar. 21 & 22 gave accurate checks on compasses, also just touching northern horizon Mar. 26 & 27.

A2 Molett 1989 pp.142-143 argues that this note explains Peary’s steering. However, as stated, the note is unrealistic for two reasons — so unrealistic that, had Peary even tried either method he would never have written the note in that form. These reasons are: [1] the rapid change of solar declination around equinox and [2] the slow passage of the sun through the horizon so close to the Pole. Below, it is shown (§E5) how a much surer orientation can be obtained by using transverse (E-W) sextant observations.

B Orientation

B1 It is hard to determine direction to the Pole when travelling over shifting ice in a world without landmarks, comparable to the difficulty of locating a tiny underwater reef in the Pacific from a canoe. Celestial navigation is in some respects hampered (in others, aided: Rawlins 1973 p.154) by the almost horizontal daily rotation of the skies. (For discussions of navigational methods proper to the problem, see, e.g., Mohn 1915, Rawlins 1973 &

[All footnotes by DR.] Thoroughly brought up in the mathematical sciences, Hanne Christiansen is one of the most technically competent of scientific historians. (She was first introduced to the field by a stimulating series of discussions with the late highly respected University of Copenhagen mathematician & historian, Olaf Schmidt, a retiring but unusually able and principled scholar.) Though she is now research associate professor in the History and Prehistory of Astronomy, we are grateful that she occasionally delights us with excursions of the present type, which point up her exceptional ability to discern what everyone else has overlooked. (Mailing address: Sauntesjev 28 A, DK-2820 Gentofte, DENMARK. Telephone: +45-3965-2524.) [Typically scrupulous note by HDC: It has pleased the publisher occasionally to couch my rather straightforward paper in nontrivial deletion (or Rawlins 1992 x] for the only statement Peary made in his memoir regarding the setting and rising on March 21st and 22nd gave an east and a west that was easily converted to a usable compass heading to the Pole from his locations at the time.” However, the §A1 memo neglects to impart this heading. (See below: fn 20.) And in 1911 Peary contradicted the memo by telling Congress that in 1909 he did not determine the direction of the compass: Rawlins 1991 §C2.

C Solar Shifting

C1 But that is not the only difficulty with the Peary memo (§A1), for the sun’s declination does not stay constant (as we assumed) — instead, it increased to completely cross the horizon — which corresponds to 7° of azimuthal motion. Such slackness suggests that observing sunset is a less than ideal method for precision-checking of compasses’ orientation.

[Rawlins 1991 §C6&D1 (or Rawlins 1992 §K4) for the only statement Peary made in his diary attempting to explain his 1909 steering: “setting course by moon, our shadows, etc.” (This is a rough method which would have been replaced by sextant observations when Peary began closing in on the Pole — a process which was aborted at birth on 1909/4/6-7 when he confirmed how impossibly far he was from his goal: fn 20.) Note the striking coincidence that this is the sole nontrivial deletion he made when reading his diary extensively to Congress in 1911. See Rawlins 1991 §C6. Note also the point (ibid §C4) emphasized by Oliver M. Miller of the American Geographical Society.

Rawlins 1992 fn 44 cooks a playful eyebrow at the Peary memo’s report that his party observed sunset in the east.

4 The Nav Fnd Rpt (pp.49 & 55) treats the §A1 memo as navigationally sound; p.55: “The sun’s setting and rising on March 21st and 22nd gave an east and a west that was easily converted to a usable compass heading to the Pole from his locations at the time.” However, the §A1 memo neglects to impart this heading. (See below: fn 20.) And in 1911 Peary contradicted the memo by telling Congress that in 1909 he did not determine the direction of the compass: Rawlins 1991 §C2.

5 The estimate is 85°33’N at Peary 1910 p.338.

6Peary’s 1911 statement before Congress. (See Rawlins 1991 §C3 and Rawlins 1992.) Acknowledged by the Navigation Foundation (e.g., Washington Post 1993/6/1).

7Near the Vernal Equinox, variation of declination during the setting process will lengthen that process; the same effect will shorten the rising process. Vice-versa for Autumn Equinox.

8 See the 1990 descriptions, by B.Schafer & W.Liller (PASP 102:796), of the large fluctuations in atmospheric refraction very near the horizon even in temperate climes, variations which it is well known will only be more exaggerated & unpredictable in the polar regions.

9At this latitude, around Vernal Equinox: during the half-hour (eq. 1) the solar disk requires to set, the solar declination will increase by about a half an arcmin.