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DIO

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News Notes: *DIO* had *quite* a 1996 . . . .

[a] On 1996/5/9, 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 *NYTimes*, *DIO* feels humbly and admiringly obliged to take explicit note of the fact that the Science Dep't of the *NYTimes* 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 accepted[1] (the Neugebauer-Muffia[2] 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 53.4*:423 (1996 July) congratulatorily praised[3] the "excellent" and "thorough" achievement of *DIO*'s pioneering edition of Tycho's Star Catalog.

[1] *DIO 6* ‡1 fn 137.

[2] Whose Princetitute-backed math-follies shine at ‡5 §B, *DIO 4.1* ‡4, & *DIO 6* ‡1 fnn 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 know joy, by sharing DR's jocular view of their pretensions: e.g., *DIO 1.3* ‡10.) NB: Nowhere here or in other organs of the cowering, shakily-"established" world-of-science-history, do we find the *scientific accuracy* of the Catalog's offending criticisms evaluated. Sense of proportion: *DIO*'s Catalog took a total of about **one** cheery page (*DIO 3* §L8 & fn 97) to *counter*-undo 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 megassault. And the dithering Hist.sci Soc. is deaf&dumber to suggestions that such matters be refereed: *DIO 4.2* ‡7 §§B29 & B44.) For further math-funnies 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-gutting Muffia muffs: *DIO 4.1* ‡4 §A. With the lone [and valued] exception of A.Jones [*JHA* 1995 May], none of these 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* §J7: 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 a rock whose Sisyphan permanence matches Muffia unfalsifiability: despite stern, criticism-Muffling cult-omertà, plus 50^y of political-economic intrigue (*DIO 1.2* fn 172), aimed at finally getting the field taken seriously, Hist.sci still draws a wink-&-a-prayer from competent mathematical scientists who know it 1st-hand. Given the foregoing 64-to-1-myopia-spectacle (& ‡5 §B4): who can blame us?

‡1 Fabricated Transit Data by Abram Robertson

by E. Myles Standish[1]

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A 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[2] recorded with a five-wire telescopic 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 (3^{rd}) 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 2^{nd} or 4^{th} wire is listed to one-quarter or three-quarters of a second of time ("xx.25" or "xx.75").

[1] [Note by DR.] Myles Standish is prime author of the internationally renowned solar, lunar, & planetary tables of the *Astronomical Almanac*, published annually by the US Naval Observatory and the Royal Greenwich Observatory. A recent Chair of the American Astronomical Society's Division on Dynamical Astronomy, Standish is one of the most deservedly admired of astronomers, both for his accomplishments and for his concern for organized science's maintenance of high scholarly and ethical standards.

[2] Astronomers have used transit circles at least since Timocharis (Alexandria, early 3^{rd} century BC), to take advantage of the fact that spherical astronomy becomes simple arithmetic on the meridian (the celestial great circle containing the Earth's poles and the observer's zenith-nadir axis). "Transit" means literally crossing that meridian. The objects Robertson observed in transit were mostly stars (predominantly bright ones), but also included solar & lunar limbs, and planet-centers.

Table 1: Abram Robertson's 1811 January Transit Data and Wire-Sums

Day	Wire 1	Wire 2	Middle Wire	Wire 4	Wire 5		1+5	2+4	2·Mid
6	$2^m54^s.6$	$18^s.3$	$19^h\ 3^m41^s.8$	$4^m\ 5^s.3$	$28^s.7$	*	$23^s.3$	$23^s.6$	$23^s.6$
	5^m16^s	$39^s.4$	$19^h\ 6^m\ 2^s.9$	$26^s.5$	50^s	*	6^s	$5^s.9$	$5^s.8$
	7^m17^s	30^s	$2^h\ 7^m53^s$	8^m16^s	39^s	*	56^s	46^s	46^s
	49^m45^s	$50^m\ 8^s$	$2^h50^m31^s$	54^s	51^m17^s		2^s	2^s	2^s
	$25^s.5$	25^m48^s	$4^h26^m11^s$	34^s	57^s	*	$22^s.5$	22^s	22^s
7	34^m50^s	35^m21^s	$20^h35^m52^s$	23^s	36^m54^s		44^s	44^s	44^s
	9^s	32^s	$4^h25^m55^s.5$	26^m18^s	41^s	*	50^s	50^s	$51^s.0$
	$31^s.5$	3^s	$5^h\ 2^m35^s$	$3^m\ 6^s$	37^s	*	$8^s.5$	9^s	10^s
	21^s	46^s	$5^h14^m11^s$	36^s	$15^m\ 1^s$		22^s	22^s	22^s
8	53^m24^s	$48^s.5$	$22^h54^m13^s$	$37^s.5$	$55^m\ 2^s$		26^s	$26^s.0$	26^s
11	15^m43^s	17^m57^s	$2^h20^m\ 9^s$	22^m25^s	24^m41^s	*	24^s	22^s	18^s
	$10^m\ 2^s$	$24^s.75$	$4^h10^m47^s.5$	$11^m10^s.75$	11^m33^s	*	35^s	$35^s.50$	$35^s.0$
	15^m37^s		$4^h16^m23^s$		$17^m\ 9^s$		46^s		46^s
	$23^m\ 4^s$	$26^s.75$	$4^h23^m49^s.5$	$24^m12^s.25$	35^s		39^s	$39^s.00$	$39^s.0$
	12^m15^s	$40^s.25$	$5^h13^m\ 5^s.5$	$30^s.75$	56^s		11^s	$11^s.00$	$11^s.0$
13	$27^m50^s.5$	$28^m18^s.5$	$18^h28^m46^s.5$	$29^m14^s.5$	$42^s.5$		$33^s.0$	$33^s.0$	$33^s.0$
	$33^m27^s.8$	$51^s.5$	$19^h34^m15^s.3$	$39^s.1$	$35^m\ 2^s.8$		$30^s.6$	$30^s.6$	$30^s.6$
	35^m48^s	$36^m11^s.7$	$19^h36^m35^s.5$	$59^s.3$	37^m23^s		11^s	$11^s.0$	$11^s.0$
15	26^m15^s	26^m43^s	$18^h27^m11^s$	27^m39^s	$28^m\ 7^s$		22^s	22^s	22^s
	$51^m25^s.5$	50^s	$22^h52^m14^s$	39^s	$53^m\ 4^s$	*	$29^s.5$	29^s	28^s
	$14^m58^s.6$	$15^m20^s.7$	$2^h15^m42^s.8$	$16^m\ 4^s.9$	27^s		$25^s.6$	$25^s.6$	$25^s.6$
	$21^m\ 3^s.8$	$30^s.1$	$2^h21^m56^s.4$	$22^m22^s.7$	49^s		$52^s.8$	$52^s.8$	$52^s.8$
	$37^m32^s.4$	$56^s.8$	$2^h38^m21^s.2$	$45^s.6$	39^m10^s		$42^s.4$	$42^s.4$	$42^s.4$
	$44^m\ 4^s.2$	$26^s.4$	$2^h44^m48^s.6$	$45^m10^s.8$	33^s		$37^s.2$	$37^s.2$	$37^s.2$
	23^s	56^s	$3^h\ 8^m29^s$	$9^m\ 2^s$	35^s		58^s	58^s	58^s
	$13^m33^s.4$	$56^s.3$	$3^h14^m19^s.2$	$42^s.1$	$15^m\ 5^s$		$38^s.4$	$38^s.4$	$38^s.4$
	$20^m52^s.3$	$21^m14^s.6$	$3^h21^m36^s.9$	$59^s.2$	$22^m21^s.5$		$13^s.8$	$13^s.8$	$13^s.8$
	$57^m32^s.1$	$58^m\ 5^s.2$	$3^h58^m38^s.3$	$59^m11^s.4$	$44^s.5$		$16^s.6$	$16^s.6$	$16^s.6$
	5^m52^s	6^m15^s	$4^h\ 6^m38^s$	$7^m\ 1^s$	24^s		16^s	16^s	16^s
	21^m54^s	22^m17^s	$4^h22^m40^s$	$23^m\ 3^s$	26^s		20^s	20^s	20^s
16	48^m23^s	$48^m58^s.5$	$17^h49^m34^s$	$50^m\ 9^s.5$	45^s		8^s	$8^s.0$	8^s
	18^m31^s	$18^m54^s.5$	$19^h46^m18^s$	$46^m41^s.5$	$47^m\ 5^s$		36^s	$36^s.0$	36^s
	47^m51^s	$48^m14^s.5$	$19^h48^m38^s$	$49^m\ 1^s.5$	25^s		16^s	$16^s.0$	16^s
17	16^s	$\mathit{3}^m39^s$	$4^h\ 6^m\ 2^s$	25^s	48^s		4^s	4^s	4^s
	13^m49^s	14^m12^s	$4^h14^m35^s$	14^m58^s	15^m21^s		10^s	10^s	10^s
	21^m18^s	21^m41^s	$4^h22^m\ 4^s$	22^m27^s	22^m50^s		8^s	8^s	8^s
	25^m31^s	$26^m\ 1^s$	$4^h26^m31^s$	$27^m\ 1^s$	27^m31^s		2^s	2^s	2^s
	$17^s.4$	$38^m44^s.8$	$4^h39^m12^s.2$	$34^m39^s.6$	$35^m\ 7^s$		$24^s.4$	$24^s.4$	$24^s.4$
	41^m49^s	42^m15^s	$4^h42^m41^s$	$43^m\ 7^s$	33^s		22^s	22^s	22^s
	47^m44^s	48^m19^s	$4^h48^m54^s$	49^m29^s	$50^m\ 4^s$		48^s	48^s	48^s
	$8^m59^s.5$	$9^m24^s.75$	$5^h\ 9^m50^s$	$10^m15^s.25$	$40^s.5$		$40^s.0$	$40^s.00$	40^s
	$\mathit{1}8^m56^s.6$	$38^m19^s.2$	$5^h38^m41^s.8$	$39^m\ 4^s.4$	27^s		$23^s.6$	$23^s.6$	$23^s.6$
18	$28^s.5$	52^s	$19^h54^m15^s.5$	54^m39^s	$55^m\ 2^s.5$		$31^s.0$	31^s	$31^s.0$
	48^s	$56^m11^s.5$	$19^h56^m35^s$	$56^m58^s.5$	57^m22^s		10^s	$10^s.0$	10^s
	$0^m\ 6^s.8$	$29^s.6$	$6^h\ 0^m52^s.4$	$1^m15^s.2$	38^s		$44^s.8$	$44^s.8$	$44^s.8$
19	59^m27^s	$50^s.5$	$20^h\ 0^m14^s$	$0^m37^s.5$	$1^m\ 1^s$		28^s	$28^s.0$	28^s
	1^m46^s	$2^m\ 9^s.5$	$20^h\ 2^m33^s$	$2^m56^s.5$	3^m20^s		6^s	$6^s.0$	6^s
	11^m13^s	11^m46^s	$3^h12^m19^s$	12^m52^s	13^m25^s		38^s	38^s	38^s

Table 1: Abram Robertson's 1811 January Transit Data and Wire-Sums

Day	Wire 1	Wire 2	Middle Wire	Wire 4	Wire 5		1+5	2+4	2·Mid
	29^m51^s	30^m23^s	$3^h30^m55^s$	31^m27^s	31^m59^s		50^s	50^s	50^s
	47^m14^s	47^m41^s	$3^h48^m\ 8^s$	48^m35^s	$49^m\ 2^s$		16^s	16^s	16^s
	25^m42^s	$26^m\ 5^s$	$4^h26^m28^s$	26^m51^s	27^m14^s		56^s	56^s	56^s
	$49^m42^s.8$	$50^m11^s.6$	$4^h50^m40^s.4$	$51^m\ 9^s.2$	51^m38^s		$20^s.8$	$20^s.8$	$20^s.8$
	59^m13^s	35^s	$4^h59^m57^s$	0^m19^s	0^m41^s		54^s	54^s	54^s
	$12^m21^s.4$	49^s	$5^h13^m16^s.6$	$44^s.2$	$14^m11^s.8$		$33^s.2$	$33^s.2$	$33^s.2$
	$28^m35^s.9$	$29^m\ 0^s.3$	$5^h29^m24^s.7$	$49^s.1$	$30^m13^s.5$		$49^s.4$	$49^s.4$	$49^s.4$
	$40^m38^s.4$	$41^m\ 5^s$	$6^h41^m31^s.6$	$58^s.2$	$42^m24^s.8$		$3^s.2$	$3^s.2$	$3^s.2$
20	24^s	$47^s.2$	$20^h\ 7^m10^s.4$	$7^m33^s.6$	$7^m56^s.8$		$20^s.8$	$20^s.8$	$20^s.8$
	$8^m43^s.8$	$9^m\ 7^s$	$20^h\ 9^m30^s.2$	$9^m53^s.4$	$10^m16^s.6$		$0^s.4$	$0^s.4$	$0^s.4$
	29^m34^s	$30^m\ 6^s.2$	$3^h30^m38^s.4$	$31^m10^s.6$	$31^m42^s.8$		$16^s.8$	$16^s.8$	$16^s.8$
	46^m55^s	47^m22^s	$3^h47^m49^s$	48^m16^s	43^s		38^s	38^s	38^s
	$17^m53^s.6$	$18^m17^s.2$	$4^h18^m40^s.8$	$19^m\ 4^s.4$	28^s		$21^s.6$	$21^s.6$	$21^s.6$
	$25^m25^s.8$	25^m48^s	$4^h26^m11^s.8$	$\mathit{34}^m34^s.8$	26^m57^s	*	$22^s.8$	$22^s.8$	$23^s.6$
	48^m31^s	$49^m\ 1^s.25$	$4^h49^m31^s.5$	$50^m\ 1^s.75$	32^s		3^s	$3^s.00$	$3^s.0$
	58^m55^s	59^m17^s	$4^h59^m39^s$	$0^m\ 1^s$	23^s		18^s	18^s	18^s
	2^m50^s	3^m21^s	$5^h\ 3^m52^s$	4^m23^s	54^s		44^s	44^s	44^s
	14^m36^s	$15^m\ 1^s$	$5^h15^m26^s$	51^s	16^m16^s		52^s	52^s	52^s
	$36^m36^s.7$	$0^s.9$	$5^h37^m25^s.1$	$49^s.3$	$38^m13^s.5$		$50^s.2$	$50^s.2$	$50^s.2$
	45^m18^s	40^s	$5^h46^m\ 2^s$	24^s	46^s		4^s	4^s	4^s
21	46^m35^s	$47^m\ 3^s$	$2^h47^m31^s$	59^s	48^m27^s		2^s	2^s	2^s
	52^m34^s	52^m56^s	$2^h53^m18^s$	53^m40^s	$54^m\ 2^s$		36^s	36^s	36^s
	10^m37^s	11^m10^s	$3^h11^m43^s$	12^m16^s	49^s		26^s	26^s	26^s
	16^m58^s	$17^{m}20^s$	$3^h17^m42^s$	$18^m\ 4^s$	26^s		24^s	24^s	24^s
	31^m28^s	31^m51^s	$3^h32^m14^s$	37^s	$33^m\ 0^s$		28^s	28^s	28^s
	$25^m\ 8^s$	31^s	$4^h25^m54^s$	17^s	26^m40^s		48^s	48^s	48^s
	47^m14^s	48^m44^s	$4^h49^m14^s$	49^m44^s	50^m14^s		28^s	28^s	28^s
	58^m38^s	$59^m\ 0^s$	$4^h59^m22^s$	59^m44^s	$0^m\ 6^s$		44^s	44^s	44^s
	6^m31^s	6^m53^s	$5^h\ 7^m15^s$	7^m37^s	59^s		30^s	30^s	30^s
	14^m19^s	14^m44^s	$5^h15^m09^s$	15^m34^s	15^m59^s		18^s	18^s	18^s
	26^m41^s	$27^m\ 3^s$	$5^h27^m25^s$	27^m47^s	$28^m\ 9^s$		50^s	50^s	50^s
	$45^m\ 0^s$	45^m22^s	$5^h45^m44^s$	$46^m\ 6^s$	46^m28^s		28^s	28^s	28^s
24	$22^m\ 7^s$	30^s	$20^h22^m53^s$	23^m16^s	39^s		46^s	46^s	46^s
	24^m25^s	48^s	$20^h25^m11^s$	34^s	57^s		22^s	22^s	22^s
25	5^m33^s	$55^s.5$	$3^h\ 6^m18^s$	$40^s.5$	$7^m\ 3^s$		36^s	$36^s.0$	36^s
	$28^m\ 6^s$	28^m38^s	$3^h29^m10^s$	29^m42^s	30^m14^s		20^s	20^s	20^s
	$51^m\ 3^s.4$	$51^m37^s.6$	$3^h52^m11^s.4$	$52^m45^s.2$	53^m19^s	*	$22^s.4$	$28^s.8$	$28^s.8$
	1^m33^s	1^m55^s	$4^h\ 2^m17^s$	2^m39^s	$3^m\ 1^s$		34^s	34^s	34^s
	22^m17^s	22^m46^s	$4^h23^m15^s$	23^m44^s	24^m13^s		30^s	30^s	30^s
	$13^m\ 8^s$	13^m33^s	$5^h13^m58^s$	14^m23^s	48^s		56^s	56^s	56^s
27	50^m45^s	$51^m\ 7^s$	$2^h51^m29^s$	51^m51^s	52^m13^s		58^s	58^s	58^s
	$54^m\ 1^s$	$54^m29^s.5$	$2^h54^m58^s$	$55^m26^s.5$	55^m55^s		56^s	$56^s.0$	56^s
	4^m58^s	$5^m20^s.5$	$3^h\ 5^m43^s$	$6^m\ 5^s.5$	6^m28^s		26^s	$26^s.0$	26^s
	$7^m49^s.8$	$8^m22^s.8$	$3^h\ 8^m55^s.8$	$9^m28^s.8$	$10^m\ 1^s.8$		$51^s.6$	$51^s.6$	$51^s.6$
	$27^m29^s.4$	$28^m\ 2^s$	$3^h28^m34^s$	$29^m\ 6^s.3$	$29^m38^s.5$	*	$7^s.9$	$8^s.3$	8^s
	$40^m28^s.5$	$40^m54^s.1$	$3^h41^m19^s.7$	$41^m45^s.3$	$42^m10^s.9$		$39^s.4$	$39^s.4$	$39^s.4$
	50^m48^s	$51^m11^s.7$	$3^h52^m35^s.4$	$52^m59^s.1$	$53^m22^s.8$		$10^s.8$	$10^s.8$	$10^s.8$
	30^m31^s	30^m55^s	$6^h31^m19^s$	31^m43^s	$32^m\ 7^s$		38^s	38^s	38^s

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.

Appended Comments by DR

D Reconstructing Robertson's Methods

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 filled in numerous gaps in an embarrassingly intermittent[3] record (or adjusted a nearly full set of real, perhaps unsatisfactory data)[4] largely by extrapolation and-or interpolation, which could be accomplished by grade-school arithmetic, using wire-intervals found either empirically (eqs. 1&2) or from a list (of such intervals) kept for the purpose (§F).

D2 E.g., if $w2$ was missing, he could just empirically interpolate:

$$w2 = (w1 + w3)/2 \tag{1}$$

Or, in case $w1$ was wanting, he could empirically extrapolate:

$$w1 = 2 \cdot w2 - w3 \tag{2}$$

D3 By chaining such elementary means, it would be possible also to create $w4$&$w5$. Thus, for any star, Robertson could observe-record merely two wire-times and quickly 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 is consistent with well over 80% of the Table 1 data set. (However, another explanation will work for numerous data here, as we will see below at §F.)

E Hints of Fabrication

E1 Standish notes (§B2) that all the $1^s/4$ and $3^s/4$ 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 $w2$&$w4$ with the half-timesec ending while $w1$&$w3$&$w5$ are integral. Both statistical asymmetries are what we expect when data are being interpolated[5] 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 ($w1$&$w3$ or $w3$&$w5$). 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., $w2$&$w3$ or $w3$&$w4$ or such.) One possible motive for such selectivity will soon (§E5) become evident.

[3] 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 1811/1/11, 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.

[4] 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 $w2$&$w3$ 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.

[5] 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 (1811/1/11 & 1/17) — and the interval is identical: $25^s1/4$. (By contrast, the correct interval by eq. 6 is 25^s.) Another explanation for the β Tau data: only 3 wire-times were observed (see also §F10), say, $w1$&$w5$ on 1/11 and $w3$ on 1/17 (all integral) — and the other 7 data were then fabricated via eqs. 1&2, adopting the 1/11 interval for 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-twentieths. Yet not 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-twentieths (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 $w2$ = $5^{\mathrm{m}}39^{\mathrm{s}}.4$ would accidentally agree with interpolation (from eq. 1) within $0^{\mathrm{s}}.05$. Ignoring hours, we have

$$(w1 + w3)/2 = (5^{\mathrm{m}}16^{\mathrm{s}} + 6^{\mathrm{m}}02^{\mathrm{s}}.9)/2 = 5^{\mathrm{m}}39^{\mathrm{s}}.45 \tag{3}$$

And the same star's $w4$ = $6^{\mathrm{m}}26^{\mathrm{s}}.5$ also agrees with interpolation, to the same amazing precision; proceeding analogously to eq. 1 or eq. 3:

$$(w3 + w5)/2 = (6^{\mathrm{m}}02^{\mathrm{s}}.9 + 6^{\mathrm{m}}50^{\mathrm{s}})/2 = 6^{\mathrm{m}}26^{\mathrm{s}}.45 \tag{4}$$

These coincidences both occur in a data-set whose standard deviation is an ordmag larger (§C1). It is more reasonable to suppose that $w1$&$w3$&$w5$ were observed and $w2$&$w4$ fabricated therefrom via eq. 1 — but both results had to be rounded[6] (to timesec-tenths) when the computed figures exhibited the ridiculous precision of $0^{\mathrm{s}}.05$. Perhaps such uncomfortable experiences nudged Robertson to prefer *adjacent* wire-pair data (§F5) whenever taking 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 *un*asterisked case. The 1811/1/13 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[7] was identically enhanced. (Indeed, for the 2nd limb data, it appears that, instead of writing the endings as "xx.75" & "xx.25", Robertson simply rounded[8] 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_{m}: eq. 5) here was childsplay[9] — circularity required that w_{m} was just equal to $w3$.

[6] When 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.

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

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

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

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, $w1$ = $7^{\mathrm{m}}17^{\mathrm{s}}$ looks like a tens-place miswrite (or miscalculation) for $w1$ = $7^{\mathrm{m}}07^{\mathrm{s}}$. And, for the 2nd star of 1811/1/11, $w4$ = $11^{\mathrm{m}}10^{\mathrm{s}}3/4$ may be another slip. (The correct mean of $w3$&$w5$ is $11^{\mathrm{m}}10^{\mathrm{s}}1/4$.)

E8 An independent bit of evidence of data-wrenching: on the handwritten record, the three 1811/1/11 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 observatory. 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 times end 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

$$w_{\mathrm{m}} = (w1 + w2 + w3 + w4 + w5)/5 \tag{5}$$

E11 But, for an asymmetic set, it is not so simple. An example is the two-limb solar record for 1812/4/20. For the 1st limb, $w1$&$w2$ were not recorded, and $w3$ = $1^{\mathrm{h}}52^{\mathrm{m}}49^{\mathrm{s}}$, $w4$ = $[53^{\mathrm{m}}]11^{\mathrm{s}}.5$, $w5$ = $53^{\mathrm{m}}33^{\mathrm{s}}.5$. For the 2nd limb, $w1$ = $54^{\mathrm{m}}15^{\mathrm{s}}$, $w2$ = $[54^{\mathrm{m}}]37^{\mathrm{s}}.5$, $w3$ = $1^{\mathrm{h}}55^{\mathrm{m}}0^{\mathrm{s}}$, $w4$ = $[55^{\mathrm{m}}]22^{\mathrm{s}}.5$, $w5$ = $56^{\mathrm{m}}45^{\mathrm{s}}$. (The precisely-uniform-interval 2nd limb data look fleshed-out by the same fabrication-approach as the 1811 January data.) Reconstructing: Robertson, using a $22^{\mathrm{s}}.5$ interval (from 2nd limb data or eq. 6), reduces the 1st limb's $w4$&$w5$ to the middle wire (i.e., subtracts $22^{\mathrm{s}}.5$ from $w4$ & 45^{s} from $w5$), and averages these to find w_{m} = $52^{\mathrm{m}}48^{\mathrm{s}}.83$. (All the data in his record are rounded to hundredths of arcsec.) This he averages with the 2nd limb w_{m}, 55^{m}, to find solar-center w_{m} = $53^{\mathrm{m}}54^{\mathrm{s}}.415$, which (rounding as usual) he writes as $53^{\mathrm{m}}54^{\mathrm{s}}.41$. This is an at-least-partly-proper record. 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.)

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

F1 Though empirical extrapolation-interpolation is a possible solution of 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 table[11] providing t for, say, every degree of δ. Since the equatorial wire-interval t_{o} was about[12] 22^{s}, he could compute any star's wire-interval t_{o} from its declination δ via the simple equation:

$$t = t_{\mathrm{o}}/cos\delta \tag{6}$$

[10] A few days later: we find Regulus' $w2$&$w5$ wire-times are identical (ending in $24^{\mathrm{s}}.7$ and $31^{\mathrm{s}}.7$, resp) for 1812/4/21&22, which is slightly odd. (Regulus' $w1$&$w4$ wire-times are not the same; $w3$ data are not recorded for either day.) And, nearby in this record, three out of the four repeated wire-times for ϵ Leo (precision half-timesec) are identical for 1812/4/19&20.

[11] In his *Histoire Céleste*, Lalande provides just such a table of computed wire-intervals, arranged for zenith distance — but, of course (see his p.576), computed from declination (as in our eq. 6).

[12] To compute his stars' intervals t, Robertson seems to have used either 22^{s} or $21^{\mathrm{s}}.8$ as t_{o} in eq. 6.

F2 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 δ — 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., α Tau $23^{\rm s}$, β Tau $25^{\rm s}$, α Lyr $28^{\rm s}$, β Gem $25^{\rm s}$, α Gem $26^{\rm s}$, α Per $33^{\rm s}$, δ Per $32^{\rm s}$ or $32^{\rm s}1/4$,[15] α CMa $23^{\rm s}$, α CMi $22^{\rm s}$, α Aur $31^{\rm s}$ or $31^{\rm s}.2$. (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 $w_{\rm m}$ computed there is slightly erroneous, due to quick&dirty use of the same[16] old $25^{\rm s}$ Pollux interval: see §F9.)

F3 Below (§G14), we will see large-scale evidence against the one-wire-time theory. Other, smaller problems with it follow here.

F4 On 1811/1/11, α Tau and β Tau were expressed to $1^{\rm s}/4$ precision ($t = 22^{\rm s}3/4$ & $25^{\rm s}1/4$, resp), though the real t in each case was actually near-integral ($23^{\rm s}$ & $25^{\rm s}$, 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.

F5 And use of a list seems unlikely to explain the case[17] of ϵ Aur (magn 3.0), where we find at least three variants for t: $30^{\rm s}$ (1811/1/21 & 1/29, etc), $30^{\rm s}1/4$ (1811/1/20), and $30^{\rm s}1/2$ (1811/2/13). (Since δ was $43°46'$, the real t was about $30^{\rm s}1/2$.) There is a provocative implication here: in a five-wire set, a variation of $1^{\rm s}/2$ in t will entail a discrepancy of $2^{\rm s}$ in the quantity $w5 - w1$. Such an error is too large for real 5-wire transit-observations. (The least ambiguous indication is that ϵ Aur could not have been on the list of major-star intervals hypothesized 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 ϵ Aur. (At §G, we will find confirmation of the suspicion that such fabrication was indeed a regular occurrence in this record.)

F6 In early 1811, the interval t for Jupiter's center[18] was $22^{\rm s}.9$. 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.

F7 Indeed, the very same day (1811/2/1), he also used the same $t = 22^{\rm s}.9$, 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 $t = 23^{\rm s}.5$. (There are no solar data for 1/17.)

F8 The 1812/5/8 observation of α Gem is a curious hybrid,[19] based on two wire-times: instead of interpolating, Robertson (or whoever) simply extrapolated (using his standard $26^{\rm s}$ interval for α Gem) from each of the two observed w. Result: $w1 = 23^{\rm m}14^{\rm s}.5$, $w2 = 23^{\rm m}40^{\rm s}.5$, $w3 = 7^{\rm h}24^{\rm m}6^{\rm s}.7$, $w4 = 24^{\rm m}32^{\rm s}.7$, $w5 = 24^{\rm m}58^{\rm s}.7$.

[13] But days are another matter. Especially for the outer planets: see §F6.

[14] A clear example at §F10.

[15] There is a $1^{\rm s}$ arithmetic slip for $w5$ on 1811/1/29.

[16] The 5-wire Pollux record of 1812/2/13 is in perfect accord with the same $t = 25^{\rm s}$, despite "High Wind". The Gemini twins' regularity may have a partly empirical cause: eq. 6 yields Pollux $25^{\rm s}.0$, Castor $26^{\rm s}.0$.

[17] A less flagrant example is 137 Tau: constant interval $22^{\rm d}.6$ on 1811/1/18, vs. $22^{\rm d}.8$ on 1811/2/2. Small difference — but large implication: t was not based on a pre-listed interval for this star.

[18] On 1811/1/15 and 2/1, all four intervals t for Jupiter's *center* are $22^{\rm s}.9$. Since Jupiter's width is several time-secs, this is quite an implicit precision-claim!

[19] The β Gem data of 1811/1/14 are probably a similar set.

F9 On 1812/5/3, we find the data for β Gem (Pollux): $w1$ = [$7^{\rm h}$]$34^{\rm m}27^{\rm s}.5$, $w2 = 34^{\rm m}52^{\rm s}.7$, $w3$ omitted,[20] $w4 = 35^{\rm m}42^{\rm s}.7$, $w5$ = "clouds". Robertson's computed $w_{\rm m}$: [$7^{\rm h}35^{\rm m}$] $17^{\rm s}.7$. Here it seems that $w1$&$w2$ were real, and he may simply have computed both $w4$ and $w_{\rm m}$ by adding appropriate multiples of the usual (§F2) $25^{\rm s}$ onto $w2$.

F10 The opportunism cited at §F6 reaches an artful pinnacle with the solar observations of 1811/2/16. First limb: $w1 = 56^{\rm m}29^{\rm s}.2$, $w2 = 56^{\rm m}53^{\rm s}.2$, $w3 = 21^{\rm h}57^{\rm m}15^{\rm s}.2$, $w4$ & $w5$ omitted.[21] Second limb: $w1 = 58^{\rm m}42^{\rm s}$, $w2 = 59^{\rm m}6^{\rm s}$, $w3 = 21^{\rm h}59^{\rm m}28^{\rm s}$, $w4 = 59^{\rm m}50^{\rm s}$, $w5$ = [$22^{\rm h}$]$0^{\rm m}12^{\rm s}$. With solar δ at about $-12°1/2$, we know (from eq. 6) that the real t was $22^{\rm s}1/2$. A reasonable reconstruction of the fabricator's work here requires only three real observations (similar to fn 5): if we assume he meant to get $w1$ & $w5$ of both limbs but missed $w5$ for limb 1 (due to wind: fn 21), then he got: $w1$ of the 1st limb, and $w1$ & $w5$ of the 2nd limb. (All three are in fact quite accurate.) The fabricator then, so near the equator, sloppily set t equal to the equatorial $t_{\rm o} = 22^{\rm s}$. Next, he non-empirically[22] extrapolated by subtracting multiples of t from $w5$. (Since he was fabricating by using a value for t that was a half-timesec low, the gap between $w2$ and $w1$ ended up quite wrong: $24^{\rm s}$.) Finally, he found the difference between[23] the two limbs' $w1$ to be $2^{\rm m}12^{\rm s}.8$ and subtracted that from the 2nd limb's $w2$ and $w3$ to get the corresponding wire-times for the 1st limb. The theory accounts for both these bizarre data-sets, in which (if we do not acknowledge fabrication here), we must believe that the observer found all intervals equal to $22^{\rm s}$ except the first, which instead was $24^{\rm s}$, that is, $2^{\rm s}$ greater. Unlikely enough even in isolation; but the ultimate peculiarity is that the weirdly exaggerated $24^{\rm s}$ interval occurred identically for both limbs (to the tenth of a timesec) — *and* at the same wire-interval (the first: $w2 - w1$). Not remotely credible.

G Detailed Proof of Computational Fabrication

G1 Up to this point, we couldn't be sure whether data were being smooth-fudged or outright fabricated. But two stars in Taurus will now settle the question: 25η Tau (Alcyone, in the Pleiades) and 125 Tau.

G2 Our search for evidence that would answer the §G1 question was a good bet to get results, because humans are fallible; thus, we have yet another[24] application of statistical common-sense to this case: no one who bluffs on a large scale (whether an individual, or a bluffia-clique) can escape making the occasional muff that reveals the truth. (See, e.g., the case of Ptolemy, whose published[25] observations — on which his theories were fraudulently[26] founded — were massively faked. Some of his most amusing giveaway pratfalls are revealed at ‡5 §B5 [below], and at *DIO 1.1* ‡6 §H5 and fn 37.)

G3 The 5.2 magn star 125 Tau was observed on consecutive nights, 1811/1/28&29. Its δ was $25°47'$, so (eq. 6) actual $t = 24^{\rm s}1/2$. And on 1/28, all 4 intervals (between wire-times) are just that amount. But, the next night (1/29), all 4 intervals are equal to $25^{\rm s}1/2$: $w1 = 26^{\rm m}14^{\rm s}$, $w2 = 26^{\rm m}39^{\rm s}.5$, $w3 = 5^{\rm h}27^{\rm m}5^{\rm s}$, $w4 = 27^{\rm m}30^{\rm s}.5$, $w5 = 27^{\rm m}56^{\rm s}$. Since Robertson wrote a "5" over the last digit (altering $w5$ by $-1^{\rm s}$), the final record shows errors of $+2^{\rm s}$ in both $w4$ and $w5$. Because $w2$ is about accurate, it appears that a $1^{\rm s}$ error in $w1$ or $w3$ caused

[20] This is one of a number of 1812 cases where unrecorded $w3$ would be identical to recorded $w_{\rm m}$.

[21] "High wind" is noted beside the 1811/2/16 solar data.

[22] See §F2.

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

[24] See also §D & §E.

[25] Another point in Oxford's favor: Robertson did not publish his fabrications.

[26] I.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^s 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 (magn = 2.9), it was observed 1811/1/28 & 2/1. On both occasions, the fabricator faked most of the wire-times, using $t = 23^s$ — perhaps borrowing the interval of Alcyone's fellow-Bullstar, Aldebaran. However, Aldebaran's δ was $16°07'$, while Alcyone's δ was $23°31'$, so (by eq. 6) Alcyone's actual $t = 24^s$. Therefore, both these Alcyone records contain *perfectly* systematic errors in $w5 - w1$, amounting to *four timesec*. On each night the observer could have recorded only one wire-time (say, $w3$) and later fabricated the other four wire-times (using $t = 24^s$). Thus, the consistent falsity of the Alcyone data is neatly explained by the 5-wires-from-1 hypothesis of §F. Though, 5-from-2 (via eq. 2) works as well (assuming Alcyone $t = 24^s$ was a 2-wire empirical accident one night, copied therefrom the other night — to fatten the latter's 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^s errors in $w5 - w1$, 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[27] of prior raw-data records. (Which leaves open the possibility[28] that he was not the fabricator. However, alot 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: §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 ($w2$&$w4$ or $w1$&$w5$): 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., §D2, §F, and fn 23. But the purpose 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 (§D3) that: [a] At least a substantial fraction of the data are real. [b] Fudging or smoothing did not (§§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 §F5 & §G.) But in one sense the two crimes are the same; after all, if one is forcing a datum, then: *to what value is it being made equal*? A fabricated one. (Or, in other arenas, a plagiarized[29] 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 §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 BC[30] and who (perhaps out of caution at his data's seemingly meaningless slight inconstancy) rounded[31] all his reduced stellar declinations (of which only six survive: *Almajest* 7.3) to quarter-degree precision. 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.

G13 Returning to point [b] in §G10, we have a little mitigation of Robertson's misbehavior: his fabrications are unlike the very many[32] of Ptolemy or the very few[33] of Tycho, in that the fudging is not betrayed by large[34] departures from reality (other than statistical).

G14 Though the case of Alcyone (§G4) suggests (without proving) that the fabricator used the 5-wire-times-out-of-1-wire-time method (§F) on occasion, the previous point (§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 out of place by serious amounts of time. Probable explanation: taking at least two transit data provides a check against large blunders in time; so, the virtual nonexistence of such is consistent with there generally having been multiple real wire-times per star.

G15 Thus — though [a] padding is awful science, and [b] the stars' mean accuracy is not impressive[35] — 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.

[27] In the 1811/2/13 record, Robertson accidentally skipped the 5-wire record for Capella and wrote down the 5-wire record for β Tau before realizing his omission. He then scratched out the β Tau data and wrote Capella's on the next line, and β 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 ϵ Aur [temporarily skipped] and 125 Tau [first entry scratched].)

[28] 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 padder (§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* ‡7 fn 1] J.Lalande 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.

[29] See *DIO 1.3* §N15, and *DIO 2.1* ‡2 §H14 [bracketed].

[30] Rawlins 1991W fn 126; Rawlins 1994L §§F7&9, Table 3.

[31] Perhaps he or others took slight discrepancies between his results and Timocharis' data (c.300 BC: fn 30) as reflecting on his abilities. Did timidity cost Aristyllos the discovery of precession? By contrast, his contemporary Aristarchos distinguished between the sidereal & tropical years (*DIO 1.1* ‡1 fn 25, ‡6 fn 1), which implies recognition of precession in the 3rd century BC.

[32] E.g., ‡5 fn 16 & fn 17.

[33] *DIO 2.1* ‡4 Tables 1&2.

[34] The errors noted at §F5 & §G are statistically excessive but not great in timesec.

[35] See §C1. Note: in a typical five-wire data-set here, we do not usually know which w are-is real.

‡2 Hipparchus and Spherical Trigonometry

by Curtis Wilson[1]

A Hipparchus' Trigonometric Equation on the Sphere

That spherical trigonometry was developed or used by Hipparchus has occasionally been claimed.[2] According to Neugebauer, however, the solution of spherical triangles became possible only with the discovery of two theorems by Menelaus (first century A.D.).[3] 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:

$$\tan\phi = \frac{-\cos\frac{M}{2}}{\tan\epsilon} \tag{1}$$

where ϕ is latitude, M is the length of the longest day converted to angle at 15° to the hour, and ϵ 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" (δια των γραμμων) the arc above the horizon of a star of declination 27;20°, for latitude ϕ = 36°.[4] The problem in effect uses formula 1 backwards, with declinaton 27;20° replacing the obliquity ϵ, and M as the unknown. Hipparchus' result was 224;15°; a present-day hand calculator gives 224;07°.[5]

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.[6] OU is the trace of the horizon plane, OM the trace of the equator, ϵ the obliquity of the ecliptic, ϕ the latitude. The half circle VST represents half the Sun's path on the day of the summer solstice, with

[1] [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-Muffia-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.]

[2] I. Thomas, *Dictionary of Scientific Biography 13*, 319; D. Rawlins, "An Investigation of the Ancient Star Catalogue," *Publications of the Astronomical Society of the Pacific 94* (1982), 368; D. Rawlins *DIO 4.2* (1994), "Competence Held Hostage #2". [Note by DR. For dissent on the contended question, see *Vistas in Astronomy 28*:255 (1985) n.9 (van der Waerden), *DIO 1.2-3* fn 38 & §§G2, P1, & P2, *DIO 2.1* ‡3 §A2, *DIO 4.1* ‡3 §§D & E5-E7 and fn 17, *DIO 4.3* ‡14, *DIO 6* ‡1 §G5.]

[3] O. Neugebauer *A History of Ancient Mathematical Astronomy* (hereinafter *HAMA*; Springer-Verlag, 1975), pp.26-29, 301.

[4] Neugebauer, *HAMA*, 301-302.

[5] Carrying out the calculation by the Greek formula (formula 5 below), and using linear interpolation in G.J.Toomer's reconstruction of Hipparchus' table of chords (*Centaurus 18* (1974), 8), I obtained the result 224;08°.

[6] *HAMA*, 1310.

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 n shown in the diagram is half the excess of $M/2$ over 90°. Then

$$VT = 2r_d = \mathrm{crd}(180^\circ - 2\epsilon) \tag{2}$$

$$UR = \frac{r_d}{2}\mathrm{crd}2n \tag{3}$$

and

$$RO = \frac{\mathrm{crd}2\epsilon}{2} \tag{4}$$

so that

$$\frac{UR}{RO} = \frac{\mathrm{crd}2\phi}{\mathrm{crd}(180^\circ - 2\phi)} = \frac{r_d\mathrm{crd}2n}{\mathrm{crd}2\epsilon} \tag{5}$$

Given that half the chord of the double angle is equal to the sine, and $\sin n = -\cos(90^\circ + n)$, 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 unwarranted.

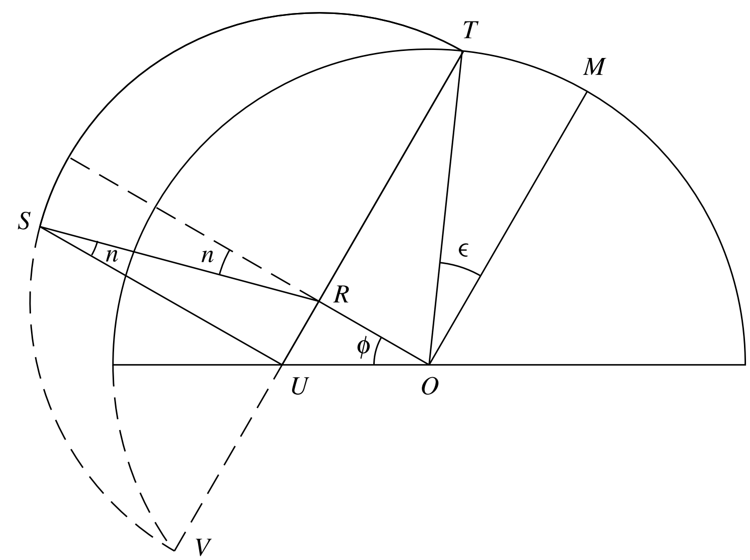

Figure 1. Rendition by KP & DR.

‡3 Hipparchos at Lindos: a Modest Confirmation

by DR[1]

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 (υ Boo) — of declination $\delta = 27°1/3$ — 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 δ is effectively given to 2′ precision,[2] and M is evidently being expressed to the nearest timemin:[3] $M = 15 - 1/20$ hours $= 14^h57^m = 224°1/4$ (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^h57^m or $15 - 1/20$ hours, as reported (§A1). By contrast, if we use $\phi = 36°$ in the calculation, the rounded result[5] is $M = 14^h56^m$ 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 ϕ from the attested Hipparchos values (§A1) for M & δ.

[1] See K.Pickering at *DIO 2.1* ‡2 §F10.

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

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

[4] Neugebauer 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 §O3.)

[5] Exact result: $M = 14^h56^m27^s$.

B2 The result: $\phi = 36°05'$. (Which is the real value — as well as the anciently-known[6] value — for the latitude of Lindos: §C.) Taking M's precision as timeminutes, we check solutions for M between $14^h56^m1/2$ & $14^h57^m1/2$, finding that this constrains ϕ to the range:

$$36°00'22'' < \phi < 36°09'09'' \qquad (1)$$

— which does not include[7] the usually-presumed Hipparchos latitude $\phi = 36°$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 ($\phi = 36°05'$N: §B2) — on Rhodos,[8] the Mediterranean island he is known to have worked at.

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D.Rawlins 1991W. DIO-J.HA *1.2-3* ‡9.
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.

[6] Rawlins 1994L fn 50.

[7] At 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.

[8] Rawlins 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.

‡4 On the Navigation of Polar Explorer Robert Peary

by Hanne Dalgas Christiansen[1]

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[2] 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[3] 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 & 1992, and the Navigation Foundation Report 1989.) A magnetic compass can be useful; but, along Peary's intended 1909 route [1] the terrestrial magnetic field is much weaker than in most regions of the Earth (Rawlins 1973 p.139), [2] compass-north is somewhat south of due west, and [3] the compass needle's north end pointed (Rawlins 1992 fn 94) about 30° to the right of the distant North Magnetic Pole [the south pole of the terrestrial magnet].

B2 Here we concentrate on the memo above (§A1, also cited in the Navigation Foundation Report 1989[4] and Rawlins 1992 fn 44). It is on a loose, undated slip of paper. Rawlins *loc cit* comments on "the excruciatingly gradual effects" of sunrise & sunset, which make it hard to judge the moment when they occur. But, while the *hourly* changes of solar altitude are surprisingly slow in the polar regions, the effects of *daily* changes are surprisingly high there (mornings & afternoons) near an equinox. (E.g., at 86°N latitude, it will take about 0.4 hours, i.e., 6° of azimuthal variation, for the rising or setting sun to move as many vertical arcminutes as the sun moves northward in declination in a day at Vernal Equinox.)

B3 To evaluate the horizon position (azimuth) of the sun for the dates mentioned, we must know the geographical latitude. A figure of 85°.6 N[5] is about right for the sunrise of March 21. (Vernal Equinox was at 1:26 local apparent time [LAT] on 70°W.) The Peary party intended to approach the Pole along the meridian of longitude 70°W (where local time is $4^{\mathrm{h}}40^{\mathrm{m}}$ less than Greenwich time). No sextant observations were taken for longitude.[6]

B4 The age-old methods of observing have focussed on azimuths of either first/last gleam or of disk touching horizon. Now, when the equinoctial sun (declination $\delta = 0$) sets at 85°.6 N, its disk (of width exceeding a half degree) takes so long to cross the horizon (moving along a path tilted only about 4°.4 with respect to horizontal, after all) that it slides a huge angular distance in azimuth between the time its lower limb touches the horizon and the time its upper limb finally disappears below the horizon. If we (fictionally) hold solar declination[7] latitude, & atmospheric refraction[8] constant, then it is easy to see that, at latitude 85°.6 N, the 32′-wide solar disk will require about

$$(1^{\mathrm{m}}/15') \cdot 32' \sec 85^\circ.6 = 28^{\mathrm{m}} \tag{1}$$

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.

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[9] for convenience at §B4) — instead, it increased

[1] [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 *DIO* style, and to add a few details — including footnotes which provide a wealth of information, besides stressing some extra mathematical niceties.] (See *DIO 6* ‡3 fn 11.)

[2] See 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.

[3] Rawlins 1992 fn 44 cocks 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 useable 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.

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

[7] Near 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.Schaefer & 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.

[9] At 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.

Table 1: Solar Lower-Limb-Touch Horizon Azimuths Along 70°W

Date	Rise			Set		
	latd	LAT	Azimuth	latd	LAT	Azimuth
1909 Mar 21	85°26′N	5:30	+82°.4	85°33′N	18:42	−79°.4
1909 Mar 22	85°43′N	5:07	+76°.6	85°48′N	19:07	−73°.1

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[10] (latd) and the local apparent time (LAT) when each event occurred. (For −40°F temperature,[11] 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 touching[12] 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 (nmi). (I.e., solar declination increased 24′ during the 24^{h}.)[14]

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 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 ϕ_{b} (along the 70°W meridian) where an observer could see the midnight sun's bottom (lower limb) touch[15] the horizon:

$$\phi_{\mathrm{b}} = 90°\mathrm{N} - 1°57' - 46' + 16' = 87°33'\mathrm{N} \qquad (2)$$

D5 Table 2 supplies declination data at 0:00 LAT for March 26, 27, & 28, as well as (with 46′ of refraction)[16] the latitudes[17] (for those same times): touching and claimed.

D6 From Table 2, it is clear that for 0^{h} 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^{h} on 1909/3/27, the sun's center (not lower limb) would

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

Date	Decl	Touching Latitude	Peary Latitude
1909/3/26	1°57′	87°33′	86°38′
1909/3/27	2°20′	87°10′	86°53′
1909/3/28	2°44′	86°46′	87°05′

[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 $r\&p$ 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.

[15] Due to declination-variation, the Sun's lower culmination here (c.87° 1/4 N) occurred, for a fixed observer, about 5^{m} 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 disablingly-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 86°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 3/23): "Henson still in his igloo as usual."

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:[18] "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:

$$\cos A_t = (180' - 32')/180' \tag{3}$$

An exact equation is:

$$\cos A_t = \sin\delta \sec\phi \sec h - \tan\phi \tan h \tag{4}$$

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 ±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".

[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^h) 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^h — 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 point 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, ϵ = obliquity of ecliptic, α = 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 $E = 3.6\cdot(1 + v)$; so, for Peary's claimed speed (v = 2 knots), E is over 10 nmi to the left of the Pole.

E3 Ordmag $1'$ is the smallest change in altitude h visible to the naked eye (even under ideal conditions — i.e., free of glare, irregular horizon, intermittent clouds, snowblindness, & the distractions of wearing travel); and rough calculation for latitude 87°N ($180'$ from the pole) shows that a $1'$ shift in h corresponds to an azimuth change ΔA near lower culmination of (analogously to eq. 3, ignoring refraction):

$$\Delta A = \arccos[(180' - 1')/180'] = 6° \tag{5}$$

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 h, so eq. 5 must be altered to:

$$\Delta A = \arccos[(180' - 1'.3)/180'] = 7° = 28^{\mathrm{m}} \tag{6}$$

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.)[19]

E4 Of course, one could do better with a sextant, but no claim is made that this was used in connection with the §A1 note, and the expedition records contain no such sextant[20] observations. It is therefore a fair conclusion that the remarks in the Peary §A1 memo do not relate to instrumental observations.

E5 Incidentally, a much better and simpler way to navigate is by transverse[21] (E-W) observations via sextant. To estimate latitude ϕ, one may merely measure the difference

[19] ΔA 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 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.

[20] 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 §A1 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, 1909/4/7 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 [§E5: factor of 20] type of steering observations only *after* arrival at the point one was steering for?! See Rawlins 1973 pp.114 & 149.)

[21] It is not required that transverse sextant shots be on the prime vertical in order to steer by them. (But §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 tranverse 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 transverse observations for aiming at the S.Pole. See Rawlins 1992, the *DIO 2.2* paper

between observed and assumed E or W altitudes h to find orientation, or monitor the rate of ascent or descent. The altitude $h_\circ$ of the sun on the prime vertical (E or W) is given by:

$$\sin h_\circ = \sin\delta / \sin\phi \tag{7}$$

Ephemeris tables provide the precise declination δ, and a noon reading gives latitude ϕ. With a log-trig table (and a page or two from each table will suffice), it is easy to compute-predict h, 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 (§§E1-E2). Ignoring small variations in δ and in the equation of time, the ascent-rate dh/dt is just:

$$dh/dt = \dot{h} = \cos\phi \sin A \tag{8}$$

where A = azimuth; so, near the prime vertical, the sun's rate of ascent $\dot{h}_\circ$ is virtually:

$$\dot{h}_\circ = \cos\phi \tag{9}$$

In our earlier example (§E1), ϕ = 87°N, so $\dot{h}_\circ$ = 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 §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.

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which triggered the *Wash Post* 1993/6/1 article which itself caused the skeptical 1993/6/11 story in *Science* (Amer 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* ‡8 fn 11.) More hits on it will soon risk SPCA-wrath at deadhorse-abuse. NGS greeted Bryce (fn 22) with standard-slowbleed p.r.: 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 (§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 (§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] §A1; [b] Rawlins 1991 §C6; [c] *ibid* §§C2&D7 vs. Peary 1910 p.211.)]

‡5 Unpublished Letters

A Banned in England: Another Astronomer-Royal Suppression

In reaction to our publications on the Neptune affair (*DIO 2.3* ‡9 & *DIO 4.2* ‡10), some have responded with disbelief that Astronomers Royal would suppress material. This is a peculiar reaction, considering that it is a matter of record that the greatest of Astronomers Royal, George Airy, suppressed key parts of [a] his own 1846/7/9 letter to Jas. Challis (*DIO 2.3* ‡9 §B2), and [b] Challis' 1846/10/12 letter to Airy (*ibid* §D7).

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 Quarmby Lawrence for assistance with my exam of the file containing this find: Adams mss 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 §A3 contemplation of censoring Airy-Sedgwick letters should be of particular interest to our readers, since *DIO 2.3* ‡9 §A6 specifically stated in 1992 that the remains of the Airy-Sedgwick 1846 Neptune correspondence indicated to DR that it had been protectively censored. *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 *S&T* (Patr. Moore), *Astronomy* (Sheehan&Baum), & *Sci. Amer* (O Gingerich); all omitted the trifle that the key RGO file walked in the 1960s (*DIO 4.2* ‡10). *Astronomy*'s lawyeresque 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&feathered — not even after asking why *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 unevadable bomb: Brit-hero Adams' final Neptune solution (Hyp X: *DIO 2.3* §§B4, 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 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 fn 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[1] 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 $n-2n'$ [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[2] 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[3] 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 unfeeling machine):[4] though he called James Challis (history's chief fall-guy for Britain's Neptune disaster) censurably inconsistent in his defenses of Adams (see *DIO 2.3* ‡10 §D2) and "perfectly dreamy" in theoretical areas (*ibid* fn 28), he was sympathetic regarding the attacks (continuing to this day) upon Challis' miss of Neptune, which we have revealed (*ibid* §B4) was actually due primarily to Adams' wide range of disparate solutions (*ibid* Table 2) to an enormously difficult problem. (A problem Adams had the admirable courage to challenge even before being fully conversant with the relevant science: *ibid* fn 4.) When M.Lalande's 1795 near-miss of Neptune was discovered by Sears Walker in 1847 (the US' first internationally recognized contribution to astronomy),[5] Airy's comment (same 1847/4/29 letter cited above) was perfect: "Let no one after this blame Challis."

A12 Postscript. Indeed, Challis is *less* blameworthy than either Galileo or M.Lalande, who missed finding Neptune (1613 & 1795, resp) despite *explicitly recognizing positional discrepancies* between two nights' observations of it. Note: all 3 men were looking for solar system objects, though the planet-discovery ambition of Michel Lefrançais Lalande (whose uncle J.Lalande sponsored & published the *Histoire Céleste* project) seems generally unknown to historians. Lalande's intent is obvious from his transparently consistent references[6] to Uranus as "Herschel". After mapping the heavens down to 9th magnitude stars, he intended to remow[7] the sky within a few degrees of the ecliptic, which would reveal the upcoming planet "Lalande" by its motion. So, what-could-possibly-go-wrong with this surefire plan? [a] The 2nd (follow-up) survey died.[8] (Funding expired?) Later, it re-flickered to life briefly. Note the unpublished 1804/6/13 record which, with luck, could have included Neptune.[9] [b] The Lalandes made the mistake of expecting success only by a lengthy, methodical mass-search. (Same as Airy & Challis assumed. And this before either was born.) It didn't occur to Lalande in 1795 that a planet might turn up in only *one* survey; but, by chance, Neptune lurked in the tiny sliver[10] of overlap of the 1795/5/8 and 5/10 zones, so it could have been found *without* the followup-survey-that-never-matured. . . .

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[1] The 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.

[2] [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 (esp. Neptune), for several centuries, to an accuracy finer than the residuals which Leverrier & Adams were dealing with, when they solved the Uranus mystery.

[3] *Mem.RAS 16*:385-459 (p.389), or *Mon.Not.7*:121-152 (p.125). Both 1846.

[4] Airy even attempted poetry. (His is probably even less known than Abe Lincoln's more impressive efforts in that direction.) See Airy's sincere memorial to the uplifting deeds and depressing end of James Cook (quoted at p.138 of Dava Sobel *Longitude* NYC 1995).

[5] Full story excellently told by J.Hubbell & R.Smith in the *J. Hist. Astron. 23*:261-291 (1992).

[6] *Hist 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 nonpunctality.

[7] The original mss of the *Histoire Céleste* are in the Paris Observatory archives: (A.)C.5bis. 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 franç. [Lalande] & Burkh. [Johann Burkhardt] to discover a planet beyond Herschel, if there exists one. . . ."

[8] The *Histoire Céleste* mss were officially presented to the National Institute on 1800/11/2. (See Delambre's handwritten note on p.321 of vol.33.) However, hope melted (after the 1800/10/25 enthusiasm) into a few nights devoted to filling in (mostly non-zodiacal) sky-areas previously skipped: 1800/11/13, 1801/1/8, stopping on 1801/1/15. (All 4 days of observations were published in 1801 in the *Histoire Céleste* pp.570f.)

[9] See 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 down half a right angle. But not until 1/2 hour later did he move south into the zodiac (re-sweeping some of his 1798/5/22 area), starting with ν Sco — at virtually the same ecliptical latitude as Neptune, and only 8° of ecliptical longitude past it. Lalande's last chance was gone. (Speculation: Did he belatedly wonder whether the disparate 1795 observations were of a planet? If so, did he briefly take a stab at chasing it down in 1804, unfortunately assuming too rapid a motion during the 9y past?)

[10] When mowing a lawn, one deliberately arranges a little overlap. Neptune happened to be in the 1795/5/8&10 sweeps' slim overlap — which is the only reason the planet, exceptionally, got observed twice. Naturally, the two positions exhibited Neptune's motion — but Lalande assumed that the discrepancy was just due to a blunder; therefore (and this *was* a blunder — which cost him eternal astronomical fame), he avoided the labor of further investigation by just suppressing the 5/8 Neptune place, while publishing the 5/10 one with a mark of doubt (":") beside it. See *Comptes Rendus 24*:666 (1847/4/19), *Histoire Céleste* pp.156&158, & original mss vol.23 pp.8 & 27. Mercifully, M.Lalande died in 1838, without ever knowing what he had (just to save a few minutes of checking-time) let slip through a net he spent 10 years scrupulously weaving.

B CoveringUp the CoveringUp 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-Muffia. Though these results are known[11] to Muffia & Princetitute personages, not one of the errors (see also *DIO 6* ‡1 §H4)[12] has ever been acknowledged. Standard archonal integrity. The following will add yet further material to that which the Princetitute will simply tuck under its increasingly Himalayan rug. Similarly, its courageous Muffia'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-pretense 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*, pp.166-167; also Toomer's *1984 Almajest*, p.464 n.99), though he does not remark the revealing fact that Ptolemy (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* paper, p.27, similarly protects the reader from understanding Ptolemy's 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 $4^{\rm d}$], Ptolemy 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 Ptolemy's first observation; [and point] No.11, for Nov.19, coincides with the second observation." These points are taken directly from Tuckerman (who uses $5^{\rm d}$ intervals for Mercury). But N has forgotten[13] 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 $1^{\rm d}/2$ (not $1^{\rm d}$) before 11/15.25, and pt.11 is $1^{\rm d}/2$ (not $0^{\rm d}$) after 11/19.25. . . . if we follow N's error and compare evening positions, the position shift in the $4^{\rm d}$ interval grows to [$0°.9$], which disagrees even worse with [Ptolemy's $1°/4$. The discrepancy here *is larger than Ptolemy'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 $1°/4$ [$13'$] beyond pt.10 before station [at $212°43'$].

[11] E.g., *DIO 4.2* ‡7 §B28 (& §B14).

[12] Just another instance of Princetitute-biggie Neugebauer so slavishly copying Ptolemy that all his ancient mentor's errors become his own. A longtime colleague of Neugebauer has confided his realization of precisely such routine Neugebauer behavior. But, because of Neugebauer's long connexion to the Princetitute, no science-history publication besides *DIO* can mention it publicly. (By admirable contrast, the *American Journal of Physics* permitted DR to point out a posegay of such errors [by Neugebauer & claque] in n.30 of his 1987/3 *AJP* paper.)

[13] An equally ethical archon's similar confusion: *DIO-J. Hysterical Astronomy 1.1* ‡8 §E4. David Hughes' response to these & other cometic errors? (Of up to c.30°!) No response.

[14] And c.1 moonwidth is the error in the 11/19 datum at p.411 of Muffia-circle archon O.Pedersen's *Survey of the Alm* (Odense Univ 1974); more on this minefield: *DIO 1.1* ‡5 fn 6 & *DIO 6* ‡3 fn 9.

B4 Both §B3 Princetitute foulups 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-sculpted (*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-archonal 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 archonal misbehavior, reserving (p.2 fn 3) criticism instead for he 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.$213°.1$ (*idem* 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-proof (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's position ($213°48'$); *however*, it neatly matches[16] the position ($213°37'$) given by the *CanInscr* 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-charade. 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*.

[15] Disgusted 1997/1/15 appraisal, by a wellknown veteran observer of academic pretense.

[16] Self-evident reason the original −264/11/19 longitude survived: this Mercury "observation" 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 $1°/4$ (not $1°/2$, as previously) west of the 11/19 longitude, he merely took a moment to alter his report (of $4^{\rm d}$ differential longitude-motion), to make it agree. Sloppy. (For another instance of such *precisely* revealing Ptolemaic sloth, see *DIO 1.1* ‡6 §H5.) Resulting hybrid gap: $213°37'$ (*CanInscr* for −264/11/19.25) minus $213°1/3$ (*Almajest* 9.10 for −264/11/15.25). This equals $17'$ — which is 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 doubly-suspect (notoriously discrepant [Toomer *loc cit*] *and* altered-orbit-ensnared) observation-pair report had a $4^{\rm d}$-motion-error ($1°/4$) that *just-so-happened* to match the difference between the *CanInsc* and *Almajest* theories here. This, when there is no question that Ptolemy kept constant his Mercury mean motion, allegedly math-based upon the −264/11/15 longitude (pseudo-proof at *Almajest* 9.10) — *despite* alterations (prior *CanInsc* 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 #5] & n.24. For the great mathematician van der Waerden's delighted appreciation of the finality of its bearing on the Ptolemy debate, see *DIO 1.1* ‡6 fn 37. The Princetitute-Muffia'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 academe as a brilliant **and highly ethical** scientist.

[17] As slick a trail as any is Ptolemy's Mars orbital eccentricity of 0.10000 ! — allegedly (*Almajest* 10.7-10) based on observations, but so overneat 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 crushingly clear proof of Ptolemy's fraudulence here, see mathematician H.Thurston at *DIO 4.2* ‡6.

C Hysterical Velikovskians Flee Own Frankenstein-Mongoose !

To: *DIO* 1996 . . .
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,[18] and since considered by some his "most unrelenting critic",[19] 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.

C2 The ban of Ellenberger was stipulated by at least two speakers, Charles Ginenthal[20] and Prof. Lynn Rose (Philosophy, SUNY Buffalo). Rose is — rather ironically in the present context — author of "The Censorship of Velikovsky's Interdisciplinary Synthesis".[21]

C3 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).

C4 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.

C5 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 Carlinsky imparted what he had learned during a May visit in Portland with one of the organizers of the upcoming November meeting.

[18] See *Zetetic Scholar* Nos. 3-4 (1979) and No. 5 (1979), 1980 May-June *Bib. Arch. Rev.*, 1980 Oct. *Astronomy*, 1981 April *Physics Today*, and 1983 May *Science Digest*.

[19] So described in the program for the 1990 August conference on Velikovsky in Toronto, Canada. *Aeon*'s 1992/4/15 subscription renewal-form noted, "The abrupt about-face of Leroy Ellenberger, combative secretary of *KRONOS* [*sic*, see §C12], and hitherto a devoted supporter of Velikovsky, has likewise provided fuel for those who would relegate the author of *Worlds in Collision* to the dustbins of history." See, e.g., Ellenberger: "Falsifying Velikovsky", *Nature 316*:386 (1985/8/1); "A lesson from Velikovsky", *Skeptical Inquirer 10*:380-381 (1986 Summer); "Immanuel Velikovsky 40 Years later: Not to Be Taken Seriously", *New York Times* 1987/5/16 p.14; "Velikovsky Revealed", *Venture Inward* (1990 Jan-Feb) p.49; book review, *J. Sci. Explor 10.4*:561-569 (1996); and H. Bauer, "Velikovsky" in G. Stein (ed.) *Encyclopedia of the Paranormal* (Prometheus Buffalo 1996) pp.781-788.

[20] Founding Editor-in-Chief of *The Velikovskian*, and compiler of *Carl Sagan and Immanuel Velikovsky* (1990, 1995), and contributor to *S. J. Gould & I. Velikovsky* (1996).

[21] *Pensée 1*:29-31 (1972); reprinted in *Velikovsky Reconsidered* (N.Y.City 1976), whose fallacies-per-page count is estimated in "Applied Philosophy of Science 101 — The Annotated Rose: A Propaganda Piece Analysed", distributed at Toronto "Reconsidering Velikovsky" Conference, 1990/8/17-19.

[22] After Arthur Koestler's coinage of the term in *The Call Girls* (N.Y.City 1973).

[23] *Meta Research Bulletin*, PO Box 15186, Chevy Chase, MD 20825-5186, phone 202-362-9176. TVF sincerely contends: some V-ists are as openminded as centrists, and the 1994 ban was atypical.

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.[24] 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."[25] 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,[26] was banned from Portland, while Ginenthal, who actually had previously (§C7) been a disruptive influence, was on the program — demanding Ellenberger be barred.[27] Ginenthal had boycotted the 1994 Haliburton meeting as a protest against Ellenberger's attending.[28]

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 astrodynamicist 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.

[24] The Ellenberger-vs-Rose schism-spatfight is summed up in M.Gardner's *The New Age: Notes of a Fringe-Watcher* (Prometheus Buffalo 1988) pp.70-71. Detailed delineation in the section "Dénouement" of Ellenberger's invited memoir, "Of Lessons, Legacies, and Litmus Tests: a Velikovsky Potpourri", whose Part 1 appeared in *Aeon 3.1*:86-105 (1992). Part 2, containing "Dénouement", plus a sweeping appreciation of the lofty scholarly merits & intellectual stature of the Velikovsky movement's leadership, was cancelled by the humorlessly enraged editor, against the staff vote. (These merits are manifested most prominently in the scholarship & openmindedness of L. Rose, whom Ellenberger has occasionally needled with such choice flattery as: "α-class epigone".) After its suppression at *Aeon*, Ellenberger's "Dénouement" was instead posted on talk.origins in a longer 1994/6/20 message titled "Ellenberger Contra Cochrane: The Second Reply & Talbott, Too". It is archived and can be retrieved at http://abob.libs.uga.edu/bobk/cle/cle-contra-cochrane.txt; see, too, the file cle-talbott.

[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!)

[26] Ellenberger participated previously without incident in meetings at Princeton (1980 & 1981), San Jose (1980), Toronto (1990), and Haliburton (1992).

[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.

C10 By the time Ellenberger was informed he would be *poisona non grata*, after leaving (1994/11/20) a message on superVelikovskian[29] 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 Slabinski[31] 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 seen fraternizing with Ellenberger. Talbott made clear *Skeptic* could have any reporter in the world, except Ellenberger, when editor-publisher Michael Shermer tried to get Ellenberger reinstated. 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 disruptiveness, 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 postings 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 by 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'y.", 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 his many initiatives and memoranda to staffers 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 hold-over 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",[32] as ex-Ed.-in-Chief L. Greenberg wrote a mutual correspondent in 1991 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, protesting the hypocrisy manifested by those who fail to follow the same standards of scholarship to which they hold their critics accountable[34] 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 Velikovskian publications & conferences.

C13 Epilogue by DR. After the Portland conference (1994), Shermer invited a 2-stage exchange,[36] to begin a winding-down[37] of *Skeptic*'s involvement in the Velikovsky debate, on which *DIO* will also publish nothing further.[38] We are, significantly, giving Ellenberger our last word on the matter. Primary reason: counterbalancing 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 was[39] — is long since over,[40] in even half-serious scholarly circles. And the skeptics have won.

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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 *idée 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.

[31] V. J. Slabinski, "A Dynamical Objection to Grubaugh's Polar Configuration", *Aeon 3.6*:1-10 (1994) (answering 1993 *Aeon 3.3*:39-48). Ellenberger performed all numerical analysis and computer simulations and prepared "Appendix B" (pp.8-9).

[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." In *Kronos 12.3* (1988), polite unbarbarian Greenberg kissed off Ellenberger as "a disaffected zealot who long ago drifted beyond the pale of rational objectivity." Further with respect to "polite academic society": when R. Davis, Emer. Prof. of English, Columbia Univ, in *The New Leader* (1977), panned V's *Peoples of the Sea*, mirrorless Greenberg protested to *NL* that Davis had "departed the world of reality never to return".

[33] See, e.g., S. V. M. Clube and W. M. Napier, "The microstructure of terrestrial catastrophism", *Mon. Not. R. astr. Soc. 211*:953-968 (1984); S. V. M. Clube, "The dynamics of Armageddon", *Spec. Sci. Tech. 11*:255-264 (1988); D. J. Asher, *et al*, "Coherent Catastrophism", *Vistas in Astronomy 39*:1-27 (1994); S. V. M. Clube, "Hazards from Space: Comets in History and Science", in W. Glen (ed.), *The Mass-Extinction Debates: How Science Works in a Crisis* (Stanford 1994), pp.152-169; and V. Clube and B. Napier, *The Cosmic Winter* (Oxford and Cambridge, MA, 1990).

[34] E.g., Greenberg&Rose, "L. Sprague de Camp: Anatomy of a Zetetic", *Kronos 3.1*:45-67 (1977).

[35] Contra Rose in *Kronos 12.1* & *12.2*, see S. Mewhinney, "Ice Cores and Common Sense", *Catastrophism and Ancient History 12.1*:5-33 & *12.2*:117-146 (1990), and Ellenberger, "Litmus Tests in the Ice", section in ". . . a Velikovsky Potpourri, Part 2" (see fn 24), distributed at C. S. I. S. Meeting, Haliburton, Ontario, 1992/8/25-26. Further on Ginenthal's unique gifts as physicist, see Ellenberger, "Magnetism, Dynamos, & Neptune", section deleted from ". . . a Velikovsky Potpourri, Part 1" (see fn 24); later posted on talk.origins, 1994/4/25.

[36] E. Cochrane, "Velikovsky Still in Collision", *Skeptic 3.4*:47-48 (1995). Ellenberger, "An Antidote to Velikovskian Delusions" [web http://abob.libs.uga.edu/bobk/velidelu.html], *Skeptic 3.4*:49-51 (1995). Followup by each: *Skeptic 4.2* & *Skeptic 4.3*; the latter is the 1st public treatment to take the discussion of Velikovsky beyond *Worlds in Collision* and Sagan's flawed analysis, which organized skepticism has unthinkingly over-worshipped. [See *Kronos 3.2* (1977), S. F. Kogan (letter), *Physics Today* 1980 Sept pp.97-98 (sponsored by Freeman Dyson for Velikovsky's older daughter in the interest of fair play); and Ellenberger (letter), *Physics Today* 1991 April p.72.] Preferable to Sagan is D. R. Moorcroft's unpublished "Taking a Leaf from Velikovsky & Examining It", available from Ellenberger (see §C11).

[37] Now concluding with imminent *Skeptic 4.4* [p.107] (1997) reviews of *Sagan & V* and *Gould & V*.

[38] Those who wish to hear the Velikovskian side of these issues (and-or to 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, POBox 5215, Aloha, OR 97006. *Aeon*, 601 Hayward, Ames, IA 50014. Chas. Ginenthal (718-897-2403), c/o 65-35 108th Str, 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* ‡14 asks an oft-overlooked question: "if even the most logically & evidentially one-sided controversies are . . . indefinitely irresolvable, then — why investigate anything?"

‡6 Science-History's Dark Ages Get Darker

The Passing of B. L. van der Waerden
A Mathematician's Appreciation

by Hugh Thurston[1]

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 text-books, and one text-book 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[2] and Indian astronomy. He was noted for his modesty and his openmindedness. (See, e.g., *DIO* volume 1 ‡1 footnote 2, ‡6 footnote 4.) His last substantial work was *Die Astronomie der Griechen* (1988) in which he (in the final page's final sentence) passed on to *DIO* the task of setting straight the early history of Greek astronomy.[3]

[1] [Footnotes by DR.] Cantab mathematician & cryptographer Thurston's book, *Early Astronomy* (Springer-Verlag [hardback 1994, paperback 1996]) follows in the tradition of van der Waerden in bringing to bear on ancient mathematical astronomy an exceptional combine of [a] extremely high mathematical competence and [b] openmindedness.

[2] 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 benefitted 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 §D) — which is apt, since *DIO* aims at encouraging unbiassed 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 benighted 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 1 (1963 ed. p.291), which has been in my library since I bought it in Harvard Square, 1/3 of a century ago.

[3] 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 §I1) have been submerged for another 2000 yrs. [Now finally published in 2002: *DIO 11.2* ‡4 (revised&retitled in 2003).]

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