Vol. 2 No. 2 1992 August ISSN 1041-5440

DIO

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The Journal for Hysterical Astronomy

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Errata in *DIO 1.1*:

‡ 1 fn 21: for 1989/12/16, read 1989/12/15
‡ 2 §G2: closest refers to distance-ratio, not absolute distance
‡ 5 fn 13: for *HAMA*, read *History of Ancient Mathematical Astronomy* 1975
‡ 7 fn 6: for development of A's propositions, read development of A's hypotheses
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‡ 8 References: for Hughes & Drummond, read Hughes & Drummond 1984.

‡5 Amundsen's "Nonexistent" S.Pole Aiming Data

Heckathorn Exocets Yet Another Dizzy Institutional Fable
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Context & Summary

The official 1989-1990 National Geographic Society (NGS) defense of Robert Peary's dubious 1909 North Pole claim was put forth by NGS' hired consultant, the "Navigation Foundation" (NavFou). It was primarily based on [1] photogrammetry, and [2] devaluing DR's 1970-1973 attacks (§B5) on the Peary expedition's mysteriously[1] dataless steering. The NavFou's variously bungled photogrammetry convinced almost no one (including several pro-Peary scholars), and was — with creditable candor — openly disbelieved in the world scientific community.[2] So we now turn to the other strut holding up NGS' case: the NavFou's contention that there was nothing odd about Peary's lack of transverse solar data for aiming poleward. The NavFou report's apologia on this point are heavily based upon the hitherto-universally-accepted understanding that undoubted 1911 South Pole discoverer Roald Amundsen likewise took no transverse solar observations for steering. (By "transverse data", I mean: observations of the Sun's altitude, taken on bearings approximately[3] perpendicular to the intended path, since these will control a poleward expedition's transverse wander off its intended meridian.) But polar researcher Ted Heckathorn has now recovered such data from Amundsen's glorious 1911 trip and thereby: [a] undone another whacky piece of academic-society conventional-wisdom, and [b] atomized the already ludicrous & baseless case for Peary's 1909 steering. The *National Geographic*'s published claim, that Peary aimed at the Pole using Amundsen's steering method, is additionally demolished by comparison of Amundsen's extensive 1911 record of compass variation observations & resultant compass courses (fn 74) versus Peary's total 1909 lack of such data within 350 miles of the N.Pole, plus Peary's explicit public denial (§K3 & fn 44) that he observed any such magnetic data in 1909. (Note: here, as in all DR writings, miles are strictly the nautical miles used by navigators; 15% larger than the layman's familiar "statute" mile, a nautical mile is one arcmin on the Earth's surface — so 60 mi = 1° of latitude.)

[1] Fn 44, fn 48, §K4.

[2] See, e.g., *Science* 1989/12/22, *Scientific American* 1990/3 & 1990/6. See also *Nature 344*:902 (1990/4/26). Right in National Geographic's hometown, DR has urged (*Wash Post* 1991/8/13) that the National Academy of Sciences adjudicate the Polar Controversy — and DR has stated (*idem*) that he is willing to abide by the NAS' verdict on whether Peary's N.Pole claim is scientifically acceptable. That investigation should additionally include a comparison of DR's photogrammetry (1990/10/22 Amer Astron Soc presentation; *DIO 1.1* ‡1 fn 14 & ‡4 or RV fn 22) vs. the NavFou's (NG 127f & NU). NGS has, understandably, not picked up the challenge. *Scientific American* (1990/6) noted: "Rawlins . . . takes extensive issue with the suspiciously good statistics of the [NavFou]'s photographic analysis . . . he called on the National Geographic Society to make the photographs widely available for independent analysis." Nonetheless, NGS has refused to comply with Ted Heckathorn's 1991 request for a copy of the uncropped version of Peary's 1909/4/7 "Pole" photo #G4 (NU 14 Fig.3). (NGS also refused an earlier *Wash Post* request for copies of the "Pole" photos.)

[3] E.g., transverse observations even 60° of azimuth from the prime vertical are still useful in this regard: their utility will be 1/2 that of data taken near the prime vertical (since the cosine of 60° is 1/2).

A Ted Heckathorn

A1 I have often said[4] of the immortal astronomer-numerologist J.Kepler that: his discovery of any one of his 3 laws of the planets' orbits would alone have ensured his high place in scientific history. By analogy, Ted Heckathorn (a Seattle historian, formerly in US intelligence) must now be ranked among the immortals of the Cook-Peary North Pole controversy, generally known (without ambiguity in polar circles) as The Polar Controversy or simply: The Controversy.

A2 I offer this judgement despite Ted's & DR's large disagreement[5] on the value of F.Cook's 1908 N.Pole claim, which I regard as precisely null, while Ted is agnostically-sympathetic to it. The implicit lesson, which I recommend to other academics: neither professional ranking nor severe disagreement on interpretation should inhibit one's appreciation of the productivity of opposite numbers nor firm friendship with them. (See, e.g., fn 46 & fn 98.) I should add that [a] Ted is a person of rigorous integrity, and [b] his knowledge of the details of the Polar Controversy is even more encyclopedic than his massive library of books on polar exploring.

A3 Ted's first key find was his 1989 August recovery (at the US National Archives) of a Peary "Pole" photo of the Sun & horizon. (I.e., Peary had aimed his camera into the Sun — highly unusual procedure, which revealed an obvious desire to record the solar altitude. For one time only.) National Geographic's 1989/12/11 "Navigation Foundation" (NavFou) official report's supplement (NU) later acted as if this key photo were its own discovery. But Ted found & released it first (torpedoing at a stroke the NGS' main secret-weapon-salvation hope for vindicating its ill-received NavFou report). This is why DR & Tom Kelly were first into print[6] with the discovery — and its damning implications for how sneaky Peary was at Camp Jesup (the purported "Pole" Camp): the photo deliberately shows the Sun's altitude at a moment when it was just "right"[7] — but similar photos for times with the Sun at a crossbearing (c.6 hrs later) are glaringly absent[8] from the Peary Papers, as pointedly noted in *Scientific American*'s properly skeptical 1990/6 evaluation.

A4 Soon after, Ted examined the diary of 1909 expedition member J.Goodsell and there found a startling item which the diary's professorial editors had overlooked when publishing an abridged version of it: only a few days before the very observations which the NavFou (wrongly)[9] brings forth as Peary 1909 expedition chief scientist Prof. R.Marvin's aiming data, Goodsell reports (p.369) his 1909/3/14 encounter with Marvin: Marvin's watches had stopped! (In the admitted[10] absence of Peary timesight observations, these watches were critical[11] for finding longitude — and thus, contra NavFou, direction to the Pole.) Marvin then reset them from Goodsell's watch, which was later found (3/25) to be 22^{m} slow vs. the ship's chronometer it had agreed with, just 37^{d} earlier. Linear extrapolation suggests c.15^{m} of error on 3/14. Thus, if the ship's chronometer was correct (as the NavFou insists: NG 7, 80), and Marvin's 1909/3/22 "1 PM ship's time"[12] data were taken virtually at Local Apparent Noon (as the NavFou also insists: NG 7, 56 & 224), then the expedition was at this time nearer 80°W longitude than to its purported 70°W meridian of travel — a matter unaccountably omitted from Peary's account, as well as from the NavFou 1989/12/11 official report (NG 180). And the westward trend of the expedition (Herbert's suggestion) is further indicated by DR's (hitherto unannounced) key discovery that the expedition's last extant pre-Camp Jesup "noon" observation (Bartlett 1909/4/1) was just before 3 PM AST (SPH 300 & 305) — that is, roughly 2 hrs after 70°W noon.[13]

A5 Ted's latest discovery is the main subject of the present paper. Before presenting it, we will briefly examine the Controversy background that makes his latest bomb so devastating.

[4] E.g., *DIO 2.1* ‡4.

[5] Ted & DR are as one on the utter incredibility of Peary's 1909 claim. I doubt that any serious polar scholar remains, who continues unqualifiedly to accept the Peary 1909 imposition.

[6] *Washington Times* 1990/2/22: a balanced article, thus enraging National Geographic since it mentions both my Betelgeux Document error (which I consider it a matter of honor always to remind readers of) and the NavFou's errors.

[7] I.e., when the altitude was about equal to the known (tabular) solar declination, as is true all day at the Pole.

[8] Each usable photo roughly establishes a Sumner line, but the first principle of celestial navigation is that: it takes 2 such lines to fix geographical position. To accept 1 Sumner line as proof of Peary's attainment is thus tantamount to accepting a single sextant observation! (Twice a day, any day of the year, right here in Baltimore, I could take such a sextant sighting or photo showing the "right" solar altitude, thus proving my N.Pole location, to the NavFou's apparent satisfaction.)

[9] Notably: neither Marvin nor Peary ever alleged Marvin's sextant altitudes were for steering. Since both limbs were taken, we know that these data were not intended to detect the moment of culmination, as alleged at NG 57-58, 61, & 224.

[10] NG 61.

[11] The NavFou resists this obvious conclusion by claiming that Marvin's 1909/3/22 & 3/25 data were culmination sets (NG 56-57, 224), a view almost no one else accepts (besides Moore, who originated it: MC 118) — including Peary-supporter Bill Molett. See fn 9 & fn 46.

B The You're-Another Defense of Peary's Alleged Course-Setting

B1 We recall that the standard GOP alibi for Nixon's Watergate lies & crimes was: other Presidents did the same — a defense which falls short both as justification and as fact. Likewise in the Peary controversy: the very stuffiest appropriate academic institutions have, over many years, managed to convince themselves unqualifiedly (by their usual technique, of simply repeating each other without checking anything)[14] that Amundsen's 1911 pioneer trip

[12] NG 214 or PZ 351. Marvin's time-error on 1909/3/22 was probably about 15^{m} (though it could easily have been worse), so Marvin's 3/22 latitude shot was at roughly 1:15 PM or 13:15. The expedition members carried 60°W time (Atlantic Standard Time); so, if the expedition were near the Cape Columbia (70°W) meridian (NG 180 puts it at 71°W), then local mean noon should have been at 12:40 on the chronometers. Thus, 13:15 was c.35^{m} or about 9° past 70°W noon — i.e., local mean noon at c.79°W. However, if we include the eq.time, the (loosely) indicated culmination time (NG 224), and the difference between culmination & transit, the suggested longitude is several degrees east of this. It has been suggested (RR note f) that the ship's chronometer was c.10^{m} fast, which (if the figure is valid: contra NG 80) would cancel part of the Goodsell error. Regardless, the critical point is that we are left with a lack of reliable chronometer data: there are full chronometer comparisons just before departure (PM 0299-0303) but nothing at all during the trip or after the return — atypical of Peary's usual concern about such matters. (See, e.g., his thorough chronometer timesight tests before & after his magnificent genuine 1900 trip to the northernmost land on Earth: fn 91.) Thus, given the obvious nontrivial error-rates of the pocket watches (PM 0299-0303, fn 91) and the explorers' occasional accidental plunges into frigid water (PM 0062, 0251, PZ 274, 278, HE 130-132), one is left without much confidence in the validity of the times given (all too infrequently) in connection with data taken en route to the N.Pole or wherever. Note Scott's sage cautions (fn 35) regarding spotty chronometer reliability in polar work, even without occasional unexpected baths. HY 162 notes that one of Scott's two S.Pole watches became undependable. (The Nansen & Johansen 1895 N.Pole attempt nearly proved fatal due to watch-stoppage. The two watches had differed by 12^{s}/day before then, anyway: HF 300.) As we recall (§A4), it is no speculation that watches stopped dead during the 1909 trip: the fact is directly attested by Goodsell.

[13] Bartlett much later insisted (NG 56) that his single (& single-limb) " noon" observation (NG 218 or PZ 359) was about at (though not of) the Sun's culmination-time. If so, then, to experience local noon at c.3 PM AST, he was scores of miles west of 70°W — nearer 100°W. But, if instead he was on 70°W and overcast weather is the explanation for his lack of a series of recorded data, then his latitude was (see eq. 12) roughly 20 miles further south than his (inappropriate) meridian math (NG 219, PZ 359) placed him.

[14] That's exactly how the British concluded that dog transport was ineffective in the Antarctic. And, when Amundsen & Scott (in their separate fashions) proved that this conventional-wisdom was a crock, the Royal Geographical Society, at a whining&dining affair bemedalling Amundsen, simply insulted him by suggesting that dog-travel wasn't as noble as man-hauling: HU 191&465, 188, 419, 513, 574, AM 71-72. (Evidently, using motor-sledges or ponies — not to mention outspending & outmanning the opposition by huge factors — was permissible under British ethical rules of fairness & manliness-etiquette: see HU 418f. Hanssen's brief, blunt remark gets right to the truth, HU 563: "What shall one say of Scott and his companions who were their own sledge dogs? . . . I don't think anyone will ever copy him.") Little wonder Amundsen wrote (AM 71): "by and large the British are a race of very bad losers." (He prefaced this remark by asking that it be "read in the light of" his intelligent comparison of national bad sportsmanship to war-fever: "in times of war . . . soldiers . . . retain a high respect for their foes in arms, while the noncombatants at home seem to feel [obliged] to indulge in hymns of hate against their enemies . . . ." The poor-loser remark is much quoted; the requested preface: seldom, if ever.) While Bowers merely regarded Amundsen as a "sneaking, back-handed ruffian" (HU 462), the Secretary of the Royal Geographical Society applied the ultimate Brit-snob putdown, slandering Amundsen as: a "professional" (HU 548).

to the South Pole was accomplished *without transverse solar altitude observations* (which would supply valid compass variation V & longitude W). (The delusion is particularly strange, in light of the 1911 trip's cloudy periods, large diversion through mountains & other zigzagging — not to mention the terrestrial magnetic field's rotation & divergence.)[15] That contagious institutional detachment from reality culminated with the National Geographic-NavFou world-publicized 1989/12/11 announcement (NGD 47, NG 6, 61-62, §E1-§E2) that, since Peary also proceeded without transverse observations, his claim is redeemed by Amundsen's like omission. (This despite the fact that the NavFou's initial 1989/2/1 attack on DR's work had repeatedly insisted on the import of Peary's use of transverse "timesight" data! See NG App.A *passim*.)

B2 Printed analyses of Amundsen's position data were confused from the very outset. A.Alexander's 1912 work (AS 2:400-403) contained several serious foulups; aside from the criticisms made by Hinks (HY 164), I will add that Alexander's map shows the compass needle pointing in two grossly discrepant directions simultaneously (both false), and his zones of position-uncertainty exhibit nothing like the proper near-elliptical shape. (See also fn 80.)

B3 The next published analysis was an improvement, but is now almost unknown: in 1915, Norwegian meteorologist Henrik Mohn analysed Amundsen's weather data. Mohn had access to Amundsen's original observationsbook (MM 40). He tabulated Amundsen's field position & compass variation results (MM 42-44), not the raw data (except at the S.Pole: MM 77-78); and he recomputed none of the complex spherical trig Amundsen used (eq. 3), nor did he compute longitudes. Understand, Mohn could easily have found (via Amundsen's computed azimuths A and our simple eqs. 6 & 7) Amundsen's 1911 longitudes *from the very same solar altitudes* on which are based Amundsen's frequent compass variation determinations (fn 74). These longitudes would, after all, have provided the positions Mohn needed to locate the very meteorological data that are the subject of his paper! But he instead used crude interpolation[16] for estimating a few longitudes near [a] Amundsen's rise from the Ross Ice Shelf to the S.Polar Plateau, and [b] the approach to the S.Pole itself. (It is possible that Mohn simply did not know how to perform the rigorous computation of longitude:[17] eqs. 6 & 7, or eqs. 10 & 11. But octagenarian Mohn died soon after this paper's publication, so perhaps he was simply worn out from the labor. In any case, his paper has numerous merits and credits, including knowledgeably citing several systematic & random errors[18] that might infect the data: MM 45&47.) This article, despite its flaws, contains important material that is either rare or unavailable elsewhere, e.g., the altitude[19] profile for

[15] Clouds prevented Sun shots from 86°47′S until 88°16′S (AS 2:111-112); the mountain-passage up to the plateau shifted the path c.5° of longitude to the right; deflections from a straight line are obvious from computed longitudes (Table 3 & §J2) and from periods of known zigzagging (HU 443, 455, MM 49 fn 80; see also F136, §B6). The magnetic field direction's rotation (as Amundsen traveled) is cited at MM 45 & §J7; the transverse steering instability-factor caused by magnetic field divergence is noted at F138.

[16] Some of this work involves spherical trig, but at a very simple level.

[17] Mohn is obviously aware (as noted at §B4) of the variables that go into the computation (eqs. 3 & 5) of compass variation V; but he never performs the computation himself. Mohn (MM 41): "During the journey to the Pole the variation of the compass was determined by means of the altitude and compass-bearing of the Sun. From the altitude and declination of the Sun, and the latitude, was computed the azimuth of the Sun; and this, compared with the [Sun's] compass-bearing, gave the error or variation of the compass." However, this is not the same (though just as difficult: §H3) as finding longitude.

[18] One suggestion just might be misleading: Mohn suggests errors for the Pole observations from imperfect levelling of the artificial horizon. Since the temperature at the Pole (MM 65) was always about 15°C (or more) higher than the freezing point of mercury, it may be that the observers used the mercury artificial horizon (AS 2:19), not the two glass ones. (A mercury horizon has no levelling error.) However, I note that the photo at HU 458d, of a Norwegian shooting the Sun, shows a glass horizon, as does a photo (AS 2:112 opp) of one of the sextant observations at the South Pole.

[19] HU 515 (& see HU 469) says that Scott failed to find altitude (above sea level) for the Pole: "the one original piece of work left to Scott was finding the height of the Pole. But that was impossible because [the thermometer for] the hypsometer was broken; the whole expedition in a nutshell. . . . The temperature was −30°C." But the truth is that Scott's field altitude for the Pole was over 25 times more accurate than Amundsen's ultimate value.

the whole trip (MM 58), the correction to one[20] of Amundsen's watches (MM 77, fn 64), raw data for the first (1911/12/16) of the two long series of altitudes & compass bearings of the Sun shots near the South Pole (MM 77), Amundsen's complete field-computed latitude & compass variation results (MM 42-44), plus en-route records of the temperature & atm pressure (*idem*) — taken thrice daily, as a rule (AP 2:94). This important article was long overlooked, and I must thank David Drewry & Roland Huntford (despite my criticisms of their superficial reading of it) for first drawing my attention to the paper.

B4 Next, we come to a generally valuable & expert 1944 paper (HY) by Royal Geographical Society President A.Hinks (Cambr U), which discussed the 1911 observations of S.Pole winner Roald Amundsen and the 1912 observations of loser Robert Scott. We should be grateful to Hinks for performing the labor[21] of not only exhuming & computing the original observations but also sorting out (HY 169) Amundsen's confusing mixed use of dates on both sides of the International Date Line (fn 29). However, Hinks repeats an earlier failure (in his and J.Wordie's 1937 *GJ* review of Hobbs' *Peary*) to inquire carefully into aiming procedure. (Evidently, he was not aware of Mohn's paper, which explains the 1911 steering calculations in general terms, though without computing anything: fn 17.)

B5 In his 1973 book,[22] *Peary at the North Pole: Fact or Fiction?*, DR zeroes in on the most glaring oddity of Peary's navigational story: his failure to determine his compass variation or longitude. *Fiction* noted (F87):

> efficient practical navigation toward the point called the North [or South] Pole requires knowledge of position: latitude (north-south) tells how far (colatitude) one has to go to reach 90°N. [or S.] latitude (North [or South] Pole) — and especially longitude (east-west) for aiming, since (in practice,[23] for polar travel) every bit of longitude uncertainty causes an equal number of degrees of unsurety in one's aim toward the Pole. [Geographical position is] customarily determined from sextant (or theodolite) observations of the sun's altitude (angle above the horizon). In simplified terms: latitude is most

From Amundsen's data, MM 65 (& 58) induced altitude 2454 m for the Pole, while Scott's diary made it (HY 177) 9500 ft = 2896 m. The S.Pole's actual altitude is 2912 m (National Geographic's fine 1981 *Atlas of the World*, p.221). Curiously, the values for altitude which Amundsen recorded in the field were a great deal more accurate than those eventually developed & published by Mohn: note the large relative discrepancy of Amundsen's rather accurate field values (AS 2:34, 47, 51, 76, 84, 102, 107) vs. Mohn's systematically erroneous corresponding altitudes (MM 58 & 63-67). Despite high respect (BAM 1:viii) for Mohn, the Scott expedition's G.Simpson corrects (BAM 1:290f) serious flaws in Mohn's analysis. Nonetheless, Simpson's best-estimate for the S.Pole's height, 2796 m (BAM 1:294) is not as good as Scott's field estimate. Thus, curiously, for both expeditions, the extended scientific analyses' values for height were worse than the explorers' own on-the-spot estimates. Moreover, the best post-expedition British estimate of the S.Pole's height (2847 m) was based (*idem*) upon Amundsen's barometric data, not Scott's. (Simpson's Scott-data-based height values for the entire trip are provided at BAM 3:234.)

[20] Each member of the Norse 5 man party carried a watch, 3 of them being chronometer type (AS 2:19).

[21] One is equally grateful to the NavFou for looking through a cubic tall tale of Peary papers & photos, in order to lay out such an incredible 1909 record for us. Ted Heckathorn writes DR (1991/12/30): "I think [the NavFou] should rank right up there with you, Herbert, and Thomas Hall in making the Peary Myth topple like a Stalin statue."

[22] Unfortunately, Huntford did not consult *Fiction*. However, HU 614 does cite a polar book published the same day as *Fiction* (1973/6/29): H.Eames' *Winner Lose All*, a Cook-apology not now taken seriously by any academic. The 2 books were frequently reviewed in tandem — to Eames' disadvantage virtually everywhere but at the ecstatic *NYTimes*, which elevated *Winner* (published by Little-Brown, a heavy *NYTimes Book Review* advertiser) to the exclusive *NYTBR* "New & Recommended" list for 3 weeks. See David McCullough's 1973/6/24 (p.6) *NYTimes Book Review* promo-raview of *Winner* and Christopher Lehmann-Haupt's even more credulous dust-jacket review (daily *NYTimes* 1973/7/31). That neither "reviewer" actually read *Winner* through is self-evident, since the latter half of the book teems with fantastically vitriolic abuse of a certain newspaper: the *NYTimes*! E.g., *Winner* claims that the *NYTimes* broadcast the anti-Cook trust's "party line" (pp.275, 279, 305), a.k.a. "the nonsense out of New York" (p.236 *and* p.237 — *Winner* has a weakness for repetition). We are even told (p.238) that "the *New York Times* brayed". Such writing is deemed "charming and literate" by Lehmann-Haupt (*NYTimes* 1973/7/31). I congratulate Eames on a magnificent experiment upon the integrity of book reviewing at the *NYTimes*. (See *DIO 1.1* ‡2 §A1.)

[23] Principle repeated at F231. Of course, if all one's chronometers go funny, then this rule ceases to apply; however, none of the explorers discussed here reported such a catastrophe.

> easily found [eq. 1] by *noon* observation of the sun. Longitude may then be computed [eqs. 10 & 11] from a *non*-noon solar observation — a more difficult calculation.

B6 After Peary's 1909 return, the US Navy's Hydrographic Office wished to map his bathymetric soundings, so (after receiving the latitudes for Peary's soundings), the US Coast & Geodetic Survey said[24] it additionally wanted "the data for determining their positions." Peary simply avoided this request for raw data (which would have had to include his nonexistent transverse solar data for longitudes: §K2), instead evasively replying (1909/10/28): "will say" all 1909 soundings "were made on the meridian of Cape Columbia" (SPH 307). *Fiction* commented (F136, emph in original):

> The unsuspecting Survey did not learn until later the incredible fact that, throughout the entire northward trip, [Peary] claimed he never took, needed, or even *contemplated* a *single* non-noon astronomical observation to determine how far transversely to the left or right he might have strayed from the Cape Columbia meridian, his alleged beeline. It is this peculiar feature of Peary's account (utterly unique in the annals of Polar travel) that places the burden of locating en route the correct direction to the Pole entirely on the compass, which was also regarded with unprecedented casualness: no checks of compass variation, over a route where the [compass] variation changed by 13° — vs. a claimed [net][25] compass-steered aim of 0°.6 accuracy.

B7 DR then contrasted Peary's superlax 1909 navigation fable (413 beeline miles of severe detours, over jagged, drifting sea-ice to the N.Pole) versus DR's understanding of Amundsen's navigation. DR stated (F140) that Amundsen

> and his South Polar party, 80 percent of whom were navigators, though they traveled over an unobstructed nondrifting plateau with an undistracted eye[26] ever on the compass, and [sledged to the North Pole] only 95 miles[27] — less than one-fourth [the length] of Peary's trip — [after] the last point of determination[28] of true South [1911/12/8, nonetheless:] they ended up [on 1911/12/15][29] about six miles to the right of the South Pole, over five times worse [alleged] angular aim than Peary.

Amundsen's mis-aim = at least arcsin(6′/95′) = 3°.6 — vs. Peary's implicitly claimed 0°.6 fantasy (fn 25).

C Clott of the Antarctic

C1 But in 1979, D.Drewry & R.Huntford ashcanned this entire DR line of reasoning by claiming that using non-noon aiming checks was needless. (Peary had called it a "waste of time": F139, F231, SPH 317.) This judgement was published in the *Polar Record* of Cambridge University's Scott Polar Research Institute. It flatly stated (p.331, emph added) that Amundsen

[24] Letter of 1909/10/21 (SPH 307), promising confidence for the data.

[25] Peary's 1909 tale has him 4 mi off (to the left of the Pole) after 413 mi of travel; arcsin(4′/413′) = 0°.6 = 0.01 rad (1% dead reckoning accuracy: F154).

[26] AS 1:212, 2:108, HU 484.

[27] Latitude 88°25′S (AS 2:117). The Amundsen obsbk (AP 2:79) indicates it was probably nearer 88°24′S, thus colatitude = 96′.

[28] F140 (F301) cited Amundsen's account (AS 2:117, referring to data like that of Table 1 and calculations like eqs. 3 & 5 and §G5): "the sun came out all at once, and we had an excellent opportunity of taking some good azimuth observations, the last of any use that we got on the journey." See fn 72.

[29] Throughout, I adopt the Australian dates used in Amundsen's obsbk. His published accounts usually give the American dates (one day less). See §B4. MM 42f uses the Australian dates; MM 60f specifies both.

> took no longitude sights [transverse or off-noon sights] during the whole polar journey, depending instead on a single longitude fix at Framheim, his base at the Bay of Whales.[30] Thereafter he trusted to *latitude observations alone*, combined with *dead reckoning* based on compass courses and distances run. The compasses were checked by frequent azimuth observations,[31] the logical method at high latitudes. Amundsen's navigation was specifically designed for simplicity and time-saving on the march, *based on the comparatively easy meridian observation* [eq. 1]. In contrast, Scott used conventional marine navigation [sph trig: eq. 10] as employed at lower latitudes. His navigator, Henry Bowers, made ex-meridian observations and longitude sights, spending considerable time and effort on calculations [eq. 12 & eq. 10] for a few kilometres, sometimes a few hundred metres of meaningless accuracy. Amundsen's navigational notebooks [fn 33] demonstrate the precision and elegance of his own system.

For this reason, the authors state (*idem* & HU 454) that they found it difficult to locate some Amundsen geographical discoveries. (We will see below that the data exist for demystifying that situation. A start is made at §J here.) Photocopies[32] of both parties' computations are visually compared. (See HU 458c, discussed here at §H3.) The slightest glance at the work shows that the Norwegian meridian math is simple addition & subtraction, while the British sph trig longitude calculation is long, messy, loggy, & complex: see below at eqs. 10 & 11 (§H1-§H2). The Norse equation is merely standard meridian arithmetic:[33]

$$L' = \delta' + Z \tag{1}$$

where L' = south geographical latitude = $-L$ (with L = north geographical latitude), δ' = Sun's south declination = $-\delta$ (declination is tabulated in national almanacs), Z = zenith distance = $90° - h$ (with h = true altitude of Sun's center above horizon).

C2 Huntford's invaluable book did much to rectify a long injustice to Amundsen caused by the classic British censorship & snobbery[34] that sanctified Scott of the Antarctic. (Readers of DR's *Fiction* will know how strongly I have always disapproved of the relative glorification of Scott over Amundsen: see, e.g., F278-280.) Huntford's work has torn aside decades of coverup, accomplished via, e.g., out&out "perjury" (HX 362) by Cambridge University's F.Debenham, founder & first head of the Scott Polar Research Institute. However, Huntford's revisionist (usually just) portrayal of Scott as the ultimate polar bungler goes rather too far in attacking Scott for the "singular inefficiency" (HU 483) of performing precise navigation! In fairness to Scott, certain facts should not be overlooked:

[30] Drewry & Huntford footnote: Amundsen "may have taken a longitude fix at his last depot on the Ross Shelf at 85°5′ [latitude south, 1911/11/17], according to a reference in his diary. There is, however, no trace of it in the navigational records, and possibly it was discarded at the time as manifestly unreliable." Curiously, at p.41 of the very Mohn paper cited at DN 336, it is stated unequivocally: "There is only one [field] determination of longitude, on the 17th of November [1911], 0°49′E of Framheim." Since Framheim was at 163°37′W, this longitude was 162°.8 W, which is precisely the value which Mohn lists (MM 63) for this 85°05′S camp. (One notes that Amundsen often observed bearings of mountains from his camps, e.g., DN 333-334. On the return, these would have the advantage of positioning a needed food depot regardless of chronometer-failure. The disadvantage, as Amundsen learned, is that topography's recognizability is not so easy as the Sun's. Still, DN 332 makes intelligent use of a few bearings to fix several Amundsen camp positions that were previously not known at all securely.)

[31] See fn 72.

[32] Below, I use the HU 458c photocopies of the British sph trig math (with thanks to Huntford for taking the trouble to reproduce these). It should be noted that another sample of the British expedition's raw data & math has been published at BTN 26 opp. However, it does not use sph trig, so near the S.Pole (§G12).

[33] All primed variables are here taken as positive to the south. An alternate version of eq. 1 is: $C' = h - \delta'$. (Note that, for null $A = H$, eq. 9 reduces to eq. 1.) For photocopies of the Norwegian 1911 expedition's meridian math, see AP 2:85 (obsbk p.33) or HU 458c.

[34] See, e.g., fn 14, HU 548-549. Huntford's comparison of British post-S.Pole sour-griping, to US reaction after *Sputnik*, is perfectly apt.

[a] Whatever the level of his personal integrity in other departments (HU 189, 194), Scott never lied about his geographical position; indeed, he died to attain genuinely a Pole of the Earth, the same feat Peary faked the last leg of, in order to survive (F122).

[b] Peary's failure (§B6) to supply longitudinal data to place his soundings was scientifically disgraceful; thus, I cannot agree with attacks on Scott for taking pains to get precise position data. Scott had emphasized the import of doing this and of publishing the results (fn 35) — so it is not ridiculous but inspiring to see how determined he was to keep the implicit promise. (What would his topographical altitudes be worth without longitudes?)

C3 To praise Amundsen and denigrate Scott, Huntford offers these comments (HU 480-482, emph added):

> In November 1909, [Arthur] Hinks, [RGS] Lecturer in Surveying and Cartography at Cambridge University, held a seminar[35] at the Royal Geographical Society in London on determining position near the Poles. . . . Scott was present, but he politely ignored what the specialists had to say. He eventually arrived on the Polar Plateau using conventional Naval routine, which Bowers, a conventional Naval officer, obediently followed.
>
> What it meant was this. Every day, around noon, Bowers took an ex-meridian sight for latitude [eqs. 1 & 12], and *in the evening* another for longitude [eqs. 10 & 11]. The calculation of both is tedious, taking (in those days without pocket calculators) perhaps an hour of laborious arithmetic . . . . Scott clearly approved of this as praiseworthy devotion to duty. He did not consider that with the exhaustion of man-hauling at high altitude, rest was more important. It was, in any case, an unintelligent waste of effort. Bowers was racking his brains for a few hundred yards of meaningless accuracy.
>
> At high latitudes, the convergence[36] of the meridians makes the degree of longitude small. At the head of the Axel Heiberg Glacier, 86°S, for example, it is a bare four miles, instead of sixty miles at the equator.

C4 Which means[37] that a 1° error in longitude will land one up 4 miles to the left or right of the Pole. For Scott's party — manhauled, not dog-powered — this was not a negligible point. To Scott, even a few hundred meters was serious distance. And since a brain's computing takes alot less food-fuel than manhauling sleds a hundred meters, Bowers' work was not wasted. (See fn 35.) The alleged brainstrain may even have been healthy: physically, Bowers held up best of all the 5 men in Scott's dying Pole party.[38] The valid Huntford criticism here applies to Scott's failure to adopt grid navigation, but this criticism applies at least equally to Amundsen: *idem*. The British march at the South Pole is a wonderfully straight-arrow hit: see Hinks' striking map (HY 163) of the line of successive British poleward camps — and his just admiration (HY 162): "The navigation was first-rate; no camp is as much as a mile right or left of the meridian along which they were trying to march". Scott's navigation difficulties & en-route meanderings were no worse than Amundsen's (though more fateful, and aggravated by a depot-marking system far inferior to Amundsen's, as Huntford rightly emphasizes). They merit criticism (e.g., HU 483), but Scott's near-perfect piercing of the Pole was much superior to Amundsen's and deserves admiration, not derision. Finally, the miraculous finding of the party's bodies & the records which have become an undying chapter of British lore,[39] even the inspiration of a powerful Ralph Vaughan Williams symphony (7^{th}): these records might have been lost (along with Scott's eyewitness & photographic confirmation of Amundsen's success: §K5) were it not for the "meaningless" navigation Bowers & Scott insisted upon. Indeed, the eventual British search party's 1912/11/12 southward path was so near Scott's 1912/3 northward path that the slight mound of Scott's by-now collapsed & drifted-up tent (at the last campsite, 1912/3/21) was fortunately visible as the later party passed it! Navigator Charles Wright was the person who made the visual discovery. The historical importance of Wright's feat recommends more than casual remembrance of the man.

C5 Huntford's criticism of Scott's aiming continues (HU 482, emph added):

> The Hinks seminar indicated that longitude fixes were therefore mostly unnecessary. [DR: Misleading; see fn 41.] What was required was to steer accurately due south towards the Pole, for which the *simpler* observations for latitude and compass error [variation V] would do. Amundsen saw the report of the Hinks seminar in the *Geographical Journal* [fn 35], and took the advice that Scott ignored. [DR: Quite untrue.][40] He used *the meridian sight for latitude, which is simple to calculate and takes a few minutes* [eq. 1, fn 33].

[35] After Hinks' talk, Scott twice commented (HF 306, 308) on the unreliability of chronometers in polar work, & he seconded a Hinks recommendation. Scott (HF 306): "May I add that I think the lecturer's suggestion that records of positions should be treated like other scientific data and published is a good one." In the light of this comment, it is rather ironic to see Scott lambasted for allegedly rejecting Hinks entirely. (That Scott ignored Hinks' grid or cartesian approach to polar navigation is a separate question. Amundsen also ignored it: fn 40. However, I believe Peary & Byrd both properly envisioned polar navigation in Hinks' terms. So does DR. See RR 35 & note ga, and BUL.) Computation of Amundsen's newly found transverse solar data (83°-84°S) by the Hinks method (eqs. 9 & 5) will yield compass variation V values about 1° higher than Amundsen's calculations gave (and see discussion at §G12). But "conventional" spherical trig (eq. 3 instead of eq. 9) produces close agreement, as we shall see below here (§G7): compare the Table 1 field solutions to the sph trig solutions ("V") & the grid solutions ("V via grid") set out in our Table 3. Thus, Amundsen obviously used the very sph trig approach which Expert consensus has so consistently denied he used (§C, §D, §E). And, for Amundsen's mid-Nov situation, cartesian eq. 9 will produce longitudes W about 1° lower than values rigorously computed via sph trig. (Again, contrast sph trig vs. grid values in Table 3.) Indeed, Amundsen gave up shooting transverse data a few days (over a latitude degree) short of the S.Pole precisely because he did not think cartesian (AS 2:117, MM 40, 43, & 45). *Otherwise, he would not have ended up 6-7 mi to the right of the Pole.* (See fn 40.) Part of the error may have been from rightward rotation (fn 15) of the needle as the party moved south (with a small clockwise component of motion around the South Magnetic Pole: §J7), after the last check of compass variation. Also, Amundsen was tense about Scott's possible priority and so may have deliberately permitted such natural veering toward the hypothetical British incoming path, which would come in on Amundsen's right. Had Scott been so inaccurate that his aim was 7 mi off (remember, he died on the return, merely 11 mi short of One Ton Depot), then revisionist historians would have scored this as just one more sign of his clumsiness & failure to learn: the Royal Navy had earlier banished him from battleship command for a mishap in (HU 230) "a delicate situation in which the slightest error of course or speed could mean disaster." And scientific review of his 1901-1904 *Discovery* expedition results had suggested incompetence and presciently wondered aloud whether anything could prevent such mismanagement from continuing — before "exceptional disasters" occurred (HU 242).

[36] See fn 41 & fn 95.

[37] See the insufficiently-understood DR principle cited here at §B5.

[38] E.g., Bowers' seems to have died last (HU 544, 564). However, that circumstance may also have been due to his small size, since he got the same food share as the others.

[39] I am no admirer of Scott's generalship. (See, e.g., HU 420 or HX 361-362.) And, incidentally, neither was R.Vaughan Williams, who composed the *Sinfonia Antartica* in Scott's memory. (Second wife Ursula VW's biography of RVW recounts his private outrage at Scott's taking 5 men instead of 4 to the Pole & thus fatally disrupting tent & food arrangements.) But the literary quality of Scott's last words (if one overlooks some pathetic alibiing) went far towards redeeming his life. (HU 544 creditably notes both pro&con here.) He and Peary were the finest writers of the Pole seekers; but, as an explorer, Peary was far his superior in virtually everything else — except honesty of reportage of geographical position. I must emphasize (again, in simple justice to Scott) that this is not exactly a trifling point! Had Scott lied by merely ordmag 10% of Peary's 1909 exaggeration, he would have lived. That is, Scott died to perform honestly the very deed which Peary faked in order to get home alive. Is academe now too amoral to find anything admirable in that? Note: the geographical integrity of Amundsen & Scott (both of whom ultimately died in action, unlike Cook & Peary) rescued polar exploring from the slimy after-effects of the Cook-Peary 1909 double-hoax, which had frozen fiscal support for the work (WP 283-285, BL 248). I have never encountered a word of geographical community gratitude to either man for this achievement. The reason is plain: Peary's 1909 fraud catapulted National Geographic into world respectability (F190, GN 51), from which it made so much money that it now supplies grants to real geographical societies (if that is not oxymoronic), whose gratitude-reservoir is, after loot-receipt, too exhausted to fuel the slightest expression of their ethical debt to a couple of explorers who lost their lives to perform genuinely heroic deeds. That eminent geographers' silence & tolerance of fakery has been bought with money generated by the most successful science hoax of the century, is a spectacle which might cause some academic communities a touch of shame. But, luckily, geographers are stronger than that.

[40] See fn 35. Hinks suggests (HF 301) that one take sights at any hours, so long as the bearings are somewhat perpendicular. But, Amundsen, after his last trusted transverse sight (evidently on the evening of 1911/12/8, AS 2:117, MM 43, fn 28), used nothing but meridian sights for celestial navigation, until — now steering by compass alone — he concluded the full outward march (682 mi from Framheim to the Pole). Amundsen even deliberately took a "noon" sight just 5 mi short of the full distance to the Pole (MM 45). Mohn (MM 40, 43, & 45) suggests that

He believed that saving mental as well as physical energy was a vital rule of safety.

. . . For speed and simplicity, he depended on accurate steering and dead reckoning.

C6 The consistent theme of these criticisms (§C1 & §C5) of Scott is that Amundsen did better by ignoring transverse (longitude) observations,[41] following Hinks's grid method (contra fn 35, fn 41, & fn 40), & relying primarily on meridian (noon or midnight) observations for latitude. However, Scott's actual opinion was expressed as soon as the cited Hinks lecture was finished, and it shows that what Huntford takes to be Amundsen's navigational superiority over Scott (namely, using meridian sights exclusively) had nothing whatever to do with Scott's rejection of Hinks' approach. (See fn 41.) Hinks' 1909 talk had concluded (fn 40, HF 301) by noting that, since noon was increasingly indistinct as one neared the Pole, the time of shooting the Sun wasn't critical. (See also DR: F114.) Scott rejected the advice by stating (HF 306): "I remain a firm believer in the meridian sight, for latitude is, after all, the most important point for the polar explorer to determine."

D Moore Logic

D1 In 1983, mountaineer & former U.Alaska President Terris Moore published an unsubtly-irritated counterargument to *Fiction*'s comments on Peary's steering. (These had been adopted in explorer David Roberts' fine 1982 book *Great Exploration Hoaxes*, Sierra Club.) Moore (MC 115):

> The essence of their [Roberts' & Rawlins'] number one charge is that although Peary did make observations for longitude at many key locations on all his preceding expeditions, in 1909 he made no observations for longitude anywhere between leaving land at Cape Columbia [83°07′N, c.70°W] and what he asserted at his farthest north was 89°57′. So how could Peary have made corrections en route for transverse motion resulting from inevitable aiming errors in his simplified direction-finding by compass and dead reckoning?

attempts at transverse shots were made on 12/10 (& perhaps even 12/12), but the results were not considered reliable according to AS 2:117, fn 28. This further confirms that Amundsen's party had no mathematical understanding of Hinks' scheme. And that is why the Norwegian first stab missed the Pole by over 6 mi (fn 35).

[41] This is a misunderstanding of Hinks' method. For position-fixing, transverse observations are just as crucial near the Pole as anywhere. But HU 490 says: "The meridians converge to vanishing point, so that longitude is meaningless, and only latitude remains. Fixing the position of this strange spot [the South Pole] is an alien and arduous exercise." In fact, as noted in *Fiction*, the geographical Poles are the easiest places on Earth to navigate at — genuinely or fraudulently. (See RR 35, F154.) This is self-evident from Hinks' (cartesian) method. Huntford's quoted comment is typical of the very confusion (a common one, even promoted — at its convenience — by the NavFou, see below: fn 95) which near-Pole cartesian navigation is supposed to clear up. For readers who are mystified by this point, I recommend §B5's clarifier and Hinks' paper, which clearly states (HF 302): "two sights on well separated bearings give as good a fix close to the Pole as anywhere else. The apparent trouble about the indefiniteness of local time and longitude is artificial, due to use of unsuitable [spherical] coordinates." (Similarly at HF 301.) See also the DR discussions at F87, 135-143, 230-231. Incidentally, the "Hinks method" (cartesian navigation near the Pole) is so obvious that, when Hinks himself introduced it, he added (HF 302): "Perhaps it is well known. The writer has not come across it." I am sure that, over the years, numerous astronomers (including G.Littlehales & DR) have independently discovered it. In this connection, DR has published: [a] Proof (RR 35) that, in a couple of lines of simple arithmetic, the cartesian approach produces (to a within a few meters!) the same alleged Peary 1909/4/7 geographical position as the fancy & massive traditional-navigation sph trig solution Peary's mathematicians produced (NG 229-239) for his N.Pole "data". [b] A simple equation for reducing sph trig to plane trig near the North Pole, namely, $h = \delta + C \cos A$, where h is true solar altitude, δ is solar declination, C is colatitude, and azimuth A = hr angle $H = G - W - 180^\circ$ (eq. 7) — with G = Greenwich App Time (midnight epoch), & W = longitude West. Note that C mapped as a function of W is a straight line. (Some adjustment is selfevidently needed for use near the South Pole: eq. 9.) The equation was published at RE 137, accompanied by the observation that, in polar coordinates, this straight line is effectively the Sumner line of Hinks' method. Huntford cites this Norwegian Geogr Soc paper at HU 626. (Though, he gives the *NGT* vol. no. as 38 instead of 26.)

The answer is that Peary's method was actually the same as the simplified navigational method subsequently employed by Amundsen and his navigators in their attainment of the South Pole in 1911. And what was that?

The clearest description of this method which both used may be found in the article "Amundsen's Route to the South Pole" published in the January, 1979 issue (page 331) of the *Polar Record* (Cambridge University's Scott Polar Research Institute).

D2 Moore then approvingly quotes, verbatim (MC 115-116), very much the same material found above at §C1. All this appears to put the authority of Cambridge University behind the claim that one can go straight to the Pole without solar observations for aiming! (And *DIO* is nothing if not worshipful of Cambridge University, since Cambr U has given the world the Editor-for-Life of the extremely handsome *Journal for the History of Astronomy*, which is frequently admired in the pages of this journal.)

D3 Having echoed Drewry & Huntford in clobbering Scott for "spending considerable time and effort" on sph trig computations of transverse solar shots (MC 117, quoting DN 331: §C1) and praising Amundsen's navigation as allegedly "based upon the comparatively easy meridian observation" & arithmetic (MC 116, quoting DN 331: §C1), Moore continues to scoff at the idea of transverse Sun shots, as he defends Peary's lack of such, stating that, compared to meridian shots (MC 118, emph added):

> *time sights* for longitude . . . are much more *difficult, time consuming, uncertain, and unnecessary*; and Peary's navigators were not going to bother with them.

So, Moore asserts that only a fool would shoot the Sun roughly at right angles to one's path when aiming at a Pole, and Pres. Moore has let the Cambridge University paper talk him into absolute certainty that the great Amundsen would not be so stupid — and that, indeed, Amundsen's eschewing of the sph trig nightmare of transverse sight reductions was one of the prime keys to the success of his South Pole trip! Boy, was DR dumb to think otherwise (§B5), and Moore is delighted to believe he has shown so.

E The NavFou Piles On

E1 National Geographic's Peary-defense-lawyer consultant, the NavFou, eagerly leaped right into the trap innocently[42] baited by Huntford & (not so innocently)[43] by Moore. The *National Geographic*'s 1990/1 article by the NavFou (NGD 47, emph added) laughed off

> questions about Peary's method of navigation, based largely on a presumed need for longitude observations . . . .
>
> We have determined that Peary's method of navigation by dead reckoning corrected by observations, as he described[44] it to the congressional committee, was appropriate and completely adequate for the polar region. It was, in fact,

[42] Huntford is not in the least attempting to promote Peary's claim (or Cook's): see HU 491 & 493. Huntford's writings are generally admirable for their rectification of the longstanding gross imbalance of history's treatment of Scott vs. Amundsen. Huntford's book (HU) inspired the intelligent 6 part British video docudrama, "The Last Place on Earth".

[43] Alaska Univ Pres. Terris Moore, as politically correct on Henson as on the Peary claim, calls doubts of Henson's navigational expertise "condescending" (MC 118) and testifies (*idem*) that Peary's companion Matt Henson (whom Moore says he knew for 20 years): "fully understood the use of GMT, GHA [Greenwich Hour Angle], and the Equation of Time to obtain longitude and simple meridian passages." This is just ethnic-appeal fiction. I challenge Moore (or anyone) to produce a scrap of Henson output supporting any of it: diary, letter, audiotape, transcript, whatever. That Henson had been tutored in math over the 1908-1909 winter is true, and was reported long ago by DR (F128). But that is a far cry from an essential understanding of refined celestial techniques of steering and finding longitude. On these points, see PY 117 & NG 54 n.7, and RD 203! (p.226 of the 1947 edition), & §K5.

[44] In fact, Peary described to Congress no method whatever to explain his steering, nor did he provide a digit of steering data he'd determined. See SPH 299, 316-317, F231. A loose, undated Peary memo, PM 0267 (cited at NG 55),

evidently written after his return to land, claims: "The sun setting due E. & W. Mar.21 & 22 gave accurate checks on compasses, also just touching northern horizon Mar.26 & 27." (The pretense that these excruciatingly gradual effects were precise events is almost as amusing as Peary's claim of observing sunset in the east.) Understandably, none of this embarrassingly crude methodology is acknowledged in Peary's publications or testimony — and, most telling: [a] none of the (alleged) resultant compass variation V data survive; [b] Peary publicly denied (§K3) to Congress that he took any V data on this trip. (Thus, far from salvaging Peary, this memo convicts him of . . . shall we just say: inconsistency?)

> the method used by Roald Amundsen in his successful trek to the South Pole in 1911. Contrariwise, Robert F. Scott's navigator, on his doomed South Pole expedition, wasted precious time struggling with the reduction of *complex conventional* but nonessential longitude *sights*.

That complex sph trig work is what ignorant DR asked of Peary for 1909. (DR looks dumber & dumber, doesn't he?)

E2 And the extensive official NavFou "Report to the National Geographic Society" states that Peary's lack of longitude observations was no-problem (NG 61):

> First, a longitude sight (Peary would have taken it [by] a "time sight") was unnecessary and would have been time consuming.[45] Such a sight requires an accurate time and latitude, and would have necessitated far more observations (at hours apart) than those Peary took.

(DR especially enjoys this mode of NavFou rhetoric: defending Peary's lack of observations by pointing to his lack of observations.) The NavFou instead whimsically claims (quite falsely)[46] that the Peary expedition aimed by culmination sights (NG 7, 55) — which are much more time-consuming than transverse shots, and less accurate for steering (fn 50). The NavFou continues (NG 61-62, emph added):

> It is interesting to compare Peary's navigational method and his independence of longitude sights with that of other polar explorers, such as Roald Amundsen. . . . Volume 19 of the *Polar Record* (1978-1979), the publication of the Scott Polar Research Institute, contains an article by D.J.Drewry and R.Huntford, "Amundsen's Route to the South Pole," which offers *the first real analysis of the Norwegian explorer's navigation*.

The NavFou then quotes exactly[47] the same Drewry-Huntford material quoted by Moore (noted §D2) — even the same ellipsis (MC 115-116 & NG 61).

E3 The NavFou concludes (NG 62):

> Peary proposed[48] to waste no time and effort on unnecessary and useless sights, and he did not do so. Those who have subsequently insisted that he should have taken longitude sights to ensure that he stayed on the 70th meridian [DR: NavFou avoids noting that Peary's lack of transverse data makes his soundings worthless: §K2 & §C2 item [b]], and who have presumed that he was dependent on maintaining accurate time for months away from civilization in order to do so [Herbert & Rawlins: NG 78] appear to have missed some of the practical aspects of elementary polar navigation.

We are about to see which party has misunderstood "elementary polar navigation".

[45] Parts of NG 61-62 sound alot like MC 118 (e.g., §D3). I am not sure if MC is even cited.

[46] During the 1991/4/19 US Naval Institute debate, Peary's more credible supporter, Bill Molett, demolished the NavFou's amusing culmination claim by pictorially comparing (via slide projected on screen) [a] the gradual curve of the Sun's culmination to [b] the scattered 1909/3/22&25 R.Marvin "noon" data which the NavFou claims determines culmination: fn 9. (The current paper thoroughly vindicates Molett's creditable public disputation of the NavFou's proposed culmination-aiming scheme for Peary. See Molett's 1990 exchange with the NavFou in *Navigation*, Institute of Navigation, Wash, DC — where the NavFou again falsely claims that Amundsen used no transverse sextant observations. I ask that the Inst Navig correct the record by printing an appropriately edited version of the present *DIO* paper, following the guidelines of the *DIO* publisher's statement.) DR simultaneously handed D.Davies (the NavFou rep on hand 1991/4/19) readouts of least squares fits of quadratics through these data, showing that both 2nd derivatives are valueless for determining direction: the 3/25 value is as large as its own standard deviation — while the 3/22 value is actually positive, which happens at midnight, not noon!

[47] In fairness, it should be said that the NavFou did not simply lift the quote straight from Moore without consulting the original.

[48] This sounds as if Peary announced his alleged new&simplified navigation *before* getting himself into the dataless fix he found himself in at Camp Jesup (for reasons induced for the first time at F149-150). No such announcement or even private note (before or after) has been produced in support of this typically convenient NavFou speculation.

E4 In fairness, it should be noted that the unlucky NavFou is far from the first to defend Peary by fantasizing his navigation for him. (This only became necessary because Peary himself never publicly specified how he steered: fn 44. Indeed, he deliberately suppressed his actual slack method: §K4.) Why do Peary's defenders persist in making the very same mistake which DR's 1970 Dartmouth article openly warned of (RR note i), as did his 1973 book (F143):

> A further indicator of the obviousness of Peary's aiming folly is the embarrassing fact that his biographers Green and Hobbs both speak with naïve admiration of the careful navigational methods that they assume Peary *must* have used for checking dead reckoning estimates on the shifting pack; likewise, believing explorers Henry Bryant, Vilhjalmur Stefansson, and Richard Byrd all remark innocently on Peary's skillful conquest of the special navigational difficulties caused by the *moving* ice.

I can almost hear Peary's nerve-frayed shade: begging these apparent authorities please to shutup-shutup about the sore point of moving ice. The NavFou instead tried claiming (NGD 46, NG 62f & 180) that, during the 1909 trip, there hardly *was* any net ice-drift. The NavFou evidently missed the irony of DR's remark (F99) on "Peary's ultimate 1909 navigational tale, a tale requiring that the Arctic Ocean's ice stand as still as Joshua's sun, for 36 days straight." Likewise, in 1972, the *Polar Record* of Cambridge University was taken in by my late friend G.Potter's delightful 1970 satire, on Peary's N.Pole whopper — soberly treating this playful paper as a serious defense of the Peary claim. DR commented (F62): "No wonder the slightly less obvious original [1909 prank was so] successful!"

F Heckathorn Finds Amundsen's Transverse Data

F1 The foregoing NavFou compost pile (§E) is founded upon the supposedly rocksolid premise that Amundsen's records exhibit no transverse data of any kind — a conclusion based upon an apparently thorough examination of Amundsen's original navigational records, appearing (DN) in a Cambridge University publication.

F2 As noted above, Ted Heckathorn has repeatedly turned up missing pieces of Polar Controversy puzzles. He has done this by: [a] not accepting the accepted, and [b] looking in places which supposed experts either didn't think of, or had searched superficially. And it was this touch which induced Ted to discover Amundsen's supposedly nonexistent transverse (azimuth-longitude) observations. Even more remarkable: Ted did not have to travel to Norway to make this astounding discovery. He found a photocopy, of a manuscript excerpt from these "nonexistent" data, right in the (admittedly rare)[49] Norwegian edition (AP) of Amundsen's book! Ted bought this book (over 30^{y} ago) for all of $3. And with this $3 weapon, he has sent to-the-bottom the National Geographic Society's belovéd 1989 Peary-whitewash NavFou Report, which cost NGS ordmag 100,000 times as much. Cost-efficiency-ratio-wise, it's like a tiny Exocet sinking a fat aircraft carrier.
Only better.

F3 Now that we know the truth, it will be educational to arrange a retrospective comparison of voucher-authorities, on the question of whether Amundsen used transverse longitude observations:

NO:	YES:
Scott Polar Research Institute (Cambridge Univ)	Ted Heckathorn, historian
National Geographic	DR, gadless gadfly
Navigation Foundation	
Terris Moore, President, Univ Alaska	

[49] Hinks says (p.168) that he got hold of what might have been the only copy in England in 1944 (belonging to Hugh Mill). DN 336 cites the usual English edition (AS). But HU uses throughout (& lists at HU 611) the Norse edition (AP).

The impressive authorities in the left column are, naturally, as correct as ever.

F4 Polar bibliophile Heckathorn has found (as also did the NavFou: NG 136 n.6) that foreign editions sometimes include material not in the edition we are familiar with. Ted discovered that the Norwegian edition of *The South Pole* includes photocopies (absent from the English edition) of numerous Amundsen observationsbook pages. In the foregoing context, by far the most startling is a photocopy (AP 2:115) of 1911/11/9&13 data (from Amundsen's observationsbook p.127), shot transversely to the Poleward path. The handwritten figures displayed are captioned: "En side av azimuth- og laendeobservationene." In English, this is: "A page of azimuth- and longitudeobservations." Hmmm. (Thus, clearly captioned photographic disproof — of the amusing SPRI-Moore-NavFou-NGS fantasy that Amundsen went to the S.Pole without transverse observations for longitude — has been right on the public record for 80 years. . . .)

F5 On 1991/12/30, Ted turned these data over to DR for evaluation. I have recomputed them and find that the Amundsen calculations are "conventional", "complex", and "time consuming" spherical trig celestial navigation — exactly the sort of math which Scott & Bowers have long been unjustly excoriated for performing, math which is necessary[50] (in some form, standard or cartesian) for efficient travel towards a geographical Pole.

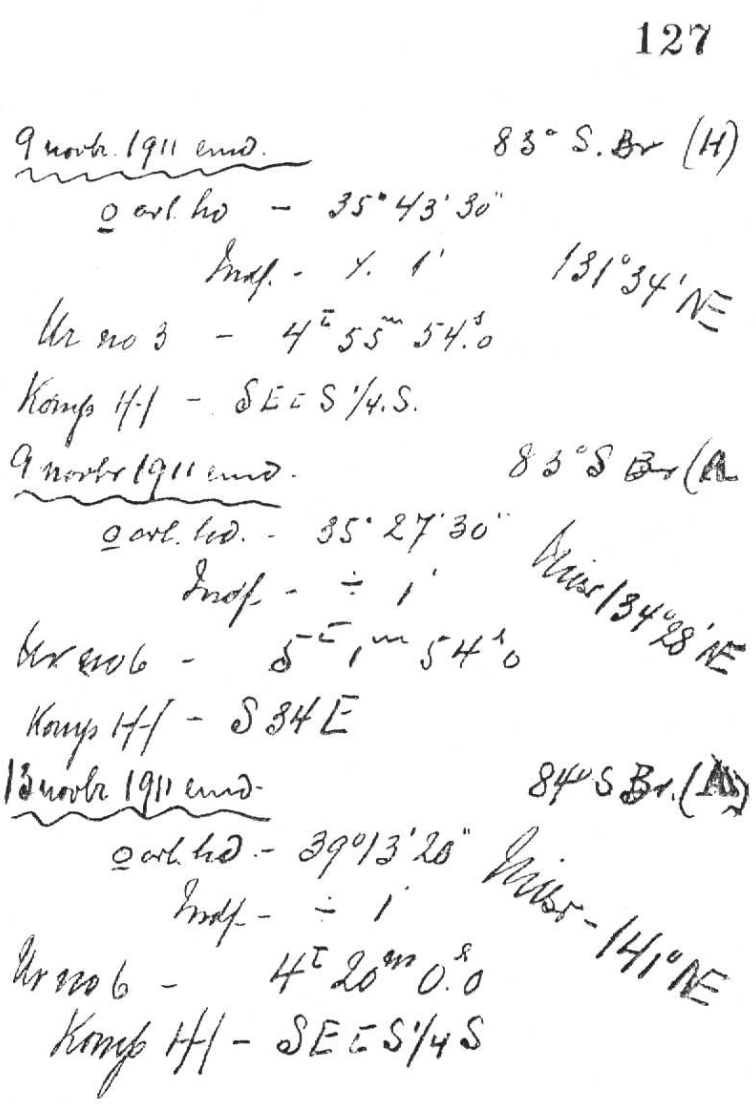
F6 I will begin by setting out here, in Table 1, the data on the observationsbook page reproduced by Amundsen (AP 2:115; photocopy here at p.70).

Table 1 Amundsen's On-Site Recorded Data & Calculated V

Obs #	Date	T (Chron#)	L	hh	B	V
1	1911/11/09	16:55:54 (3)	-83°	$35^\circ 43' 30''$	30° 15/16	$131^\circ 34'$
2	1911/11/09	17:01:54 (6)	-83°	$35^\circ 27' 30''$	34°	$134^\circ 28'$
3	1911/11/13	16:20:00 (6)	-84°	$39^\circ 13' 20''$	30° 15/16	141°

where T = chronometer time (on Norse chron #3 or #6), L = latitude, hh = sextant apparent double altitude of Sun's lower limb, B = compass bearing of Sun's center, V = recorded compass variation (result of Norse sph trig calculation in the field: e.g., §G7).

[50] It goes without saying that, around noon (or midnight), one may find longitude W by the method of equal altitudes (which establishes by symmetry the time T and bearing B of culmination); but this is so time-consuming, and the result is so vulnerable to vagaries of Arctic refraction for low solar altitudes, that Peary obviously would not have used it in early Spring near the North Pole, given the lowness of both thermometer and Sun. (When he used it for the 1900 compass variation measures cited at fn 92, temperature & Sun were both high; even so, the process took an hour or two in all cases, and the results were so rough that they were expressed only to the nearest half-degree. His presumed 1906/4/12 use of culmination does not produce a figure for longitude W — or latitude L for that matter. See PX 128 1906/4/12-13. Even the published PY 129 "improvement" of this diary entry's vague lower-limit result does not provide W. Compare to quantitative L, W, & V at PY 117, 1906/3/30.) More important, there are no diary or calculational records supporting Peary's 1909 use of this or any other method for finding true north via sextant or theodolite.



FRA OBSERVATIONSBOKEN:
En side av azimuth- og længdeobservationene.

F7 The observationsbook consistently records index correction IC = $-1'$. For observation #1, the navigation tables carried by the party gave refraction & parallax ($r\&p$) = $2'51''$ & solar semidiameter (ssd) $16'11''$. Starting from these data, we may induce the expedition's likely celestial navigation computations for the 3 azimuth-longitude observations: see DR reconstructions below (§F10, §G3-§G10). The math is rough & even sloppy in places; however, [a] the sph trig part of the calculations (the hardest part of the pure math involved) is carried out with complete competence; [b] the results are fully adequate for steering purposes, since the compass bearing B of the Sun's center (a key raw datum for aiming towards the Pole) is observed & recorded to no better than 1° precision.

F8 We know that the solar position table used by the expedition was based on Greenwich Apparent (not Mean) Noon, since such data were copied out (for 1911/9) on observationsbook p.7 (reproduced at AP 2:77). These data agree to about $0''.1$ with the same table of the *American Ephemeris & Nautical Almanac* for 1911 p.146. So we may recover Amundsen's 1911/11/9&13 math by using the 1911/11 data of the same reference work (p.182).

F9 Presuming Amundsen discarded his astronomical math scratch-sheets (not raw data), we look to the extant record for clues to the computers' precision habits. A surviving meridian-shot latitude calculation[51] shows that arcsec precision was normally maintained, though the usual arcmin precision of the azimuth calculations' outcomes suggests that the precision in the latter part of the azimuthal math was to the arcmin.

F10 Since the Amundsen field records used the Australian date (fn 29), we know that Obs #1 was taken between 1911/11/8 & 11/9 of the tables. The computer (perhaps Helmer Hanssen)[52] found solar declination δ by interpolation: the hourly rate for 1911/11/9 is $-43''.51$. Now, one of the commonest[53] errors[54] in such work is confusion of local & Greenwich time when rapidly entering position tables (based on the latter). For Obs #1, the computer found the declination of the Sun's center by using the recorded (local mean) civil time (c.17^h after midnight), not GMT[55] (c.16^h after GMNoon). So, multiplying $-43''.51$/hr times 17^h and adding the result to the 1911/11/8 tabular GMN declination $-16^\circ 19' 56''$ yielded $\delta = -16^\circ 32' 15''$. For Obs #2 (a few minutes later), the other computer (perhaps Amundsen) used the tabular 1911/11/8 hourly rate (decl change), $-44''.20$: this times 17^h, added to $-16^\circ 19' 56''$ (tabular GMN δ), gave $\delta = -16^\circ 32' 27''$. (Presumably this was done by a different person than was Obs #1; he certainly used a different chronometer: chron #6 now instead of #3.) Maybe trying to save time (or as a safeguard against inconsistent procedure), observer A used — invalidly — the same hourly rate ($-44''.2$/hr) in both his calculations (Obs #2&#3; both are based on the same chron #6). So, for Obs #3 (1911/11/13): $\delta = -17^\circ 28' 24''$ (tabular GMN δ) + $16^h \cdot (-44''.2/\text{hr}) = -17^\circ 40' 11''$. (All 3 results are listed here in the δ column of Table 2.) Similarly, for ease, all 3 calculations

[51] Observationsbook p.33 (photocopy at AP 2:85). Note that this and other crucial worksheets are co-signed. The extensive 1911/12/17-18 round-the-clock Pole observations, Set B, were co-signed by all 4 navigators: R.Amundsen, H.Hanssen, O.Wisting, [S.] Hassel. (Set B and the 1911/12/16 Set A, co-signed by Amundsen & Hanssen, are printed at MM 77-78; Set B also in original observationsbook pp.168-169, photocopy at AP 2:136-137.) By contrast, Peary has no such co-signed data sheets for the disputed last third of his purported 1909 trip to the North Pole.

[52] On the data page, "(H)" appears at the upper right of the entry for Obs #1, while "(A)" appears at the upper right for Obs #2&3. These letters may refer to Hanssen & Amundsen, respectively. Observer H used chronometer #3; observer A, chronometer #6.

[53] Over years of reviewing nonastronomers' astronomical calculations of this type, I have repeatedly encountered such a confusion. (In this case, its effect is trifling: the net error is merely ordmag 1 hr, thus the corresponding declination error is well under $1'$.) It should be noted that Amundsen did not make this error in the latitude computation for 1911/11/15 (photoreproduced at AP 2:85). In fairness to Peary's high engineering & surveying skills, I should add that (in my opinion) Peary would be extremely unlikely to make such a slip.

[54] The small Norwegian math errors proposed here (producing Table 2) are not hypothesized independently. They are DR-inductions, fitted to explaining navigationally-insignificant errors in the Norse results (Table 1 reported vs. Table 3 correct).

[55] Though the term "Greenwich Mean Time" is now commonly used interchangeably with Greenwich Civil Time, I use the former to signify noon epoch (the latter being midnight epoch). GMT was used in most national ephemerides prior to 1925; GCT, from 1925.

used (invalidly) the same $r\&p$ ($2'51''$) without temperature or pressure correction[56] and the same ssd ($16'11''$).

G Recovering Amundsen's Spherical Trig Calculations

G1 To compute the compass variation V (and thus compass direction toward the South Pole) from the data of Table 1, Amundsen first needed to find the Sun's true azimuth A from his transverse observation of the Sun's altitude h. Today, we would use the formula:

$$\cos A = \frac{\sin \delta}{\cos h \cos L} - \tan h \tan L \tag{2}$$

where A = azimuth, δ = declination of Sun's center, h = true altitude of Sun's center, L = geographical latitude.

G2 In the era before pocket calculators, such precision work was done by logarithms. But solving eq. 2 by logs is taxing, due to the subtraction. So navigators preferred[57] the following standard formula (e.g., *Bowditch Navigator* 1903 p.111):

$$\cos \frac{A}{2} = \sqrt{\cos s \cos(s-p) \sec L \sec h} \tag{3}$$

where north polar distance $p = 90° - \delta$ and $s = (h + L + p)/2$. To illustrate, we will next reconstruct the complete Amundsen reduction & computation of Obs #1.

G3 From Table 1, which is taken from Amundsen's observationsbook (p.127, reproduced at AP 2:115 & here at p.70), we have lower limb double altitude $hh = 35°43'30''$. Applying IC = $-1'$ and dividing by 2: apparent altitude of Sun's lower limb = $17°51'15''$. Subtracting $r\&p$ (*BowdNav* 1903 Table 20B) = $2'51''$ leaves: $17°48'24''$. Adding ssd = $16'11''$ (*AENA* 1911 p.182), we have true altitude of solar center $h = 18°04'35''$, as shown in Table 2. In brief:

$$h = (hh + \mathrm{IC})/2 - r\&p + ssd \tag{4}$$

G4 Now to set up the sph trig process. Above (§F10), we reconstructed the Norwegians' adopted $\delta = -16°32'15''$; thus, $p = 90° - \delta = 106°32'15''$. Since (Table 1) $L = -83°$, we have $s = (h + L + p)/2 = 20°48'25''$. And $s - p = -85°43'50''$.

G5 To solve eq. 3, Amundsen's computer now consulted 5 place log-trig tables like those of *BowdNav* 1903 Table 44. From these, he found log sec h = 10.02198, log sec L = 10.91411, log cos s = 9.97071, log cos $(s - p)$ = 8.87185. Summing and halving (which accomplishes the square root in eq. 3) results in log cos $(A/2)$ = 9.88933, the arccos antilog of which is $39°11'$. Doubling[58] yields solar azimuth $A = 78°22'$. Since this is an evening observation, our A sign convention is positive to the west, counting from north = 0°.

G6 Our next math step is: finding the desired compass direction to the S.Pole (which is merely the negative[59] of the compass variation V). For this, one combines the foregoing result (solar true azimuth A) with the observed compass bearing B of the Sun's center, using simple arithmetic:

$$V = B + (180° - A) \tag{5}$$

The observationsbook (AP 2:115) records that the Sun's center was sighted at compass direction "SE by S 1/4 S",[60] which means 1/4 of a compass point south of "SE by S". Since SE by S = 33°3/4, and a compass point = 11°1/4, then subtracting 1/4 of 11°1/4 from 33°3/4 yields the Amundsen party's Observation #1 of the compass bearing of the Sun: B = 30°15/16 E $\doteq 30°56'$ east of compass south. These values[61] are listed in Tables 1&2.

G7 To find the compass variation V, one simply (following eq. 5) adds this datum (probably in rounded form, $30°56'$) to the supplement of A (namely $180° - 78°22' = 101°38'$), yielding reconstructed $V = 132°34'$ (Table 2). Unfortunately, the hurried 1911/11/9 computer failed to carry during the addition, so he got and recorded $V = 131°34'$ (Table 1). Similarly (again by eqs. 3-5), for Obs #2, reconstructed $V = 134°28'$ (Table 2), and recorded $V = 134°28'$ (Table 1); for Obs #3, reconstructed $V = 141°00'$ (Table 2) and recorded V = 141° (Table 1).

Table 2 Reconstructions of Amundsen's Math

Obs #	h	δ	L	$A/2$	A	B	V
1	$18°04'35''$	$-16°32'15''$	$-83°$	$39°11'$	$78°22'$	$30°56'$	$132°34'$
2	$17°56'35''$	$-16°32'27''$	$-83°$	$39°46'$	$79°32'$	$34°00'$	$134°28'$
3	$19°49'30''$	$-17°40'11''$	$-84°$	$34°58'$	$69°56'$	$30°56'$	$141°00'$

G8 Computing west longitude W from scratch is (as we'll see below: §H1, eq. 10) as complex as computing V. However, once V is known, W is trivially simple to calculate. (Their inter-relation is why Amundsen himself describes his transverse solar altitudes as "azimuth- and longitudeobservations": §F4.) This point has evidently escaped previous commentators on the Norse 1911 navigation (fn 72). The elementary equation relating W and V is that[62] relating azimuth A and hour angle[63] H:

$$\sin H = \sin A \cos h \sec \delta \tag{6}$$

— and we find longitude W from H by simple subtraction:

$$W = G - H - 12^{\mathrm{h}} \tag{7}$$

where G = Greenwich Apparent Time, which is found from

$$G = T - K + M + EqT \tag{8}$$

(T is the time, read from one's chronometer; M is the west-longitude meridian the chronometer is supposed to be set for, 163°37'W for either Framheim-based Norse watch; EqT is

[56] Without access to Mohn's paper, Hinks believed (HY 171) that Amundsen did not bother to record temperature & pressure for accurate refraction computations. Amundsen made the records (MM 60-68; sample at AP 2:94) but evidently did not correct for $T\&P$ when computing refraction in the field. The effect here on the computed true altitude will be ordmag $10''$, which will cause ordmag an arcmin of error in these 3 computed azimuths. But this is of little account when the solar compass direction is recorded to whole degrees at best.

[57] For tabular solution of eq. 2, three tables are needed: log-trig, log, & natural trig. Inconvenient. Another approach: hav$A$ = [havδ' − hav$(L' - h)$]secL'sec$h$. (For computational speed, *BowdNav* haversine tables have traditionally melded nat & log tables, e.g., 1916 ed, Table 45; 1981 ed, Table 34.)

[58] It happens that this step ensures (since all B have even endings) that all our computed compass variation results must come out even in arcmin. And, indeed, all three Norse-calculated arcmin endings for V are divisible by 2 (Table 1: $34'$, $28'$, $00'$).

[59] We here define the compass variation as the angle the S.end of the needle makes with respect to true south. Therefore, when Amundsen observes on 1911/11/13 that the needle's S.end points 141° west (right) of true south, he knows that his course (to aim at the South Pole) must be 141° east (left) of compass south. Similarly, see F131.

[60] This is the same compass direction which US navigators call "SSE 3/4 E". See the compass card and a full list of the 128 compass quarter-points at *BowdNav* 1903 pp.15-16. Since SSE = 22°1/2 east of south, if we add 3/4 of 11°1/4 (a "point") to it, we have 30°15/16. (There are 32 points on the compass card, so a point = 360°/32 = 11°1/4.) Amundsen usually (except for Obs #2, where precision is to the nearest 1°) measured solar compass bearing B to the nearest quarter-point, which is 1/4 of 11°1/4 or 2°13/16. This roughness accounts for the slight discrepancies in the computed 1911/11/9 results for direction to the Pole. (The different result for 1911/11/13 is largely due to the intervening 60 mi shift of position.) Note that the division of the compass is history's oldest sophisticated purely-binary measuring scheme, using 128 quarter-points; 128 is the 7th power of 2.

[61] The direction is given precisely in *BowdNav* 1903 p.16: $30°56'15''$.

[62] *BowdNav* 1903 p.112.

[63] A celestial body's "hour angle" H is the angular amount by which it has passed the meridian (largely due to diurnal motion). Local H equals local sidereal time minus the body's rt asc.

App−Mean equation of time, tabulated in national ephemerides; K is the chronometer error. Both T & G are midnight epoch.) Correct computations[64] would have yielded the results set out in Table 3.

G9 A sample longitude calculation will make clear how simply one can solve for W after finding V: from Table 1, for observation #1, we have T = 16:55:54. Substituting this and the appropriate values (Table 3, fn 64, & §G8) into eq. 8, we have (in hrs): $G = 16^h55^m54^s - (-16^s) + 163°37'/15 + 16^m10^s.8 = 4^h06^m49^s$.

G10 Then, substituting A, h, & δ (already known[65] from Table 3) into eq. 6, we find $H = \arcsin[\sin 78°20' \cos 18°04'25''/\cos(-16°31'42'')] = 5^h04^m49^s$.
Substituting this H, as well as G (§G9), into eq. 7, we have W = 4:06:49 − 5:04:49 + 12^h = longitude 11:02 W = 165°1/2 W, as listed in Table 3 (where we use $L' = -L$, $\delta' = -\delta$).

Table 3 Rigorous Calculations

Obs #	h	δ'	L'	A	V	(V via grid)	W	(W via grid)
1	18°04′25″	16°31′42″	83°	78°20′	132°37′	(133°41′)	165°30′	(164°27′)
2	17°56′24″	16°31′47″	83°	79°28′	134°32′	(135°37′)	166°07′	(165°04′)
3	19°49′33″	17°38′59″	84°	69°42′	141°14′	(142°12′)	165°12′	(164°16′)

G11 The simplicity of the cartesian (Hinks or "grid") approach is easily demonstrated. Instead of computing A as previously (eqs. 2 or 3), we merely use an easy DR equation (fn 41, adjusted for the S.Pole region):

$$\cos A = (h - \delta')/C' \tag{9}$$

where C' = south colatitude = 90° − L' = 90° + L, while δ' = south declination, and we use $H = A$ (which of course is virtually true[66] near a Pole). This yields $A = H$ = arccos[(18°04′25″ − 16°31′42″)/7°] = 77°1/4. Adding B to the supplement of this (eq. 5) produces 133°41′, as listed in Table 3.

G12 Finding longitude W via grid math is just as simple: using eq. 7, merely add G = 4:06:49 (§G9) to the supplement of H = 77°1/4 (§G11) to find W = 164°27′, as listed in Table 3. It was already noted (fn 35) that, so far from the Pole (7° of latitude), these grid-math-based values are off by about 1°; however, such crude results would still have been useful to Amundsen, given the imprecision of B. (And, close to the Pole, the grid approach is as accurate as a practical navigator could possibly wish: fn 32, fn 41.)

G13 By contrast, the rigorous eq. 3 (sample solution at §G5) involves precisely the type of standard navigators' sph trig which eminent institutions & authorities have been denying Amundsen used (§F3) — but which §G7 showed was in fact his method. Given that the three Amundsen transverse solar observations (& math) we have recomputed here (Table 2) were made in 1911 mid-November, we note a conventional-wisdom comment on his 1911/11/28 position (HU 454, emph added):

> When the region was finally mapped, [Amundsen's] dead reckoning position at the Butcher's Shop [where most dogs were killed, 1911/11/21-26] proved to have been within a mile of the true one, fixed by his own latitude observation and bearing on Mount Fridtjof Nansen. . . .
>
> Since he had depended for longitude *exclusively on compass and sledge-meter* after leaving Framheim, now 400 miles behind, this was by any standards an astonishing degree of accuracy. Without maps or the footsteps of others to guide him, however, Amundsen had no way of knowing this at the time.

This conclusion is doubly peculiar in that the position of the Butcher's Shop (85°36′S) is explicitly (DN 331) taken from Mohn, who states (MM 41-45) that his estimation of longitude (167°.7 W) is based not on dead reckoning since Framheim (78°38′S) — 418 mi ago! — but is founded on the single Amundsen field longitude-calculation at the foot of the Norse ascent (85°05′S), merely 31 miles ago. This instance highlights how innocent Huntford is of Amundsen's repeated use of transverse solar altitudes for controlling his steering — yet, as we have seen (§D & §E), it is Huntford's contingent evaluations that have been the foundation of Peary-apologists' fantastic insistence that an explorer would think nothing of steering hundreds of miles to the Pole without taking transverse solar data for aiming. One must keep in mind that Amundsen's S.Pole trip was enormous in sheer distance: the equivalent of going from Spain to Poland & back, by foot — carrying all one's food & supplies both ways for most of the distance.

H Scott's Navigational Math

H1 To illustrate that the British method for finding longitudes was (contra previous unanimous Expert opinion: §C3, §D3, §E2) no more difficult, tedious, or time-consuming than the Norwegian math for finding compass variation, we will now examine the Scott expedition's math for the transverse observation made on 1912/1/13, shortly after crossing the 89th parallel. Examination of a photocopy of this calculation (HU 458c) shows that [a] it was performed by Scott's navigator, H.Bowers,[67] and [b] it uses the formula:[68]

$$\sin\frac{H}{2} = \sqrt{\csc C' \csc p' \sin s' \sin(s' - Z)} \tag{10}$$

where zenith distance $Z = 90° - h$, south polar distance $p' = 90° - \delta'$, and $s' = (Z + C' + p')/2$. (H & C' are as already defined, above: fn 63 & §G11, respectively.)

H2 During the evening of 1912/1/13 (3^d before encountering Amundsen's black flag near the Pole), Bowers took the solar upper limb's altitude: 21°57′07″.5. After correcting for $r\&p$ and ssd to find true h, he subtracted this from 90° to find zenith distance Z = 68°21′21″. Adding the afternoon march's distance (obtained by odometer), onto his noon meridian shot's latitude result, indicated that $L = -89°08'.9$, so (§G11) $C' = 0°51'06''$. Tabular δ' = 21°41′40″, so p' = 68°18′20″. Thus, $s' = (Z + C' + p')/2 = 68°45'23''$, and $s' - Z = 0°24'02''$. Turning now to log-trig tables, Bowers estimated: log csc C' = 11.827868, log csc p' = 10.031905, log sin s' = 9.969440, log sin $(s' - Z)$ = 7.844540. (All

[64] Using precise ephemeris data, we have ssd = 16′11″ for 11/9, 16′12″ for 11/13; equation of time EqT = 16^{m}10^s.8 for Obs #1, 16^{m}10^s.6 for Obs #2, 15^{m}47^s.7 for Obs #3. Parallax is 8″.4 for 11/9, 8″.3 for 11/13. Using the $T\&P$ given at MM 62, atm refraction is computed according to an original DR procedure (summary in *DIO 2.1* ‡4): 3′09″.4 for Obs #1 (−13°.2 C = 260°K, 736.0 mm Hg = 981 mb), 3′10″.8 for Obs #2 (same $T\&P$ as Obs #1), 2′57″.5 for Obs #3 (−18°.9 C = 254°K, 743.3 mm Hg = 991 mb). In Table 2, no chronometer correction was applied. For Table 3, we adopt the error vs. Framheim Mean Time (163°37′W) given for chron #3: 56^s − 3^s/day from 1911/10/16. November comparisons of chron #6 to chron #3 are tabulated at AP 2:107 (obsbk p.115): −57^s for 1911/11/9, −77^s for 1911/11/13. Thus, the chronometer errors K (chron minus FMT) for the 3 observations are, respectively, −16^s, −73^s, & −105^s. (Again: the Peary 1909 record contains no en-route watch comparisons.)

[65] These do not of course differ much from the rougher field-computed values of Table 2.

[66] Depending upon the convention adopted (for A & H): A may equal (instead of H) the negative and-or supplement of H. (See, e.g., fn 70.) This does not change the fact that, near the Pole (where $|h| \approx |\delta|$), eq. 6 renders A & H effectively identical. The simplification thus introduced — into the problem of navigating in the immediate vicinity of a geographical Pole — has been intuited by every Pole-attaining navigator. (See, e.g., fn 32.) But neither Amundsen nor Scott realized that the same (cartesian) simplication is useful even 100 mi from the Pole.

[67] The handwriting is Bowers' (HY 166). Bowers was generally called "Birdie", presumably for his beaklike nose. (See photo HU 458b: seated 4th from right.) He was the youngest, cheeriest, strongest (by weight), most durable, sharpest-eyed, and most mathematically able of the brave 5 man Scott party who died returning from the South Pole.

[68] Though of different form, eq. 10 is identical to $\sin h = \sin(L + p) - \sin^2(H/2)\cos L \sin p$ (DR's equation at RE 137, from which it is easy to prove eq. 12). But eq. 10 is more cleverly arranged for use with log tables. Eq. 10 is also the same as that found at *BowdNav* 1903 p.89, except that all the arguments in Scott's eq. 10 are consulted in the sine column of the tables, a useful consistency. (Note that use of eq. 3 is just as consistently confined to the cosine column.) Another form of eq. 10 is: $\text{hav}H = [\text{hav}Z - \text{hav}(L' - \delta')]\sec L' \sec\delta'$.

four interpolations are imperfect in the 6th place, perhaps[69] from dependence upon compact 5 place tables — by a computer who was nonetheless striving for the highest possible precision.) Adding these logs and (to perform the square root of eq. 10) halving the sum, Bowers then took twice the arcsin of the antilog, finding hour angle[70] $H = 93^\circ 14' 03'' =$ 6:12:56. Subtracting $EqT = -8^m 23^s.6$ from H produced the hour angle of the mean Sun: S = 6:21:19.6. Using eq. 8 (with null M for Greenwich), Bowers subtracted his (erroneous)[71] chron error value $K = 2^h 17^m 16^s.8$ from the chronometer mean time $T = 9^h 44^m 42^s.4$ and added 12^h, thereby obtaining GMT = 19:27:25.6 (where GMT = $G - EqT - 12^h$). Using the simple equation (essentially identical to eq. 7)

$$W = \mathrm{GMT} - S \tag{11}$$

Bowers found W = 19:27:25.6 − 6:21:19.6 = −10:53:54. Multiplying by 15°/hr gave $W = 163^\circ 28' 30''$E longitude, which is the result displayed at the end of the computation (HU 458c). Written sideways on this page is a correction, entered when it was found that the adopted chronometer error K was off by 10^m (fn 71). This shifts the deduced W from 163°1/2 E to 161°E, a distance of c.2 mi. (The corrected figure was used the next day: fn 77.)

H3 My prime purpose for examining the sph trig navigational math of Amundsen (eq. 3 & §G5) and Scott (eq. 10 & §H2) is to emphasize the falsity of the hitherto-infectious idea (§C1) that Amundsen's navigation was essentially simpler, a notion which the understandably-desperate Peary-defense team has fallen deeply in love with (§D & §E). Yet, as we have seen above, the central calculations of Amundsen's navigation were based on a sph trig formula (eq. 3) whose complexity is just *exactly* as great as that used by Scott (eq. 10) — as a moment's inspection-comparison reveals. (Amundsen's sph trig found A, while Scott's sph trig found H. Recall from eq. 6 that finding one from the other entails trivial labor. Thus, both men's calculations are not just equally non-simple; they also produce effectively the same knowledge.) Huntford's dramatic pictoral juxtaposition (HU 458c, §C1) contrasts the simple (addition-subtraction) Norwegian meridian calculation with the lengthy (sph trig-log) British calculations. The underlying misconception (that simple addition & subtraction is all Amundsen used) occurred primarily because modern historians who have looked through the Amundsen record have not recomputed his results (as here at §G5); had they done so, they would quickly have realized that Amundsen's work required standard navigational math (sph trig), applied to standard steering observations (transverse solar shots) — just the same as the British work. The analyses of Huntford, Moore, & NavFou rejected this reality and promoted instead the idea that Amundsen's Sun sights were entirely meridian shots.[72] But, as shown here (eq. 3 & §G5), Amundsen's steering was naturally controlled not by simple meridian observations but by transverse observations: standard navigational practice — and the basis of the very 1973 DR discussions (e.g., §B6-§B7) in *Fiction*'s Chap.10 ("Quiver in the Arrow") to which Moore & the NavFou took such violent & now deliciously ironic[73] exception. In 1973, DR had stated (§B5-§B7)

[69] But BTN 12 says that six-place tables were available.

[70] Literally, the supplement of the directly computed H. See fn 66.

[71] The adopted chron error K should have been 2:07:16.8. See HY 162.

[72] Huntford (§C1 & §G13), Moore (§D1-§D2), & the NavFou (§E1-§E2, NG 61-62) state that, unlike Scott, Amundsen saved Sun-shooting & computing time by basing his "simple" navigation on "the comparatively easy meridian observation" plus "dead reckoning" by compass "checked by frequent azimuth observations". They did not comprehend (as Mohn apparently did: fn 17) that "azimuth observations" (AS 2:117, fn 28) obviously didn't mean merely sighting the Sun's compass bearing B (item#364 of *BowdNav* 1903 p.111) — it entailed additionally a simultaneous transverse sextant shot of solar altitude h by Amundsen (just like Scott), for use in sph trig computation of solar azimuth A in eq. 3 & V in eq. 5 — both equations illustrated at *BowdNav* 1903 pp.111-112 item#365. Misunderstanding Amundsen's plain reference to such utterly standard altitude-azimuth navigational procedure doesn't say much for the pretended navigational expertise of SPRI, Moore, or the NavFou. (The central azimuth equation of Amundsen's steering, our eq. 3, has been in the *Bowditch Navigator* seemingly forever; see, e.g., 1903 ed. p.111, 1916 ed. p.147, 1984 ed. p.586.)

[73] §D3 & §E1.

that: [a] transverse celestial observations are required for steering, & [b] Amundsen had used such data to check which way was true south, only 95 mi from the South Pole. This contention has been totally vindicated by Heckathorn's finding of raw Amundsen transverse solar observations and by the availability of Mohn's complete list[74] of the sph-trig-computed compass-variation results of Amundsen's whole 1000 mi series of such observations. So, the upshot of Heckathorn's discovery (and the foregoing discussion): exit another curiously widespread establishment exercise in weird-science mythology.

I Ex-Meridian Overprecision & Fatigue

I1 Despite his misunderstanding of the rôle of transverse observations in the 1911-1912 S.Pole race, Huntford's criticism (§C3) of the Brits' overdone ex-meridian math is on-the-mark. He reproduces an ex-meridian sight computation (HU 458c) for 1912/1/14. It appears to be in both Bowers' & Scott's handwriting (HY 166): the data recorded by Scott, the computation mostly by Bowers, & then touched up by Scott. Here we do indeed see wasted labor, in 2 senses: [a] needlessly cumbersome technique (vs. §I3), [b] insufficient gain in accuracy of location (§C1, §C3, & §I2).

I2 The lengthy 1912/1/14 calculation was performed merely to correct for the fact that the Brits' noon meridian[75] observation had been a bit late. To calculate the required correction k (which must be subtracted from the meridian-arithmetic-computed south latitude L': eq. 1), Bowers used the formula[76]

$$k = [1''.963 H^2] \cdot [\cos L' \cos \delta' \csc(L' - \delta')] \tag{12}$$

where H is taken in timemin. The main British computation used the erroneous longitude −163°1/2 = 10:54 E of the previous day's math (§H2). Then, from eqs. 7 & 8 (with zero M: §H2), Bowers added 24^h and computed $H = T - K + EqT - W + 12^h$ = 3:45:46.5 − 2:07:13.5 − 0:08:41 + 10:54 + 12^h = 24:23:52. This result was then misread[77] as H = $24^m 23^s$ and was substituted into eq. 12, along with $L' = 89^\circ 15'.2$ & $\delta' = 21^\circ 34' 08''.6$. Bowers used csc Z in place of $\csc(L' - \delta')$ in eq. 12 (a clever saving of labor via eq. 1, with negligible loss of accuracy). He had just observed $Z = 67^\circ 41' 46''$ (true Z, after correction for $r\&p$ and ssd). Based on the foregoing values, the first bracketed term in eq. 12 was $1167''$. Bowers added its log (3.067071) to those of the 3 components of the 2nd bracket: 8.114990 + 9.968470 + 10.033770 + 3.067071 = 1.184301, the antilog of which is $15''.3$ — merely a quarter of a nautical mile, a trivial distance.

I3 Not only is the result negligible, but, moreover, this entire computation can be accomplished (to all needed precision), in about a minute, from the simple equation (which is easy to derive from DR's eq. 9):

[74] The 6th column of MM 42-43: 8 Norse sph trig determinations of V between Framheim and 88°.4 S. Evidently (§J3) most such work was doubled to guard against error: 2 navigators independently observed h & B and independently computed V therefrom. V was observed at Framheim (MM 37); and, at the Pole itself, 37 more B data were taken (MM 77-78). The full 1911 Norse record of compass variation V: 107° at 78°38'S (Framheim), 119° at 81°S, 129° at 82°S, 133° at 83°S, 140° at 84°S, 140° at 84°2/3 S, 145° at 86°S, 143° at 87°S, 147° at 88°.4 S, & 146° at 88°.8 S. (The last V was deemed too rough for use, AS 2:117.) The B & chronometer data taken at the Pole show that the needle's N.end pointed along c.18°W there.

[75] Due to variation in solar declination, culmination occurred about 8^m before transit.

[76] *BowdNav* 1903 p.97; coefficient $1''.963 = 5''\pi/8$. Eq. 12 follows easily from the 1st equation in fn 68.

[77] Once the 10^m longitude error (§H2, fn 71) of 1912/1/13 had been apprehended, the ex-meridian correction was recomputed for the revised longitude $W = -160^\circ 58' 15''$ (HU 458c). But this too was slightly misread, as $-160^\circ 15'$. (The repeated difficulty, of merely reading numbers, evidences the toll of weeks of brutal wear and poor nutrition, so ably & convincingly documented by Huntford: e.g., HU 510.) Division by 15°/hr yields W = −10:41; subtracting that instead of −10:54 in the calculation of §I2, yields 24:10:52 (instead of 24:23:52), thus the observation was c.11^m after transit. Multiplying $1''.963$ times the square of 11^m yields $237''.5$ for the first bracketed term in eq. 12, the log of which is 2.375664. (This appears on the calculation sheet: HU 458c.) Using this in §I2 instead of 3.067071, we find $k = 3''.1$, instead of $15''.3$. This was too minuscule even for the British Navy, so the calculator didn't bother to complete the revised deduction of k.

$$k = C' \cdot (1 - \cos H) \tag{13}$$

Thus, $k = 44'.5 \cdot (1 - \cos 6^\circ) = 89' \cdot \text{hav } 6^\circ = 15''$. The difference between the 2 methods of computation amounts to a few meters.

I4 Hinks was the first to make public (HY 165) the fact that Bowers had miscomputed the solar declination by $2'$ (2 mi) while working up the first Sun sight on 1912/1/17. Thus, when the British party thought they were a mile past the Pole, they were in fact a mile short. Huntford stresses (HU 516, emph in orig) that this error created the irony that the British "*never quite got to the Pole at all*." This may seem to imply that Scott didn't go as far as the Pole, whereas it is clear that in truth he traveled almost exactly far enough. (Yes, he went a bit to the right; but this was largely due to following Amundsen's initially erroneous path: fn 35. Until encountering the Norse track, Scott had been aiming almost precisely at the Pole: §C4.) But, as one sees from the map at HY 174, he got diverted by Amundsen's wayward trail — and that deflection & resultant meandering cost him 5 precious miles in all. (That is, the circuitous Scott path near the Pole, reconstructed on HY 174, is 5 mi longer than a straight line to&from the Pole would have been.) Since Scott later died 11 mi from his One Ton Depot, this 1912/1 extra distance could have contributed to his fatal inability to forge ahead from his later final camp of 1912/3/21. On 1912/1/18, Scott passed about a half-mile from the exact South Pole, according to Hinks' map (HY 174) or Huntford's (HU 517, obviously much based on HY 174). Since the errors of the observations are ordmag 1 mi, Scott may be said to have reached the South Pole.

I5 We recall that there was another British error of 2 mi (§H2). But we note that such slips are much less than Amundsen's occasional navigational errors. E.g., the 1° error in one of the Norwegians' 1911/11/9 compass variation computations (§G7), would (if uncorrected-for later) have been worth 7 mi at the Pole. And his actual rightward miss of the Pole was (from other, later errors) about this large. Given the massive labor being accomplished by Scott's party — the longest manhauling polar trip ever (suicidal though it was) — it is hard to fault Bowers merely for being fallible (fn 77). I prefer Hinks' judgement, when he notes the British 2 mi error at the Pole (HY 177): "Apart from this one mistake by an overtired man, the observations and deductions made by Bowers were excellent: far better than was allowed in the official account of them." And I trust that my own criticisms, of a very few lapses in the enormous effort that went into Huntford's thorough, revolutionary, & often wise book,[78] will be viewed with the same generous perspective.

J Amundsen's Path to the Pole

J1 We are now in a position to determine the hitherto somewhat uncertain (§C1) track of Amundsen's 1911 South Pole journey, which is *the* grand, shining feat of all polar history. Astonishingly, the accurate longitudes of Table 3 (as computed in §G10) are the first[79] W computed from Amundsen's sextant data since the expedition members roughly did so themselves, in the field, back in 1911 ! Given the greater care taken here, the positions should be more accurate. The results show the need for some revision[80] in the following:

[78] Aside from numerous solid, intelligent observations, Huntford's book also has its deft touches, e.g., the reference (HU 237-238) to Scott's wife-to-be Kathleen as "a predatory female; more predatory than usual, that is."

[79] It will be noticed that, for the trip over the Ross Shelf, neither hypothetical field-calculated longitudes (Table 2) nor correct longitudes (Table 3) are provided by Mohn, who simply lists (MM 60-62) the Framheim longitude ($163^\circ.6$ W) for all camps from start until 1911/11/14, as Amundsen approaches the mountains blocking his path to the Pole (MM 62).

[80] There is an odd kink in the Amundsen path depicted at NGS 1981 *Atlas* p.220: when approaching the Pole, Amundsen is shown (when c.80 mi from it) jumping suddenly from 170°W to 157°W — which he certainly did not do. Presumably from confusion of V & W, A.Alexander's wildly false incoming path (AS 2:401) for Amundsen is parallel to 146°W. Hinks & Huntford's maps more reasonably bring Amundsen in on paths parallel to 169°W

[a] Mohn's assumed straight path (along $163^\circ.6$ W) over the Ross Shelf (MM 62), [b] the Huntford map (HU 481), and [c] the 1911 path shown on the generally excellent National Geographic 1981 *Atlas of the World* (p.220). At both 83°S & 84°S, all 3 of these sources put Amundsen c.2° of westerly longitude less than his actual now-recovered (formerly Nonexistent) longitude observations (Table 1) actually place him, according to Table 3 — which has Amundsen further west in mid-November than any previous account. E.g., Mohn (MM 62) places Amundsen at the Framheim longitude ($163^\circ.6$ W) on 1911/11/14; but we have found (Table 3) that, on 11/13, he was ordmag 10 mi to the west of this, at about longitude $165^\circ.2$. (Note: this and other longitude estimates for Amundsen depend upon the accuracy of the chronometer correction, given at fn 64. If this is valid,[81] then the longitudes are far more accurate than the compass variation findings, which are based on solar compass bearing data given only to a quarter point of the compass, c.3°. It should be understood that, in Table 3, our computed compass variations V are virtually independent[82] of the chronometer, while our computed longitudes W are entirely independent of the imprecise solar compass bearing observations B.)

J2 And the Amundsen longitude calculation at $85^\circ 05'$S put him (MM 41) at $162^\circ.8$ W (about where NGS has him), not a longitude degree to the east of that (as shown at HU 481). After 84°S, Amundsen swerved well to the left (as at MM 44 & 62-63, HU 481) when he approached the Queen Maud Mts, presumably attempting to get as far south as possible on the Ross Shelf before ascending these enormous obstacles in his path, which he was about to challenge and — amazingly — conquer. From the afternoon of 11/15 until the morning of 11/17, Amundsen aimed his expedition about 6° to the left of the true-south compass course of 140° he'd determined on 11/13 (§J3): his course was "NE by N" or 146° 1/4 left of compass south.

J3 From the records listed by Mohn (MM 37, 42-44; fn 74), we have all the Amundsen compass variation determinations V & courses for the entire round trip. (Photocopies of the observationsbook's handwritten daily compass course records for $80^\circ.4$ S to 82°S are provided at AP 2:128; those for $86^\circ.4$ S to the Pole, at AP 2:79.) For 1911/11/9, V = 133° NE (MM 42; see also HU 458d), the mean of the 2 determinations of V that day (Table 1). For 1911/11/13, $V = 140^\circ$ NE. (This may have been the mean of two 1911/11/13 readings, only the first of which was published in AP: $V = 141^\circ$ NE in Table 1. See fn 74. We recall that 2 independent determinations were performed on 1911/11/9.)

J4 Drewry & Huntford have done such a scrupulous & expert job of reconstructing Amundsen's previously-unsolved path through the mountains (DN 332, HU 443) that I will not try adding to it. (After 1911/11/29, this path is not very sure: fn 80.) But much less certain reconstruction has been done regarding Amundsen's approach to the Pole itself, a situation I will here attempt to improve upon. Hinks suggests that Amundsen came in along c.169°W (HY 164, 160); Huntford, c.168°W (HU 443).

J5 We begin by examining a curious implicit contradiction in the Hinks map of the South Pole vicinity, portraying the Poleward paths of Amundsen & Scott. Amundsen's compass

(HY 160) & 168°W (DN 331&332, HU 443, 491), respectively. HU 443 (& DN 331) has his course $184^\circ 22'$ azimuth (east of north) on 1911/11/26-27, then swerving to $178^\circ 45^\circ$ for 11/27-29, and then reverting to $184^\circ 22'$ after 11/29. (Obviously, true south is 180° by this convention.) Consulting Mohn's table of course-data, we see what has happened: the authors (somewhat loosely, considering the arcmin precision displayed in the DN & HU discussions) took the 1911/11/27 observation-based compass variation value, $V = 145^\circ$NE (MM 42) to apply to this entire region, thus 35° east (right) of compass north is taken as true south, which makes "NE 1/2 N" equal $184^\circ 3/8$ ($184^\circ 22'$ 1/2) and "NE by N" equal $178^\circ 3/4$ ($178^\circ 45'$). Actually, Amundsen's intended course was (see MM 42) the latter not the former after 11/29 (until 12/5 at c.$87^\circ.1$ S). However, HU 443 carefully says "No reliable compass course" for this stretch, and indicates plenty of zigzagging; see also DN 332. Below, we will confirm (§J7) that Drewry & Huntford made a remarkably good estimate of Amundsen's actual mean course during this murky period.

[81] If the chronometer correction is badly in error, then the longitudes here revealed will also be mistaken — virtually at a rate of $1^\circ/4$ for every timemin of chron error. This near the Pole, the corresponding distances will be trivial, so our deduced transverse positions (Table 3) are likely to be good to ordmag a mile.

[82] The calculation of solar declination (necessary for finding V) depends on the time, but this is far less sensitive than longitude-calculation's dependence upon use of correct time.

course was unquestionably "NE by N" or 146°1/4 (AP 2:79, MM 43). And Scott's compass course was 180° or simply: compass north (§K1, HY 164, 174; also maps at HY 163, 172, 174). Thus, Amundsen & Scott's paths must intersect (see map at HY 174 or HU 517) at an angle of nearly $\theta = 180° - 146°1/4 = 33°3/4$. Now, the 2 paths cross at the 1912/1/16 Scott camp, which was virtually identical (HY 164) to the 1911/12/14 Amundsen camp. Bowers' observations make the camp's colatitude $C' = 14'.4$ (HY 164), which agrees well with Amundsen's $C' = 15'$ (AP 2:79, AS 2:119, MM 43, HY 164). Finally, we know that Amundsen missed the Pole by no more than 7 mi to the right.[83] So, the contradiction is simple: $\arcsin(7'/14'.4) = 29°$, but $\theta = 33°3/4$. In brief, one component of the foregoing deductions is false. It is this 5° disagreement which has Amundsen's path coming into the Pole parallel to 173°W, in both Hinks' and Huntford's maps (HY 174, HU 517) — contradicting the paths both state in their texts (§J4), by very nearly this 5° discrepancy, naturally. And there is an even more obvious problem with Hinks' map (HY 174): it shows Amundsen's course approaching the Pole about 27°1/4 E of magnetic north — whereas Amundsen's own records (AP 2:79, MM 43) state that his course was 33°3/4 E of magnetic north, a discrepancy of about 6°1/2.

J6 In any case, there is an extremely simple way to find Amundsen's approach to the Pole:

[a] His incoming compass course was 146° (§J5).

[b] Compass north was nearly along 18°W (§K1). Thus, since 146° + 18° = 164°, we know that Amundsen approached the South Pole along a path closely parallel to the 164°W longitude meridian.[84]

J7 Next, we estimate Amundsen's absolute longitude on 1911/12/8, at $L' = 88°25'$ ($C' = 1°35' = 95'$), where[85] he made his last effective (fn 28) compass variation determination (MM 43: 1911/12/8), $V = 147°$ — and then responsively (MM 43) locked in his compass course at "NE by N" (146°1/4) for the rest of the Poleward trip. Following this course, he missed the Pole (to the right) by 7 mi, from a distance of 95 mi; so, a first-order estimate of his 1911/12/8 westerly longitude is 164°W + $\arcsin(7'/95') = 168°+$. But, we must also realize that (as he moved slightly clockwise around the S.Magn Pole) the compass needle rotated (fn 35). This rotation was roughly 3°. The mean steering error contributed by the effect equals about half that amount. Adding this to 168°+, we can estimate that Amundsen's 1911/12/8 position was about 88°25′S, 170°W. This result roughly vindicates the inspired Huntford suggestion (HU 443) that, during the days after Amundsen crossed 86°21′S, he was wandering somewhat rightward of true south (though his intended compass course was a little leftward: fn 80). Thus, by 1911/12/8, his westerly longitude ended up not 168° or less but nearer 170°.

J8 We now know Amundsen's 1911 movements[86] alot better than previously. And we can probably learn more. (Note: the present paper's analyses of Amundsen's 1911 work

[83] Hinks' careful analysis put Amundsen's final Polheim camp near $L = 89°58'.75$ S ($C' = 1'.25$) & $W = 71°.6$ E (HY 173). After the Norwegians' 1st camp near the Pole, they shifted their path 82° (literally, 81°9/16 ≈ 81°.6) to the left, from "NE by N" (AP 2:79, MM 43) to "NW 1/4 W" (AS 2:130, MM 47) and traveled 5.5 mi by odometer (AS 2:129). The indicated Norse miss-distance is about $5'.5 \sin 81°.6 - 1'.25 \sin(164° + 71°.6) = 6'.5 = 6.5$ mi, which (despite the foregoing's divergences from Hinks' treatment) is about what is shown on the map at HY 174. Computations (based upon an incomplete round of altitudes) of the 1st Amundsen camp's position put it about 6 mi from the Pole (AS 2:129, 400). Since this is good to ordmag 1 mi and since the §J5 contradiction eases for larger miss-distances, I prefer going with about 7 mi.

[84] Which means that Hinks' 173°-parallel path (HY 174, also HU 517: §J5) is nearly 10° off — while the chart (BTN 29, HY 175) of the official British report (BTN), which Hinks repeatedly (often justly) denigrates, was in fact about right on this point. Perhaps fortuitously. The BTN chart's depiction of Amundsen's approach may well have been based (from forgetting the Norwegians' severe detour west, as they went up through the mountains onto the polar plateau) merely upon the crude — but in this case accidentally correct — assumption that he must have come at the Pole parallel to the Framheim meridian, which happens to be nearly 164°W.

[85] Amundsen stayed at this camp-depot 1911/12/8-10.

[86] He made (MM 48) no astronomical observations during the return trip: mostly 1912, and almost entirely along the outward trail.

have deliberately been based strictly upon data which have been on the accessible public record for most of this century.) So, I urge that the Norwegian Polar Institute transmit to *DIO* photocopies of the full original data of Amundsen's 1911 observations (& chronometer comparisons). If this is done, *DIO* will compute and publish the longitudes for each of the 8 or 9 en-route camps where Amundsen took transverse solar data, so that the trail of his legendary 1911 expedition will become as accurately known as we can make it.

K Bunker Buncombe

K1 It is time now to compare Amundsen's massive 1911 proofs (of success) to Peary's 1909 bare evidential cupboard. As already noted (fn 74), there are a great deal more en-route compass data given in the Norwegian observationsbook than those published by photocopy in Amundsen's book (Table 1). The direction of the compass at the S.Pole was first recorded by discoverer Amundsen. According to my analysis of Amundsen's compass data[87] taken at the South Pole, the north end of the compass needle pointed nearly[88] along the meridian of 18°W. This result has been verified by later explorers. Scott evidently[89] made it 19°W, since he found variation $V = 179°40'$ (BTN Map 3; HY 164, 174) along 161°E longitude (§H2). Peary's 1909 expedition produced no compass directions whatever. (He brought back from the N.Pole's vicinity no specific scientific data of any sort[90] which might be checked by later attainers of the spot.) When alibiing this outrage, the NavFou suddenly loses interest in making §E2-style comparisons to Amundsen's steering. Indeed, the NavFou doesn't mention Amundsen at all at this point and instead (§K4) simply says it was "unfortunate" that Peary made such a glaring omission — unprecedented for him (F130).

K2 Last-ditch defenders of Peary predictably will seek comfort in Amundsen's apparent failure to compute longitudes regularly in his observationsbook. Comments:

[a] No need for immediate computation of longitude, since finding compass variation got him where he was going. (Peary didn't find either longitude *or* variation.)

[b] Amundsen's caption (§F4) to his book's photocopies of 3 transverse solar altitude observations shows that he knew they were "longitudeobservations". For placing his topographical altitudes for science, these longitudes could be computed later, as in Table 3. By contrast, Peary's 1909 ocean depth soundings are virtually worthless since one cannot compute his longitudes in the entire absence of transverse solar data (§B6 & §E3).

[c] As Heckathorn's sharp eye has already noted, Amundsen's observationsbook locates, e.g., his 80°S & 81°S depots, in precise, explicit longitude terms. (See pp.3-4 of observbk, reproduced on AP 2:138-139, "79°58′.9 S [latitude], 163°53′W [longitude]" and "80°58′S [latitude], 164°34′W [longitude]".)

[87] Sets A & B are given fully at MM 77-78. A photocopy of the original handwritten Set B appears at AP 2:136-137 (from pp.168-169 of observbk). (Note that MM 78 miscopies the last double altitude by $+1'$.)

[88] The error will be ordmag 1°. Note that the results given by Mohn for the two groups of Pole magnetic data are impossible. We will distinguish Set A (14 data, 1911/12/16) and Set B (23 data, 1911/12/17-18). For Set A, Mohn makes the direction of the needle = 118° − 103° = 15°W, while, for Set B, he has it = 58° − 41° = 17°W. (See MM 48.) But the agreement (2°) is too good, since a raw comparison of the solar compass bearings B for the very same times in the 2 sets shows a mean discrepancy of c.5°. (This is probably due to a systematic difference of $c.20^m$ in the time of observing the solar bearings.) To get a consistent result, I have used only the 5 pairs of B data which agree within a quarter of a compass point (2°13/16). They indicate that: the compass needle pointed along meridian 18°W ± ordmag 1°.

[89] See §J5. The last Scott camp for which compass variation is shown in BTN (Map 3) is that for $L' = -89°09'$, $W = -161°$. [See also SL 394.] But the variation stayed so steady, at nearly 180° (along the latter part of Scott's track), that the final 51′ obviously made little difference. Unfortunately, BTN (by geologist F.Debenham) neglected to publish most of the raw theodolite data of the Polar journey. (MM likewise provided none of Amundsen's raw sextant data.) This despite Hinks' expressed wish (seconded by Scott, fn 35): "it is important that full details of all the observations [sights] made on recent expeditions should be published" (HF 301).

[90] Peary's soundings did not touch bottom for the last 2/3 of his alleged trip. Amundsen not only recorded his compass direction throughout the entire trip, but also recorded his height above sealevel the whole way (MM 58). All verified by later explorers. Peary has nothing to compare with this.

[d] Amundsen did compute [longitude] W at $L = 85°.1$ S (§J2).

[e] As already shown here in detail (§H3), Amundsen's compass variation (transverse solar) sights & math are equivalent to Scott's longitude (transverse solar) sights & math — equivalent in all departments: results (via easy eq. 6; see §H3), difficulty, & sph trig-log math technique. Incidentally, on the point as to whether Peary used Amundsen's azimuthal equation (eq. 3) instead of finding longitude by Scott's equation (eq. 10): quite aside from Peary's denial (§K3) that he took compass data in 1909, we have the stark fact that, whereas the Peary Papers (PX) contain dozens of fully computed examples[91] of his use of eq. 10, there are no examples[92] of his use of eq. 3.

K3 When comparing the Peary 1909 N.Pole claim to the Amundsen 1911 S.Pole claim, the crucial point one must not be diverted from (by NGS consultants) is this: Amundsen's records provide (fn 74) a continuous, observation-based record of the compass variation for the whole trip, including the direction of the compass at the Pole itself. These data cannot be faked. By contrast, Peary explicitly denied to Congress (SPH 299, 310, F226-228, F234) that he had ever taken the very compass data Moore (MC 115, §D1) & the NavFou (NG 62, §E2) have stated was the basis of his 1909 "navigational method"! Even Peary's one compass variation estimate (WSW or 112°1/2 W, PZ 232, F131&138), at c.84°N, was not based on 1909 observations but was merely an estimate from his 1902 expedition. In that case, he of course used the standard transverse Sun-sight for longitude. (See Peary's longitude timesight & compass variation results: PX 124, 1902/4/14.) So, for 1909, not only do we have no compass observations from Peary (for the whole trip), but only one compass *course* record survives — 360 mi from the Pole! By contrast, Amundsen's 1911 compass course was recorded *daily* (§J3) from Framheim all the way to the S.Pole.

K4 And we must contrast not only the 1909 Peary vs. the 1911 Amundsen, but the 1909 Peary vs. the real explorer Peary — before he began hoaxing[93] bigtime in 1906&1909. Peary's customary navigation method was standard (§K3), not "simplified" as proposed by certain defense-lawyer exercises in wishful thinking (§E). The only compass datum Peary produced (V = 95°W), under 1911 congressional crossquestioning (regarding his 1909 trip), was over 400 mi from the Pole (SPH 299, F226), and it wasn't even his own — merely an obsolete British 1875-1876 measurement (see NN map). In brief, Amundsen took a full line of compass variation observations from homebase (Framheim) to the South Pole (fn 74) — while Peary by his own account took none, even though he stated publicly (see PZ 211-212, 276, F131, RV fn 12) that he had steered to the Pole by compass! (His diary never mentioned using compass steering. For the shocking 7 words he suppressed when reading his 1909/4/2 diary entry to Congress, 7 words which reveal the crude truth of Peary's actual 1909 steering method, see RV §C6 & §D1. Note that the NavFou suppressed the same words: NG 85.) The NavFou (NG 57 n.9) attacks this unanswerable DR point by slickly confusing the NavFou's own baseless fantasy (about Peary's steering by compass) with fact. The NavFou then adds by-the-way (NG 60 n.10) that it was merely "unfortunate" that Peary oops-forgot to record the very compass data the NavFou imagines (§K3) he used! Even if this were true, it would be weird enough to cause rejection of Peary's claim. But remember: it *isn't* true. The truth is far, far worse: our NavFou has just conveniently ignored Peary's own on-the-record statement (§K3) that he never *took* any such compass observations in 1909.

K5 Summing up the comparative situation: Peary has no compass data from the North Pole, while Amundsen recorded *thirty-seven* such data at the South Pole (fn 74, fn 87), producing the hitherto unknown (thus not fakable) 18°W compass direction there at that

[91] These are found throughout the genuine part of Peary's exploration career. E.g., PX 127: 1900/4/8, 5/9, 5/10, 6/11, 7/1, 7/23. It should be acknowledged that the late Tom Davies (NavFou) first recovered the raw data for the 5/10 calculations: PX 122.

[92] See, e.g., PX 122 (1900/5/10, 5/13, 5/16), results appearing at RR 38 opp. These compass variation determinations are just simple arithmetic based on the familiar method of equal altitudes (fn 50): no sph trig at all.

[93] For Peary's clumsiest fake (1906 "Crocker Land"), see RV §B1-§B2.

time, which Scott's records later confirmed (§C4, §K1). All these Pole compass records constitute [a] unfakable[94] new scientific data[95] [b] are co-signed by competent navigators (fn 51), and [c] are entered into a bound data register whose pagination is continuous and pre-printed. Comparison to Peary's data from the Pole is simply embarrassing: nothing but trivially-fakable sextant altitudes (I could fabricate such a set in minutes: RR 35, F154), no co-signing of anything by companion Henson (fn 43, PZ 292-293, 362), and all on loose,[96] unnumbered slips of paper! (Peary's 1909 diary was written on a flimsily bound little note pad, without numbered pages, and it is generally acknowledged that pages may have been removed or inserted — and that he may have used more than one such notebook: NG 172-173.) We conclude with the temperate observation that: comparing Peary to Amundsen (§D & §E) is not exactly the smartest tactic the former's defenders could have chosen. . . .

The same source-abbreviations used in the notes for *Peary at the North Pole: Fact or Fiction?* (Bibliography & Reference-Key pp. 308-313) are adopted above as well, with the additional items:

AP	Roald Amundsen *Sydpolen* (2 vols). Christiania 1912.
BAM	Brit Ant Exped 1910-1913. Meteorology (3 vols). Calcutta 1919-London 1923.
BTN	Brit (*Terra Nova*) Ant Exp 1910-1913. Rep't on Maps&Surveys. London 1923.
NGD	Thos.Davies *National Geographic 177.1*:44; 1990/1.
NG	*Robert E. Peary at the North Pole*, Report to NGS by NavFou; 1989/12/11.
NU	Supplemental Report to NG, 1990/4/16.
DN	D.Drewry & R.Huntford *Polar Record 19*:329; 1979.
HF	Arthur Hinks *GJ 35*:299; 1910.
HY	Arthur Hinks *GJ 103*:160; 1944.
HU	Roland Huntford *Scott & Amundsen* NYCity 1980 (British ed 1979).
HX	Roland Huntford *Shackleton* NYCity 1985.
MM	H.Mohn *Videnskapsselskapets Skr. 1* Mat.-naturv. Klasse 1915 #5.
MC	Terris Moore *American Alpine Journal* 1983 pp.114, 323.
PM	US National Archives' 1971 microfilm: Peary 1909 records (ref by frame-#).
PX	Peary Papers, US National Archives; ref by box-#.
F	Dennis Rawlins *Peary at the North Pole: Fact or Fiction*? Washington 1973.
RV	Dennis Rawlins *DIO 1.1* ‡4; 1991.

[94] Superhoaxer F.Cook, R.Peary, the NavFou, and *National Geographic* have all stated that the compass needle points to the North Magnetic Pole. (See F91, 226, 234, RE 137, & NGD 48.) So has Ed Stafford (StN 50), Peary's grandson, writing in 1971 in the *US Naval Inst Proceedings* (where Stafford serves as combination mascot & resident genius). But, as *Fiction* noted in detail (F234), this is far from true. Along Peary's alleged 70°W meridian, approaching the North Geographical Pole he sought, the needle pointed about 30° (half a radian) to the right of the North Magnetic Pole. However, for Amundsen & Scott, the S.end of the needle did point approximately at the South Magnetic Pole. (At the South Geographical Pole, the needle pointed only about 0.1 radians to the right of the S.Magnetic Pole.)

[95] The NavFou falsely states that Peary could not have used his compass data to establish his claim. (So, how come Amundsen did so? Massively: fn 74.) NG 60 n.10 (echoing fn 41; see also NGD 47): "as for using the [compass direction] record as a later 'proof' of his attainment of the Pole, the problem introduced by the convergence of the meridians would have applied." Yet the same report, just 8pp earlier, debunks the very same obscuration (NG 52): "it is technically correct to say that variation changes dramatically near the Pole. Yet the rate of change in the direction the compass needle points is no more of a problem there than in many other locations. What changes dramatically near the Pole is not the direction that the compass points, but the direction of the meridians of longitude. Only because variation is measured with respect to the observer's meridian of longitude, does it change rapidly." The NavFou report then (NG 52-53) correctly explains how compass "grivation" can be used with grid navigation near the Poles, contradicting in even further detail its above-quoted alibi (NG 60 n.10) for Peary's total lack of compass direction — either as scientific productivity or as his record of the expedition's daily steering courses. Since Amundsen's record is full on both of these points while Peary's is entirely blank, it takes nerve to say (with Moore, NGS, & the NavFou: §D & §E) that Peary's navigation is credible because it is like Amundsen's. A*lot* of nerve.

[96] F154, SPH 294, 313.

L Appendix: Coverup Cubed

L1 The 1991/4/19 debate at the US Naval Institute's annual meeting (noted at *DIO 1.1* ‡1 §C10, ‡4) on the Adm.Peary N.Pole hoax & its succession of coverups, was held as scheduled. However, after the early 1991 death of the head of the the Navigation Foundation (NavFou), which had issued the National Geographic 1989/12/11 whitewash of Peary, the NavFou refused to send *any* of its other seven Boardmember navigators (all seven of whom had unanimously[97] signed onto the conclusion) to defend the report against the 3 leading skeptics: N.Pole explorers Wally Herbert & Ralph Plaisted, and astronomer DR. (The NavFou boardpersons were not entirely indisposed. At least 3 watched the debate from the audience: T.Carraway, Dale Dunlap, & M.Davies. None said a word during the Q&A sessions following.) Instead (after USNI pressure), Doug Davies, the capable if reluctant lawyer son of the late NavFou chief teamed up with willing, experienced (& witty) Air Force navigator Wm.Molett,[98] to defend Peary. But the Navy quickly[99] realized that the Bad Guys were going to win, and soon scrapped previous plans to seek maximum public coverage & to invite a prominent moderator (e.g., CBS' Charles Kuralt had been considered). The *NYTimes* science dep't claims it was not even informed the debate was to occur. (However, the *NYT* has since behaved as if it had never wanted to cover the event.) Before the debate, DR requested that the USNI have appropriate departments (e.g., math, navigation) at the US Naval Academy pre-referee both parties' technical presentations. In vain.

L2 As expected, the debate was one-sided, especially the afternoon session. (See *Wash Post* 1991/6/9, 6/22, 8/13.) So, the Naval Institute solved that problem by just scuttling (for the 2nd time: fn 99) its plans to publish papers by all 5 debate participants.[100] It next invited a DR book review of A.Counter's then-current Henson biography. The review submitted by DR remarked USNI's last-minute pre-debate request that DR not mention the NavFou's Vespucci disaster (*Wash Post* 1989/12/12, *DIO 1.1*:29) — so the USNI covered that embarrassment by killing the review's publication, too! Gee, that's a neat way to prevent folks from thinking anyone's covering up anything.

L3 And so the debate was preordained as a heads-we-win-tails-you-lose proposition. Had the Peary forces done well, the Navy would have promoted the result. Since that didn't happen, the matter is down the Memory Hole. Standard.

L4 During a lull in the devastating afternoon session (which the Naval Institute claims it inadvertently neglected to videotape),[101] American Museum of Natural History archivist Tom Miller mentioned that the Museum possessed lots of arctic photographs taken by Peary's sole literate companion at the "Pole" camp (Camp Jesup), Matt Henson. Later, the NavFou investigated this collection and counted (I am told) 106 photos from 1909. The NavFou's Doug Davies made the intelligent suggestion that this was awfully close to the

[97] What sort of scholars unanimously certify a report they can't defend? (All 8 NavFou boardmembers also refused a 1990/4/21 debate with DR, proposed by the Explorers Club Washington Group.) That the NavFou's late President overdominated the other 7 Boardmembers of the NavFou is self-evident. However, hiding from facing those parties your report has attacked is a disgrace — revealing that apparent NavFou unanimity (on its Report) represented little more than the former chief's strong persona. Such shy behavior may spawn a suspicion that the NavFou was essentially: Snowjob & the Seven Dwarfs.

[98] Bill, Ted Heckathorn, & DR had a jolly breakfast together the day after the debate. That Bill has not lost his edge was evident as soon as he heard DR mention that he gives extra points to his Loyola students for every academic mistake they catch him at (in lectures, etc.). (The late Tom Davies *definitely* gets a point.) Bill commented: "They must all get As."

[99] In 1990, it had been planned to publish (1991/2 issue of *USNI Proceedings*, before the 1991/4/19 debate) condensed papers from all five panelists. Instead, only the NavFou chief's paper was published. (He was a US Navy Rear Admiral. Like Peary.)

[100] This plan was universally understood by all parties. (And this was the procedure carried out for the 1992 USNI annual meeting's Columbus-landfall debate.) Some participants took pains to fulfill their obligation to submit such papers, not believing it credible that the Navy could be engaging in fake invitations.

[101] A video of the morning session is available from USNI. A brief 1991/6 ad for it was evidently typeset before the Navy debacle at the 1991/4/19 debate. That is the last *USNIP* mention of this determinedly forgotten USNI event.

number of N.Pole photos Henson said[102] Peary had stolen from him: 110 photos.

L5 DR visited the AMNH 1991/11/26 and found 108 photos.[103] Added to the two Henson photos used in Peary's 1910 book (*The North Pole* opp. p.290), this comes to precisely 110.

Comments:

[a] Contra early NavFou (& Peary family) guesses, none of the Camp Jesup photos[104] recovered at AMNH are of the slightest photogrammetric use: there are no shadows because the sky is overcast.

[b] The other photos are from elsewhere, but none have useable flat horizons with complete shadows, so that, again, the Sun's altitude cannot be determined.

[c] I have heard that Doug Davies has hoped at least to use the indicated azimuths for checking Henson's timetable vs. Peary's. (These timetables differ by about 12^h. I used Peary's timetable for my 1990 photogrammetry.)[105] However, [a] one cannot be sure of the inevitably-zigzagging sledges' precise direction at any given time, and [b] no time of day (or time-elapsed on the march) is given with any Henson photo. So using the photos for azimuths involves some guesswork. If we assume a direction parallel to the Cape Columbia (70°W) meridian, then some photos are taken between 6 AM & noon. But this does not firmly eliminate either man's timetable. Two sunlit photos (2A11395 & 2A11393) were published[106] by Henson with captions indicating they were from the final spurt to Camp Jesup, but Peary's diary[107] and Henson's account[108] agree that it was not sunny at the time, as shown in these photos.

[d] The coincidence that the number of Henson's photos agrees with the number he cites as missing is most probably significant. Henson evidently confused the number of photos he knew still existed (110) with the number that had disappeared. But if the NavFou hopes to use this apparent confusion to counter charges that Peary stole Henson's Camp Jesup photos, it will have to tuck some other coincidences under the rug.

L6 The stark facts are: only 4 Camp Jesup photographs by Henson survived, after Peary looted Henson's collection. Two of these photos were shot under cloudy skies and are (as noted in fn 104) at the end of the roll of film. But the 2 photos reproduced in Peary's book are in sunlight. (Both are shaky & photogrammetrically valueless.[109] Peary captions them as 1909/4/7.) They were thus made after the two overcast-scene photos. (Both men's accounts agree that the weather was cloudy through the last march and for hours after arrival, but then cleared up — Peary said at 1909/4/6 8 PM — for the rest of the time at Camp Jesup. Peary says they stayed 30^h there, e.g., PZ 287&301. Henson says 33^h: HA.) The obvious conclusions are: [i] Henson reloaded his camera after the 2 photos of the overcast scene at Camp Jesup, and [ii] he shot photos later there in sunlight. So, if we are to believe that Peary did not appropriate plenty of Henson photos (which might contain shadows indicating the Sun's actual altitude), we must believe that Henson, having reached the goal of his life, suddenly ceased shooting photos in sunlight, after taking only the 2 which Peary

[102] See F157. The original charge that Peary had appropriated Henson's photos (and would not give them back in response to requests) appeared in Henson's written story for *Boston American* 1910/7/17 (HA) pp.1-2.

[103] These were contributed to the AMNH by Henson in 1910. That would be about the time of the failure of his lecture tour (promoted by Wm.Brady). Thus, I presume that these were the photos he used for his lectures.

[104] Only 2 photos in the Henson collection at AMNH are from Camp Jesup. AMNH numbers: 2A11309, 2A11399. Both figure in Henson's 1910/4 article for *World's Work* (HW), where 2A11309 is reproduced, but with a caption saying that "STOP" appears on it. (Actually "X" appears on this photo, while "STOP" appears on photo 2A11399.) As the caption indicates, such marks signify "the end of the roll of film". That Peary grabbed all of Henson's Camp Jesup photos except "one or two fuzzy, end-of-roll would-be rejects" was earlier stated by DR (F157).

[105] Cited *DIO 1.1* ‡1 fn 14, ‡4 p.29.

[106] HW 12829 & 12833, respectively.

[107] Weems *Peary* 1967 p.267: "weather thick".

[108] HW 12837: "dense mist" all day.

[109] Note the awful problem with any Henson photo showing a clear horizon with photogrammetrically-useful shadows: generally speaking, Peary couldn't be sure (as for his own well-timed photos of Sun & shadows) what time they were take at. By coincidence, no such Henson photo survives.

published. Simple logic (and the gist of Henson's recollection) compels one instead to face the probability that he took dozens of photos around Camp Jesup. Their nonavailability (in the context of Henson's published charge that Peary stole lots of his photos) is — like the blank pages in Peary's 1909/4/7 diary — a statement which is easily translated into an obvious bottom line: given such massive gaps in the evidential record, there is no way that a self-respecting scientific society can certify the Peary N.Pole claim. (See fn 2.)

L7 As noted, the US Naval Institute requested both a paper and a book review from DR. At the 1991/4/19 debate's afternoon session, since Bert Peary Stafford (imitating father Ed's 1989/12/11 manners by just yelling out of the audience without raising his hand) had challenged DR's photogrammetry and suggested a bet, both DR's invited submissions to the USNI assented to this wager, and specified conditions & fiscal amounts. (And see fn 2.) But neither proposal was published.

L8 Instead, the *US Naval Institute Proceedings*' 1991/8 issue printed a highly flattering piece on the (admirable) nonpolar adventures of the late head of the NavFou, portraying him as particularly expert at navigation. The article's author? Peary's grandson, Ed Stafford! (Coziness is next to godliness.) And, since the USNI debate, that is the only article the US Naval Institute has published, relating to that debate — a debate which comprised over half its annual 1991 meeting. Nothing fixes like the fix.

L9 There are genuine first surface-attainers of the North Pole living today, e.g., Ralph Plaisted and Wally Herbert. No one, familiar with the Geographic's integrity (and the personal bigotry of the Grosvenor family that runs the "National" Geographic), is surprised that NGS would — just to save itself the embarrassment of admitting its Peary error — attempt to prevent Plaisted and Herbert from receiving the honors due them, while they live. Nor is anyone, familiar with the longstanding academic insecurity of the gelatin-spined geographical community, surprised at its craven silence, especially considering that Peary was President of the American Geographic Society (1903-1907) and the 1904 International Geographical Congress. All this we expect. But, as one who is well aware of the high scholarship that the US Navy has historically sponsored (S.Newcomb, W.Chauvenet, A.Michelson, & R.Newton, to name a few), I am very sorry to see the Navy joining the Geographic (& the now-silent[110] *NYTimes*, which also sponsored Peary's 1909 trip) — to help keep the lid on the by-now-obvious truth of a monumentally successful scientific hoax, perpetrated by a great US Navy admiral.

[110] §L1. The *NYTimes*' embarrassment at having co-funded Peary's 1909 "N.Pole" hoax still (over 80^{y} later) prevents it from stating frankly the truth of the 1909 claim: it is simply this century's greatest science fake. The *NYTimes* cooperatively gave the NavFou's failed 1989 whitewash front-page & editorial attention. (*NYT* 1989/12/12 p.1, 12/15 editorial.) DR's immediate dissent was creditably noted at both places. However, as the NavFou Report has since utterly collapsed under expert scrutiny, only the *Washington Post* has informed the larger public (§L2) of these developments. Since the 1989 NGS press-blitz proclaimed the NavFou Report to be The Last Word, other large publishers have cooperatively followed their longstanding instincts by helping out poor-little-David National Geographic (against awful-Goliath DR) by deepsixing all evidential discussion of the Polar Controversy. This pattern commenced as soon as NGS (initially described as coming-out-swinging: *Science* 1989/12/22) found that its Report had become a joke among scientists, so that silence (i.e., running off the battlefield indefinitely) became the Geographic's preferred tactic of dealing with its NavFou Report embarrassment. (Such tactics cannot be successful without the cohesive assent of other media. And, since these media listen primarily to wealth, the Geographic is able to choose the time&place of all future encounters. In the US, such inequities are entirely typical of the media's idea of Free Speech.) Question: how can the *NYTimes* [a] give front-page attention to a pseudoscientific NGS Report (by National Geographic's hirelings) defending a *NYTimes*-supported fraud, [b] then not even show up at the *long*-awaited face-to-face showdown pitting the Report's out-of-their-depth authors against an expensively-gathered, internationally-recognized expert opposition (Herbert, Plaisted, DR), & [c] expect to have the *NYTimes*' pretensions to integrity taken seriously? (The *NYTimes* claims it was not told that the debate was occurring. But it has shown no interest in finding out how that purported noncommunication occurred, nor in rectifying its resultant omission to inform its readers of the demise of an NGS Report which the *NYTimes* had only recently deemed to be worthy of front-page coverage.) For further entertaining details on the *NYTimes*' record vis-à-vis Peary, see fn 22.

Acknowledgements: For valued assistance, I thank Barbara Rawlins & Mary Beverungen of the Loyola-Notre Dame Library, and scientist Keith Pickering of Watertown, MN. Also, my longtime friend, mathematician Debbie Zito (Mrs. Tom Rayner, Belair, MD). And, for entrusting to DR the remarkable finding of Amundsen's 1911 longitude observations, I especially thank the discoverer, Ted Heckathorn of Seattle.

DIO

DIO & the supplemental *Journal for Hysterical Astronomy* are unleashed thrice yearly by:

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Box 19935
Baltimore, MD 21211-0935 USA.

Telephone (answering machine always on): 410-889-1414.
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 ISSN 1041-5440. This printing: 2014\4\19.