DIO &

The Journal for Hysterical Astronomy
Table of Contents

<table>
<thead>
<tr>
<th>§5 Amundsen’s “Nonexistent” 1911 South Pole Aiming Data</th>
</tr>
</thead>
<tbody>
<tr>
<td>§A Ted Heckathorn .......................... 56</td>
</tr>
<tr>
<td>§B The You’re-Another Defense of Peary’s Alleged Course-Setting .......................... 57</td>
</tr>
<tr>
<td>§C Clott of the Antarctic ................. 60</td>
</tr>
<tr>
<td>§D Moore Logic ................................ 64</td>
</tr>
<tr>
<td>§E The NavFou Piles On ...................... 65</td>
</tr>
<tr>
<td>§F Heckathorn Finds Amundsen’s Transverse Data ..................................... 67</td>
</tr>
<tr>
<td>§G Recovering Amundsen’s Spherical Trig Calculations .................................... 71</td>
</tr>
<tr>
<td>§H Scott’s Navigational Math ................ 74</td>
</tr>
<tr>
<td>§I Ex-Meridian Overprecision &amp; Fatigue ................ 76</td>
</tr>
<tr>
<td>§J Amundsen’s Path to the Pole .............. 77</td>
</tr>
<tr>
<td>§K Bunker Buncombe .......................... 80</td>
</tr>
<tr>
<td>§L Appendix: Coverup Cubed .................. 83</td>
</tr>
</tbody>
</table>

Upcoming

In Future Issues of DIO:
Warren Report Was Right: Lone Gunman, Not Conspiracy, Killed JFK.
Ancient Planet Tables’ Long-Cycle Ancestries.
Ulysses of the Polar Seas: the Kane Mutiny — Oscar Villarejo vindicated.
The Unslandering of Sloppy Pierre.
Ancient knowledge of the 781 year eclipse cycle.

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
§ 8 §A8 & fn 2: Squareza still associated with CSICOP (Consultant)
§ 8 References: for Hughes & Drummond, read Hughes & Drummond 1984.

§5 Amundsen’s “Nonexistent” S.Pole Aiming Data

Ted Heckathorn Exocets Yet Another Dizzy Institutional Fable
Amundsen’s Spherical Trig & South Pole Path Reconstructed
Scott’s Long-Ridiculed Poleward Navigation: Now Vindicated
Scratch One More NavFou Alibi for NGS’ Peary N. Pole Hoax

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 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. 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 data for steering. (By “transverse data”, I mean: observations of the Sun’s altitude, taken on bearings approximately 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 said5 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) most 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 disagreement5 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 was its own discovery. But Ted found & released it first (torpedosing at the stroke the NGS' main secret-weapon-salvation hope for vindicating its ill-received NavFou report). This is why DR & Tom Kelly were first into print5 with the discovery — and its damning implications for how sneaky Peary was (purporting “Pole” Campus (10) to show the Sun’s altitude at a moment when it was just “right”) — but similar photos for times with the Sun at a crossbearing (c.6 hrs later) are glaringly absent6 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)7 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 absence of Peary timesight observations, these watches were critical11 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°7 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.

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.

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 stufiest appropriate academic institutions have, over many years, managed to convince themselves unqualifiedly (by their usual technique, of simply repeating each other without checking anything)8 that Amundsen’s 1911 pioneer trip

12 NG 214 or PZ 351. Marvin’s time-error on 1909/3/22 was probably about 15° (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, so (for Columbus’ rules) the plush “noon” for the expedition at 13:15 was 12:40. Thus, Dr. Bartlett’s “noon” observation was at 13:15 (71°W) local mean time should have been at 12:40 on the chronometers. Thus, 13:15 was c.53° 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 that of this. It has been suggested (Dr. Bartlett note f) that which of 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 concerns about such matters. (See, e.g., his meter timesight tests before & after his magnificent genuine 1909 trip to the northmost 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 inferentially) in connection with data taken en route to the N.Pole or wherever. Note dr. Bartlett mentions (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/day before then, anyway: HF 300.) As we recall (§A4), it is no speculation that watches stopped dead ahead of the 1909 trip: the fact is directly attested by Goodsell.

Bartlett much later insisted (NG 56) that his single (& single-limb) “noon” observation (NG 218 or PZ 359) was at about (not at all) the Sun’s culmination-time. If so, then, to experience local noon at c.3 PM AST, he was some 66 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.

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 becomodelling Amundsen, simply insulted him by suggesting that dog-travel wasn’t as noble as man-hauling: HU 191a, 146, 188, 419, 513, 574, AM 71-72. (Evidently, using motor-sledges or ponies — not to mention outstanding & outmanaging the opposition by huge factors — was permissible under British ethical rulings involving small-scaled enterprises.) However, “professional” (HU 548), 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, \$1E1-3E2) 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, para.)

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 observations book (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 \& our simple eqs. 6 \& 7) Amundsen’s 1911 longitudes from the original observations \& the latitude, which are based on 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 interpolation\(^{16}\) to estimating for a few lengths 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: 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\(^{17}\) that might infect the data: MM 45\&46.) This article, despite its flaws, contains important material that is either rare or unavailable elsewhere, e.g., the altitude\(^{18}\) profile for 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 their 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 note, in passing, that Hinks was trying to do 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 aiding 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 latitude. 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 north or south one is’ 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

\(15\) 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 \& \$32) 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 \& 37E; the transverse steering instability—factor caused by magnetic field diversion 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 latitude 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 leveling 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 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 459d, of a Norwegian shot of 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.

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

\(21\) One is equally grateful to the NaVFou for looking through a hectic 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 (1972/7/29): HU 614 “Eames’ Winner Lost All, a Cook-apology not taken seriously by any academic. The 2 books were unfortunately 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 list of “Recommended” for weeks. See David McCullough’s 1973/6/24 review of a “prom-crowd-review of Winner and Christopher Lehmann-Haupt’s even more cedulous 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 terms with fantastically vitriolic abuse of a certain newspaper: the NYTimes! E.g., Winner claims that the NYTimes “rushed [to the] Cook’s party line” (pp. 257, 293, 395), a.k.a. “the fake news 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 1.7 §2 A.1.)

\(23\) 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 it additionally wanted “the data for determining their positions.” Peary simply avoided this request for raw data (which would have had to include his non-existent 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 on 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]2 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 on an unobstructed non-drifting plateau with an undistracted eye26 even on the compass, and [sledged to the North Pole] only 95 miles27 — less than one-fourth [the length] of Peary’s trip — [after] the last point of determination28 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 ashcaned 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 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, longitude sights, spending considerable time and effort on calculations [eqs. 12 & eq. 10] for a few kilometres, sometimes a few hundred metres of meaningless accuracy. Amundsen’s navigational notebooks [In 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.) Photocopies32 of both parties’ computations are visually closely 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:

\[ L' = \delta - Z \]  

(1)

where \( L' = \) south geographical latitude = \(-L \) (with \( L = \) north geographical latitude), \( \delta = \) Sun’s south declination = \( -\delta \) (declination is tabulated in national almanacs), \( Z = \) zenith distance = \( 90^\circ - h \) (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 & snobery33 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.Denbigh, 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 far in attacking Scott for the “singular inefficiency” (HU 483) of performing precise navigation! In fairness to Scott, certain facts should not be overlooked:

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).
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 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.
[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 this doing 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 seminar35 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.

Whatever it was meant. 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 convergence36 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 means37 that a 1° error in longitude will land one up 4 miles to the left or right of the Pole. For Scott’s party — manhandled, 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 alost less food-fuel than manhandling 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 go.” Scott’s navigation difficulties & en-route meanderings were no worse than Amundsen’s (though more fatigued, 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): 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 1921/3 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 latitude and compass error [variation V] was enough. After Hinks’ talk, Scott twice commented (HF 306, 308) on the unreliability of chronometers in polar work, & his small size, since he got the same food share as the others. 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 reportage of geographical position. I must emphasize (again, in simple justice to Scott) that this is not exactly a pre-eminent geographer’s silence & tolerance of fakery has been bought academic communities a touch of shame. But, luckily, geographers are stronger than that.

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 position on other scientific data and published afterwards are of importance, 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: in 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 §GI2). 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, Scott’s possible over-estimation and under-estimation of reportage of geographical position. I must emphasize (again, in simple justice to Scott) that this is not exactly a pre-eminent geographer’s silence & tolerance of fakery has been bought academic communities a touch of shame. But, luckily, geographers are stronger than that.

36 I am no admirer of Scott’s generalship. (See, e.g., HU 420 or IX 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 reportage of geographical position. I must emphasize (again, in simple justice to Scott) that this is not exactly a pre-eminent geographer’s silence & tolerance of fakery has been bought academic communities a touch of shame. But, luckily, geographers are stronger than that.

37 The insufficiently-understood DR principle cited here at SB5.

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 IX 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 reportage of geographical position. I must emphasize (again, in simple justice to Scott) that this is not exactly a pre-eminent geographer’s silence & tolerance of fakery has been bought 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, following Hinks’s grid method (contra fn 35, fn 41, & fn 40), and 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 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 editor 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 previous expeditions, in 1909 he made no observations for longitude anywhere between leaving land at Cape Columbia [83°07′057″N, 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 (and 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).

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 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 32:120) for his N.Pole “data”. [b] A simple equation for reducing sph trig to plane trig near the North Pole, namely, h = cos A, where h is true solar altitude, A is solar declination, C is colatitude, and azimuth A = har angle H = G − W − 180° (eq. 7) — with H = 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 Summer 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 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).

D5’s clarifier and Hinks’ paper, which clearly states (HF 302): “two sights

E The NavFou Piles On

E1 National Geographic’s Peary-defense-lawyer consultant, the NavFou, eagerly leaped right into the trap innocently baited by Huntford & (not so innocently) 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 it to the congressional committee, was appropriate and completely adequate for the polar region. It was, in fact,
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”.

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, Vilhjalur 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 62 & 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 true navigation 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) Norwegian edition (AP) of Amundsen’s book! Ted bought this book (over 30y ago) for all of $3. And with this $3 weapon, he has sent to-the-bottom the National Geographic Society’s beloved 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.

---

45 Hinks says (p.168) that he got hold of what might have been the only copy in England in 1944 (belonging to Hugh Milb. DN 336 cites the usual English edition (AS). But HU uses throughout (& lists at HU 611) the Norse edition (AP).
Now that we know the truth, it will be educational to arrange a retrospective comparison of voucher-authories, on the question of whether Amundsen used transverse longitude observations:

<table>
<thead>
<tr>
<th>NO:</th>
<th>YES:</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scott Polar Research Institute (Cambridge Univ)</td>
<td>Ted Heckathorn, historian</td>
</tr>
<tr>
<td>National Geographic:</td>
<td>DR, gadless gadfly</td>
</tr>
<tr>
<td>Navigation Foundation</td>
<td>Terris Moore, President, Univ Alaska</td>
</tr>
</tbody>
</table>

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

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 observation book 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 observations book p.127), shot transversely to the Poleward path. The handwritten figures displayed are captioned: “En side av azimuth- og lændeobservationene.” In English, this is: “A page of azimuth- and longitude observations.” Hmm. (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. . . .)

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 in some form, standard or cartesian for efficient travel towards a geographical Pole.

I will begin by setting out here, in Table 1, the data on the observation book page reproduced by Amundsen (AP 2:115; photocopy here at p.69).

<table>
<thead>
<tr>
<th>Table 1 Amundsen’s On-Site Recorded Data &amp; Calculated V</th>
</tr>
</thead>
<tbody>
<tr>
<td>Obs #</td>
</tr>
<tr>
<td>-------</td>
</tr>
<tr>
<td>1</td>
</tr>
<tr>
<td>2</td>
</tr>
<tr>
<td>3</td>
</tr>
</tbody>
</table>

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).

FRA OBSERVATIONSBOKEN.
En side av azimuth- og lændeobservationene.

"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."
The observationsbook consistently records index correction IC = -1’. For observation
#1, the navigation tables carried by the party gave refraction & parallax (r & k) = 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.

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 observations
book p.7 (reproduced at AP 2:77). These data agree to about 0’’. I take the same table of the American Ephemeris & Nautical Almanac for 1911 p.146. So we may recover Amundsen’s 1911/11/&&13 math by using the 1911/11 data of the same reference work (p.182).

Presuming Amundsen discarded his astronomical math scratch-sheets (not raw data), But solving eq. 2 by logs is taxing, due to the subtraction. So navigators preferred the following standard formula (e.g., Bowditch Navigator 1903 p.111):

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

where A = azimuth, \(\delta\) = declination of Sun’s center, \(h\) = true altitude of Sun’s center, \(L\) = geographical latitude.

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 the following standard formula (e.g., Bowditch Navigator 1903 p.111):

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

where A = azimuth, \(\delta\) = declination of Sun’s center, \(h\) = true altitude of Sun’s center, \(L\) = geographical latitude.

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:

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

where A = azimuth, \(\delta\) = 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 the following standard formula (e.g., Bowditch Navigator 1903 p.111):

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

where north polar distance \(p = 90^\circ - \delta\) 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.69), we have lower limb double altitude = 17’’37.51’’ east of compass south. Similarly, see F131.

H. Hanssen, O. Wisting, [S.] Hassel. (Set B and the 1911/12/16 Set A, co-signed by Amundsen & Hanssen, are printed at PM 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 third of his purported 1909 trip to the North Pole.

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.

Over years of reviewing nonastronomers’ astronomical calculations of this type, I have repeatedly encountered such errors (in this case, the effect is so trifling: the net error is merely ordaining 1’ to the correct (by a different person than was Obs #1; he certainly used a different chronometer: chron #6 now instead of #3.)

To illustrate, we will next reconstruct the complete Amundsen reduction & computation of Obs #1.

Now to set up the sph trig process. Above (§F10), we reconstructed the Norwegian’s adopted \(\delta = -16^\circ32’’15’’\); thus, \(p = 90^\circ - \delta = 106^\circ32’’15’’\). Since (Table 1) \(L = 83^\circ\), we have \(s = (h + L + p)/2 = 20^\circ48’’25’’\). And \(s = p - 85^\circ43’’50’’\).

To solve eq. 3, Amundsen’s computer now consulted the provided table-logs trig tables like those of 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 = 18’04’’35’’, as shown in Table 2. In brief:

\[ h = (h + L)/2 = r + k + ssd \]

Now to set up the sph trig process. Above (§F10), we reconstructed the Norwegian’s adopted \(\delta = -16^\circ32’’15’’\); thus, \(p = 90^\circ - \delta = 106^\circ32’’15’’\). Since (Table 1) \(L = 83^\circ\), we have \(s = (h + L + p)/2 = 20^\circ48’’25’’\). And \(s = p - 85^\circ43’’50’’\).

When computing refraction in the field. The effect here on the computed true altitudes steered to about 10’’, which would originate an error of r in these computed azimuths. But this is of little account when the solar compass directions are rounded to whole degrees at best.

It happens that this step ensures (since all \(L\) have even endings) that all our computed compass variation results must come out even in arcmin. And, indeed, all 3 Norwegian-calculated arcmin endings for \(V\) are divisible by 2 (Table 1: 34’, 28’, 0’’).

We here define the compass variation as the angle 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.

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 ordaining 10’’, which would originate an error of \(\delta\) in these computed azimuths. But this is of little account when the solar compass directions are rounded to whole degrees at best.
result (solar true azimuth $A$) with the observed compass bearing $B$ of the Sun’s center, using simple arithmetic:

$$V = B + (180^\circ - A)$$  

(5)

The observations book (AP 2:115) records that the Sun’s center was sighted at compass direction “SE by S 1/4 $S$”, which means I/4 a compass point south of “SE by S”. Since SE by S = 333°34′, and a compass point is 1 1/4′, then subtracting 1/4′ of 11°14′ from 333°34′ yields the Amundsen party’s Observation #1 of the compass bearing of the Sun: $B = 30°15′16″ E = 30°56′$ east of compass south. These values 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>
<thead>
<tr>
<th>Table 2</th>
<th>Reconstructions of Amundsen’s Math</th>
</tr>
</thead>
<tbody>
<tr>
<td>Obs #</td>
<td>$h$</td>
</tr>
<tr>
<td>1</td>
<td>18°04′35″</td>
</tr>
<tr>
<td>2</td>
<td>17°56′35″</td>
</tr>
<tr>
<td>3</td>
<td>19°49′30″</td>
</tr>
</tbody>
</table>

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 longitude-observations”: §F4.) This point has evidently previous commentators on the Norse 1911 navigation (in 72). The elementary equation relating $W$ and $V$ is that azimuth $A$ and hour angle $H$:

$$\sin H = \sin A \cos h \sec \delta$$  

(6) — and we find longitude $W$ from $H$ by simple subtraction:

$$W = G - H - 12^h$$  

(7) where $G = $ Greenwich Apparent Time, which is found from:

$$G = T - K + M + EqT$$  

(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 Framhjem-based Norse watch; $EqT$ is the mean error of equation of time, tabulated in national ephemeresids; $K$ is the chronometer error. Both $T$ & $G$ are midnight epoch.) Correct computations65 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): $G = 16°53′54″ - (16) = 163°37′15″ + 16°10′3.8 = 4°06′49″$. 

G10 Then, substituting $A, h, \delta$ (already known from Table 3) into eq. 6, we find $H = \arcsin(\sin 78°20′ \cos 18°04′25″ \cos(−16°32′14″)) = 5°04′49″$. Substituting this $H$, as well as $G$ (§G9), into eq. 7, we have $W = 4°06′49″ - 5°04′49″ + \lambda = 11°12′28^{\circ} = 156°32′1/2 W$, as listed in Table 3 (where we use $L = -L, \delta = -\delta$).

<table>
<thead>
<tr>
<th>Table 3</th>
<th>Rigorous Calculations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Obs #</td>
<td>$h$</td>
</tr>
<tr>
<td>1</td>
<td>18°04′25″</td>
</tr>
<tr>
<td>2</td>
<td>17°56′24″</td>
</tr>
<tr>
<td>3</td>
<td>19°49′33″</td>
</tr>
</tbody>
</table>

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 (in 41, adjusted for the S.Pole region):

$$\cos A = (h - \delta') / C'$$  

(9) where $C' = $ south colatitude = 90° – $L' = 90° + L$, while $\delta' = $ south declination, and we use $H = A$ (with of course is virtually true near a Pole). This yields $A = H = \arccos[(18°04′25″ - 16°31′42″) / 77°2°1/4$. 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’ spin trigs 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, emb added):

64 Using precise ephemerals data, we have $s = 16°11′$ for 1119, 16°12′ for 1131; of time $EqT = 16°03′8′$ for Obs #1, 16°10′6 for Obs #2, 15°37′7 for Obs #3. Parallax is $8′′4.9$ for 1119, $8′′3.1$ for 1131. Using the $T&P$ given at MM 62, atm refraction is computed according to an original DR procedure (summary in DIO 2.1:14): $3′′09′′.4$ for Obs #1 ($−13°2.2 = 260° K, 736.0$ mm Hg $= 981$ mb), $3′′08′′.2$ for Obs #2 (same $T&P$ as Obs #1), $2′′57′′.5$ for Obs #3 ($−18°9 = 234° K, 743.3$ mm Hg $= 991$ mb). In Table 2, no chronometer correction was applied. For Obs #2, we adopt the error $163°37′W$ given for chron #3, $56′′ - 3′′/day$ from 1911/10/16. November comparisons of chron #6 to chron #3 are tabulated at AP 2:107 (obsbk p.115): $−57′$ for 111/119, $−77′$ for 111/113. Thus, the chronometer errors $K$ (chron minus FMT) for the 3 observations are, respectively, $−1°.−7°$, $−105°$. (Again: the Peary 1909 record contains no en-route watch comparisons.)

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

66 Depending upon the convention adopted (for $A$ & $H$) $A$ may equal (instead of $H$) the negative and-op of 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 intuitively by every Pole-ataining navigator. (See, e.g., fn 32.) But neither Amundsen nor Scott realized that the same (cartesian) simplification is useful even 100 mi from the Pole.
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-calibration 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’ fantastical 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;[66] 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 \(Z = 90^\circ - h\), south polar distance \(p' = 90^\circ - \delta\), 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° before encountering Amundsen’s black flag near the Pole), Bowers took the solar upper limb’s altitude: 21°57’07”/’. After correcting for \(r_{kcp}\) and \(s_{sd}\) 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 \(L = -89°08’9.9, \) so (§G11) \(C' = 0°51’06”\). Tabular \(\delta' = 21°41’40”\), 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 \(\sin s' = 0.969440\), log \(\sin (s' - Z) = 7.844540\). (All

---

[66] 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 \(\sin = \sin L + p - \sin H/2)\cos L/2)\) (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: \(\tan H = \tan Z - \tan (L' - \delta) = \tan L' \tan L'\).
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 Heckathom’s finding of raw Amundsen transverse solar observations and by the availability of Mohn’s complete list3 of the sph-trig-computed compass-variation results of Amundsen’s whole 1000 mi series of such observations. So, the upshot of Heckathom’s discovery (and the foregoing discussion): exit another curiously widespread establishment exercise in weird-science mythology.

I Ex-Meridian Overprecision & Fatigue

II Despite his misunderstanding of the role 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 meridian35 observation had been a bit late. To calculate the required correction $k$ (which must be subtracted from the meridian-arithmetic-computed south latitude $L' = 1$ eq.), Bowers used the formula76

$$k = \left[1^{\circ}.9634H^2 \cdot \cos L' \cdot \delta' \cdot \sec(L' - \delta')\right]$$  

(12)

where $H$ is taken in timemin. The main British computation used the erroneous longitude $-163^\circ 1/2 = 10.54{\text{E}}$ of the previous day’s math (§H2). Then, from eqs. 7 & 8 (with zero $M$: §H2), Bowers added 24$^\circ$ and computed $H = T - K + E^2 T - W + 12^\circ = 3.4546.5 - 2.0713.5 - 0.0841 + 10.54 + 12 = 24.2352$. This result was then misread77 as $H = 24^\circ 23^\prime$ and was substituted into eq. 12, along with $L' = 89^\circ 15^\prime 2^\prime$ and $\delta' = 21^\circ 34^\prime 08^\prime$.6. Bowers used $\sec Z'$ in place of $\sec(L' - \delta')$ in eq. 12 (a clever saving of labor via eq. 1, with negligible loss of accuracy). He had just observed $Z = 67^\circ 41^\prime 46^\prime$ true ($Z$, after correction for $r\kappa p$ and $ssd$). Based on the foregoing values, the first bracketed term in eq. 12 was $11677^\circ$. 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 $15^\circ 3^\prime$ — 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):

$$k = \left[1^{\circ}.9634H^2 \cdot \cos L' \cdot \delta' \cdot \sec(L' - \delta')\right]$$  

(13)

Thus, $k = 44^\circ 5.4(1 - \cos 6^\circ) = 89^\circ 90^\prime 6^\circ = 15^\circ 24^\prime$. 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$^\circ$ (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 Norske 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 took 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$^\circ$ 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 manhandling 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 first79 $W$ 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 revision78 in the following:

77 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 ($163^\circ 6^\prime W$) 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$^\circ W$ to 157$^\circ W$ — 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$^\circ W$. Hinks & Huntford’s maps more reasonably bring Amundsen in on paths parallel to 160$^\circ W$.
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 θ = 180° – 146° 1/4 = 33° 3/4. Now, the 2 paths cross at the 1912/11 Scott camp, which was virtually identical (HY 164) to the 1911/12/14 Amundsen camp. Bowers’ observations make the camp’s colatitude $C' = 14.4^\circ$ (HY 164), which agrees well with Amundsen’s $C' = 15^\circ$ (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: 1. $\text{arcmsin}(7/144.4) = 29^\circ$, but $\theta = 33^\circ 3/4$. In brief, one of the foregoing deductions is false. It is, and (§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 (§4), by very nearly this $5^\circ$ 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.

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

[a] His incoming compass course was 146° (§35).

[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.

Next, we estimate Amundsen’s absolute longitude on 1911/12/8, at $L' = 88^225'$ ($C' = 135^\circ \pm 2/3$).87 He made his last effective (fn 28) compass variation determination (HY 1911/12/8), $V = 147^\circ$ — 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 westerly longitude is 164°W + $\text{arcmsin}(7/95) = 168^\circ +$. 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 $3^\circ$. 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' 170° W. This result roughly vindicates the inspired Huntford suggestion (HY 443) that, during the days after Amundsen crossed 88° 25', 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° but 170°.

We now know Amundsen’s 1911 movements86 a lot better than previously. And we can probably move further. (Note: the present paper’s analyses of Amundsen’s 1911 work

83 Hinks’ careful analysis put Amundsen’s final Polheim camp near $L = 89^\circ 58'\ 75.5$ S ($C' = 1^\circ 25'\ & 171.6^\circ$ (HY 173). After the Norwegians’ 1st camp near the Pole, they shifted their path 82° (literally, 81.9’/16 or 81.6’/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 distance is about 5.5 sin 81.6’ × $1/4$ sin (164° 7/4) = 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 map of the entire 1st Amundsen’s 1st camp’s position put it about 6 mi from the Pole (AP 2:129, 400). Since this is good to ordnag 1 mi and since the §35 contradiction eases for larger missing distances, I prefer going with about 7 mi.)

84 Which means that Hinks’ 173° parallel path (HY 174, also HU 517: §35) is nearly 10° off — while the chart (BTN 29, HY 175) of the official British report (BTN), which Hinks repeatedly (often justify) denigrates, was in fact about right on this point. Perhaps fortunately. The BTN’s chart’s depiction of Amundsen’s approach may well have been based (from forgetting the Norwegians’ severe detract 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 (and 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 observations book than those published by photography 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 taken at the South Pole, the north end of the compass needle pointed nearly $15^\circ$S along the meridian of $18^\circ$W. This result has been verified by later explorers. Scott evidently $\theta$ made it $19^\circ$W, since he found variation $V = 179^\circ 40'$ (BTN Map 3; HY 164, 174) along $161^\circ E$ longitude ($\theta$2). 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 $\theta$ which might be checked by later examiners of the spot.) When alibiing this outrage, the NavFou suddenly loses interest in making $\theta$2-style comparisons to Amundsen’s steering. Indeed, the NavFou doesn’t mention Amundsen at all at this point and instead ($\theta$4) 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 observations book. 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 ($\theta$4) to his book’s photocopies of 3 transverse solar observation altitudes shows that he knew they were “longitude observations”. For placing his topographic charts in position, these longitudes could be so computed later, as in Table 3. By contrast, Peary’s 1909 ocean depth soundings are virtually worthless since one cannot compute his longitudes in the entire absence of transverse solar data ($\theta$6 & $\theta$3).

[c] As Heckathorn’s sharp eye has already noted, Amundsen’s observations book locates, e.g., his $80^\circ S$ & $83^\circ E$ 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]”.)

$\theta$8 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 168-169 of observbk). (Note that MM 78 misquotes the last double altitude by $1'$.) $\theta$8 The error will be ordaining. $\theta$9 Note that the results given by Mohn for the two groups of Polar 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 $= 118^\circ$ — $103^\circ = 15^\circ$ W. While, for Set B, he has it $= 58^\circ$ — $41^\circ = 17^\circ W$. (See MM 48.) But the agreement ($\theta$) 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 $c.5^\circ$. (This is probably due to a systematic difference of $c.20^\circ$ 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 ($\theta$13/16). They indicate that: the compass needle pointed along meridian $18^\circ 3^\theta W$; ordaining $1^\circ$.

$\theta$9 See J5. The last Scott camp for which compass variation is shown in BTN (Map 3) is that for $L = 80^\circ 09’$, $W = 161^\circ$. [See also SL 394.] But the variation stayed so steady, at nearly $180^\circ$ (along the latter part of Scott’s track), that the final $5^\theta$ obviously made little difference. Unfortunately, BTN (by geologist F.Debenham) neglected to publish most of the raw tabulated 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).

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.

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 recorded compass dataset (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, $\theta$1) & the NavFou (NG 62, $\theta$2) have stated was the basis of his 1909 “navigational method”! Even Peary’s one compass variation estimate (WSW or $112^\circ$W, FZ 232, F131&138) at c.$84^\circ$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 ($\theta$3) from Framheim the whole way to the S.Pole.

K4 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 observations book than those published by photography 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 taken at the South Pole, the north end of the compass needle pointed nearly $15^\circ$S along the meridian of $18^\circ$W. This result has been verified by later explorers. Scott evidently $\theta$ made it $19^\circ$W, since he found variation $V = 179^\circ 40'$ (BTN Map 3; HY 164, 174) along $161^\circ E$ longitude ($\theta$2). 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 $\theta$ which might be checked by later examiners of the spot.) When alibiing this outrage, the NavFou suddenly loses interest in making $\theta$2-style comparisons to Amundsen’s steering. Indeed, the NavFou doesn’t mention Amundsen at all at this point and instead ($\theta$4) simply says it was “unfortunate” that Peary made such a glaring omission — unprecedented for him (F130).

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 (in 74, fn 87), producing the hitherto unknown (thus not fakedal $18^\circ W$ compass direction there at that...
time, which Scott’s records later confirmed (§C4, §K1). All these Pole compass records constitute [a] unfausable new scientific data [b] 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, unnamed 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:

NGD Thos. Davies National Geographic 177,144: 1990/1.
NG Robert E. Peary at the North Pole. Report to NGS by NavFou; 1989/12/11.
HX Roland Huntford Shackleton NYCity 1985.
PM US National Archives’ 1971 microfilm: Peary 1909 records (ref by frame-#).
PX Peary Papers, US National Archives; ref by box-#.

94 Superhoxer F.Cook, R.Peary, the NavFou, and National Geographic have all stated that the compass needle points to the North Magnetic Pole. (See P91, 226, 234, RE 137, & NGD 48.) So has Ed Stafford (SN 30), 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 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, deems the very same obscurance (NG 52): “it is technologically inaccurate 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.

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 noverominated 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 jelly 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 can commit (at 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 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\textsuperscript{103} Peary had stolen from him: 110 photos.

L5 DR visited the AMNH 1991/11/26 and found 108 photos.\textsuperscript{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\textsuperscript{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°. I used Peary’s timetable for my 1990 photogrammetry.)\textsuperscript{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\textsuperscript{106} by Henson with captions indicating they were from the final spurt to Camp Jesup, but Peary’s diary\textsuperscript{107} and Henson’s account\textsuperscript{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.\textsuperscript{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° there, e.g., PZ/287&301. Henson says 33°: 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

\textsuperscript{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.

\textsuperscript{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.

\textsuperscript{104} Only 2 photos in the Henson collection at AMNH are from Camp Jesup. AMNH numbers: 2A11309, 2A11399. Both are in Henson’s 1910/4 article for World’s Work (HW), where 2A11399 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).

\textsuperscript{105} Cited DIO 1.11 in 14, ¶p.29.

\textsuperscript{106} HW 12829 & 12833, respectively.

\textsuperscript{107} Weems Peary 1967 p.267: “weather thick”.

\textsuperscript{108} HW 12837: “dense mist” all day.

\textsuperscript{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 taken at. By coincidence, no such Henson photo survives.

\textsuperscript{110} §L1. The NYTimes’ embarrassment at having co-funded Peary’s 1909 “N.Pole” hoax still (over 80’ later) prevents it from stating frankly the truth of the 1909 claim: it is simply this century’s greatest science fake. The NYTimes cooperatively transferred the failed voyage’s 1989 whitewash front-page & editorial attention. (NYT 1989/12/12 p.1, 12/15 editorial). DR’s immediate dissent was credibly noted at both places. However, as the NavFou Report has since clearly collapsed under expert scrutiny, only the Washington Post has informed the larger public (SL2) 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 deepjocussing all evidential discussion of the Polar Controversy. This pattern is not uncommon in science (as initially described in Chapter 12822). As the NavFou 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 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).}
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 is primarily a journal of scientific history & principle. At present, most DIO copy is written by Dennis Rawlins (DR) and friends (see DIO 1.1 fn 12). Each author has final editorial say in his own article. If refereeing occurs, the usual anonymity will not — except (if the author wishes) in reverse.

The J.HA is rumored to be edited by the intrepid feline explorer Admiral Purry, longtime member of the National Geographic Society (election through NGS Board of Trustees; certificate 1973/1/1) and of the American Federation of Astrologers.

Both journals’ writings are to be considered as automatic submissions to the appropriate handsome (centrist) academic journals. I.e., permission is hereby granted to these journals’ article-space, correspondence columns, and-or approved authors, to print matter from any issue of DIO (or J.HA), edited to these journals’ alleged standards. Indeed, DIO encourages handsome journals’ open refereeing & publication, in whole (except DIO vols.3 & 5) or in part, of DIO articles which clarify problems these journals (e.g., Journal for the History of Astronomy) purportedly exist to elucidate. No condition is set except this single one (which will presumably serve as a fully sufficient impediment to said hypothetical publication): DIO’s name, address, & phone number are to be printed adjacent to the published material & all comments thereon (then or later), along with the additional information that said commentary will be replied to (if at all) in DIO’s pages, not the quoting journal’s. (Copies of the quoted material & attendant comments are to be sent to DIO when published & not before.)

DIO invites communication of its readers’ incredulity, appreciation, nausea, empathy, scorn, support, and-or advice. (Those who wish to be sure of continuing — or not continuing — on the mailing list should say so. It is hoped that professorial readers will encourage their university libraries to request receipt of DIO.) Written contributions are encouraged for the columns: Unpublished Letters, Referees Refereed, and regular Correspondence. (Note: all letters received are accounted public domain. Comments should refer to DIO section-numbers instead of page-numbers.) Deftly or daftly crafted reports, on appropriate candidates for recognition in J.HA’s pages, will of course also be considered for publication. (A subject’s eminence may enhance J.HA publication-chances. The writer’s won’t.)

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

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

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

©1992 DIO Inc. ISSN 1041-5440. This printing: 2020\12\21.
A Fresh Science-History Journal: Cost-Free to Major Libraries

DIO — The International Journal of Scientific History.
Deeplv funded. Mail costs fully covered. No page charges. Offprints free.

• Since 1991 inception, has gone without fee to leading scholars & libraries.
• Contributors include world authorities in their respective fields, experts at, e.g., Johns Hopkins University, Cal Tech, Cambridge University, University of London.

• Journal is published primarily for universities’ and scientific institutions’ collections; among subscribers by request are libraries at: US Naval Observatory, Cal Tech, Cornell, Johns Hopkins, Oxford & Cambridge, Royal Astronomical Society, British Museum, Royal Observatory (Scotland), the Russian State Library, the International Centre for Theoretical Physics (Trieste), and the universities of Chicago, Toronto, London, Munich, Göttingen, Copenhagen, Stockholm, Tartu, Amsterdam, Liège, Ljubljana, Bologna, Canterbury (NZ).

• New findings on ancient heliocentrists, pre-Hipparchos precession, Mayan eclipse math, Columbus’ landfall, Comet Halley apparitions, Peary’s fictional Crocker Land.

• Entire DIO vol.3 devoted to 1st critical edition of Tycho’s legendary 1004-star catalog.

• Investigations of science hoaxes of the – 1st, +2nd, 16th, 19th, and 20th centuries.

Paul Forman (History of Physics, Smithsonian Institution): “DIO is delightful!”

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

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