

Vol. 10 2000 January

ISSN 1041-5440

DIO

The International Journal
of Scientific History

Triple Issue: Copublished with the University of Cambridge

Amundsen: Cheated & Uncheated

R. Amundsen-O. Wisting
First at EACH Pole

Byrd North Pole Claim's Burial
Slides from Decent to Indecent

Bernt Balchen's Air Double-Priority
& Skepticism Finally Vindicated

Byrd's Courage & Navigational Pioneering
Merit Admirable Nonetheless

DIO & The Journal for Hysterical Astronomy:

DIO's Byrd Report: Co-Published with *The Polar Record*

Table of Contents

Page:

News Notes & Amundsen's Pre-eminence	3
History of DIO Report & Summary of US Geography's Triple North Pole Hoax	4
The "National" "Geographic" "Society" & Real Geographical Societies' Snickers	6
Preface: DIO, Byrd, & the "North Pole" Diary	7
§A Introduction	9
§B Historical Upshot	13
§C Byrd's Various Alleged Speeds: Tied Up in Knots? or Pure Bosch?	17
§D Published Sextant Data (Official Typescript)	22
§E Long-Secreted Actual 1926 Sextant Data (In-Flight Diary)	26
§F Proof of Fabrication Procedure in the 1926 Byrd Data	32
§G Precision Anomalies	38
§H Sumner Straightline-Jacket	42
§I How Far North Did the Airplane Get?	43
§J How Far North Could Byrd & Bennett See?	45
§K Temporal Bookends	48
§L How an OSU-Promoted Expert Works Backwards to an Answer	50
§M Superswift Flying & Certifying	55
§N Summing up the 1926 Evidence	59
§O Appendix 1. Reconstructing Actual DeadReckoning & 9:15 Stress	62
§P Appendix 2. Restoring the Diary's Flawed 1925 Sextant Sights	65
§Q Appendix 3. Ohio State University: Neutral? Or for the Byrds?	66
§R Addendum 1: American-Experience, PBS, and Fraud	69
§S Addendum 2: Portney's Complaints	70
§T Addendum 3: It Ain't Over 'Til the FatCat Sings	76
⊙1 In Which the Byrd Center Does Some Erasing of Its Own . . .	81
⊙2 The Undropped Flag-Blizzard	82
⊙3 NP Fictional Productions	84
⊙4 Drift-Indicator: Aim vs Distance	86
⊙5 Byrd's Scientific Equipment and National Geographic's Exam	87
⊙6 OSU Expert vs OSU Expert	88
⊙7 Unsteady Heights & Byrd's Suppressions of Them	89
⊙8 Steering-Mysteries & Open Options	91
⊙9 The Loose Sheet of Figures: Mid-Fabrication in Mid-Ocean	92
⊙10 Speculative Reconstructions of Some Marvellously Agreeable Fakes	93
⊙11 The "Splendid Check"	93
⊙12 Verlegen Hook, Misreadings, & Ungenerous Apologists	94
⊙13 Velikovskiy Lives! — How Cults Eliminate Contradictions	96
⊙14 In Which Goerler Forgets the Day He Was Famous	97
⊙15 Another OSU-Promoted Expert Wrestles with Numbers	98
⊙16 TimeFudge-Emulation & Byrd at 1 Mile/Hour	98
⊙17 Can Leashed Judges Leash a Rampaging Approval-Orgy?	99
⊙18 In Which Byrd's Defenders Juice Him Up to 130 Miles/Hour	101
⊙19 Odd Hominems: the Fruits & the Fates	102
⊙20 FlameKeepers vs Scholars: Why Are Outsiders Making the Analytic Finds?	103
⊙21 Nuts, Sluts, Grosvenors, & Clones	104
Conventions	105
References & Abbreviations	106

List of Figures

Page:

1 National Geographic's Official Diagram of Byrd's Alleged 1926/5/9 Path . . .	10
2 Byrd's Initial Version of His Flight-Path	11
3 Byrd's Frantic 1926/5/9 Diary Record: Oil-Leak Discovered	18
4 The 1st Sextant Shot, Diary Page 8	28
5 The 2nd Sextant Shot, Diary Page 11	30
6 Loose Sheet of Calculations in Byrd's Hand	33
7 From Initial [a] & Final [b] Reports: 7:07 Sextant Data Discussion	34
8 From Post-Flight "Flight-Chart": Detail Lower Right Corner	35
9 Bizarre <i>Skyward</i> Map, as Used by OSU & <i>Mercator's World</i>	56
10 Two Genuine North Pole Discoverers	61
11 Portney's Smart Tornado (Dorothy & Toto Should Have Been So Lucky) . . .	72
12 Portney's 1973 Diagram of Byrd's Purported Path & Error Zone	74
13 Portney's 1992 Diagram of Byrd's Purported Path & Error Zone	75

News Notes & Amundsen's Pre-eminence

[On 2000/12/8, the two leading investigators of polar hoaxes, Robert Bryce and DR, appeared together on the *History Channel*, celebrating the 1st national recognition of experts' now-near-unanimous though hitherto-private opinion: Roald Amundsen's priority at *each* geographical pole. The program made known to the public at last the horrific injustice done to Amundsen by the establishment of his day, who had so damned and exiled him that he was by now all but forgotten by the US public.]

To put it simply: once one clears away the also-rans and the hoaxers, Amundsen's record of priorities exceeds that of *all other polar explorers combined*. He was:

1. First to winter in the Antarctic.
2. First through the Northwest Passage.
3. First to the South Pole.
4. First circumnavigator of the Arctic Ocean.
5. First to the North Pole.
6. First to the Ice Pole (the Arctic Ocean point farthest from land-masses).
7. First to cross the Arctic Ocean.
8. First polar explorer to die attempting to save the life of an enemy (§A10).

[On 2001/5/12, Reuters' A. Doyle reported live at 1:25, from over the North Pole, that a group of celebrants were popping corks to toast Roald Amundsen's priority there exactly 75^y earlier, when he radioed ("live") his attainment to the world. The 2001 group had been flown to the Pole by the Norsk Polarinstitut, accompanied by its director, Olav Orheim.

The *History Channel* and Norsk Polarinstitut events vindicated not only Amundsen but also DR's 1973 book, *Peary at the North Pole: Fact or Fiction?*, which (at its p.280) had been the first ever to publish — nearly three decades earlier — the fact that *all three* prior claims to the N.Pole were fakes, thus, Amundsen was first there. Given the inexcusably glacial slowness with which establishments accept any truth upsetting to money, I am *extremely* fortunate to have lived to see the truth of Amundsen's unique greatness finally come to permanent worldwide light.

As a naturally happy sort, I've not personally been very directly hurt by the delay. But I regret that genuine North Pole discoverers Roald Amundsen, Lincoln Ellsworth, & Umberto Nobile and their closest family (as also my parents) could not fully share in this exquisite moment of justice (though Nobile's widow survived long enough to know the 1996/5/9 death of Byrd's fake claim), which we owe to the perception and the historical equity-sense of Jessica Louchheim, Jonathan Grupper, and the *History Channel*.

Thanks are also due to the news reporters who carried the uphill fight against Byrd's hoax over the years: Jim Bready, C.B.Allen, R.Montague, Nicholas Wade, & John Wilford.]

History of the *DIO* Report & Summary of US Geography's Triple North Pole Hoax

This issue of *DIO* is devoted to publication at last of DR's well-known 1996/5/4 report to the Byrd Polar Research Center at Ohio State University, a report that was put together in roughly 10 days and was the acknowledged basis of the 1996/5/9 pageone *New York Times* story which courageously undid that newspaper's own 1926/5/10 pageone acceptance of Richard Byrd's "North Pole" flight of the previous day.

At the dawn of what we hope will be a more open and enlightened millennium, the better-matured version of the report is now being simultaneously published by the leading polar journal of the world, *The Polar Record*, journal of the University of Cambridge's renowned Scott Polar Research Institute — and here, in subsequently-much-expanded (and partly popularized) form, by *DIO*. (The greater size of the *DIO* version is merely due to DR's inability to let-alone an article during the period between SPRI's deadline and our publication. Absolutely no censorship — explicit or implicit — was applied by SPRI Editor Beau Riffenburgh.)

The Cambridge publication concludes with a note explaining that, while double-publication is unusual, the Byrd report "is considered to be of such significance to the polar community that it has been published here despite an expanded version being published this same month in *DIO*, [*The*] *International Journal of Scientific History*."

In the present rendition, some subjects are covered in more detail, and we have added a few extra Figures.

As *DIO* readers already know (see this and other info on inside cover of the 1997 reprint of *DIO 4.3*): five days after the initial report, the *NYTimes* Science Dep't got behind the story. On that memorable day of just reckoning in polar history — 1996/5/9 — all three TV networks concluded their national evening news with gracious acknowledgements of Amundsen's priority, an outcome I had sought for a quarter-century, though I never expected to see it.

As one who has occasionally been critical of the *NYTimes*, I am obliged and delighted to say that its coverage (written by John Noble Wilford, instigated by Nicholas Wade) was accurately and effectively realized.

[The courage of Wade, Wilford, and Riffenburgh was next joined by that of Jonathan Grupper and Jessica Louchheim of the *History Channel*, a confluence which climaxed in a beautifully executed *History Channel* investigative report for "This Week in History", airing 2000/12/8 (re-broadcast 2001/5/7, etc): see p.3.]

The Byrd hoax — stubbornly certified as genuine by National Geographic's Gilbert Grosvenor — was the capstone to a successive threesome (⊙21) of US explorers' pretended attainments of the North Pole, a scientific disgrace unmatched for persistence. (This paralleled the persistence of 3 generations of Grosvenors' inability ever to admit they had confused mythology with science — thereby holding hostage the reputation of their variously useful family enterprise, the "National" Geographic Society: ⊙21.) This triple-hoax blot on US science was initiated by National Geographic Society chief Gilbert Grosvenor's publicity-driven passion for awarding gold medals to exploration-claims without alertly checking data, specifically: Peary 1906 (Farthest North), Peary 1909 (N.Pole); Byrd 1926 (N.Pole) — all of which later turned out to have been fakes.

The 1906 award — by showing insiders (*DIO 1.1* †4 §B4) that exploration data were not being carefully checked — unleashed Explorers Club President Frederick Cook's ludicrous 1908 N.Pole hoax (⊙5 & ⊙17), the swift demise of which then ensured that (to avoid total public ruination for the United States' exploring-reputation) embarrassed US judges must approve the great Peary's lesser 1909 N.Pole exaggeration. When, in early 1926, the threat loomed that Roald Amundsen of Norway might reach the Pole and then challenge Peary's claim (fn 123), protection of the Peary fake thus in turn required the Byrd fake's

unquestioned national recognition, complete with yet another perfunctory "exam" by bad-old-reliable National Geographic. (Which triggered *the only Congressional Medals of Honor in history which are definitely known to have been fraudulently obtained*: Byrd & Bennett 1926 December: G 135. This item in particular has caused Rob't Bryce to regard *DIO*'s treatment of Byrd as insufficiently severe.) For details of causal inter-relations during this veritable cascade of fraud, see, e.g., §M2, §T14, ⊙5, ⊙21. Note: the US' venerable geographical institution, the American Geographical Society (though it awarded Peary high honors for the mostly genuine accomplishments of his great 1898-1902 expedition) never fell for any of the clumsy fakes just cited — nor did any Scandinavian geographical society. (Peary's most undeniable exploration-hoax — more blatant than even his ridiculous 1906 April "Farthest North" — was his non-existent 1906 June "Crocker Land". See fn 66. The evidences on both of these frauds are fully discussed at *DIO 1.1* †4 §B.)

But the responsibility is not just NGS' because early-20th-century organized US geography (being on rather few university campuses) was then too unsure of its academic status to risk investigating its community's potential embarrassments. Sir Martin Lindsay, who had in 1934 led what was then the longest self-supporting journey in arctic history, later (*The Times* of London, 1977/11/6) ticked off the secrets which polar experts had carried privately for decades: "Scott was not a great explorer . . . Shackleton tended to bound and to booze. . . . [Sir Douglas Mawson ate] the corpse of his companion. And Peary almost certainly never reached the North Pole,* though he reached the region; the Americans were determined to accept their man's testimony. All this is known to those of who have been polar explorers, but we have never dared to say so for fear of hurting feelings."

But the lordly Hubbard-Bell-Grosvenor clan, after running the "National" Geographic Society for over a century, has become immunized (by its dubiously-gained power: §M2 & ⊙21) from public criticism of its own inability to feel the slightest compensatory obligation to be sensitive about others' feelings. E.g., Ralph Plaisted and Wally Herbert are well known in the professional exploring community to be the first true surface attainers of the North Pole (1968 & 1969, resp), since National Geographic's 1909 Peary hoax is now just a joke there. Yet, NGS continues to ignore evidence and distort history, thus denying Plaisted and Herbert general acclaim for their priority, while they are still alive.

Is the National Geographic Society hoping to achieve a genuine priority: to become the world's first ineducable "educational" enterprise?

* The sensible reason why even an explorer as courageous as Peary turned around (1909/4/7) short of the Pole was: his precious northward pre-pickaxed & pre-iglooeed (Rawlins 1973 pp.142 & 159) trail, which he of course wished to use when speeding back southward with his 1909/4/1 Bartlett Camp, was vulnerable to obliteration by shifting ice & storms (*ibid* pp.118-121) — so, [a] he must return there quickly, but [b] he'd best take all his food & supplies with him in case the trail was unrecoverable for the return. (Navigator Bob Bartlett's party was simultaneously re-knitting the weeks-long trail from Bartlett Camp back to land. This key camp, well south of 88°N, was the last rendezvous&rest place before the Peary party's final 5^d dash towards the Pole and was the farthest north where Peary was accompanied by a fellow navigator. Bartlett was ordered to return south from this point, though he alone of Peary's 1909/4/1 personnel could have confirmed Peary's alleged navigational procedures & data from that point.) If this fragile-trail life&death-factor didn't weigh on Peary, then, at Bartlett Camp on 1909/4/1: *why didn't he unload (& cache) most of his food etc.* — instead of unloading navigator-witness Bartlett! — in order to hugely decrease his sledges' weight for (and thus increase the credibility of) his upcoming final northward dash, where he was to claim speeds (*ibid* pp.158-159 & *DIO 9.3* †6 fn 42) *double* those of the pre-April 1 light-sledge advance party, though all five of his post-All-Fool's Day northward party's sledges were fully-loaded (Rawlins 1973 pp.111, 134).

The “National” “Geographic” “Society”: What Real Geographical Societies Privately Think of It

It has often been snickered that the wealthy National Geographic Society is neither national nor geographic nor a society — primarily a glorified media outfit, dominated by the same rich Hubbard-Bell-Grosvenor family for *five* generations: ☉21. (The double-White House genes of the Adamses, Harrisons, & Bushes lag 60% behind this record.) Most academic organizations have hitherto treaded softly regarding NGS (see, e.g. Rawlins 1973 p.255): the tendency is delightfully illustrated by DR’s 1982/7/6 conversation with my late friend Sir Edward Shackleton (son of the immortal explorer and sometime head of the Royal Geographical Society — also Leader of the House of Lords). DR openly videotaped the encounter, as Shackleton recalled only half-way through (fortunately), with amusing results. (I promised him not to release this material while he lived. But he is now departed, and this enlightening conversation deserves preservation.) We had gotten into discussing professional reaction to DR’s 1973 book, *Peary at the North Pole, Fact or Fiction?* (Wash DC, 1973), which ended general trust in Peary’s false 1909 claim (near-universally accepted in the decades previous to 1973).

DR: . . . [there was] a good [and funny] review [in *Arctic* by N.Corley, Librarian, Arctic Institute of North America, predicting that DR’s book will be read by the Peary faithful “on the verge of apoplexy”] . . . And [Brig. H.W.]Love [Executive Director (i.e., head) of AINA] gave it [DR’s 1973 book] an anonymous [generally positive] review [*Choice* 1973 Nov which] called National Geographic a “pseudoscientific” organization . . . [DR: Love’s *main criticism* of DR’s book was that it might lead readers to confuse NGS with genuine geographical societies such as AGS.] My wife, a librarian, figured out who [the reviewer] was, so I wrote [Love], thanking him, and got a [1974/1/3] letter back [asking for continued anonymity]. . . .

ES: I would have thought it was a bit much to call the National Geographic “pseudoscientific”. I wouldn’t have called it scientific at all.

DR [laughing]: True, but this was Love’s term, not mine.

ES: . . . Interestingly, they have contributed quite a bit of money to some of the Royal Geographical Society’s serious expeditions. . . . so they’re a good institution.

DR: . . . And now . . . , with *Life* [magazine] gone . . . [NGS does] the best photography for popular journals.

ES: Marvellous.

DR: There’re many pluses. [NGS] did an excellent article a while back [1970 Aug] on the planets by [Kenneth] Weaver, I think it was. And, it’s just: [NGS is] very mixed. Sometimes . . . crazy stuff [see Rawlins 1973 Chap.4], and of course the [NGS-dominating Grosvenor] family tradition is all for Peary, so [NGS] can’t do much about it.

ES: Of course.

DR: The next stage [in *Fiction*’s scholarly reception] was [a wonderful four-page review in the] *Annals of the Association of American Geographers* [1975 March (vol.65) pp.79-82]. . . .

ES: Are we recording all this, by the way?

DR: Oh, yes.

ES [laughing]: Well, I’d better withdraw my remarks about [National Geographic] not being a scientific institution.

Preface: DIO, Byrd, & the “North Pole” Diary

Only days after *DIO 4.3* was mailed out (late — as usual), with two articles prominently questioning Richard Byrd’s 1926/5/9 claim to have reached the North Pole by air, a miracle [☉1] occurred at the Byrd Polar Research Center at Ohio State University: Byrd’s original 1926 diary record of the flight was “re-discovered” by Raimund Goerler of (G 5) the Byrd Polar Research Center Archival Program. (Goerler is described as OSU’s chief archivist by the *Columbus Dispatch* 1996/5/15 p.B2.)

When news of the find first began spreading about in polar circles, I was off traveling in warm New Zealand and warmer Tahiti.

Shortly after I returned to Baltimore, polar historian Ted Heckathorn phoned and relayed the information. But, when I checked at the Byrd Center, I learned that the diary had already had a non-outside expert examination. OSU map specialist John Bossler had already concluded that the diary’s sextant data were just unimportant confused practice calculations. (Bossler made no connection of the data to the 1926 official report.) I told Goerler by phone that if I did visit for an exam I would positively keep an open mind (a promise which, given the unambiguous nature of the evidence that was to emerge, turned out not to matter) — and I repeated what I had mentioned at my first meeting with Goerler (1993/10/21, OSU): of the first three North Pole claims (Cook 1908, Peary 1909, & Byrd 1926 — all three generally regarded as dubious), the only one I was not sure was false was Byrd’s. Indeed, it was the only one of the three about which I could truthfully say that (though surprised) I would not be utterly shocked if it turned out to be valid.

When Ted urged DR to join him at the Byrd Center’s upcoming meeting, I was hesitant, since there seemed little likelihood that anything of moment would turn up (which shows how smart I am). But, it would be good to see the Center and Ted again; so, on 1996/4/11, I drove out to Columbus. That very evening, I first got to know Byrd’s daughter, Bolling Byrd Clarke, a Swarthmore graduate (from a day when few women went to college at all, much less to one of such high rank). Bolling is irresistibly lovable, embodying Byrd’s personal virtues without the frailties. (I asked Bolling and her older sister Kay — who looks so much like her father that I guessed who she was, even before being introduced — whether their immortal father was a doting parent. The laughing reply was: well, yes — when he was home!) [I note that this occurred on Easter Monday — only a few hours before the next-morning discovery that would launch me heavily into the Byrd 1926 records, an involvement which has independently reminded me of a gooiey-dense Easter-egg hunt, because wall-to-wall anomalies are so unavoidably underfoot as one reads these records.]

Late the next morning, at DR’s request, Goerler drove Ted and DR over to the archives to look at the 1926 diary. When it was brought out, I touched it with the reverence it was due: a book that had *physically* flown deep into the Arctic 1925-1926 (**pre-Lindbergh**) and then (in 1927) across the Atlantic (soon after Lindbergh) — back in a day when almost no one else had the guts to do it.

Within a few minutes, we happened onto p.11 of the diary (our Fig.5 = D 11 = G 84), and my eye caught the time 7:07:10 Greenwich Civil Time written there. Recalling it as the *exact* time of one of the 1926 sights reported to National Geographic, I excitedly burst out that this was no practice calculation.

We squinted to read the nearly-erased solar altitude, 19°25’30’’ — and I leapt upon a copy of Byrd’s typed report to NGS that was sitting just to the left of the diary and feverishly fingered through the pages until finding the 7:07:10 GCT solar altitude in the official report. There it was: 18°18’18’’ — *a completely different figure*. I instantly learned how the expression my-skin-froze came to be. From this time, the Byrd claim was irreversibly doomed.

The attendant calculations for both figures showed that there was no misprint: the enormous discrepancy was real. At that unforgettable moment, I knew that this was the definitive end to the Byrd 1926 claim. And I wished that the incomparable Roald Amundsen

and the great flyer-navigator Bernt Balchen had been still alive to be here with us, at this archivally precious moment-of-truth. (The immediate entry of the discovery in my pocket diary was for 1996/4/12 11:41 EDT.)

On Byrd diary p.8 (our Fig.4 = D8 = G 82), another erased solar altitude (4:39:19 GCT) was discernable. The diary contained no other 1926 sextant data.

I told Goerler that Ted and I would say nothing of this (☉14) and would leave it to Ohio State & the Byrd Center to bring out the truth. I would write a report which the Center could use all, part, or none of. (Before leaving, I reached out and touched the diary one last time. I felt the same sense of privilege when I [thanks to the kindness and empathy of Christopher Walker] finally touched the 100 BC Babylonian tablet BM 55555 at the British Museum [1996/9/25 12:13 UT] — the key astronomical cuneiform text whose obverse line 11 first proved Babylonian use of Greek astronomical observations: *DIO 1.1* †6.)

Later, after dinner, some laptop-computer work showed that a simple hypothesis fit these two data and Byrd's takeoff place&time: a straight-north path at 70 mph (below: §I4).

I returned home on 1996/4/13. Some weeks later, after receiving from OSU photocopies of the original records, I spent a frantic ordmag 10 days writing the report — FedEx-ing it to Ohio State University on 1996/5/4 17^h EDT, and OSU instantly relayed a copy to the *NYTimes*. And the rest is history. Revised.

The text of that epochal report follows, with occasional revisions and sometimes quite substantial augmentations. Besides the footnotes: end-notes for added enlightenment are indicated by bracketted references, using the astronomer's Sun-symbol:

☉

Since the initial 1996 public revelation of the Byrd diary's controversy-ending contents, the diary has been published (very peculiarly — see, e.g., ☉1): Raimund Goerler *To the Pole*, Ohio State Univ Press, 1998. Our references to it are indicated by the prefix "G" — that is, "G 82" refers to Goerler 1998 p.82. (Also: NARA signifies the US National Archives, where "RG" specifies Record Group.)

References to the Byrd 1925-1927 diary are indicated by the prefix D if on numbered pages, or by prefix "d" if on the un-numbered pages at the back of the diary, which we here number in reverse. E.g., "d 3" refers to the 3rd page from the Byrd diary's back.

I am sorry that OSU, after at first effectively stipulating to Byrd's obvious 1926 failure, has since chosen to encourage a ridiculous and hopeless "defense" of the indefensible. This foolish strategy has thrown away much of the honor (☉14) which OSU had gained from the 1926/5/9 *NYTimes* revelation — and has turned a staid funeral into an undignified one, for Byrd's 1926 hoax.

Acknowledgements:

I've thanked some scholars in the body of the text. But deserving of extra mention for help at various stages, are: Ted Heckathorn, Keith Pickering, Bob Bryce, Linda Olsen, B.Krutsikh, Gerry Gregorek, Bill Johnson, Tracy Lener, Nancy Wallace, Ruth Eaton, & Clinton Moon. I also express gratitude to the key truth-outing rôle of a friend of my father (engineer&flyer Lou Rawlins, director of the Baltimore Airport), namely, versatile Bernt Balchen: pilot, engineer, navigator. (And a WW2 resistance hero as well. See, e.g., the writings of Carl Finstrom.)

[Note added 2004: I also thank Bess and Audrey Balchen for contributing enlightening items, despite their understandable disagreements with some of the following.]

Byrd's Heroic 1926 Flight & Its Faked Last Leg

by Rawlins¹

Byrd Exaggerated Latitude — But Saw Very Close to North Pole Byrd's NGS-Certified Hoax Covered for Peary's NGS-Certified Hoax US Geography Fear-Greed Led to 3 N.P. Fakes: Cook, Peary, Byrd Fourth-Claimants Amundsen-Ellsworth-Nobile 1st to Reach Pole National Geographic Exiled & Robbed the Greatest Polar Explorer NGS&OSU Defend Byrd Fake by Faking Existence of Serious Debate

A Introduction

A1 On 1926 May 9, at Kings Bay, Spitzbergen,² navigator Richard Byrd and pilot Floyd Bennett took off towards the North Pole in the primitive Fokker trimotor airplane, the *Josephine Ford*. Byrd's claim that the plane reached the Pole is now generally dismissed on several reasonable grounds. (Among the simplest: his unique failure to drop his cargo of over a hundred US flags, intended to mark the Pole so that Roald Amundsen's dirigible, the *Norge* — "Norway" — could verify [☉2] the claim on reaching the North Pole a few days later. I.e., Byrd had planned to do to Amundsen what Amundsen had done to Scott in 1911-1912 at the South Pole.)

A2 But it has also long been widely suspected and rumored that Byrd didn't even try.³ This prominent charge will here be proven false.

A3 Ohio State University archivist Raimund Goerler has brought forth the original diary-record of the 1926/5/9 "North Pole" flight of the justly famous polar explorer Richard Evelyn Byrd, Jr. (It had been found at OSU in the mid-1980s — but was then *re*-submerged for another decade. See ☉1. Further on the secrecy surrounding Byrd's records for many previous decades, see fn 22. As to Byrd's main reasons for keeping his records out of public view, see §E8, §G6, & §G7.)

A4 The 1926 diary will not restore Byrd's N.Pole claim. Quite to the contrary⁴ — as will readily be seen from our depictions (to follow) of its key pages (esp. Figs.3-5), which

¹Rawlins publishes *DIO* and is author of *Peary at the North Pole: Fact or Fiction*, Wash DC, 1973.

²A Norwegian possession, also called "Svalbard".

³I confess that I had myself (qualifiedly) wondered aloud on the point: Rawlins 1994 §§C-D.

⁴That the diary would spectacularly end the Byrd 1926 claim was not at all anticipated by OSU, which (when it made known the document's "recovery") believed it would help *restore* this tattered myth. (Before rumors of its release reached DR through Ted Heckathorn in early 1996, one expert [J.Bossler, OSU] had already reviewed the diary for OSU and had reassured the Byrd Center by judging the sextant data to be nothing but trivial practice shots: see Preface, above.) The 1926 claim was already so widely disbelieved that the situation for Byrd's supporters was in the what-have-we-got-to-lose category. The same thinking (e.g., *DIO 9.2* †5 §D3) led to the opening of the Cook and Peary papers — accompanied by the same loyalist complaints of occasional errors in critics' prior speculations, though these errors were largely due to a paucity of evidence caused by decades

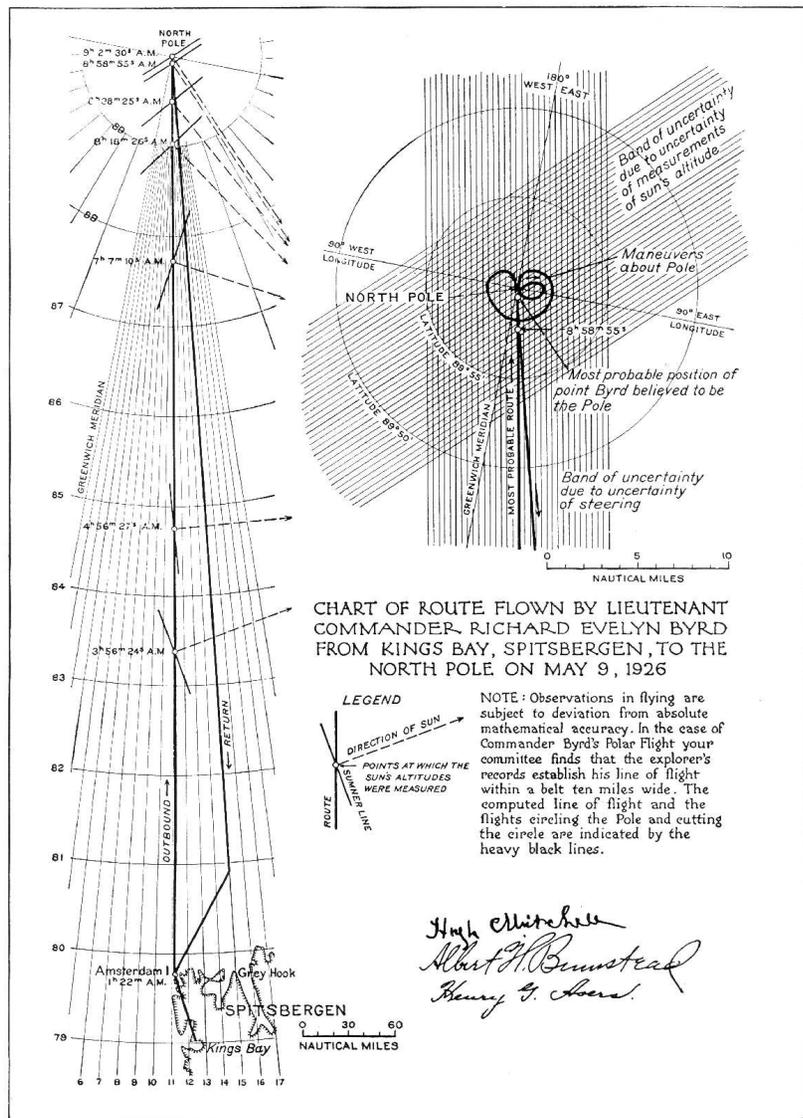


Figure 1: National Geographic's Official Diagram of Byrd's Alleged 1926/5/9 Path.

From *National Geographic* 1926 September p.386. The straightline-perfection of the flight depicted here is unique in the history of aerial exploration. (Remarkable implications summarized at §G — where we show the physical impossibility of the observations on which the NGS report and above chart are based.) However, the super-rushed National Geographic “expert” examining threesome’s report (*NGM 50:385-388* or *G 142-146*) on the Byrd 1926 flight fell distinctly short of perfection, as did the accompanying chart (above).

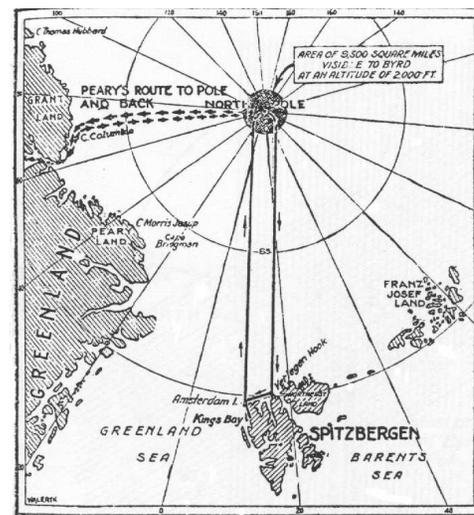


Figure 2: Byrd's Initial Version of His Flight-Path.

Published by the *NYTimes* 1926/5/11:2:3=4. (The depiction of area visible from 2000 ft [⊙7] is a bit exaggerated.) The same day this map was published, the later-disremembered Verlegen Hook-to-Amsterdam Island portion of the trip here depicted was reported on page 1 by Byrd's *NYT* correspondent. (See below at §J6, ⊙12, & fn 101.)

Comments on Fig.1 (opposite page) & the NGS Report accompanying it:

Rather than show Byrd slightly off his ideal 11°E meridian, the NGS Report's diagram (Fig.1) has fudged the 4:56:27 Sumner line to the right by almost 3 nautical miles (nmi), which forces the line's intersection (with the 11°E meridian) northward by nearly 30 nmi, so that it accords smoothly with Byrd's claimed mean speed (c.80 knots) along the impossibly straight path shown. And if the 7:07 Sumner line's slant looks out of kilter with the respect to the other Sumner lines', it's not your imagination: the National Geographic examiners messed it up by more than 10° due to confusion of Greenwich and local time. (Details here at, e.g., ⊙17.) The chart's Sumner line through the Pole (above) is drawn without accounting for the Equation of Time. The report (p.387 or G 144) figures the plane's speed as 74 knots from 8:18 GCT (sextant-shot) to the Pole; but the alleged sextant-indicated latitude (89°02'—) would imply c.80 knots from there to the N.Pole, in good accord with TA 7's dr estimate. (See §05 or G 155). The detail-diagram (upper right: Fig.1) shows Byrd barely 2 nmi from the Pole at 8:58:55 GCT, though the reported distance was over 4 nmi. (NGS report p.387 = G 144: 4.7 nmi. TA 7 = G 155: 4+ nmi.) This bloop would imply that Byrd's speed was about 40 knots! The trio's discussion of determining position “on a Sumner line” (G 142) was incomprehensibly misprinted by *National Geographic* as “of a Sumner line” (*NGM 50:387*), which is nonsense in context. But, what the NGS trio didn't even notice at all (while rushing because NGS had given Byrd his gold medal long before the trio had finished checking his data! — §M1) was far more important than slips which are mere symptoms of the hurry: the NGS trio's most crucial oversights are summarized here at §G. The report's silliest error is noted below at fn 127. The outright dishonest item in the NGS report is exposed at §M1.

are MUCH easier to read than the originals (since we have replaced script by type). But this invaluable document should go a long way towards restoring Byrd's reputation for courage and ability.

A5 In the diary, we see at last the real record of this amazingly dangerous real-life flight into the unknown (*far* into the unknown), as against Byrd's polished public report. The disagreements between the two versions are multiple and nontrivial: they leave little doubt that Byrd knew (though Bennett probably did not know until later) that he had not succeeded and that he was thus — with National Geographic rubberstamper Gilbert Grosvenor's willing commercial connivance — taking the honor of first reaching the North Pole from Amundsen, Ellsworth, & Nobile, whose *Norge* dirigible reached the Pole 3 days later (1926 May 12) during its pioneer crossing of the Arctic Ocean from Kings Bay, Spitzbergen, to Point Barrow, Alaska. (Byrd knew what he was doing. In his own words, as he pretended he had succeeded: "I had robbed them of realization of one of their long-cherished ambitions".⁵ It is the very enormity and ghastliness of the theft that motivates National Geographic to keep denying all, no matter the evidence.) The *Norge's* party included Oskar Wisting — one of Amundsen's 1911 South Pole companions — generously chosen by Amundsen in order that the two veterans could share a unique priority. (Wisting was Amundsen's Matt Henson — with the differences that: he is far less known, even though much the superior explorer.)

A6 Scientists cannot endorse Byrd's misreportage; but, in fairness and mercy to him, one must note the several items that follow.

A7 Byrd could have destroyed this highly precious diary (which accompanied him to Greenland in 1925, to the Arctic Ocean in 1926, and across the Atlantic Ocean on the *America* in 1927 — 3rd only after Lindbergh). Instead, whether deliberately or no [⊙3], he left us the evidence that ultimately told us the truth.

A8 He (a brother of Virginia's then-Gov. Harry Byrd) and Bennett had just risked their very lives — to a degree which few men ever have in peacetime. (One can only try — inadequately — to imagine the joy & relief they must have experienced when re-sighting land on the return, after the long hours of gut-roiling uncertainty over an oil-dripping motor and the accuracy of their delicate, still-experimental steering.) It is not hard to realize why they believed honors were due them.

A9 Byrd knew that he and the airplane were the scientific future of polar exploration, while the legendary Amundsen (like his dirigible) was the past.

of secrecy by the very families doing the complaining. (A pre-1996 DR misimpression was in not knowing how good a navigator Byrd was, because: [a] Not one raw Byrd sextant observation had ever been unsealed — or published — for reasons we now know all too well: ⊙3. [b] His pilots Balchen & Dean Smith correctly testified that Byrd took no sextant data on their post-1926 flights with him. See §C6.) One may theorize-speculate that depression over the 1926 imposture affected Byrd's character (as intelligently suggested by Ken Kirby's recent Byrd-cult-hated British television show) — and discouraged Byrd from working with the instrument thereafter. (Whatever the reason, no raw sextant observation in his hand later than the shattering 1926/5/9 7:07 GCT shot [Fig.5] has yet been released to the public. See fn 195. One hopes that material relevant to this point will ultimately turn up and help us better understand this heroic figure's tortured evolution.) A description of the 1926 sextant (fnn 79&80) is provided at TA 1 = G 149 and Byrd 1928 p.188. I don't know if the diary's dimensions have ever been published, so I provide them here. The pages are 6''7/8 (495 pts) wide, 8''1/8 (585 pts) high. (These 11-to-13 proportions are preserved in Figs.3-5, despite a linear size-reduction by a factor of 7/11.) For the dated pages (those signified here by prefix "D"), the 29 ruled lines are 1''/4 (18 pts) apart, starting 1'' (72 pts) from the top and ending 1''/8 (9 pts) from the bottom. For the pages at the back (those we here denote by prefix "d"), the size of the top and bottom clear-spaces are halved: 1''/2 (36 pts) from the top, 1''/16 (4 1/2 pts) from the bottom, and there are 34 lines spaced 16 1/2 pts apart. See fn 141.

⁵ *NYTimes* 1926/5/17:4:5=6. And see below at fn 192.

A10 Byrd largely used his new fame creditably. In 1927, he unquestionably crossed the Atlantic with Bernt Balchen⁶ as main pilot and survived their near-fatal desperate ditching, right off the French coast, at what would later be called Gold Beach (where the Canadians landed at Normandy on 1944/6/6, next to Omaha Beach), during which (non-instrument) pilot Bert Acosta injured his collar bone. (Acosta, officially the #1 pilot, had gotten the *America* into the air at Roosevelt Field, NYC, a major achievement in itself.) Goerler observes that the Byrd diary is water-soiled. (Note: had the plane been lost,⁷ so would the diary and thus the data revealing the full truth of the 1926 flight.) And, after Amundsen's heroic 1928 death (when the great Norwegian attempted to save his estranged former companion, 1926 N.Pole co-discoverer and the *Norge's* brilliant designer, Umberto Nobile of Italy), Byrd devoted⁸ most of the rest of his life to the polar regions. (Balchen was Byrd's excellent choice as pilot for his famous 1929 trip to the region of the South Pole — Byrd's aiming at which might⁹ have been better than some of us have previously thought, if the 1926 precedent is relevant.) Byrd ultimately became the person who will forever be remembered as the grand opener of the continent of Antarctica (using Amundsen's old 1910-1912 base, which Byrd re-named "Little America") — a person who genuinely lived an astonishingly risky, heroic life, a life which (I prefer to think) deliberately did much more good than harm.¹⁰

B Historical Upshot

B1 The 1926 Amundsen-Ellsworth-Nobile *Norge* expedition should now be recognized as first to the North Pole. *Norge* co-commander Lincoln Ellsworth was as US as Byrd: from Ohio, spending much of his youth in the town of Hudson.¹¹ Nobile designed and co-piloted

⁶ There is a website — <http://home.acadia.net/userpages/kikut/NPFlightNew.html> — created by Bess Balchen Urbahn (Bernt Balchen's 2nd wife), which has historical value in detailing [a] the persecution Balchen endured from the Byrd political machine, and [b] the process that ended with deleting from Balchen's 1958 book his now-vindicated (§N6) original analysis of the 1926 flight. Two questions: [a] If Balchen's rational 1958 analysis had not been suppressed (thus publicly undebated), would he have felt the need to draw attention to the essential truth (that Byrd had stolen polar priority from Amundsen) by finally stretching Bennett's confession (fn 13), in order to create an unignorable 1971 news story? [b] Had Balchen not thus spotlighted the 1926 flight's invalidity, would the fatal Byrd diary data ever have been announced as "re-found"? (I.e., are we — myself most of all, among the living — ultimately beneficiaries of Balchen's dying ploy? Likewise, had Bob Headland of SPRI not publicly doubted Byrd in *DIO* 4.3, would the diary have surfaced so soon after?) Finally, Bess Balchen reminds us that, while Byrd was for decades generally credited with being the first person to fly to both Poles, the more likely double-attainer is (§N7) the very person who exposed Byrd's North Pole lie, his self-created enemy: Balchen himself. (It would take alot to make an enemy of Balchen, who was known for his volunteering amiability.) Since Byrd suddenly began treating Balchen in hostile fashion right after his 1949 N.Pole flight, she suggests that this happened because Balchen's North Pole attainment threatened Byrd's fake (just as Amundsen's 1926 success threatened Grosvenor One's Peary hoax).

⁷ Once again, we have a debt to Balchen, who brought the plane in safely in horrid weather, and deliberately ditched in shallow water, which is why the plane didn't sink much. If the *America* had sunk into deep water, then (given the dazed state of the flyers), the 1925-1927 diary might have gone down with it.

⁸ Were Byrd's magnificent Antarctic years seen by him partly as a recompense for what he did to Amundsen? A provocative quote has been attributed to Byrd: "He who may have failed back there has his chance to make good here".

⁹ Though, see §§B7-C6 & fn 4. (No 1929 sextant data survive. See Rawlins 1994 fn 2.)

¹⁰ Though, we will never know how much better or worse the opening of Antarctica might have been accomplished, had other able and perhaps more honest explorers — pushed into semi-obscurity by Byrd's ambition — supervised the process instead.

¹¹ See the Ohio Historical Society's *Timeline* vol.5 # 6 pp.14-27: David Lore's "Polar Dreams, Polar Nightmares, the Quests of Lincoln Ellsworth". Ironically, the leading (academic) institution working

the *Norge*. Indisputedly the first man to reach the North Pole twice (1926 & 1928), he was always skeptical of Byrd's claim. His widow, librarian Dr. Gertrude Stolp Nobile, lived long enough to see her husband vindicated in the international press, 1996/5/9. I was privileged to be the person who personally broke the news to her a few hours before.¹² (Amundsen is also universally recognized as first to the South Pole, 1911. See, too, the conservative evaluations and conclusion by Rob't Headland of the Scott Polar Research Inst, University of Cambridge in a recent *DIO* paper: Headland 1994.) The famous prior North Pole claim of Rob't Peary (1909) no longer has the acceptance of the scientific community. (See, e.g., *Scientific American* 1990 March & June, *Nature* 1990/4/26, and Headland 1994. See also the fallout from Ted Heckathorn's key discovery regarding spherical-trig polar navigation: *DIO* 2.2 & *DIO* 2.3 ¶8 §B, *Wash Post* 1993/6/1, & *Science* 1993/6/11.)

B2 As Goerler instantly and correctly realized, the diary data are inconsistent with the detailed 1971 version¹³ of 1926 co-pilot Floyd Bennett's confession (of the flight's failure), given out by Balchen in old age. Now, with both diaries available, there is no doubt that Balchen gilded the truth in stating that Bennett had told him that the *Jo Ford* did not reach the Pole. If we may, above (§A6), suggest mitigating circumstances for Byrd, let us also do so for Balchen. Having for decades courageously told the truth — that Byrd had not quite succeeded in 1926 — to ears that were deaf (or more interested in political safety than in evaluating data), he evidently concluded that the only way to defend the truth was to counter-deceive.¹⁴ Both he & Byrd felt that their exaggerations served the higher truths cited at Rawlins 1994 fn 4. However, needless to say, I condone neither fabrication.

B3 Which brings us to the most important and original of Goerler's findings: the diary record (including even the nearly-suppressed parts) is consistent with Byrd having tried his best to reach the North Pole. That he fell somewhat short was due not to deficits of navigational ability¹⁵ or courage or ethics. Byrd was simply — as has long been suspected, e.g., in 1958 by Balchen — betrayed by a motor's oil-tank leak. This would have forced any responsible, prudent, & nonsuicidal commander — especially one with another's life in his hands — to make the (doubtless agonizing) decision to return to base.

B4 In the event, the motor did not stop and was still firing perfectly — even while visibly oil-covered — when the plane returned to Kings Bay. Though, in flight, Byrd & Bennett had no way of checking its condition from the cockpit. But: imagine Byrd's private feelings on finding that he had committed himself to an epochal hoax, which would hang over him for the rest of his life — all for nothing: fear of a motor-failure that never even occurred! (Note the parallel to Peary at Rawlins 1973 p.124, though Peary at least could never know for sure whether turning back early was unnecessary.) Defender Goerler (G 55-56, emph added): “That an oil leak in one motor would cause Byrd to abandon his effort to reach the

to deny Ohioan Ellsworth his honestly-deserved credit for polar priority is: Ohio State University.

¹²Telling Gertrude Nobile of the media storm that was about to break was one of the most pleasurable times of DR's life. (Her apartment was shortly filled with reporters, finally learning the truth of her husband's immortality.) It was as wonderful an experience knowing Nobile himself — and leaving him happily & rightly confident that his proper place in history was assured.

¹³R.Montague *Oceans, Poles, & Airmen* 1971 (p.48), purportedly an account of 1926 co-pilot Bennett's detailed private confession of the truth of the flight. I must criticize myself for occasionally (off&on) taking this story partly seriously (1973-1976). However, [a] the skepticism of my first paper (Rawlins 1972) on Byrd's 1926 flight was not based at all on Balchen's late accounts. [b] Starting in *DIO* 1.1 p.2 (1991), *DIO* was first to publish the fact (based on Balchen's own contemporary diary) that he had gone beyond what Bennett had actually told him. For this and for Bennett's very terse but invaluable statement to Balchen that there was indeed a dark secret about the 1926 flight, see Rawlins 1994 §A4 & fn 6. Compare also Balchen's direct statements to AP (1971/12/14) and to J.Visser (1972/2/27, Maxwell AFB) vs his diary (1928/10/20 entry, MAFB) or unpubl. autobio (NARA) p.142.

¹⁴ But see fn 6. And see similarly at *DIO* 4.2 ¶10 §I1 and *DIO* 9.1 ¶1 §L.

¹⁵As one who in the past has, occasionally & undogmatically, doubted Byrd's navigational qualifications (e.g., Rawlins 1994 §§C-D), it is especially incumbent upon me to emphasize this. (For origin of misimpression, see fn 4.)

North Pole seems unlikely. In planning for the flight, he had chosen a three-engine airplane specifically because he wanted to be able to *continue flying* if one engine failed.” But, when one is about half a thousand miles from home (flying over frozen, floating, fissured ice), it is not just a matter of *continuing* but of *getting back at all*. As a tense Byrd (hardly the model-of-assuredness implied by Goerler) scribbled rapidly to Bennett at the time (Fig.3, emph added here): “can you get **all the way back** on two motors”?

B5 Byrd knew that ending up short of Spitzbergen would — even assuming the landing on ridged floe-ice wasn't immediately fatal — leave him adrift upon a moving ice-cake. His 4/17 diary entry at D 108 = G 69 on his projected flight from Spitzbergen to Greenland: “If we should have a forced landing there we would be swept into the Atlantic before we could cover 50 miles [on foot] and the ice would melt under us.”¹⁶ Flying the *Jo Ford* asymmetrically on just two motors would obviously increase air-resistance and thus hurt fuel-efficiency — inevitably shrinking the range Byrd could firmly count on. Every mile north was in truth a *two-mile* commitment: the outward mile itself, as well as the eventual return-mile it would necessitate. (See Rawlins 1973 p.121.) Remember, too: *due to bucking a head-wind all the way north, Byrd had obviously used up much more fuel than expected for the distance actually achieved*.¹⁷ Flying ahead on just two motors would also have lowered airspeed from 85 mph (Fig.3) to barely 60 mph (*NYT* 5/16:4:4=5) — which, when bucking a head-wind of well over 10 mph, would have meant that progress northward would've been slowed to **under 50 mph**. And even this already-horrid-enough prospect for continuing north would have been further aggravated by the shakiness of the implicit underlying assumption: that the extra burden on the engines didn't trigger yet more trouble in the remaining pair of (for-now-healthy) motors, during the much longer time obviously required for a two-motor conclusion of the full roundtrip flight. (Especially worrisome since, again, Byrd could not know how long the wind would stay from-the-north, to help give back during the return some of the distance it cost going north.) Edsel Ford had already explicitly warned Byrd (G 44) that 1 motor wouldn't do when flying a trimotor. (This is one among numerous rare gems of information contained in Goerler's valuable if strangely-produced book.) Byrd also had to wonder how long good navigation-conditions would last (*idem*): “We wanted to make good speed, for the quicker we could get through, the better our chance to have the sun all the way on the return trip.” Given the situation, what sane commander would choose to go on to the Pole and then have to return 765 mi from there to Kings Bay? From the example of the very-nearly-fatal 1925 forced landing of Amundsen & Ellsworth's two planes (in the isolation of a similar Arctic latitude), Byrd — not yet an icon — knew that coming down on the ice to check the motor might end his life and (with the *Norge* about to take off for the Pole) would almost certainly kill his chance

¹⁶Note the relation of these happy thoughts to his intelligent 5/9 post-turnback decision to go straight to Verlegen Hook instead of taking his later-claimed (⊙12) diagonal (see Fig.1), which would have kept him over ice longer: landing on floating ice near the pack-ice edge was especially dangerous over near Amsterdam Island (where Spitzbergen wasn't holding up the southward-current-swept ice, as it would naturally do near mid-island Verlegen Hook. (He observed directly that the pack-ice touched land there: §J6.) According to his diary at D 5 = G 80 or G 77, Byrd couldn't (contra *NYT* 5/15:3:6=7 & TA 3 = G 151) discern the edge of the icepack at Amsterdam Island when close-up (see fn 26): “Now [over] ice but can't see edge of ice pack.” Unless the problem was fog (fn 26), this suggests the ice was more broken-up than usual, which would only exacerbate his §B5 floating-away nightmare. Note that the entirety of the two paragraphs of the typescript covering this part of the flight were (quite exceptionally) deleted between versions TA & TB: ⊙7. Thus he eliminated the TA 3 = G 151 statement: “Soon after leaving land we reached the edge of the Polar ice pack. The floes were not broken up to anything like the degree expected, and in several places the solid ice extended to the water's edge. Neither was it thought that the ice pack would end so near Spitzbergen.”

¹⁷Upon return, the plane was reported to have 5^h1/2 of fuel left (*NYT* 5/11:1:7=8). But, at the turnaround moment, Byrd could not be sure he'd have a north wind (and all three engines) all the way back; and he knew that he had diminished the initial fuel-load in order to get off the ground after the 5/8 takeoff failure: Byrd 1928 p.182.

for polar priority. (See Byrd's chilled remarks on the thought of landing on the ice, at *NYT* 5/17:1:3=4 and at p.197 of his 1928 book,¹⁸ *Skyward*; pp.179-185 reveals how difficult & risky it was to get the heavy plane airborne in the 1st place.) I believe Byrd's actions at this tense, critical juncture in his career could be most charitably summed up under the heading: quick thinking in an emergency — the mark of a well-trained naval officer.

B6 The Byrd family has urged no known restrictions on access to the newly-opened material. OSU archivist Goerler sent me installments by fax; some important omissions were fortunately filled in by his OSU colleague Ken Grossi (later of Oberlin College's library), who was extremely generous with his time in going over the diary's contents by phone, in order to ensure that I had clear copies of the diary pages relevant to Byrd's trips.

B7 The diary's proof that Byrd — traveling in an early, dangerously fragile airplane — went so far out on the ice towards the North Pole (virtually within sight of it) is consistent with the bravery evidenced throughout the rest of his illustrious career. Though his rôle in his several famous flights became less hands-on as his prominence rose, he was always (overcoming strong private fears — which are understandable in one who had watched¹⁹ friends die horribly in air crashes) willing to risk his own life, first-hand, on the long exploratory trips that genuinely made him a hero — if not an entirely genuine version of hero.²⁰ He also endured numerous unglamorous risks; e.g., when, in 1927, Anthony Fokker crash-landed during a test-flight, Byrd & Bennett were among those aboard: Byrd's arm was broken; and Bennett never recovered from the injuries there sustained, finally dying in 1928. (Note: this is the same Anthony Fokker who invented the propellor-synchronous airplane machine-gun. He also produced the Red Baron's WWI triplane before going on to trimotors: Fokker liked threes.) Byrd had only recently been a lobbyist. (Byrd 1928 p.137: "Those fellows up there in Congress are likeable and human and don't deserve the mud that is slung at them.") In all of history, I cannot think of another indoor lobbyist who ever transformed into a genuinely death-defying outdoor hero.

B8 There is probable indication that, had the motor's oil leak not occurred, Byrd & Bennett would have continued their adventure — and would have succeeded in hitting the Pole with high accuracy and finding their position there. Byrd's navigational scheme — admirably novel — was designed to fix his position at the Pole by the intersection of two quite differently determined lines (actually circles, the former great, the latter not quite): [i] the meridian (11°E, Amsterdam Island) he adhered to (by sun-compass) as a line-of-flight, and [ii] a nearly-transverse sextant-shot-based Sumner line. Note that Byrd's takeoff time would, for a flight whose nmi/hr speed was in the low 70s, have gotten him to the Pole only about an hour before local noon on the 11°E meridian. (See §O6.) I doubt that this coincidence is accidental. It reflects excellent navigation-logic.²¹

¹⁸ Ghosted by explorer, physicist, & 1st Peary-biographer (1926) Fitzhugh Green. (Also murderer. See Rawlins 1973 p.73 and Bryce 1997 p.1115. The woman whose charms may've helped instigate the deed was Peary's northern ex, as I later learned from Ted Heckathorn and Bob Bryce.) Green must have enjoyed (at "Byrd" 1928 p.140) describing himself as "one of the greatest authorities in the country on the Arctic", almost as much as he enjoyed expressing Byrd's carefully-phrased apparent scorn for using ghostwriters (*ibid* p.337). Byrd's prime ghost was Charles Murphy. (My previous impression that some of Byrd's later books were ghosted by Corey Ford has been questioned by knowledgeable persons. C.Ford was certainly the ghost for Balchen's 1958 book, *Come North with Me*.)

¹⁹ See the just comments of Edwin Hoyt *The Last Explorer* NYC 1968 pp.38-40 & 133.

²⁰ To the US public, Byrd was a hero so nearly on the level of Lindbergh that in 1939, the comic-strip *Buck Rogers* "predicted" that the first Earthlings-mission-to-Mars trip (in 1949) would be led by a (martyred) "Major Linbyrd". However, the legend-building that led to such pop-immortality got rather out of hand on occasion. E.g., Byrd's 1st book ascribes to him Jesus-like powers of healing; the caption to a 1927 photo appearing in Byrd 1928 opp. p.283: "The crew of the *America* visits the disabled French aviators. After Byrd's greeting, one man — a cripple since the war — rose and walked!"

²¹ A point that has not been considered in any previous analysis: Byrd may have originally planned to sextant-shoot the Moon to find an extra Sumner line. (See Rawlins 1973 p.231, on the Peary 1909

C Byrd's Various Alleged Speeds: Tied Up in Knots? or Pure Bosch?

C1 The written notes (normal communication in a deafeningly noisy early airplane), which Byrd gave to Bennett during the trip, survive in the diary, along with some telling arithmetic.

C2 Our Fig. 3 depicts diary page d 3 (G 96), at the top which Byrd writes: "The Stb [right] motor has an oil leak". Byrd 1928 p.196 avers that this was about an hour from the Pole. He adds at p.199 that he **believed it 99% certain** that the motor would stop. Note that the wireless he sent out in flight called this a "bad oil leak" (§H5). Bennett's note to Byrd (*NYT* 5/16:4:4=5): "very bad". In a real-life trip over a frigid, hostile landscape — hundreds of miles from civilization — a serious motor-oil-leak is not a yawner. (See §B4.)

C3 After spotting the leak, Byrd naturally asks Bennett (Fig.3): "Can you get all the way back [to land] on two motors"? (Not: Can you get to the Pole & back on 2 motors? Or: how much further can we go & get home on 2 motors?) Immediately following this is the question: "What has been our average air speed"? (Airspeed — the plane's speed with respect to the airmass — can be determined by on-board non-astronomical instruments, even when the ground is invisible.) Byrd's computations at this place show (Fig.3) that the reply was: 85 mph, distinctly lower than the 91 mph speed implicit in the official report's typescript account²² of a 665 nmi (= 765 statute miles)²³ trip in 8^h25^m — but *exactly* the top speed suggested in 1958 by Balchen (see 1958 appendix of Montague 1971 p.298) who

expedition's peculiar failure to shoot the Moon for longitude during its 1909/4/1 sextant work.) But, on 1926 May 9, the Moon was very near the horizon for Byrd and its crescent pretty narrow. (Had he flown a few weeks earlier or later, the Moon near its 1st quarter would have been available to provide a 2nd Sumner line roughly perpendicular to the Sun's.) Byrd's 1925 notes (though slightly confused regarding finding GCT: see fn 145) show that he found nothing shocking about taking a low-Moon sextant observation, e.g., his 1925 July 27 shot of the Moon for altitude 3°53' (D 335 = G 87 — actually a scribal error for 13°53': see discussion of 1925 sights at §P). For his 1925/5/6 lunar observation, he includes correct application of lunar parallax, a procedure sufficiently delicate that it has undone several other well-known parties, ancient (Hipparchos) & modern [the Navigation Foundation: *DIO* 1.1 ¶4 p.29 Note C; and a current *Journal for the History of Astronomy* Assoc Ed: *DIO* 1.2 fn 288]. (Also neglected in the standard astronomy textbook, G.Abell, L.Morrison, & S.Wolff *Exploration of the Universe* 1993 ed. p.25.)

²² Byrd-defender Portney has already (see §S8) acknowledged that a 5 knot (6 mph) *northward* wind-assist is needed to get the *Jo Ford* from Kings Bay to the Pole by 9:02. Actually, if computing for the 615 nmi distance from AI to the Pole (defender Jos. Portney's current preference), it's a 6 knot or 7 mph wind, since TA's AI-to-Pole interval was 7^h2/3 (implying 92 mph mean speed) — but the attested airspeed was 74 knots (85 mph): Fig.3. Note that Byrd's public reports never mentioned the 85 mph airspeed of Fig.3. (This speed had previously to be induced from Balchen's log for the post-Spitzbergen months during which he and Bennett co-piloted the *Jo Ford* around the US on a promotional tour.) Thus, the diary has now unevadably established that for Byrd to have reached the Pole, he needed a ridiculously convenient 7 mph wind from the south during the northward leg — later switching directions on cue for the southward leg: §S8! What is referred to herein as the "official report" is the 1st typescript (TA), that which Byrd transmitted to SeeNavy 1926/6/22, who relayed it to National Geographic the next day, Wednesday, 1926/6/23. The Ohio State Univ Archives possesses copies of the original TA, as well as 3 successive slightly (but importantly: §§G4-G6, §E4, ☉7, fn 80) edited versions: TB, TC, & TD. (Fig.7 displays representations of parallel excerpts from TA and TD.) The 3rd (TC) contains Byrd's handwritten editorial corrections which produced the final version, TD (effectively identical to TC), dated 1926/11/24. The TD copy sent to the Geogr Soc Philadelphia, later recovered and preserved by Herman Friis of the US National Archives, has been in my files for decades. Note that NGS told the public in 1926 Sept that (*National Geographic* 50:388) "the explorer transmitted certified copies of his data and photographic reproductions of his charts . . . to a number of the leading geographical societies of the world for their archives." Rawlins 1973 p.270: "In 1967-1968, [DR] attempted to locate these reproductions and thereby learned that, contrary to the definite statement quoted above, no other society *ever* possessed such copies." (And NGS told DR it couldn't find anything at all: fn 189.)

²³ Actually 768 mi. See fn 24.

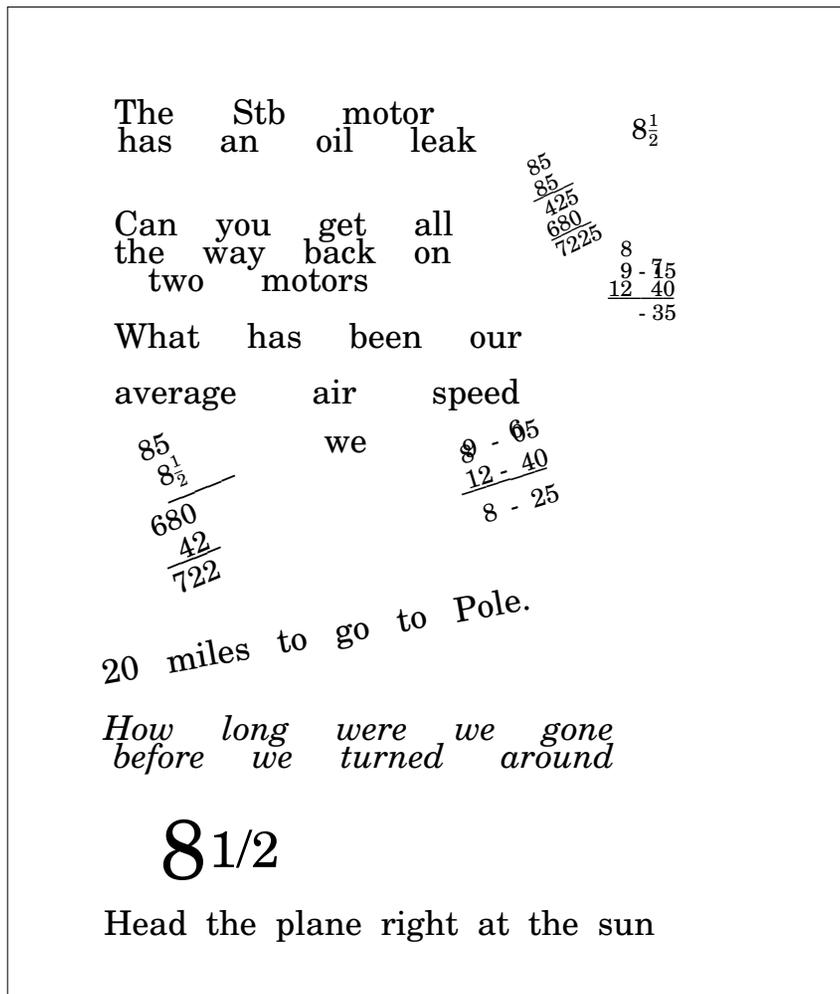


Figure 3: Byrd's Frantic 1926/5/9 Diary Record: Oil-Leak Discovered.

For the key Byrd handwritten documents discussed in this report (of which the originals are in the Ohio State University Archives), we have here produced depictions (Figs.3-6 & Fig.8) of the script portions only. N.B.: *Unersed material is in roman type. Erased material is in italics.* These illustrations are far easier to read than is the original scrawl, especially the sections Byrd tried to erase in the key diary pages Figs.3-5, where (to repeat): we show all erased material in *italics*. The original record's proportions are retained in Figs.3-6. The above page is near the diary's end (d 3 = G 96), bearing some of the Byrd-Bennett exchange near the time of turnback (the huge datum "8 1/2" is the sole entry by Bennett), as well as a note at the bottom regarding a Byrd "splendid check" (during the 2nd hour of the return southward) that is strangely alleged (☉11) to have verified the nullness of his cumulative steering-error to that point. In the actual diary, each page is 6''7/8 wide, 8''1/8 high. (See fn 4 for details.) Though we have not attempted to make each word as wide as in Byrd's original stretched scribble, the placement and (in the above rather striking case: fn 141), the slant of each message or calculation is faithfully rendered. In Figs.3-5, the superposed digits are Byrd's own write-overs (offsets exaggerated here for legibility).

often flew the *Jo Ford* in 1926-1927, becoming the Fokker factory's chief test pilot. (Note that during the northward flight, Byrd was counting on just 80 mph or 70 knots: §O.) This is 74 knots (nautical miles/hour).²⁴ Pilots Bennett and Alton Parker had privately tested²⁵ the *Jo Ford* on 1926/3/29 — speed 87 mph or 76 knots. This verifies Balchen's assertion that landing-gear skis (which he personally helped get onto the *Jo Ford* in 1926 at Kings Bay, at Amundsen's generous instigation) slowed the airplane a few knots.

C4 But the far more revealing point is: had Byrd been able effectively to use his (Pioneer Instrument Co) drift-indicator [☉4] for groundspeed, as he had originally hoped to do, then (no matter how one looks at the issue: see discussions at ☉4), his Fig.3 multiplications would have been pointless. Possible reasons for not finding reliable groundspeed via drift-indicator: [a] Difficulty in tracking (without smoke bombs) a fixed point on the icepack's kaleidoscopic surface, a problem perhaps intermittently aggravated by fog,²⁶ [b] inconstancy of plane's height, [c] inconstancy of air density, & [d] intelligent preference for sextant (see §B8, also §E10 & ☉4) to gauge distance. To its credit, the National Geographic Society's time-warp 1926 June 28-23 otherwise insufficiently curious report on Byrd's claim (publicly bemedalled by Pres. Coolidge on June 23, five days before NGS completed its "investigation"! — see §M1) remarks²⁷ that factor [c] will vitiate drift-indicator estimates

²⁴ An international nautical mile, defined as 1.852 km, is supposed to be 1 arcminute of great-circle angle on the Earth's surface, and is 1.15078 times larger than the familiar (statute) mile. But, due to the Earth's oblate shape, an arcmin (1/60 of a degree) is about a half-percent larger than 1 nmi near the poles, where we have 1.157 mi/arcmin or 69.4 mi/degree of latitude. Such refinements will generally be ignored here (especially because they would confuse the standard meaning of the word "knot"). This will slightly ease the Byrd claim's problems; e.g., the naïve distance from Kings Bay to the Pole is 665 nmi (11°05' times 60) or 765 mi, the figures we use in this report (except for the exact numbers at ☉6); but the accurate distance is 668 mi = 768 nmi.

²⁵ At Mineola, Long Island. (Carbon copy of test-results in my files, courtesy National Archives.) Parker (Lieut., US Marine Corps) was also at Spitzbergen, as a back-up pilot to Bennett. (*NYT* 5/16:4:1 reports that on 5/15, while Byrd & Bennett were using the *Jo Ford* to help save 3 stranded trappers, Parker did air-stunts to entertain Kings Bay with the expedition's other airplane, an Oriole called "Richard the 3rd" not for Shakespeare's pre-Tudor ultravillain-monarch, but for hero R.E.Byrd, Jr.'s son: G 74.) By an odd coincidence, another Alton Parker (the Dem candidate whom Teddy Roosevelt defeated in the 1904 Presidential election) died in Central Park on the very day (5/10) when Byrd's "success" hit the US. See *NYT* 5/11:1:2 & 26:1-2.

²⁶ Fog is mentioned at D 5 = G 80, but not in the official typescript. D 5 reads: "It looks like fog over the polar sea . . . about to pass Amsterdam Island . . . Now [over] ice but can't see edge of ice pack" — vs Byrd's first written story (*NYT* 5/17:4:6=7): "Amundsen [in 1925] had met fog at Amsterdam Island, which had taken him miles off his course. We so far, then, had been more fortunate than he had been. There was no fog and it was all clear on both sides." No fog mentioned at Byrd 1928 pp.191 & 248, either. If fog was indeed obscuring the ice-pack edge and then the ice surface (did this trigger Byrd's thought of using a smoke-bomb? — see fn 29 and Byrd 1928 pp.247-248) more seriously than Byrd let on (note fn 16), then he could have kept a straight course for the first few hours more by sighting land to the south than by drift-indicator. This provides an alternate explanation for the contrast in the accuracy of Byrd's 1926 steering (good) vs that of his distance-gauging (poor). But I do not prefer it because the diary data are consistent with Byrd having gone far beyond the sight of land.

²⁷ See §E10. Does this suggest that a member of the NGS sub-committee threesome privately expressed some skepticism at the neatness of the flight's data? I doubt it was tractable Hugh Mitchell (USC&GS), who had earlier testified before Congress in support of Peary's N.Pole claim, having been secretly paid \$280 by Peary for his efforts (Rawlins 1973 p.238, Bryce 1997 pp.522, 1050-1051). The oddity here is that, while the NGS report indeed rightly doubts the reliability of the d-i distance estimates in TA, it fails to remark that TA contains repeated pretenses (much muted by the time the TD version of the report was distributed: §G4) that it can precisely use these very data to interpret whether a sextant shot puts the plane *just a few miles* to the left or right of the intended flight-path. An example of this unobtrusive sham (for the 3:56 GCT alleged shot) is discussed at §E4. See also Fig.7.

of groundspeed.²⁸ NGS was also correct in pointing out that such speed-uncertainty would not in itself affect Byrd's ability to steer by drift-indicator & sun-compass. Factor [a] could hurt steering if fog were unremitting, but we will find evidence below that is consistent with assuming that Byrd's net steering was accurate, depending upon the sun-compass²⁹ designed by National Geographic's chief cartographer Alfred Bumstead, and presented to Byrd by NGS head Gilbert Grosvenor One (*NYT* 5/17:4:1=2).

C5 The flight's barograph [⊙5] shows large and sharp variations in the air density encountered by the *Jo Ford* during the trip. (Byrd's report says the air was unbumpy.)³⁰ The expedition's meteorologist, physicist Wm.Haines (loaned to Byrd by the US Weather Bureau), analysing the record (1926/5/12: G 158), interprets this as due primarily to factor [b] (height variations). (Keith Pickering agrees, citing the vertical instability of early aircraft.)

C6 Once he gets the 85 mph average airspeed from Bennett (§C3), Byrd computes in the diary (Fig.3) the distance the *Jo Ford* has traveled in 8 1/2 hours: 85 times 8.5 = 722.5. (Pre-high-school readers may consult eq. 3, below at §O5.) He there repeats the calculation (with essentially the same outcome) just to be sure of the result. The repetition suggests that this result is important to him. Again, this is not the behavior of a navigator who has been regularly keeping close track of groundspeed — or even airspeed! (And no this-checks-with-other-data statement appears in the diary.) I.e., this is pure and ultra-primitive dead reckoning math. (Byrd's 1927 trans-Atlantic flight and his 1929 trip to the vicinity of the South Pole were also by dead reckoning: see ⊙12 and Rodgers 1990 p.189, resp. See also below at §O5 & eq. 3 there.)

C7 Needless to say, all that ultimately matters is groundspeed. (Groundspeed is mph actually made good, towards the Pole in this case.) The plane's groundspeed is simply the vector sum of [a] the plane's airspeed and [b] the groundspeed of the wind (the surrounding airmass). But since nothing on this diary page tells us the speed of the wind, we cannot learn the *Jo Ford's* groundspeed from these data. (We will determine it below at §II.)

C8 But we do learn how and when Byrd may have indicated to Bennett (who had no groundspeed information) that they were at the Pole (and thus should turn & start back). Diary D 14 = G 86: "We should be at the Pole now. Make a circle & I will take a picture [camera]. Then I want the sun [via sextant]." Again: this message ("should be") smells rough — as if dead reckoning was the basis of distance-estimation to that point. Also, the proper order of business should be: sextant confirmation of latitude first, *then* photo — not the reverse.

²⁸ See Hjalmar Riiser-Larsen's real-life experiences (Amundsen & Ellsworth *Our Polar Flight* NYC 1925 pp.168f) regarding [a], and Amundsen & Ellsworth *First Crossing of the Polar Sea* NYC 1927, e.g., pp.186f & 206, regarding [c]. The expedition got to 87°47'N and 13°W (1925 p.176), so Byrd & Bennett in 1926 did not probably exceed the latitude attained in 1925 — but almost certainly saw further to the north from their greater height. Note that Byrd went mostly along a new route, since the 1925 expedition drifted far to the west: fn 29.

²⁹ See ⊙11. The use of the sun-compass (as an inverse sun-dial, based upon a 24^h clock set on local apparent time) is accurately explained at Byrd 1928 p.190 (quoted here at ⊙11). He took two sun-compasses on the 1926 trip (⊙8). The 1925 Amundsen-Ellsworth flight towards the N.Pole also used the sun-compass steering principle, apparently with less success than Byrd, largely due to fog: *Our Polar Flight* p.168. Byrd's official report does not mention using smoke-bombs to check speed (via stopwatch) on the drift-indicator: "We had smoke bombs for drift but did not need them." But the diary at D 6 = G 81 says: "I want to use a smoke bomb"; and: "where is a match box to strike with"? (See also Byrd 1928 pp.247-248.) Goerler has privately offered the intelligent speculation that no smoke bombs were dropped simply because no flame was available. Molett 1996 p.3 jettisons the concept of inertia in order to disbelieve smoke bombs' utility (contra Byrd 1928 pp.247-248): "By the time the bomb hit the ice it would already be too far behind the aircraft to time it".

³⁰ TA 4 (G 152): "There was not a 'bump' in the air over the Polar Pack. The air was smooth and the plane gave a steady platform."

C9 The Byrd-computed distance (§C6), 722.5 miles (which cannot be nautical miles, since this would put the plane far beyond the Pole) equals 628 nmi (nautical miles). For the calculation (Fig.3), Byrd uses [⊙6] 00:40 GCT (or Greenwich 12:40 AM) as his takeoff time. (See discussion of time at §K.)

C10 I note in passing here that Goerler's chief defenders of the faith (against DR's diary discoveries), Wm. Molett & G.Newsom, have an amusing disagreement which Goerler's invocation of them (G 57 n.19) doesn't tell the reader about: Molett scoffs at DR for using the takeoff time for dead-reckoning math (as Byrd does in Fig.3) — while Newsom's entire case for Byrd's (semi) success is based squarely upon these Molett-condemned data. That is, Newsom 1997 p.10 trusts *only* the Fig.3 data (based on takeoff time) — while Molett³¹ argues that no competent navigator would base dead reckoning on his takeoff time! Whose shade conjured up the Goerler-Molett-Newsom defense-trio? Lewis Carroll? Hieronymus Bosch?

C11 On the basis of the §C9 numbers, Byrd makes 2 time-interval calculations (Fig.3), 9:05 – 12:40 = 8^h25^m and (see §G7) 9:15 – 12:40 = 8^h35^m. The former, 8^h25^m, is exactly the northward-flight interval he uses in his final report, takeoff at 00:37 GCT & Pole-arrival at 9:02 GCT, at TA 3&7 = G 151&155, resp. So *it is peculiar* to find the 8^h35^m linear calculation in the original in-flight record. Directly under these calculations, he writes (maybe for Bennett): "20 miles to go to pole".

C12 Also suggesting jumpy improvisation³² in response to the oil-leak dilemma: the word "we" appears alone in the midst of the d 3 (Fig.3) calculations — an uncompleted communication, perhaps eventually (fn 32) completed in full on D 14 = G 86 ("We should be at the Pole now.") The next entry on Fig.3 (after the fragment "we") appears to be the distance to the Pole ("20"), but the datum is repeatedly written-over (fn 32 & fn 110), indicating terrible indecision, which is an understandable reaction in one being so suddenly & painfully torn between two forces: his heroic desire to travel the whole way — vs his responsibilities to his family and his companion.

³¹ Molett 1998 p.61 & his follow-up letter at *Mercator's World* 1998 Sept-Oct p.10.

³² Examining Fig.3: the mid-line positioning of "we" indicates that at least some of the math that exists on either side was already in place when this word was written. (One suspects that, on reflection, Byrd thought it too transparent to tell Bennett "we are at the Pole" so immediately after the leak was spotted.) Realizing there was too little room to write here, he instead went below his arithmetic. There, at the natural beginning (extreme left) of the very next line, he seems to have started to write a word and then wrote another — one of the two may have been "go" — over it. Though Byrd's 722 mi math (§C6) would seem to put him over 40 mi from his goal, he instead finally decided to inscribe what looks like "20" (over the accumulated script-mess: not shown in Fig.3) as the beginning of what is evidently intended as a communication to Bennett: "20 miles to go to Pole". (Whether Bennett saw this page at the time or only when Byrd perhaps asked him to aim at the Sun cannot be proved.) Both the digits in this "20" look extremely uncharacteristic for Byrd: the angle of the "2" is a huge loop and the "0" has a curved line through it — both renderings unique in the diary for those digits. He shortly switched back to writing on D 14 = G 86, a page which is — like the page opposite — entirely barren of navigational numbers, and there wrote: "we should be at the Pole now." Another possibility, a highly unlikely speculation, but one which may offer a comforting straw for those who can't tolerate any but innocent explanations of Byrd's failure: did Byrd just misgauge the distance from Kings Bay to the Pole (contra §O4)? At *NYT* 5/9:1:2, he is quoted at the takeoff as saying: "We are off for the Pole Our intention is to fly due north 720 miles in a bee line, the distance from Kings Bay to the top of the world" — a figure that is almost exactly the same as the 722 mi figure he would write in his diary just 8^h1/2 later: Fig.3. On his return, he is quoted as calling the Kings Bay-to-Pole distance 750 mi: *NYT* 5/30:1:2. (See ⊙6.) Note that such theories are not only shaky (and insulting to Byrd's intelligence) — they run counter to the attractively simple theory produced in §§O4&O5.

C13 We assume that the sextant was still working.³³ If so, then Byrd observed the apparent solar altitude with it. But this datum is not recorded in the diary. (Solar altitude³⁴ was probably at least 2° higher than the 17° 1/4 that would correspond to being at the Pole.)

C14 By the most generous interpretation,³⁵ Byrd went about 20 mi/85 mph = 14^m beyond 9:15 before indicating to Bennett that the plane was at the Pole about 9:30 — which is the time indicated by the barograph (§C16). By Bennett’s in-flight recollection (§D7) that the whole northward trip took 8^h1/2, the turnaround time was c.9:20 GCT according to the Byrd *NYT* correspondent W.Bird’s 00:50 GCT time for the takeoff, or c.9:25 GCT by the 00:55 GCT takeoff-time reported by Odd Arnesen of Oslo’s *Aftenposten* (§K1).

C15 Since we are seeking an upper limit for the *Jo Ford*’s highest latitude, we will use 9:30 (the maximum of all the time-figures just discussed here for the turnback moment), in the calculations to follow: §I4. Pickering has independently arrived at the same estimate of turnaround time. This estimate is about to be directly confirmed in §C16.

C16 The barograph record³⁶ indicates a sharp minimum (26’’.1) in the barometric pressure at about this time, which is consistent with Byrd, at his actual Farthest-North, understandably rising [☉7] to see as far as possible to the north. However, this height-peak occurs just after 9:30 GCT, a time seriously disagreeing with Byrd’s final official story (which has him at the Pole 9:02-9:15 GCT: §D6). But the actual height-peak-time is reasonably consistent with our §C15 induction of the time when the plane turned back.

D Published Sextant Data (Official Typescript)

D1 Byrd’s official report indicates (TA 7 = G 155) that his last position-determination by sextant before virtually reaching the Pole was 31 1/2 nmi (36 mi) from it. (No reported sextant observation puts him 20 mi away: *idem.*) Had this sextant observation occurred (or had drift-indicator speed estimates been of use), there would be no need to compute a shaky dead reckoning figure (indicating 20 miles to go) based upon airspeed. It is difficult to contend that Byrd moved faster than airspeed during the outward trip, since he reports³⁷ no northward wind-assistance for most of the outward trip and indeed appears to acknowledge (interpretations of his words may differ on this) a wind towards Spitzbergen, getting stronger during the last outward hour (TA 7 = G 155) — though Byrd 1928 p.199 says the wind “freshened” only after the plane turned south.

D2 Note that Byrd’s sextant-locked 61 knot mean speed for the first 6^h1/4 hours of the flight (as shown by the 7:07 GCT sextant shot at 85°16’N, discussed at §E14) requires that, to reach the Pole at the reported time (9:02 GCT), he would have had to cover the final 284 nmi between 85°16’N and 90°N in 1^h55^m — which entails a sudden speed-jump (around 7:07 GCT) from 61 knots to 148 knots.

D3 Inventing sextant data indoors is far simpler than genuine navigating via real outdoor data. In the current instance, the material is right at hand: merely add 15°/hr times the

³³ Alternate theory: did the sextant fall & break before turnback rather than, as Byrd later reported (TA 8 = G 156), just after? But this suggestion seems to conflict with the §C8-cited Byrd note (D 14 = G 86, assuming this page’s entries are all contemporaneous), asking Bennett to turn the plane so Byrd can get the Sun. Unless the sextant fell during (because of?) the turnaround. Note: the diary makes no mention of the alleged damage to the sextant — and alleged damage at the turnaround (after 9 AM) cannot explain the diary’s lack of sextant data after his bad-news shot at 7:07 (see fn 133).

³⁴ Note: the “altitude” of a celestial body such as the Sun is an angle, usually expressed in degrees. It is not to be confused with, e.g., the altitude of an airplane above sealevel, which is usually expressed in feet or meters. (We will here generally use “height” for the latter altitude, unless quoting from others.)

³⁵ As suggested by the “20 miles” at d 3 (Fig.3).

³⁶ G 158’s reproduction of the barograph is wan & reduced, so I thank Rai Goerler for (96/4/23) sending DR xeroxes (made at four varying darknesses) of OSU’s photocopy of the barograph.

³⁷ He says the wind is from the east (TA 3 = G 151, *NYT* 5/15:3:6=7), during the early part of the outward trip. (See §D8.)

GACT (§E4 & Figs.4-8) to the longitude (11°E) and subtract 180° to find the Sun’s hour angle *HA*. Then, for any latitude *L*, the Sun’s true altitude *h* is given by the familiar equation (see the discussion of St.Hilaire navigation in Bowditch 1916 p.157, 1926 p.167, 1984 1.590f & 2.904):

$$\sin h = \cos L \cos \delta \cos HA + \sin L \sin \delta \quad (1)$$

where solar Declination δ (for the sight’s date & time) is found from the *AENA* tables.

D4 As to whether Byrd knew such math: his report repeatedly (TA 1&5 = G 149&153) refers explicitly to his use of the St.Hilaire method of navigating, which analyses a celestial body’s observed altitude by: [i] assuming a time & geographical position, [ii] mathematically manufacturing (faking) the body’s altitude for the assumed space-time point, [iii] comparing (e.g., fn 62) the faked altitude to the actual outdoor-observed altitude, [iv] drawing (upon a map of one’s rough location) a line perpendicular to the celestial body’s bearing (azimuth) but offset from one’s assumed geographical position (towards the body if observed *h* exceeds computed *h*, away if the reverse). This line will be the desired Sumner line (or line-of-position). The diary shows (D 335 = G 87) that Byrd understood the St.Hilaire method. (As noted at §D3: indoor calculation³⁸ of altitude data for a celestial body is easier than actually navigating with real outdoor altitude data. Just compare fn 62 to fn 61.)³⁹

D5 *The poles are the easiest places on Earth to manufacture celestial altitude data for.* The solar altitude sextant data presented in support of all the fake North Pole claims were unwitnessed. (Peary planned it this way: Rawlins 1973 p.127. So did Byrd, since, as early as 4/16, he was planning to take only non-navigator Bennett with him on the Spitzbergen flight: G 68.) The N.Pole “observations” of Frederick Cook (1908/4/21) and Robert Peary (1909/4/6) did not even require trig; as in the Byrd case (where the sph trig of eq. 1 [§D3] was probably [though see fn 73] used at least for the first few shots: §F6), Cook’s and Peary’s N.Pole sextant data were unshared with other party members. By contrast, for Amundsen at the N.Pole (1926/5/12) and for him (1911/12/14) and Rob’t Scott (1912/1/17) at the S.Pole: the solar altitude data were largely observed by other navigators.⁴⁰ To sum up explicitly: since 1875 (introduction of St.Hilaire method), efficient navigation *required the faking of observations as part of the normal math of navigation*. (Details already given at §D4.) Thus, to claim that Byrd (or any other navigator) was not mentally equipped to fake data⁴¹ would be the same as saying he couldn’t navigate, which of course would mean that he couldn’t find the Pole. And to suppose that Byrd didn’t have the nerve to fabricate is to commit a common mistake: forgetting that those rare & resourceful men, who dared to risk their *lives* in the hostile wilderness, might also risk their reputations when faced with a different kind of danger.⁴²

³⁸ Computed results may be embellished with random scatter. See, e.g., §F6 or *DIO 2.1* ¶4 fn 19.

³⁹ And fn 61 would be much more difficult yet if the longitude were not prescribed at the outset.

⁴⁰ See, e.g., R.Huntford *Scott & Amundsen* NYC 1980 pp.493 & 515, and Amundsen & Ellsworth *op cit* Chap.10.

⁴¹ Though: note the fine point discussed at fn 73.

⁴² In any case, near the Pole, it would have been possible in a few minutes to calculate altitudes (to sufficient accuracy for the report) even more simply via the Hinks-Littlehales grid-navigation method, which Byrd navigated by in 1926. (The cartesian approach to polar navigation — evidently first published by A.Hinks at *Geogr J* 35:299; 1910 — is a special case of both St.Hilaire & grid navigation. I will refer to the Hinks & grid methods interchangeably here.) Indeed, no careful & knowledgeable mathematician would fail to compare both rigorous & Hinks calculations to check his work. Byrd’s report even spoke of applying Sumner-line curvature. At D 329, one finds the appropriate curves, marked 1 to 10; if these numbers signify multiples of a unit of a few degrees, then they seem roughly apt to the Byrd “flight-chart” (chart discussed here at §F1), of which OSU possesses two photographs — which, in both extent & scale, appears to be rather less than full-sized.

D6 The Pole-arrival time stated (TA 7 = G 155) in Byrd's eventual official report was 9:02 GCT. Rawlins 1994 §B1 notes that this same event (Pole-arrival) originally⁴³ was radioed by Byrd (1926/5/12) to SecNavy as having occurred at 9:15 GCT. Byrd evidently hesitated in making up his mind on this critical datum. It is obvious that, in the diary reckonings (Fig.3), 9:15 GCT is closely related to a distance (722.5 mi) which leaves Byrd well short of the Pole, though in his final story (TA 8 = G 156), 9:15 GCT is the time given for departure from the Pole (after 13^m of circling). Thus, one of the two simplest proofs (the other being the 7:07 GCT solar altitude alteration: §E8) that Byrd's N.Pole claim cannot stand is the 3-stage evolution of the story of what was going on at 9:15 GCT. To summarize succinctly: in the original diary, 9:15 is a time when he is a few miles short of the Pole (Fig.3). But the 1926/5/12 telegram to the Navy (fn 43) says 9:15 is the Pole-arrival time. Then, in the final report, 9:15 becomes the Pole-*departure* time, after nearly a quarter-hour of circling. To put the story's difficulty in the simplest possible terms: why would Byrd compute (§C11) the constant-speed 85 mph *straight*-line northward air-distance he's traveled up to 9:15, if he's actually been *circling* the Pole for the 13^m previous to this time?⁴⁴ Speaking as an admirer of Byrd's merits, I urge his other sympathizers to realize that there is no way that a claim based upon a multiplicity of multiple-contradictions (see also §N6, etc) can possibly maintain lasting credence in the scientific community.⁴⁵

D7 Goerler made the remarkable discovery that on the same page (Fig.3), as the 9:05-9:15 arithmetic (§C11), right under "20 miles to go to the Pole", Byrd wrote Bennett: "How long were we gone before we turned around?" (Not: "How long did we take to get to the Pole?") The reply, "8 1/2" (hours), was written under the question. (In much bigger script, with a "2" more curved than Byrd's, and with a deeply slanted fraction; this seems to be Bennett's sole writing in the diary.) In the diary, as it has come down to us, this reply ("8 1/2") is intact; but Byrd's question has been nearly erased. Needless to say, this is not

⁴³ The full "Urgent" telegram (NYT 1926/5/13:3:2): "Bennett and I reached Pole about 9:15 A.M., May 9. Found conditions as described by Peary. Were favored with excellent weather all way. Could not get report through sooner. Our wireless shut down to prevent interference with *Norge*. Navy Marine personnel splendid condition. Byrd" Comments: [a] Why should Byrd say merely "about" 9:15?! This temporary imprecision bears strikingly little resemblance to his eventual report's air of confident precision at the alleged moment of Pole-arrival. (See, e.g., TA 7-8 = G 155-156, Byrd 1928 p.197.) What the 5/12 telegram's time-datum *does* resemble is: Fig.3 (the diary). [b] How credible is Byrd's explanation for his odd three-day delay in finally providing a (still merely rough) time of arrival at the Pole? E.g., why it was more urgent to non-interfere with *Norge* radios while the dirigible was on the ground (5/10) than when it was (5/12) making a dangerous flight? And how was it that Byrd's *NYTimes* reporter at Spitzbergen, Wm. Bird, was able to get out detailed stories (much longer than Byrd's brief 5/12 message to SecNavy) on 5/9 (NYT 5/10:1:5=6) & 5/10 (5/11:1:7=8)? — stories which still gave no Pole-arrival time (rough or no), the latter one noting that Byrd and Bennett were incommunicado even 18^h after their return!

⁴⁴ If one argues that Byrd later found his chronometer was off by +13^m and so corrected his time (after 5/12), that creates a new problem: for a trip from AI to the North Pole, an error of +13^m = 3° 1/4 will cause an otherwise perfectly-steered trip to end up c.40 mi left of the Pole.

⁴⁵ This holds even if further material turns up. (Not impossible: the NGS report refers to two 1926/5/9 Byrd flight-navigation charts, but only one [photographed twice] is in the OSU Byrd material sent me. From the Byrd diary's odd history, we know it's not beyond defenders to hold material back [see ☉ I and NGS' cute 1926&1968 acts: §M4&fn 189, resp.], for release at a strategic time. And the most strategic time may turn out to be: Never. See fn 4. However, my best guess is that the other chart was suppressed because it contained a giveaway bloop, of the sort Byrd progressively deleted during his various editions of his report: fn 22. It was not sent to geographical societies: *idem.*) For, additional documents cannot erase erasures — or a host of contradictions, including extra contradictions of both parts of some of the merely-double Byrd 1926 contradictions (see §N6): i.e., we find up to three — or even four — versions of the same quantity. These are now an indelible part of the historical record. As an atheist, my favorite saying (*Nat.Hist.2:27; 77 AD*) from Pliny the Elder is: not even god can change the past. (A religious society might therefore regard the art of propaganda as a sacrilege, for it attempts to exceed the deity. This is even aside from breaking the commandment about lying.)

the sort of question one expects from a navigator who has been keeping close track of times & distances. (It also sounds like the turning-back was pretty sudden. And it doesn't feel like the words of someone who has just reached a great goal and lingered there for 13^m.) These points obviously also occurred to the eraser (fn 109).

D8 But there is a perfectly reasonable explanation for this evidence, and it relates to a matter that is a plus not a minus for Byrd. Both for aiming at the Pole and (so as not to end up like Amelia Earhart later would) for hitting Spitzbergen on the return, Byrd's prime concern was not so much distance as *steering*. There was vital cause for Byrd to keep an excruciatingly constant watch on his sun-compass and (once out of sight of Spitzbergen) faithfully to relate it to the drift-indicator (every 3^m or less: fn 47; see also Byrd 1928 p.192): if a short period of inattention permitted the plane to wander a few miles to the left or right of his intended meridian, no instrument on board could reliably correct for this. I.e., all position errors transverse to the intended path would *accumulate*.⁴⁶ That is why he persisted (as we see in the diary) in keeping after Bennett to be sure that the plane hewed to their intended direction. At one point in the diary, Byrd gets after Bennett with a triply-underlined "*must*" (D 6 = G 81) to urge steering care, because Bennett was allegedly going 5° to the right at the moment. (Below, at §O, we will realize that this stressed message was written just after his 7:07 GCT sextant shot told him that all was not well navigationally, and his first impulse was to blame Bennett's steering instead of a headwind.) Again: Byrd was fully justified in being so alertly forceful. (This partly explains why his steering appears to have been better than that of the 1925 Amundsen-Ellsworth attempt at the Pole.) Note that, though Byrd was busy reining in Bennett's purported rightward tendency,⁴⁷ there were two leftward-push factors: [i] The report notices an east component to the wind during the northward flight. (D 5 = G 80 records Byrd's concern about leftward drift from this.) [ii] If Bennett checked his course by the (erratic)⁴⁸ magnetic compass,

⁴⁶ Byrd's report suggests that sextant sights of the Sun's altitude might give him some idea of such errors. Well, they might give one a hint, but (except for Sun-shots near 6 AM or 6 PM of apparent time on the intended meridian — and, by the latter time, the flight was over), such deductions are dangerously sensitive to errors in the N-S position. (See §E4 and fn 67.) I.e., they depend upon continuous & accurate knowledge of groundspeed: awfully risky, when one's life is quite literally on-the-line — or on-the-meridian, in this case.

⁴⁷ If there really was a Bennett rightward tendency, maybe it was partly from the Coriolis pseudo-acceleration justly noted (if incorrectly computed) in Molett's 1996 ms p.3. (Coriolis' equation was discovered by Laplace before G.Coriolis was born.) If Byrd steered as he said he did (re-aiming poleward every 3^m: TA 3 = G 151), the net northward Coriolis effect would be ordmag 10 mi to the right. (Had he not persistently countered this effect, he'd have ended up ordmag 1000 mi right of the Pole.) Magnetic variation's steady leftward increase (§D8) with nothing was comparably vexing.

⁴⁸ Compass' sluggishness and swinging noted in Byrd's report at TA 4 (G 152): "it takes excellent steering to settle it down; and the course steered by it . . . may be in the general direction of the objective but it will not be an exactly straight one." (See also Byrd 1928 p.189.) Byrd seems confused about what compasses he took. NYT 5/10:2:2 says he took four and that the fourth was an earth-induction compass. But NYT 6/24:2:3 quotes him as saying on 6/23 that since the earth-induction compass had the same problems as the magnetic compass, "I did not take it on the flight to the Pole" — but the typescript which NGS was opening at the same time states (TA 1 = G 149) that on the flight: "The pilot had a periodic and an earth induction compass." In any case, NYT (5/10:1:5=6) reported that all 3 magnetic compasses aboard "deviated eccentrically after reaching high latitudes. Bennett declared that when he was piloting the magnetic compasses were wholly useless and would swing almost a quarter turn, returning very slowly. . . . Bennett and Commander Byrd alternated in the piloting, Bennett refilling the gasoline [tanks] while the Commander piloted and navigated." (Though TA 1 = G 149 claimed zero compass-deviation in 1926, note that even in non-polar latitudes, where the terrestrial field is strong, Byrd's successively-jettisoned extra-fuel gasoline cans gave the compass trouble on the 1927 flight: see G 111&112 and Byrd 1928 p.255. Such fuel cans were around in the *Jo Ford* at least until c.88°N (NYT 5/16:4:2=3); Byrd 1928 p.194: "five-gallon tins stowed all over the [*Jo Ford*] cabin." The only information Byrd gives for compass variation beyond Spitzbergen is (NYT 5/17:4) that at some indeterminate point during (apparently) the 15°E return "the compass showed variations as high

he must have contended with the already-known⁴⁹ monotonic increase of the magnetic (compass) variation during the flight. (We now know the 1926 figures more precisely than 1926 explorers did: the compass variation, along the 11°E meridian, increased from 10°W at north Spitzbergen⁵⁰ to 34°W at the Pole. And Byrd once claimed [⊙8] that he knew what the compass variations were for the trip, but no such precise new-research data appear in the official report, or the diary, from anywhere within 600 nmi of the N.Pole.) This would be a constant leftward influence if not repeatedly corrected.⁵¹ Below (§12), we will find evidence consistent with assuming that Byrd's obsessive steering-habits [⊙4] paid off by producing excellent adherence to his meridian. Considering that Byrd had hardly slept in the 24^h previous to the flight, the stamina and the achievement are both impressive. Much of the purpose for the trip was to prove that, aiming solely by sun-compass, airmen could go hundreds of miles out of sight of land and steer a safe return to base. In this limited but very important respect, the 1926 flight was a success both for the Byrd-Bennett team and for National Geographic (whose sun-compass was used).

D9 Given the lethal implications of the newfound diary for the 1926 Byrd N.Pole claim, both Byrd & NGS will now come in for just criticism in connection with it. So it is only proper that due attention be paid to what both principals did right. Likewise, though interested-party National Geographic's exams of Peary & Byrd were cursory (and though NGS mistreated Amundsen, the greatest of all polar explorers — an injustice one hopes the Society will now take steps to rectify), it is to NGS' credit that it repeatedly encouraged two exploring immortals who genuinely kept risking their lives to expand geographical horizons. That either man died in bed was remarkable good fortune. That both did was almost miraculous.

E Long-Secreted Actual 1926 Sextant Data (In-Flight Diary)

E1 We turn now to the sole 1926/5/9 sextant readings in the diary. These are two. In both cases, there has been an attempt by someone (obviously Byrd: fn 109) to erase the observed sextant altitude and the arithmetic deduction⁵² from it. (Byrd's normal habit is

as thirty degrees." Since the compass was sluggish and swung hugely, this provides no firm data, and none are in his official TA account. (Or his diary.) Incidentally, had Byrd stated unambiguously that the variation at the Pole was 30°W for the 15°E meridian, he might've helped his cause slightly, since that was about right. But instead he avoided clarity here (compare fn 50), so it is hard to distinguish between what he measured, as against: [a] random oscillation and-or [b] what he just guesstimated (rounded to the nearest 10°, unlike the AI value) from the standard magnetic chart of the day. (Which he possessed: US Hydrographic Office, Wash, DC, 1913 Feb; Byrd's own copy, attached to a 1925/6/29 letter to him from SecNavy, survives in the US National Archives, RG80:29455 — 83:26.)

⁴⁹Obvious from the USHO variation chart Byrd carried: fn 48. A giveaway Peary blank was his vagueness on compass variation: see Rawlins 1973 pp.91, 130-132, 226, 234. NGS' NavFou 1990 Report p.6 repeats a Cook-Peary slip in stating: "Peary's compass always pointed to the magnetic pole". *Idem* p.51 Figure II-2 has Peary's compass variation consistently too high by over 10°, and imagines a weirdly cramped 1% northing-gradient for compass dip.

⁵⁰Byrd's report says (TA 3 = G 151) he found variation 11°W at Amst.I. (Of course, this could have been found from the variation chart he carried. See ⊙7 & fn 48.) No imprecision; no qualifications.

⁵¹At D 339-340 = G 90-91, we find Byrd's directions for both magnetic compass & sun-compass, in careful preparation for his initial flight-plan: Kings Bay to & from Northeast Land, Greenland. (See also at *Nat Geogr* 48:527 for his commendable 1925 care on such points.) Byrd's next 1926 plan was to fly to Greenland and then to the Pole & back. When he took off early on 5/9, he intended to do the reverse: Kings Bay to Pole to NE Land to Kings Bay. But a leaking starboard motor c.9 AM abruptly changed all that. Note: if the terrifying leak had actually occurred at the Pole, then Greenland would clearly have been the nearest land. (See Byrd 1928 p.186.) But, the less the actual distance north was, the more the choice was a tossup.

⁵²Note also: whereas Bennett seems (fn 32) to have been given messages written right on the dead reckoning-math diary page (Fig.3), he was not exposed to the sextant data & computations thereof,

proper for data-recording in the field and is repeatedly evident in the diary: when he is correcting errors on data-sheets, he writes-over-strikes the mistake. He doesn't erase. See §E4 & fn 110.) But, fortunately, the observed times and altitudes are legible — as are Byrd's grid-navigation calculations upon them, to find Sumner-line angular distance from the Pole. (Byrd's 1925 practice calculations for such math are found at D 335 = G 87.) He uses a chronometer-correction of 20^s or 21^s. (This matches the 1926 May chronometer-corrections listed for chronometer Hamilton #5098, recorded at D 366 = G 94.)

E2 Since Byrd's official report says he struck north from the west coast of Amsterdam Island (Figs.1&2), which is at longitude 10°.8, I will use this meridian for all calculations below. (Byrd's report uses 11°04'E. Near latitude 80°N, the difference is 3 nmi: not critical for evaluating the data.)

E3 The 1st sextant shot was at chronometer time 4:39:40. After correction for 21^s of chronometer error, he had 4:39:19 GCT. Adding +3^m37^s for the Equation of Time, he found 4:42:56 GACT (Greenwich Apparent Civil Time, the kind of time appropriate to his navigational math). The sextant shot for the Sun's center was 16°00'00". See Fig.4, showing data recorded at D 8 (G 82). (The round value 16° suggests [despite TA 2 = G 150]: Byrd may've intelligently pre-set the sextant to this expected value and then just recorded the time when observed *h* matched predicted *h*.) He applied refraction & parallax 3'13", yielding true altitude 15°56'47".

E4 He then computed the Sun's declination by interpolation from the standard tables of the *AENA* (*American Ephemeris & Nautical Almanac* 1926 p.6). Here he initially made a small mistake, which his mental agility quickly detected. The correct declination was 17°09'. But he evidently chanced to write it a notch to the right of its proper place in the calculation, so that it temporarily looked like 17'09" instead. (If Byrd really carried an eraser — as defenders are forced to argue: §L8 — here was the chance to use it. But, even in this confusing case — see Fig.4 ! — he just typically instead wrote the correct figures right OVER the erroneous ones and kept humming.) Or, it is possible that he was confused as to the daily epoch of his tables. If this latter theory is correct, then we know the cause: starting in 1925, the epoch for such tables had switched⁵³ to midnight instead of noon. Noon had been standard with astronomers for millennia (at least since Hipparchos [see *DIO* 1.1 ¶6 eq.28], 127 BC, and the *Almajest*, c.160 AD). But in the end, he got 17°09', which was correct. From this he subtracted the observed altitude (above), 15°56'47" to get the Altitude Difference for use in his Hinks-navigation approach (fn 42, §E14). He should have gotten Alt.Diff = 1°12'13", but instead⁵⁴ wrote 1°2'13". (See Fig.4.) Thus there was a unit subtractive error in the tens place (arcmin tens in this case),⁵⁵ corresponding to a 10 nmi rightward error in the location of the Sumner line. (An error which is minor compared to the difficulty of interpreting whether a single such line indicates distance-error

both of which are on diary pages (Figs.4&5 = D 8&11 = G 82-85) which contain no messages — nor are any on the pages opposite (D 9-10 both totally blank).

⁵³It is verifiable that this recent epoch-switch confused Byrd (and alot of other navigators), as we will also see (§§P1&P3) when we examine the 1925 sights in his diary. In the case of the 1926/5/9 observation at 4:39, Byrd may have entered the *AENA* declination tables as if 4:39 AM were 16:39 hours after the tabular epoch, which would have been correct procedure before 1925. (Except that a pre-1925 navigator would have used the 5/8 tabular place. I thank Gerry Newsom for encouraging clarity on this point.) This would indeed produce 17°17', the erroneous pre-strikeover value in Fig.4. The epoch-mixup theory is recommended by Byrd's 1925 error of the same type (§P1); but the offset interpretation (which I think Newsom was first to see the merit of) better explains how "9" got so perfectly placed in the arcsec column.

⁵⁴The tens figure in the arcseconds of this result is messy-looking but readable. Evidently, Byrd at first thought he was subtracting the bottom line from the top, instead of vice-versa, so he wrote the first digit of 47 before catching himself and writing 13 instead — which is why the first digit looks like 1 superposed upon 4.

⁵⁵Byrd's weakness for tens-place slips we will twice encounter in his work on his 1925 sights: §§P2&P3.

sight	4 - 39 - 40 <u> - 21</u>	chronometer corr
Eq t	4 - 39 - 19 <u> 3 - 37</u>	
GA CT	4 - 42 - 55	
	0	
Alt Obs	16 - 00 - 00	
p&r	<u> 3 - 13</u>	
θ	15 - 46 - 47	
Dec	<u>17 19 9</u>	
	1 - 2 - 43	

Figure 4: The 1st Sextant Shot, Diary Page 8 (D 8 = G 82-83).

This 4:39 GCT sextant shot (§E3) was so nearly due east that it helped verify Byrd's excellent steering to this point — the best ever accomplished up to that time by a polar aviator.

or steering-error — or some combination. See §G2 and fnn 46&67. Comparison of TA 5 [G 153] to TD 4 regarding the alleged 3:56 GCT sextant sight, reveals the serious re-write of the original's implicit overprecision in this connection: Byrd's handwritten re-do is at TC 4. Note the NGS trio's unalertness to all this at fn 27.)

E5 Since the Sun's azimuth was only about 10° north of due east, this sight was a good check on the plane's steering. It told Byrd that the *Jo Ford* was on a (gradually-curving) Sumner line which crossed his path only about 10° west of due north and passed a little over 1° to the left of the N.Pole. This couldn't tell him much about speed, but it was consistent with his having steered well to that point. (He may have taken this sight because he had now been out of sight of land for some time and so was necessarily depending on using his drift-indicator with the sun-compass for steering.) Crudely speaking (for polar flying) if he had flown true northward, then the altitude of the Sun at azimuth c.80° should be (see eq. 2 at §E14) less than the solar declination by colatitude times cos 80°, and this is (roughly) true for the 4:39 GCT observation. However, there is some indication that the plane's northing was not going according to schedule (80 mph: §O5).

E6 Which may explain why the 4:39 GCT observation has been wholly suppressed in the official report and replaced by two others at neatly chosen flanking times, virtually 4:00 & 5:00 GACT on the nose. If the plane had gone 80 mph (Byrd's assumption at this time: §O) since 00:50 at Kings Bay, the 4:39 GCT latitude would have been 83°20'N, and the observed (apparent) altitude on 11°04'E would be higher by 8' than that actually observed (16°). (Note: Byrd's tens-place slip [§E4] virtually masked this discrepancy. See §G2.) And the pace indicated (TA 5-6 = G 153-154) by the published report's 4&5AM sextant figures puts the *Jo Ford* at about 84°11'N at 4:39 GCT. On the 10°.8 E meridian, this would produce the diary's observed solar altitude (15°56'.8) only if the plane was about 16 nmi off its intended meridian (to the west) — at longitude 8°1/4 E.

E7 The 2nd (and last) 1926/5/9 sextant observation recorded in the diary is (Fig.5 = D 11 = G 84) for 7:07:10 GCT. And Byrd's official navigation report does provide an observation at precisely this time. Indeed, this observation was worked out (our Fig.8) on the lower-right corner of the "flight-chart" provided to the NGS sub-committee that examined his claim. See below at §F for Byrd's small but fateful tampering with the recorded time-data.

E8 In any case, there is nothing small about the matter of the observed 7:07:10 GCT apparent solar altitude recorded in the diary: it **differs by more than a degree** (1°07'12" discrepancy) from the published solar altitude. (I.e., his Sumner line was 67 nmi closer to the subsolar point than the official report put it.) Official report (Fig.7[a]) apparent altitude 18°18'18" versus diary (Fig.5) apparent altitude 19°25'30" (or 45"). Note: Goerler 1998 doesn't (even at his G 57 footnote) give the page-numbers or the enormous size for this astounding discrepancy *in his own book's pages*, so we do so here (e.g., in the figures' captions).

E9 The final-version 7:07 GCT observation and calculation are in Byrd's official typed report (Fig.7[a]) = TA 6 = G 154) and are partially handwritten onto the map-"flight-chart" (see Fig.8) he submitted to NGS. This report's apparent altitude (18°18'18": Fig.7[a]) implies⁵⁶ a latitude of 87°42'N — that is, 2°18' = 138 nmi from the Pole.

E10 At this point (7:07 GCT), his typescript's dead reckoning latitude estimate (TA 6 = G 154), 87°25'N (reportedly based upon drift-indicator measures) is 17 miles less than 87°42'N; and his report seems to regard such dr estimates as the more reliable. But I agree with the NGS sub-committee report's opinion (G 143, ☉4) that they are not, so I will

⁵⁶ Although Byrd computes his Sumner-lines' distances from the Pole, neither his diary nor his report computes a latitude from these results, which were simply used for drawing the Sumner lines on a chart (based on H.O.2560). Byrd's diary & report also give no math in support of the drift-measure distances the report cites. The Sumner line computed by the NGS trio (see Fig.1) clearly has Byrd at 87°.7 N for 7:07 GCT (not Byrd's d-i value, 87°25'N: §E10), though this figure is, I believe, never given explicitly in any 1926 document or publication. The scant computed diary position-estimates are just rough airspeed-based dead reckoning (§C6).

watch	7 - 07 - 30 <u>20</u>	Dec =	17 - 10 -
GCT	7 - 07 - 10		45
Eq t	3 - 37	θ	19 - 25 - 30
GACT	7 - 10 - 47	$p\&r$	2 - 36
			<hr/>
			19 - 22 - 34
			<hr/>
			17 - 10
			<hr/>
			2 - 12 - 54

Figure 5: The 2nd Sextant Shot, Diary Page 11.

This is Byrd's diary record (D 11 = G 84-85) of the 7:07 GCT sextant observation (§E8), which shocked Byrd by showing that he was either way to the right of his intended path (as he at first hoped: §§O8-O10) or (the unlucky truth: §E11) that the plane's progress towards the Pole was being seriously slowed (down to merely 70 mph) by a wind with a strong north component (§J4). This 7:07 shot is the key one that Byrd kept secretly sculpting (while preparing to hand in his "data" to NGS judges), as the reader will quickly see by comparing the above data to the sextant and exact chronometer readings (for the same time, 7:07) at Figs.6-8. (The 7:07 original record, above, is both the best and the last sextant observation of Byrd's career: see §G6 & fn 4, resp.)

throughout use $87^{\circ}42'N$ as Byrd's claimed 7:07 GCT latitude. [Even impenetrably-loyal Byrdist J.Portney has told *DIO* Editor Keith Pickering that the barometric basis of Byrd's d-i estimates suffered variations we can only guess at.] We note that according to the chart of known atm pressure (Liljequist *Interavia* 15.5:589; 1960 p.591), the northward gradient along the *Jo Ford*'s route suggests a net pressure variation of ordmag 1% during the outward trip. However, height-uncertainty's⁵⁷ effect on d-i distance-estimate error was more important: ordmag 1/10. And this is in addition to random errors of measurement, etc. In any case, d-i distance-uncertainty was probably higher than the factor of 1/20 used as a standard-deviation by apologist Portney [in his earlier incarnations] (1973 p.217 & Fig.7 and 1992 p.258 & Fig.2).

E11 The 7:07 GCT sextant shot occurred when (for $11^{\circ}E$) the solar azimuth (117°) was nearly three times further from the prime vertical than at 4:39. So this observation gave Byrd almost three times better information regarding distance achieved. But the results of his actual 7:07 observation & calculations (Fig.5) had to be alarming, since they indicated (assuming the plane was on its $11^{\circ}E$ meridian) a groundspeed⁵⁸ of barely 60 knots — thus the *Jo Ford* was bucking a serious headwind.

E12 After I realized this, archivist Ken Grossi (then at OSU) found in the diary the following note to Bennett (d 4 = G 95): "There is a very strong wind please steer very carefully." (The official report speaks of winds but does not indicate that they were powerful.)⁵⁹ And Byrd remarks (§D8 & fn 37) a troublesome east wind from the outset, though, remarkably,⁶⁰ he claims at TA 3 (G 151) that this did not retard his northward speed. As elementary vector analysis reveals: Byrd is therefore inadvertently (vs fn 163) saying that there was a lucky small south component to the wind when he was heading north. And, better yet (as we saw at §D1), the condition suddenly reversed to a large north component when he headed south! See *NGM* 50:376, where it's little wonder that Byrd exclaims: "The elements were surely smiling that day on us". (But defender Portney goes Byrd one smile better at §S8 & Fig.11 here.)

E13 The 7:07 GCT diary calculation (Fig.5): correcting apparent altitude $19^{\circ}25'30''$ for $r\&p$ $2'36''$, Byrd found the true altitude of the solar center = $19^{\circ}22'54''$. Subtracting $17^{\circ}10'$ of solar declination (for 7:07 GCT), he learned that his Sumner line was Alt.Diff = $2^{\circ}12'54''$ or about $2^{\circ}13'$ (133 nmi) to the right of the Pole.

E14 A crude but useful linear Hinks-grid calculation could be done in minutes (examples at Rawlins 1970b p.35, *DIO* 2.2 §G11 & Table 3) as follows: adding longitude $11^{\circ}E$ to GACT (7:10:47 times 15 = $107^{\circ}.7 = c.108^{\circ}$), one has rough solar azimuth $A = 119^{\circ}$. (A

⁵⁷See ☉4 (item [iii]), which refutes one of Molett's arguments (at item [ii] there).

⁵⁸The original 1996 report mistakenly said "airspeed" here. Though it was obvious from the context that groundspeed was meant, I thank Gerry Newsom for correcting this stupid misprint. Another enlightenment: meteorologist G.Liljequist's 1960 doubts of Byrd's north-wind report are contradicted by the Byrd diary data: §§J3&J4. (Years ago, I naively took Liljequist's guesses as useful, while Portney 1973 p.214 more wisely called them overspeculative. But, ironically, Portney's own meteorology in his hilarious 1973 Figs.2&3 [our Fig.11: below at p.72] is far more speculative than Liljequist's — and is much too transparently partisan: §S8.) Byrd was correct in reporting getting a north-wind boost during the return: his misreportage was in acting as if this wind were a one-way blessing: he left out the fact that the same wind had affected his speed throughout the Poleward part of the journey, too — but adversely in that case.

⁵⁹Though the chronology of G 78 places the d 4 (G 95) note in the northward leg of the trip, I suspect that, since d 3 (Fig.3) filled up during turnaround activity, Byrd wrote the d 4 note during the return (an interpretation which might help Byrd's supporters, so I am glad to provide it), sometime after the bottom line of d 3 (Fig.3), "Head the plane right at the sun" (which correlates with TA 8 = G 156: see below at ☉11). After all, the southward hours of the flight were even tenser than the northward ones. But we are left with the same question as §O15's: why does Byrd tuck into the back of the diary his strongest remark on navigational wind-troubles?

⁶⁰The point can be forcefully made by asking: how long would it have taken Byrd to reach the Pole in his 85 mph airplane if the crosswind was 85 mph?! See math examples at fn 98.

is here measured eastward, taking north as 0° .) Approximate latitude L is found by the simple formula given in Rawlins 1972 (p.137):

$$h = \delta - (90^\circ - L) \cos A \quad \text{or} \quad L = 90^\circ + \text{AltDiff} / \cos A \quad (2)$$

(Method: TA 1-2 = G 149-150; & see D 8, 11, & 335 = G 82, 84, & 87, resp.) We have adopted the term “Alt.Diff” used at TA 5-7 = G 153-155 (where $\text{AltDiff} = h - \delta$). By the simple math of eq. 2, $L = 90^\circ + 2^\circ 13' / \cos 119^\circ = 85^\circ 26' \text{N}$. Exact⁶¹ spherical trig gives⁶² $85^\circ 16' \text{N}$. But the official report says (TA 6 = G 154) that at 7:07 GCT the airplane’s latitude was past 87°N .

E15 In the Summer of 1996, DR requested the world’s premier expert in this area, E. Myles Standish of Cal Tech’s JPL — and now also of *DIO* — to compute independently the latitudes (along $10^\circ.8$ E longitude) established by Byrd’s real and claimed 7:07 solar altitudes. Standish’s results: $85^\circ 16'$ and $87^\circ 42'$, resp — identical to DR’s figures above (§E9 & §E14) which were in DR’s 1996/5/4 report to the Byrd Center (thus appearing in the epochal *NYT* 1996/5/9 article), of which the present publication is a revised expansion.

F Proof of Fabrication Procedure in the 1926 Byrd Data

F1 When we compare Byrd’s diary to his “flight-chart”⁶³ (actually a post-flight concoction: §G7), we make the shocking discovery that *the raw time-observation for the 7:07 shot has been altered* on the latter. (Compare Fig.5 to Fig.8.) The diary computes as follows: chronometer time 7:07:30 minus 20^s correction = 7:07:10. (See Fig.5.) But (see Fig.8) the “flight-chart” Byrd gave NGS has it (as does a loose-sheet calculation, Fig.6: see §F5) chronometer time 7:07:31 minus 21^s correction⁶⁴ = 7:07:10. Though the difference is tiny (1^s), the revelation is huge — and devastating: it is black&white proof that an alleged *raw datum* (the **directly-observed** time of the 7:07 sextant shot) was fabricated indoors to equal 7:07:31 when (as one sees from the on-site diary: Fig.5) the actual outdoor time observed on the chronometer was unquestionably 7:07:30. Finally, we note in passing that: that 7:07:31 is the only purportedly-raw precise datum on the “flight-chart”: §G6. That this datum is demonstrably fabricated betrays just how genuine the “flight-chart” actually is.

F2 I.e., the *derivative* datum 7:07:10 GCT was kept fixed (obviously he’d been using it while working up numbers for his report), while the *observed* datum was fudged. Which means that Byrd was engaging in the dishonest practice which scientists call working-backwards-from-the-answer. (Hardly a novel phenomenon: see fn 74.)

F3 Xeroxes of two photographs of this “flight-chart” (Fig.8) are in the OSU Archives, **but this key document was not published in Goerler’s OSU Press book**,⁶⁵ perhaps

⁶¹ If lacking an ephemeris program, one may simply compute (with $h = 19^\circ 23'$, $\delta = 17^\circ 10' 1/3$ and $HA = 107^\circ.7 + 10^\circ.8 - 180^\circ = -61^\circ.5$): $\tan x = \cos HA / \tan \delta$, which gives $x = 57^\circ 04'$; and $\sin(L + x) = \sin h \cdot \cos x / \sin \delta$, so, $L = 142^\circ 20' - 57^\circ 04' = 85^\circ 16'$.

⁶² The reader can very easily check whether $L = 85^\circ 16'$ corresponds to $h = 19^\circ 23'$ by using eq. 1 (§D3), with $\delta = 17^\circ 10' 1/3$ and (see fn 61) $HA = -61^\circ.5$. This procedure is much simpler than fn 61 — i.e., *faking* the observation (computing h from L) is easier than *using* the observation (computing L from h). On this, see §§D3&D5. Note also that the bottom-line of Fig.6 reads *h-minus-h*, which for normal Hinks navigation means (non-navigators will have a hard time accepting it, but this is how Hinks navigation works): the difference between a real observed h and an h faked for the N.Pole: §D4. The catch in Fig.6 is that the former h is *also* faked, as we know from now having the real h from the diary: §E8. So, in the document Fig.6 depicts, Byrd is just comparing one fake to another.

⁶³ The 1926 “flight-chart” & 1927 flight-chart are not reproduced in the book. And the in-flight 1927 diary entries at D 122-128 are given in text at G 111-112, but the book does not photo-reproduce them.

⁶⁴ From Byrd’s pre-flight chronometer data (§E1), we see that the correction could be either 21^s or 20^s . In the diary, he used 21^s for the 4:39 observation (Fig.4) and 20^s for the 7:07 observation (Fig.5).

⁶⁵ This despite the fact that this document’s lethal implication for Byrd’s claim was stressed in the very Rawlins report cited by Goerler at G 57 n.19. See §Q3. Fig.8 is based on the lower-right corner of

t =	7 - 07 - 31
	21
GCT	7 - 07 - 10
Eq t	+ 3 - 37
GA CT	7 - 10 - 47
Alt	18 19 18
p&r =	2 - 46
θ =	18 - 16 - 32
Dec =	17 - 10 - 18
h-h =	1 - 6 - 14

Figure 6: Loose Sheet of Calculations in Byrd’s Hand.

This is visual proof that Byrd was fine-sculpting his final ultra-smooth story, as shown by comparing these numbers to those of Figs.5 (1926/5/9), 7 (1926/6/22), & 8 (1926/6/23?). (This document represents a very private intermediate practice step, concocted sometime — obviously between Figs.5 and 7 — during the process of arriving at his final preference for what the 7:07 solar altitude would be reported as.) There are only two empirical data here (t and “Alt”), and one or both differ from the other above-cited versions of the 7:07 record. The raw time t (supposedly read right off the chronometer) is 1^s higher than that in the diary calculation (Fig.5) for the real 7:07 observation, though the “deduced” GCT is identical! (see §F2) — an ironclad proof of backwards calculation. And the apparent solar altitude (“Alt”, allegedly read directly off the sextant) is $1'$ higher than that in the official report (Fig.7). (See fn 67.) And “Alt” is $1^\circ.1$ lower than the real observation: Fig.5. (Note: what Byrd renders here as *h-minus-h* [fn 62] is what he usually calls “Alt.Diff.”) The sheet (original at OSU Archives) is 10 cm by 15 cm, so (©9) the paper was presumably manufactured in continental Europe.

[a] FROM SECRET BYRD REPORT (to National Geographic 1926/6/22):

At 7^h-07^m-10^s G.C.T. took altitude sun's centre
18°-18'-18".

G.C.T. =	7 ^h -07 ^m -10 ^s	Sext Alt. =	18°-18'-18"
Eq t =	$\frac{(+)\ 3\ -37}{7^h-10^m-47^s}$	p & r =	$\frac{(-)\ 2\ -46}{18^\circ-15'-32''}$
G.A.C.T. =	$\frac{(+)\ 3\ -37}{7^h-10^m-47^s}$	True Alt. =	18°-15'-32"
		Dec =	$\frac{17\ -10\ -18}{1^\circ-5'-14''}$
		Alt. Diff =	1°-5' -14"

to be laid off towards sun since
computed altitude is less than
observed.

Dead reckoning position at time of sight lat. 87°-25' long
11°-04' East.

This sight indicated that the plane was about five miles
to the left and ahead of the course. Brought plane back on
course during hour ending at 8^h-22^m G.C.T.

[b] FROM BYRD REPORT (to real geographical societies 1926/11/24):

At 7h-07m-10s G.C.T., took altitude sun's centre.

G.C.T. =	7h-07m-10s	True Alt. =	18°-15'-32"
Eq t. =	$\frac{(+)\ 3\ -37}{7^h-10^m-47^s}$	Dec. =	$\frac{17^\circ-10'-18''}{1^\circ-5'-14''}$
G.A.C.T. =	$\frac{(+)\ 3\ -37}{7^h-10^m-47^s}$	Alt. Diff. =	1°- 5'-14"

to be laid off towards sun since
computed altitude is less than
observed.

This sight indicated that the plane was to the left and
ahead of the course. Brought plane back on course during hour
ending at 8h-22m G.C.T.

Figure 7: From Initial [a] & Final [b] Reports: 7:07 Sextant Data Discussion.

Above are exact representations (with typo-remnants eliminated) of the two different versions of the 7:07 data in Byrd's official typewritten report of his alleged 1926/5/9 navigation: version [a] (TA 6 item #24, 1926/6/22; G 154) was strictly private (to SecNavy 6/22, relayed to National Geographic 6/23), and it was suppressed for decades — while genuine geographical societies (those that received anything) instead were sent strictly the later, bowdlerized version [b] (TD 5 item #20, 1926/11/24; photocopy at Byrd Center Archives), from which the alleged raw "Sext Alt." datum had been excised (likewise for all the report's other sextant calculations), since the TA version expressed the raw readings of the Sun's altitude to the nearest arcsec, which Byrd had realized (during the 6/22-11/24 interim) was an impossible precision on his sextant. (See §G6 and fn 22.) Note also that (as explained at conclusion of fn 67) version [a]'s miscomputed estimate of course-error ("five miles") has vanished in version [b]. For yet another slyness here: note that the paragraphs of Byrd's typescript report had to be re-numbered to cover for the removal of entire paragraphs (e.g., ⑦), which is why TA's item 24 evolved to TD's item 20.

t =	7 - 07 - 31
	21
	7 - 07 - 10
Eq t	+ 3 - 37
GACT	7 10 - 47
	θ 18 - 15 - 32
Dec =	17 - 10 18

Figure 8: From Post-Flight "Flight-Chart": Detail Lower Right Corner.

This is a slightly stripped-down (§§F4&G6) Hinks-method calculation comparing "observed" 7:07 true (i.e., r&p-precorrected) solar altitude θ to true altitude at Pole (solar declination, "Dec"). Obtained from OSU Archives' xeroxes of two photos of the original (itself inscribed upon gov't chart USHO 2560). (One of the photos shows some of the last line, but not all — so it is omitted here.) Parallel arithmetically correct result provided in Fig.7. (The original of one of the two photos of the "Flight-Chart" was marked "Photographed" in Byrd's hand.)

because it renders Byrd's tampering as undeniable as it is sanely unalibi. (When Byrd-sympathizer Newsom of OSU was faced with this Byrd flatout data-alteration on 1998/3/9, he responded by speculating that perhaps Byrd was just too tired⁶⁶ to record data during the trip, so he wrote them down from memory the next day! This is what the alpine-sledge evidence against Byrd has inevitably reduced The Defense to. See also below at ☉16.)

F4 The raw apparent altitude ($18^{\circ}18'18''$) for the 7:07 shot is not on the "flight-chart" (Fig.8) — instead, only the true altitude ($18^{\circ}15'32''$) is given (fn 113), which suggests that this is not an in-flight record. And proof of this suspicion is found in two separate post-return alterations Byrd made in time-data (see §G7) which appear on this document. Note: the "flight-chart" isn't cited as an enclosure in either Byrd's 6/22 cover letter to SecNavy or in SecNavy's 6/23 cover letter to NGS accompanying Byrd's 6/22 TA report, so one assumes Byrd carried it with him to Washington 6/23. Thus, it was first seen (G 142) by the NGS trio between 6/23 & 6/28. This implies that it first appeared after TA. Thus, in our serial display here of the evolution of the 7:07 observation, the "flight-chart" calculation seems to belong between TA and TD (Fig.7 [a]&[b], resp). The fact that Fig.8 strikingly does not include the raw sextant altitude $18^{\circ}18'18''$ — a datum which entirely disappeared from his written output after TA — suggests that between the typing of TA (in the days or hours before 6/22) and a sweaty moment on 6/23, Byrd realized the enormity of his blunder

OSU's xeroxes of two photos of the chart, a $1''3/4$ by $2''$ piece. (Both photos truncated the calculation, but the result must be identical to TA's.)

⁶⁶ The "tired" alibi has been relayed and implicitly endorsed by Goerler (see below at ☉13). Note our earlier comments (at *DIO 1.1* ¶4 end-note [B]) on the attempts of NGS' consultants (the NavFou) to alibi Peary's failure to enter his uncorroborated (and utterly nonexistent) 1907-announced "Crocker Land" into his 1906 diary, on either of the two 1906 June dates (6/24 & 6/28) when he is supposed to have discovered it while accompanied only by Eskimos (not even Matt Henson). (See his 1907 book *Nearest the Pole* at pp.202-207, material which is virtually verbatim out of this 1906 June diary — except for the two passages on Crocker Land at pp.202 & 207, which have been interpolated whole into the text at these two pages. If real, Crocker Land would have been the only separate-new-land discovery of Peary's entire career, which is why he at *ibid* p.280 called this "discovery" the penultimate achievement of his entire 1905-1906 expedition. Rawlins 1973 pp.72-77.) The NavFou just said Peary was TOO TIRED to write down his vision on either date — even though he wasn't too tired to write huge diary entries on those dates. As for his Crocker Land, this is what Peary wrote (1906/6/24 diary entry): "No land visible". How tired would you have to be to write that passage, when you are an arctic explorer who has just discovered possibly THE MOST NORTHERN LAND ON EARTH. So, the "tired"-alibi is getting, well — tired. (Perhaps from a mix of [a] risible inadequacy and [b] numbness from repetition.) But let's not ignore our very own discovery here: that OSU's Newsom is echoing at long-distance THE most original and demented alibi ever concocted even by National Geographic's kept-consultants, the NavFou. It's a synchronicity as inspirational as that in ☉2 — and we'll see it again at ☉14. One more item in our sleep-parade: Henshaw Ward recovered a 1910 All-Fool's Day letter from Peary's magazine-serial ghostwriter (his beautiful and very close friend Elsa Barker) to Peary, on his failure to write anything in his continuous diary while he was at "the Pole" (his only "entry" was on a loose page, like Fig.6 here; see also ☉9 below): "Your eyes being so tired from the observations, there are no entries in the journal." (Details: Rawlins 1973 pp.284&230.) Though exploration discovery & record-breaking are generally believed to be exciting (which is why books on them sell), it's perplexing to find that in actuality, these famous explorers were themselves put right to sleep by the experience. Stranger yet, nobody ever found Amundsen or Scott too tired to write, after their and their navigationally-able companions' indubitable attainments of the South Pole. [Compare Byrd 1926 success-elation diary-nonentry: p.105's appended Note [A].] Is anticipation of an NGS medal soporific? All 3 snoozer claims (Peary 1906, Peary 1909, Byrd 1926) got gold medals from NGS — but not from the Amer Geogr Soc. (AGS did give medals for both men's genuine records: Peary 1900, Byrd 1929.) And the mysterious correlation between snoring and lack of corroboration is as strange as the massive inertia we must conclude is carried by navigational witnesses — the only rational explanation for why, when Peary in 1906 & 1909 shed these hefty creatures, his sledges suddenly blazed poleward at unprecedented paces. Such previously-unannounced correlations are themselves new discoveries in the lucrative, promising field of embarrassed-institution-alibiing, and we look forward to seeing them investigated and utilized still further, by the hired agents of pop-geography mythicism.

in providing over-precise raw sextant readings, a giveaway slip explored below in §G6.

F5 OSU has found (though, again, Goerler **did not publish**) a loose [☉9] sheet of paper (Fig.6) which also contains — in Byrd's hand — the same (§F1) fabricated raw time, 7:07:31, not the diary raw time, 7:07:30. Even weirder, it contains yet *another* version⁶⁷ of the 7:07 solar altitude — intermediate between diary and typescript! NB: Byrd is fudging *raw* data here (an observation, not a calculation). In this case, the claimed apparent altitude is $18^{\circ}19'18''$ (Fig.6: loose sheet) — not $19^{\circ}25'30''$ (Fig.5: diary) *or* $18^{\circ}18'18''$ (Fig.8: typescript). That is, we have here **three different** versions of the same alleged sight — what he is supposed to have read, *right off the sextant*. Again (as with the Pole-arrival time: §D6), this is an obvious case of gradual doctoring-cooking of data. (See fn 67 for a speculative explanation of this tiny alteration.)

F6 And Byrd's typescript exhibits another provocative hint of working-backwards: all six "Alt.Diff" (Altitude Difference) endings given⁶⁸ are within $1''$ of quarter-arcmin pre-

⁶⁷ Not a mere scribal error: all the page's math is consistent with $18^{\circ}19'18''$, as one sees from Fig.6. It seems almost incredible that Byrd could care about such a fine difference unless he was last-minute super-smoothing his report (see also ☉16), while trying to keep important bottom-lines (way over-)rigidly fixed. (See similarly at §F6.) In this connection, note the odd sentence at TA 6 = G 154, where Byrd attempts to discuss what is indicated by the slightly discordant fabricated 7:07 shot: "This sight indicated that the plane was about five miles [nmi] to the left and ahead of the course." (TA 4 = G 152 specifies that all "miles" in TA are nmi.) The dr position is stated at TA 6 = G 154 to be $87^{\circ}25'N$, $11^{\circ}04'E$. Now, we definitely have a screwup here (possibly originating in a familiar [fn 55] tens-place slip at an early rough stage of the fabrication), since $18^{\circ}27'$ is the apparent altitude that would be accurately calculated (St.Hilaire procedure: §D4) for the dr position, while $18^{\circ}18'.3$ is given (TA 6 = G 154) as the observed value, so that the difference was roughly $9'$. (That is: 9 nmi, not the stated 5 nmi. Note import of this: §G2.) The discrepancy is resolved if we assume that (while composing the comment on dr-vs-sextant) Byrd simply forgot r&p when checking (by eq. 1 [§D3]) the altitude for the claimed dr position. By Byrd's data (dr-position $87^{\circ}25'N$, GACT 7:10:47, & decl $17^{\circ}10'$), using Hinks' straight-Summer-line approach (eq. 2 at §E14), the expected (St.Hilaire-method) true altitude was $18^{\circ}24'.6$. His "observed" altitude, when he was just short of completing the refined touches to his report, was probably that of Fig.6: $18^{\circ}19'18''$. So the Alt.Diff was about 5 nmi — which provides a neat explanation of the origin of the above-cited TA 6 statement (Fig.7 [a]) that he was 5 nmi left of course. But, then, why did he later almost immediately change the "observed" datum from $18^{\circ}19'18''$ (Fig.6) to his final version, $18^{\circ}18'18''$ (Fig.8)? Try this: he recomputed the St.Hilaire-method altitude by rigorous sph trig (or otherwise accounted for the curvature of the 7:07 Summer line) and thus realized that the accurate (sph trig-computed) altitude was about $1'$ less than the Hinks-method altitude. Countering the effect of this last-minute refinement required only that he reduce the "observed" altitude by $1'$ — this way, no statement regarding his latitude or transverse drift need be changed *at all* in his (presumably by-then virtually pat) official report. (One other point in passing: the statement "about five miles [nmi] to the left and ahead of the course" sounds odd — as if it were originally "left of the course" but then "ahead" was inserted to satisfy the realities of fn 46. After all, "course" is navigational for direction, not distance.) In the end, Byrd must have realized his error in omitting r&p: in the report's final version, he drops "about five miles" from the sentence in question: in Fig.7, compare [a] to [b]. (Further on this: see parenthetical conclusion of ☉10.)

⁶⁸ The Alt.Diff is not provided for the sights at "the Pole": TA 8 = G 156. However, the mean true altitude reported (TA 8 = G 156 also on the "flight-chart": photocopies in OSU Archives) is $17^{\circ}12'14''$, which (like the mean apparent altitude) is within $1''$ of quarter-arcmin precision. Moreover, it is alleged in all four versions of the report, e.g. TA 8 = G 156 (also TB 7, TC 5, & TD 6) that the mean observed raw (apparent) altitude at the Pole was $17^{\circ}15'14''$, the only 1926 Byrd *h* which is the result of averaging (a reasonable procedure, since he was allegedly circling at one spot). This being the only *h* where $1''$ precision superficially seems defensible, Byrd didn't kick it out of the typescript final edition or the "flight-chart". (We know that for his 7:07 shot, he eschewed averaging his two endings, since such precision is meaningless, given the shot's actual uncertainty: see Fig.5, where he justly ignores the alternate $45''$ ending. Besides this and the Pole mean *h*, all his other sextant readings, — real or imagined — follow standard naval single-shot routine: see Figs.4&5 and the count at G 142.) He says that this 9:02 special crowning *h* is an average of four sights (which is why he rightly does not give a precise to-the-second time to it) — averaging would be possible at the Pole because he was allegedly not changing his position as during the northward leg. (See Bowditch 1984 vol.1 p.420.) The catch:

cision, which was the sextant's actual precision (though the sextant readings are given in the typescript to 1" precision! — see §G6). This coincidence strongly⁶⁹ suggests that Byrd's fabrications⁷⁰ were originally built upon 1'-precision reductions⁷¹ of 1/4-precision sextant "data" — but, when 1" precision was introduced⁷² in subsequent refinements of the arithmetic, Byrd kept the original Alt. Diff results fixed (to keep his story's vital data, the latitudes, undisturbed)⁷³ and so merely adjusted each sextant "observation" to ensure this (occasionally tossing in 1" scatter for effect: fn 38). The giveaway: starting with high-precision data and ending up with virtually rounded data. In real-life⁷⁴ science, it happens the other way about.

G Precision Anomalies

G1 TA's precision sometimes borders on the humorous. E.g., for 8:18:26 GCT, TA 6 (G 154) gives a *dead reckoning* latitude (reportedly based on d-i data) 89°01'40". For 8:38:25, TA 7 (G 155) has an even more-precise-looking dr latitude, 89°28'35". I.e., both dr latitudes (⊙10) were given to precision 5" (barely 150 meters) after about 600 nmi of northward travel! Relative precision 0.01%, for data uncertain by more than 15% (⊙4).

there is no legitimate way by which four observations of whole or half (or even the occasional [unused: §G6] quarter) arcmin precision can produce an average that ends in 14".

⁶⁹ Byrd's fabricated Alt.Diffs (TA 5-7 = G 153-155): -2°14'31", -0°28'30", 1°05'14" (Fig.7), 0°42'30", 0°23'00", 0°03'59". The probability of 6 data out of 6 accidentally hitting to within 1" of a quarter-arcmin multiple is (3/15)⁶ or 1 in 15625. And note that 5 of the 6 Alt.Diffs are within 1" of a half-arcmin multiple, raising the odds about an ordmag higher. The exception is the 7:07 shot (fn 67&160). And it seems to have been specially & persistently worked over. It is also the only TA sextant sight given, for when there really was one. (And that was the only real sight where 15" precision was even hinted at: §G6.) Thus: [a] Its agreement with the current hypothesis could be accidental. [b] One can try examining the situation if it is exempted from the sample; thus, we ask the odds of hitting by chance 5 times out of 5 within 1" of a half-arcmin multiple: 1 in 10⁵ or 100,000. (Note: mod 15", all Byrd's Alt.Diffs are either 0" or -1" — an even more unlikely coincidence, hinting at systematic late touch-ups.)

⁷⁰See, e.g., fn 67. We even find something hinting at the language of fabrication; at "the Pole", Byrd describes conversion of apparent (observed) altitude to true altitude (TA 8 = G 156, emph added): "Applying p & r of three [arc]minutes we get true altitude 17°12'14". Shouldn't this read "got"? (Similar language elsewhere in TA.)

⁷¹That is, one-arcmin precision both for Decl (as in diary) and for p&r (as in the last 3 of the typescript's 7 observations: TA 7-8 = G 155-156).

⁷²For the real in-flight calculations in the diary (Figs.4&5), both 1926/5/9 solar declinations are expressed only to arcmin precision, while the official report's declinations exhibit arcsec precision. (Actually 1/10 arcmin precision, masked as arcsec precision.) The former precision is to be expected during hurried flight conditions (§E4), the latter precision is to be expected at home. So where were the typescript's calculations most likely made? — during his arctic flight (as Byrd pretended) or later?

⁷³So strong an attachment to a set of must-keep-fixed bottom-lines does not seem consistent with a person who was fully at ease with the steps best used initially for refined fabrication of the typescript's first three sextant data. Byrd understood the Hinks version of the St.Hilaire method and so could easily have fabricated data by such, including Sumner-line-curvature, which he said he accounted for in the first two: TA 5-6. But eq. 1 (§D3) is more accurate than this. Therefore, if he couldn't cope with eq. 1 and so got a few solutions from others on the *Chantier* (see, e.g., ⊙9), he might have preferred (though see fn 67) to stick with their mathematical consequences and thus adjust differentially (& in reverse) for the above-cited refinements if he wished his "deduced" latitudes to be what his story originally wanted. Note: there is no written evidence that Byrd ever used eq. 1.

⁷⁴The very same precise→rounded inversion of reality was what gave away the working-backwards nature of the ancient mathematician-astrologer Ptolemy's pretended derivations of planetary elements (2nd century AD). See mathematician Hugh Thurston's *DIO 4.2* p.58 (1994) analysis.

⁷⁵When in 1909 Dr. F.Cook claimed a sextant-based 1908/4/21 latitude 89°59'45", navigators were incredulous. Byrd does something similar though not as clumsy at TA 7 = G 155, in putting his 8:59 GCT dr position at 89°56'N, 11°04'E. For his outdoing of Cook's ineptness, see §G6.

Thus, TA 6&7's implicitly-claimed precision was over 1000 times better than the implicitly-admitted (⊙4) precision. (See similarly at §G6.) One notes that subsequent editions (TB, TC, TC) of the Byrd report delete the 5"-precision dr claims, which remained unknown until now (outside NGS & the rest of the Byrd inner circle).

G2 Possible muddled origins of the numbers behind these long-hidden Byrd super-claims are discussed in detail in ⊙10; but the larger essential point can be succinctly stated: given that the NGS agrees (§E10; also Portney, and see here at ⊙7) that the d-i-based distance-estimates are very rough and their errors semi-cumulative (ordmag 10% error — i.e., several dozens of miles by 7:07 GCT & later), how can such phenomenally neat agreements of sextant data & dr data be taken at all seriously? (See similar giveaway features of ancient fake science at *DIO 8*†1 ⊙16.) This *especially* applies to Byrd's claims that, using such d-i-determined distances, he could simply consult his oblique sextant-shots to know whether he was *just a few miles* left or right of his meridian and thus correct his steering! (See §E4. It is astonishing that the NGS trio did not notice this claim's contradiction of the trio's own perceptive remark on d-i distances' unreliability.) This, even though his math for the major example (TA 5 = G 153) of such alleged magical *Jo Ford* navigation is so erroneous — by a factor of nearly two — that he deleted the misbegotten numerical basis (5 nmi) of the alleged course-correction from the final edition of his navigation-report. (Details of math & deletion at fn 67; related error at §E6.)

G3 Similarly, for the most steering-sensitive sextant-sight (that at 4:56:27 GCT: TA 5-6 = G 153-154), the agreement of the sextant-sight with the dead reckoning transverse-position is ordmag 1 nmi. This, after over 200 nmi of travel.

G4 The official report to NGS contains a curious sentence & slip in connection with the 8:38:25 sextant sight, which indicated a Sumner line passing 23'00" from the Pole: "Dead reckoning position [latitude] 89°28'35" [longitude] 11°04'E. This sight indicated also that the plane was still on the course and near the dead reckoning position and about thirty one and a half [miles] from the pole." In all later versions of the report, the passage has been rewritten (deleting⁷⁶ the over-exact "dr" 89°28'35"N) to read simply: "This sight indicated that the plane was about thirty one and a half miles from the pole." Problem: the 8:38:25 sextant altitude actually puts the *Jo Ford* not 31 1/2 nmi but 29 nmi from the Pole [⊙10]. Again: this latitude was dropped from every later version of the report (fn 76 & ⊙10).

G5 Note that if the last three pre-Pole sextant shots (8:18, 8:38, 8:58) are computed correctly, they demand that Byrd went 87 knots in the first 20^m span but only 74 knots in the second 20^m span, an implicit speed-discontinuity⁷⁷ he covered by simply mis-stating (§G4) the latitude mathematically given (for his meridian) by the purported 8:38 sight.⁷⁸

⁷⁶Likewise, *all other* dr estimates (of latitude attained) for times of sextant sights were dropped from the versions of the official typescript subsequent to the version (TA) which the National Geographic Society had approved (e.g., see Fig.7: compare [a] to [b]), presumably because the sextant-dr fits were too obviously over-neat. (Even Byrd-defender Molett's private 1996 ms p.2 says Byrd's TA sextant shots "seem almost too good to be true", but then says "smooth air and careful observations would give him pretty accurate readings" — which does not prevent Molett from at *ibid* p.1 ascribing the 7:07 sextant-shot "mistake" to unsmooth flying!) Compare TA 6-7 (G 154-155) to TB 5-6, TC 4-5, & TD 5 (all in OSU archives). TD was the version distributed to a few real geographical societies.

⁷⁷This discontinuity is not dreadful, since wind-blown wander from the meridian might account for it. The more important concern here is the over-neatness (& artificial arrangement) of the report.

⁷⁸All of the typescript's alleged 1926/5/9 sextant readings (TA 5-7 = G 153-155) are provided here:

At 3:56:24 GCT, $h = 14^{\circ}57'07''$.

At 4:56:27 GCT, $h = 16^{\circ}43'26''$.

At 7:07:10 GCT, $h = 18^{\circ}18'18''$.

At 8:18:26 GCT, $h = 17^{\circ}56'31''$.

At 8:38:25 GCT, $h = 17^{\circ}37'18''$.

At 8:58:55 GCT, $h = 17^{\circ}18'35''$.

(TA 8 = G 156: Mean of 4 shots at 9:02 GCT, $h = 17^{\circ}15'14''$. See fn 68.)

G6 Yet another glaring feature of the official 1926 Byrd report betrays its artificial nature: the overprecision of the purported sextant readings (fn 78). Each shot is alleged to have been read precisely to the *arcsec*, the several endings being: 07" (3:56 GCT), 26" (4:56), 18" (7:07), 31" (8:18), 18" (8:38), 35" (8:59). However, Byrd's actual private diary sextant readings are invariably read in far rounder fashion (as are those of other contemporary polar explorers); his real readings' endings: 30" on 1925/5/6 and 00" on 1925/6/21 (both from D 336 = G 88), 00" for 1926/5/9, 4:39 GCT (Fig.4), and 30" for 7:07 (Fig.5), where we see that he also here wrote (above the 30") an alternate ending of 45" (3/4 arcmin), which his math then understandably ignores. (Note: the double-version of the arcsec ending suggests that this shot was taken to the instrument's highest possible precision; it is obvious — contra §§L5&Q4's apology-swamp — that the 7:07 sextant observation was the most carefully made of any in the entire extant manuscript record of Byrd's life.) That is, all his *real* sextant readings were no more precise than quarter-arcmin precision — usually half-arcmin or whole-arcmin. (See also fn 69. Pickering points out: the d-i readings have the same precision.) In all the diary sextant readings, Byrd never computes with an observed *h* which he treats as more precise than a half-arcmin. **Fact: no vernier on a real portable 1926 sextant read to arcsec.**⁷⁹ (TA 1 = G 149: "We used the artificial horizon bubble sextant in general use in naval aviation."⁸⁰ And there was no index correction; *idem*: "I had [the sextant] adjusted so that it had no error". Though ☉5 shows that the rushed NGS sub-committee could not have checked Byrd's sextant before he got his NGS medal, the committee claimed: "We have verified all [Byrd's] computations. We have also made a satisfactory examination of the sextant and sun compass used by Commander Byrd." Text at G 142.) Not even *Doc Cook* ever made this super-giveaway blunder. (Cook's alleged sextant readings were all to simple arcmin-fractions: see, e.g., §G1 and Rawlins 1972.) Such a spectacular slip in fabrication cannot have occurred without Byrd having acquired a habit of working calculationally backwards (see §F6 & fn 67) which had become so routine that it got careless. Anyone trying to pass off Byrd's gross 67' discrepancy at 7:07 GCT (over 1°! — §E8) as unimportant must face the fact that he is implicitly contending (§S7 [i]) that Byrd's precision was *thousands of times greater than his accuracy*. (*DIO* readers have encountered such hilarities before, when dealing with obsequious officers of prestigious institutions.)⁸¹ See like outlandish ratio at §G1. Byrd was presumably warned privately

⁷⁹ Bowditch 1926 p.99 refers to the standard Navy drum-vernier sextant (Byrd's type: TA 1) as having a maximum-precision "reading of 10"; all sextants, however, are not so closely graduated." (See also *ibid* p.103 and here at fn 4.)

⁸⁰ In the typescript edition sent to geographical societies, Byrd makes only one alteration in the report's paragraph describing his sextant. He extends the first sentence so that it reads (see TC 1 or TD 1, where we put the appended words in italics): "We used the artificial horizon bubble sextant in general use in naval aviation, *except that it is larger (battleship size).*" It is obvious that by the time he turned out this version (11/24), Byrd knew that he'd blundered on precision. (Indeed, recall that TC = TD is the edition that deleted all the over-precise true observed altitudes: §G6.) But implying a large sextant (which allows slightly higher precision) is a weak attempt to dig himself out of this disastrous hole: [a] We know from the diary (§G6: Figs.4&5) that — with a 15"-precision barrel vernier — his sextant actually read a little less (not more) precisely than the 10" barrel vernier which the 1926 Bowditch noted as tops in Navy sextant precision: fn 79. [b] TA 1 says he used the same bubble sextant for 7 years, so we can gauge its size from two photos: *NGM 50:379* and Byrd 1928 opp. p.80. The size is standard. (To change the precision from 15" to 1" would require a linear size enhancement of ordmag 10, thus an enhancement in volume & weight of roughly 10 cubed or ordmag 1000. Not likely for even a battleship, much less an airplane. Unless Byrd didn't mind hoisting an instrument larger than himself. One is reminded of the portability of Ulugh Beg's famous arc)

⁸¹ See, e.g., Roy, *Astr. Soc.* sometime-veep David Hughes at *DIO 1.1* ¶8 §E4. Note also at *DIO 2.2* ¶1 §M that Hughes has — like the *J. Hist. Astron.*'s new Assoc. Ed. and fave Ptolemy-apologist (see *DIO 9.1* inside cover) — never acknowledged undeniable errors undercutting an entire paper, much less retracted the paper. How is one to respect these institutions, when they do not seem to even care (any more than do their bungling, unrefereed leashed-pet-authors) about being honest with readers?

that his raw data were way too precise: by the time he was preparing (TC) his report for the version (TD) he distributed to geographical societies, he had systematically stripped the report of every single one of his apparent (observed) sextant altitudes — merely giving (non-raw) true altitudes! (See versions TC & TD, OSU Archives. A similar TA→TD clean-up of overprecision was discussed above at §E4. See also data-lacunae noted in ☉4 & ☉7.) An example of the process (for 7:07 GCT) is provided here in Fig.7: compare version [a] (suppressed for a half century by National Geographic) to [b] (the version sent to geographical societies). We note that the only two sextant altitudes that can be found on Byrd's "flight-chart" are both true (non-raw: already corrected for r&p): 18° 15' 32" for 7:07 and 17° 12' 14" for "the Pole". (See §F1.) Which suggests that this document is more cautious (and thus presumably later) than TA. Hmmm: the only previous explorer to try putting across a set of pre-corrected true altitudes to support a Pole-claim was Doc Cook.⁸² Note that TD (distributed to a few institutions), the only version of the official report ever seen (until now) outside of the tiny NGS-Byrd family circle, *contains no raw data at all*. (All the times in TD are post-chronometer-correction, and all the TD solar altitudes are given subsequent to r&p-removal or averaging.) This struck DR the moment he saw document TD three decades ago. I suspect it affected knowledgeable officials at AGS similarly. AGS never recognized the 1926 claim. Byrd's elimination of this slipup may relate to persistent rumors⁸³ [e.g., Roberts 2001 p.165] that AGS privately got the truth out of Byrd (see ☉3), but said nothing publicly because: it might pain the US public.⁸⁴ Which can be nonloosely translated: it might lower organized geography's income.

G7 On the "in-flight" original chart submitted to his NGS examiners, Byrd handwrites the times of his departure and his arrival at the Pole as 00:37 and 9:02, resp, both GCT. The mystery here is that his in-flight linear time-calculations in the diary (Fig.3) instead show 00:40 subtracted from 9:05. (It is striking that, in TA, both these times and all his northward d-i times are 2 mod 5, suggesting that such a correction has been belatedly added to a set of figures all of which were originally rounded to 5^m precision — a theory published years ago in Rawlins 1994 fn 9, and now vindicated by the finding that all three time-data

⁸² See F.Cook *My Attainment of the Pole* 1911 p.301. But both Cook (Rawlins 1972 p.135, Rawlins 1973 p.86) and Byrd (above fn 68) made lethal blunders in these attempts. Note: National Geographic consultants Doug (& the late Tom) Davies deserve credit for stressing sextant-precision limitations while pointing out a dumb 1989 DR error (though in a hypothetical context where super-precision would have been useful): retraction at, e.g., *DIO 1.1* ¶1 §C3. Many of Byrd's defenders seem to draw satisfaction from harping on DR's sole & *publicly-withdrawn* error (even while they are themselves so inwardly insecure that they are afraid to publicly retract any of their own dozens of errors), but none has yet realized the ironic grave he is thereby (in attacking DR's overprecision-error) digging for Byrd's claim, by thus spotlighting the Byrd-killing (fn 79) issue of overprecision. And the NGS-Byrdite strategy here implies something observers should realize: while DR's 1996 report concentrated on evidence and not trashing defenders, the Defense knows it cannot win by that route, so it has consciously opted for deceitfully acting as if a flock of Experts are arrayed (against an Unreliable lone skeptic) in favor of Byrd's claim. (That NGS-OSU 1996-8 sham went permanently down the drain when the eminent SPRI honored DR's Byrd report: p.4.) But, since this tactic is needless when the evidence supports one's position, the implicit message here is plain: the evidence *doesn't* back it. (As for why DR's response here is more critical of Defenders than in 1996, see §R3 & ☉13.)

⁸³ Without judging the merit of these accounts, I note (in this provocative connection) that in the left margin of TC is a handwritten note: "send [TD] to Am. Geo. Society." But the AGS said it was unable to find this report when DR asked for it while researching his polar book (over 3 decades ago: fn 22).

⁸⁴ The pretense that coverups' purpose is just for the benefit of the lied-to public is touching. Similar AGS attitudes noted at Rawlins 1973 p.64; the kneejerk-establishment reporter there cited was the famous science writer Walter Sullivan (AGS Board), who did his best to ignore or counter criticism of Peary & Byrd: classic "science press" courage. He'd never publish evidence against either unless he could in the same article seem to gut it by citing supposed refutations. When the furor about Balchen's 1971 charges made silence impossible, forcing his eventual 1971/12/19 *NYT* story on Peary & Byrd, he said aloud while writing it (as recalled 1979/2/23 by a more honest science reporter who happened to be in the same room with Sullivan at the time): "I don't want to do this story."

in the actual record at Fig.3 are indeed expressed to 5^m precision, these being 00:40, 9:05, 9:15.) Byrd seems to have corrected both 00:40 and 9:05 by -3^m (which is 2 mod 5) after comparing the first to the ship's log upon his return (fn 102). But the chart is supposed to have been rendered *during the flight*. Add this oddity to the (§F1) alteration of the raw 7:07:30 chronometer-time (corrected to 7:07:10 GCT by subtraction 7:07:30 – 0:00:20 in diary [Fig.5] vs. 7:07:31 – 0:00:21 on “flight-chart” [Fig.8]), and we have two distinct indications that the “flight-chart” was not completed in-flight — a conclusion independently suggested by two plain items noted earlier here: [a] The differing versions (Fig.5 vs. Fig.8: diary vs. “flight-chart”, resp; see §E8) of the 7:07:10 GCT altitude & calculations. [b] The 3-stage triple-contradiction evolution (§D6) of 9:15 GCT's rôle in the story of the flight — which even contains one more (non-independent) contradiction: Byrd himself writes privately at d 3 (Fig.3) that, after 8^h35^m of flying north from Kings Bay, he's still tens of miles short of the Pole; but his official-report typescript & flight-chart say (§C11 & §G7, resp) that he reached the Pole only 8^h25^m after leaving Kings Bay.

H Sumner Straightline-Jacket

H1 We now consider the constraints upon Byrd's position (& northing) created by the real (diary) 7:07 GCT shot (§E13) — and its demand that the *Jo Ford* be on the corresponding sextant-based Sumner-line (that is, the only locus of points on Earth where the Sun's true altitude was 19°23'). There are several ways of looking at the matter, but they all come down to this: the Sumner line demands that, if the *Jo Ford* was anywhere near its claimed speed-schedule, then the airplane was far to the east of its claimed bee-line 11°E meridian.

H2 If Byrd had really traveled at 85 mph groundspeed, then (by 7:07:10 GCT) he'd be at latitude 86°39'N (83 nmi north of 85°16'N) — where, for the Sun (27° south of east) to be at the diary-recorded true altitude (19°23', Fig.5), he'd have to be (by planar approximation) roughly 83 nmi-tan 27° = c.42 nmi to the right of his intended 11°E meridian. (This offset would slightly lower the latitude; to restore it to the crudely projected 86°39'N requires further refinement: precise astronomical math will produce a fit to the 7:07 GCT Sumner line, for that latitude, at longitude 24°E; this is c.45 nmi to the right of 11°E.)

H3 And to have traveled as far (527 nmi) parallel to this meridian as his report's implicit sextant-based latitude claims (87°42'N), he'd have to be (crudely using the 67'.4 difference between true *h* reported [Fig.7] vs recorded [Fig.5]): $67.4 \text{ nmi} / \sin 117^\circ = 76 \text{ nmi}$ to the right of 11°E to yet be on his real 7:07 GCT Sumner line. This would be about $(138^2 + 76^2)^{0.5} = 158 \text{ nmi}$ from the Pole, or 87°22'N latitude (c.40°E).

H4 For the *Jo Ford* to be as far north as the official-report-typescript's *alleged* sextant shot entails (for longitude 11°E), namely 87°42'N (which is about as far north as the plane later got, incidentally), yet with the Sun at the *real* (diary) true altitude 19°23' (Fig.5) at this time, Byrd's actual longitude would have to have been about 56°E: roughly 138 nmi/(2^{0.5}) or nearly 100 nmi off his intended 11°E meridian.⁸⁵

H5 D 14 contains a note to Bennett (G 86): “Radio that we have reached the Pole and are now returning with one motor with bad oil leak. But expect to be able to make Spitzbergen”. (There is no record that a message was received at Kings Bay c.9 GCT. To the contrary, see below at Tomaselli's account.) *Pravda's* Lebedenko reports an agreement that the plane was to send a wireless signal every half hour but that this regularity stopped roughly at 5 GCT. I note that after the wireless (§O2 item [c]) sent around 4:22 GCT, Byrd tells Bennett to “pull in your wire”. Whether from omission or damage or whatever, it is possible that, from this point, transmission was weak. Milan's reporter at Kings Bay, C.Tomaselli, reported the news (*Corriere della Sera* 5/11:3:3): Byrd was at 87°N (8:30 GCT), and returning after

⁸⁵ A curious coincidence: Byrd's final-report 7:07 GCT position is almost exactly as close to the Pole as his actual 7:07 Sumner line was. Rather as if the real [diary] 7:07 altitude is being treated as a local-noon sextant shot!

attaining the Pole (11 GCT). These reports match the diary's last two wireless messages approximately; however: [a] The times are much later⁸⁶ than those for these latitudes in Byrd's report. [b] The leaking motor was, I believe, a surprise at Kings Bay, when the plane returned there earlier than expected. Trying to shed another straitjacket and deny that the *Jo Ford* returned startlingly early, Goerler resorts (G 52 n.16) to the thickest type of smoke-blowing (plus needless advertisement of OSU's archives) while clumsily exploiting what was presumably an equally clumsy W.Bird miscalculation (unless W.Bird had advance knowledge that Byrd's talk about going to Peary Land was now just a diversionary deception: Rawlins 1973 p.261): “Actually, the speed of the plane may not have been unrealistic. . . . William Bird . . . reported that Byrd's plane was ‘poleward’ and was expected back in sixteen to twenty-four hours.” See telegram . . . BP [Byrd Papers], folder 2536.” See a folder out in Ohio?! — a folder possessed by an archivist who (©13) reneges on “see” when he feels like it? Consulting the identical text at *NYT* 5/9:1:1 might be a lot simpler. It would also allow you to encounter the same W.Bird saying a little later at *NYT* 5/11:2:1=2: “Commander Byrd was not expected to arrive before 7 o'clock” — i.e., more than 18^h after takeoff. (The earlier 16^h figure is probably based on W.Bird's confusion of miles and nmi — right in the same telegram — which, when he suggests that Byrd might skip Greenland, leads him to believe⁸⁷ that the trip would then be only 1300 mi, and this at 80 mph would indeed take about 16^h. The most amazing part of Goerler's 16^h claim is that the same telegram (he cites) quotes Byrd saying, “We intend to explore by going to Peary Land via the Pole”. This sentence appears openly at *NYT* 5/9:1:2, with a map showing the path; so the whole trip as originally planned, including the proposed 100 mi Pole-circling noted here at §K4, would be over 1600 nmi, which in Goerler's 16^h would require a mean speed exceeding 100 knots. That's almost 120 mph — which no sane pilot would ask an 85 mph plane (§C3) to try for even a single hour over the Arctic Ocean, much less for sixteen. Now, if you're wondering whether Goerler can ever beat this hilarious G 52 speed-claim, you won't have to be patient for long. Just 4 pages later, Goerler shows Byrd going even faster! Details here at ©18.

I How Far North Did the Airplane Get?

I1 If we used Byrd's estimate of the Kings Bay takeoff time (00:40 GCT), the speed determined by the 7:07 GCT sextant sight would be less⁸⁸ than 60 knots. But I adopt the

⁸⁶ Perhaps the times given Tomaselli were by the ship's clock, but he added an hour thinking they had been GCT; if so, then the two radios' times were really 7:40 GCT & 10:10 GCT. The former time is roughly consistent with the theory proposed at §O8; the latter does not seem unreasonable for the turnaround time induced at §C14-§C16. Especially since Byrd mightn't want to commit himself to a Pole-claim without a pause to consider options. And W.Bird says at *NYT* 5/11:2:1=2 that after the last sure radio at 4 AM GCT, the only suspected signal was reported as a maybe by a *Norge* radio operator about 10 AM GCT.

⁸⁷ W.Bird also states: “no [radio] signals had been identified after 4 o'clock in the morning, and anxiety was prevalent that something had happened.” Why is Byrd's reporter denying the existence of radios which the diary indicates were sent and which other reporters perhaps received? To keep story-options open, did Byrd censor *NYT* reporter W.Bird in Spitzbergen? — as we know he later censored *NYT* reporter Russ Owen at Little America.

⁸⁸ Byrd's northward speed beyond Amsterdam Island was indeed less than 60 knots if his 45^m is correct from Kings Bay at 00:37 GCT to Amsterdam Island at 1:22 GCT (both times at TA 3 = G 151). (The latter datum was not neutrally witnessed — and the time is cited neither in the diary at D 6 = G 81 nor in any newspaper story I've seen before his NGS medal on 6/23. I.e., it seems to have been unknown until weeks after the flight. Same for almost all other TA times: ©8.) The 53 nmi between Kings Bay & AI in 45^m, requires a mean speed of 71 knots or 81 mph (about consistent with fn 136 — but see ©7). This might have been possible if the northeast wind (fn 37 & §E11) was somewhat blocked by Spitzbergen's mass. If Byrd really did (or thought he did: fn 134) 80 mph on this leg, then his idea of the mapped KB-AI distance can provide an alternate explanation for his assumption that

00:50 time of his *NYT* reporter (W.Bird). Thus, the 7:07 shot was taken 377^m into the flight, and 85°16'N is 381 nmi north of Kings Bay (78°55'N, 11°.9 E):⁸⁹ just over a nmi/minute, or 61 knots (a rounding of 60.6 knots, yielded by exact calculation) which is 70 mph.

I2 Note that applying similar arithmetic to the other (4:39 GCT) sextant shot finds virtually the same speed: 82°45'N (the shot's indicated latitude) is 230 nmi north of Kings Bay; so, for 4:39 (229^m after 00:50 takeoff), we have speed 230 nmi/229^m = 60 knots or 69 mph. The remarkable consistency of the speeds (61 knots and 60 knots) indicated by the only two diary sextant shots is: [a] an encouragement to accepting a linear (constant due-north velocity-vector) hypothesis, and [b] a credit to Byrd's steering and the accuracy of his off-denigrated (§S5) sextant shots.

I3 To test the constant-velocity hypothesis, we compute the solar altitudes for 4:39:19 & 7:07:10 GCT, for a 70 mph trip (on the real AI meridian: 10°.8 E) starting at 00:50 GCT. This speed will take the plane to latitudes 82°47'N (4:39:19) & 85°17'N (7:07:10). The corresponding true solar altitudes predicted by this simple theory: 15°57' & 19°23', respectively — both well within 1' of the real true solar altitudes actually found (for these times) in the diary, Figs.4&5, resp.

I4 This establishes that the data are consistent with [a] excellent straightline steering along 11°E (though see ☉7) and accurate sextant shots by Byrd (both better than I had previously thought likely), and [b] 70 mph groundspeed (note independent confirmatory evidence of this speed at ☉7). From this linear theory (whose very simplicity recommends it, by Occam's principle), we may then easily extrapolate, to get the best estimate we can for the *Jo Ford*'s most probable actual latitude at its turnaround, using 9:30 GCT as the latest possible time (§C15) for this moment: multiplying 61 knots times the 8^h40^m interval (between the 00:50 GCT Kings Bay takeoff and 9:30) yields 529 nmi or 608 statute miles; adding this to Kings Bay's latitude, we have:⁹⁰ 78°55'N + 529 nmi/(60 nmi/degree) = 87°44'N. or about 87°3/4 N. At 61 knots, that's over 2 hours short of the Pole.

I5 Since Balchen is rightly to be criticized for exaggerating Bennett's confession, we should also recognize that in 1958 he computed from the *Jo Ford*'s top possible practical speed (correctly estimated by Balchen at 74 knots: §C3) that it got to 88°1/4 N. (See R.Montague 1971 p.298.) For a guessimate (made without access to Byrd's diary, keep in mind), this is admirably close to our best figure for Byrd's highest northerly latitude (87°3/4 N) — and it departs from our result largely just because it does not include [a] the Verlegen Hook touch (see below at §J6) and [b] wind. Note: a steady parallel wind will always slow⁹¹ a round trip's mean speed. (A crosswind will also do so — but only about half as much, in undrastic cases. See fn 98.)

I6 An advocate wishing to push Byrd's turnaround-point a little farther north of AI can, of course, argue that if, at 7:07 GCT, the *Jo Ford* was c.20 nmi to the east (right) of his chosen 11°E meridian (a deviation caused, say, by chronometer-slowness of around a

the plane was going 80 mph.

⁸⁹Byrd placed Kings Bay at 11°3/4 E longitude (D 339 = G 90).

⁹⁰Note that using Odd Arnesen's 00:55 GCT takeoff time has little effect on this result, largely because, in the above math, the deduced 61 knot-speed is a high rounding for 60.6 knots. As for using Byrd's 00:40 GCT estimate: this would reduce the deduced speed to 59 knots, thus making the 9:30 GCT latitude 87°36'N.

⁹¹Which may explain a curious initial contradiction (noted by K.Pickering): *Norge* meteorologist F.Malmgren reports (Amundsen & Ellsworth *First Crossing of the Polar Sea* NYC 1927 p.266) that Byrd (right after his 1926/5/9 flight) had said the winds he & Bennett had encountered were slight. (But this was before he'd committed himself to a 9 AM turnaround, which necessitated claiming wind-assistance for the 2nd half of the trip: fn 43.) A steady wind will always lengthen a round trip's total time. (Math at fn 98.) A neat trick-question shows why: if a car goes a 120 mi distance north at 40 mph, and returns south at 60 mph, what is its average speed? Answer: 240 mi/5^h = 48 mph, not 50 mph. If this is perplexing, then change the speeds to 100 mph & 0 mph (a 50 mph airplane flying parallel to a 50 mph wind), and consider how long the round trip will take!

quarter-hour),⁹² then the plane's latitude could be about 20 nmi/tan 27° or nearly 40 nmi north of 85°16'N — at roughly 86°N — and still be on the Sumner line corresponding to the diary's 7:07 sextant observation. (Similar geometry at §H2.) Such an implicit pace would get the *Jo Ford* to about latitude 88°2/3 N by 9:30 GCT (merely 88°1/6 by 9:02 GCT). However:

[1] The hypothetical mis-aim, carried all the way to the turnback point, would be roughly 30 nmi to the right — thus the suggested alibi would destroy the very N.Pole claim it was trying to salvage.

[2] Aiming errors can be of either sign (i.e., if the 7:07 sight was instead taken 20 nmi to the west [left] of 11°E, then the plane's latitude was merely 84°1/2 N). In the present case, saving Byrd's claim would require opposite mis-aim signs at 4:39 and 7:07 — that is, for a theory [a] to move the plane at a speed higher than 70 mph (61 knots) and [b] to satisfy both diary sextant data, it must zigzaggily demand *leftward* steering error at 4:39 GCT and then major *rightward* steering error at 7:07 GCT. (See as jolly a joke at Fig.11.)

I7 Comments upon such convenient (anti-Occam) proposals: [i] They are unlikely *a priori*. [ii] They contradict the Byrd report claim (§E4) that he was to the left of his meridian at 7:07 GCT. [iii] The lowness of the 4:39 GCT solar altitude (16°) contradicts any theory that Byrd was drifting steadily to the right. [iv] To get Byrd anywhere near the N.Pole in this fashion would require assumptions of steering errors far larger than the (within-1°) sun-compass&drift-indicator steering-precision which Byrd claimed (TA 1&3 = G 149&151) — and which would have been necessary for him to hit his goal, and (to a lesser degree) to hit mid-Spitzbergen on the return. (Moreover, we must not lose sight of the dominant evidential fact here: *the diary's sextant data disagree with the official report's*; thus, sinuous sympathetic advocacy may indeed dispute our straightforward estimate here of Byrd's northernmost latitude, but his 1926 claim to the North Pole cannot survive the current revelation of data-alteration in the record.) In sum: to elevate Byrd's latitude beyond a trifle, one must lethally slander his steering — and thus risk leaving the story of the flight as an inchoate shambles. As noted at §B8, Byrd's whole principle of navigation depends upon determination of two intersecting lines: the 11°E meridian and his Sumner lines; it is fruitless to attempt solving difficulties with the latter by destroying⁹³ the former.

J How Far North Could Byrd & Bennett See?

J1 Our most likely position for Byrd's turnback point (87°3/4N) is 135 nmi from the Pole. His barograph's pressure, 26.1 inches of Hg, indicates (fn 202) a height of 4100 ft (though see ☉7); from this vantage point, I find that the flyers could see (over the curve of the Earth's surface) about 75 nmi⁹⁴ to the distant ice horizon, the dip of which was more than a degree. The conventional formula⁹⁵ produces (again for 4100 ft): distance = 74 nmi, in close agreement.

J2 And we can add about 6 nmi to the distance the flyers could see, by treating ice-pressure-ridges as small mountains (of heights ordmag 10 m). Thus, without assuming

⁹²Possible, though the effect is not at all evident in the 4:39 GCT solar altitude, which would be the more sensitive (to steering) of the two diary altitudes. (See also discussion of time at fn 44.)

⁹³See above (§H4) for the wildly rightward meridian (56°E) Byrd must be on at 7:07 GCT, if he is to satisfy the diary's 7:07 solar altitude and still be at the latitude (87°42'N) implicit in his reported 7:07 altitude.

⁹⁴The 75 nmi distance (including a trivial correction for polar flattening) is not particularly sensitive to alterations of temperature or pressure, because: at 4100 ft, one could see 68 nmi even on an airless Earth; so the atmosphere's enhancement adds merely c.10% to the total distance seen.

⁹⁵Without polar flattening (or correction for non-standard *P&T*). Astronomers who specialize in these matters will know that the horizon's distance in naut mi (arcmin) may be swiftly computed by multiplying dip (in arcmin) by 6/5. This ratio is based on the fact that the atmosphere gives horizontal light-rays a curvature that is close to 1/6 of the Earth's curvature.

bizarre atmospheric refraction, the maximum see-distance to be added to the northernmost *Jo Ford* latitude would — including the 75 nmi (§J1) to a flat horizon — be 75 nmi + 6 nmi = 81 nmi = about $1^\circ 1/3$. (This is a visibility-circle of almost double the area Byrd was implicitly claiming — for reasons set out in ☉7.) So Richard Byrd & Floyd Bennett very likely saw past 89°N — the first humans ever to see into the 90^{th} degree of northerly latitude. Had there been mountainous land at the Pole (of height ordmag 1 km), they should have spotted it. (They were searching keenly for such: Byrd 1928 p.196. But see also ☉7.) So Byrd & Bennett can be said to be the first explorers who determined by direct observation that no such land existed there.

J3 And they may have seen a little further than even these generous estimates.⁹⁶ However, [a] the 70 mph (61 knots) constant-average-groundspeed theory (§I3) closely fits the two sextant data taken along Byrd's intended northward path (at 4:39 & 7:07) for a flight starting from $78^\circ 55'\text{N}$ at 00:50 GCT, and [b] a north wind is reported by Byrd himself to have assisted his rapid return trip — thus (since the diary sextant data are evidence for the existence of a north wind on the outward trip, too), we may reasonably (§J4) assume a north wind averaging about 15 mph (13 knots) for the whole trip.

J4 This finding will now help us analyse the return trip. If the plane's 85 mph northward airspeed was wind-slowed to 70 mph groundspeed (§I4), then the southward component of the wind was (assuming merely moderate crosswind component)⁹⁷ roughly 85 mph – 70 mph = 15 mph. Thus, the return trip groundspeed may be estimated as 85 mph + 15 mph = 100 mph. (I see that Byrd makes it very similar at *NYT* 5/17:4:1=2&3=4, where he figures the wind at 20 mph, with a southward component ranging between 10 mph & 20 mph; he makes the net speed to be 105 mph, which is 85 mph plus 20 mph.) From 9:30 GCT (turnaround: §C15) to 16:20 (W.Bird's report of the time of return to Kings Bay) is $6^{\text{h}}50^{\text{m}}$. At 100 mph, the plane would only require a little over 6.1 hours to cover the 610 statute mi directly from $87^\circ 3/4\text{N}$ to Kings Bay at $78^\circ 55'\text{N}$. Which leaves about $3/4$ of an hour unaccounted for.

J5 There are several possible explanations for this discrepancy, e.g.: [a] variable winds, [b] steady crosswinds,⁹⁸ [c] fuel-conservation piloting (☉12), [d] temporary confusion regarding position, [e] the nondirectness (§J6) of the return. In order to hit Spitzbergen along 15°E , Byrd evidently intended to fly along or nearly parallel⁹⁹ to that meridian.¹⁰⁰

⁹⁶Our underlying linear extrapolation (§I4), which is the best we can do with such sparse raw data, depends upon the ideal assumption of constant speed; so one can be a little optimistically flexible and suppose that the flyers saw even closer to the Pole. (But I won't fudge numbers to get them there. That's been done.) However, if we put Byrd off his meridian in order to evade the implications of the 7:07 GCT sextant shot, then (as explained elsewhere here: §H) one must reject Byrd's steering, which is the main achievement and glory of the actual trip.

⁹⁷See fn 98.

⁹⁸The transverse component of the wind slowed the plane during both outward and homeward legs. (See §I5.) E.g., a constant 20 mph crosswind would add more than 30^{m} to the flight of an 85 mph-airspeed plane over a 1530 mi roundtrip. (For a no-wind situation, the flight would take $18^{\text{h}}00^{\text{m}}$.) The effect would be even greater if (as is obviously the case here) there were also a serious (rather steady) wind parallel to the intended path. We here provide examples. For a 20 mph entirely-parallel wind, the added time would be over an hour: the total time $T = 765 \text{ mi}/(85 \text{ mph} - 20 \text{ mph}) + 765 \text{ mi}/(85 \text{ mph} + 20 \text{ mph}) = 19^{\text{h}}03^{\text{m}}$. For a 20 mph entirely-transverse wind, $T = 1530 \text{ mi}/(85 \text{ mph}^2 - 20 \text{ mph}^2)^{0.5} = 18^{\text{h}}31^{\text{m}}$. For a wind with parallel and transverse components each equalling 20 mph, $T = 19^{\text{h}}47^{\text{m}}$ (nearly $3/4$ of an hour greater than T for the purely parallel-wind case). Note the illustrative extreme-examples provided at fnn 91&60.

⁹⁹For mathematical details, see D.Rawlins 1970a p.36. Byrd's story has him near $87^\circ.4\text{N}$ for his 11:00 GACT Splendid Check (☉11); if he were then actually on, say, 25°E instead of (as he believed) 15°E , he would (by flying at the 15°E apparent-noon Sun) travel parallel to the $14^\circ.8\text{E}$ meridian, a trivial convergence. (The intended and actual flight-paths would not meet until arriving at the sub-solar point, in north Africa, at 20°N , 15°E .)

¹⁰⁰Byrd's 1926 report says he went north to the Pole along 11°E and returned south along 15°E . But his report unaccountably fails to mention (at the turnback moment: TA 8 = G 156) re-setting

(His sun-compass alone could not tell which [☉11].) Note: our realization that the Pole was not his starting point for the return trip means that he was not actually travelling along 15°E , but merely about parallel to it, or to the meridian of a more easterly, more safely mid-Spitzbergen longitude if he chose it (Byrd denies he did: *NYT* 5/17:1:3=4), perhaps $c.20^\circ\text{E}$.

J6 Byrd's now-forgotten initial tale (*NYT* 5/11:1:7=8, illustrated with map,¹⁰¹ reproduced here as our Fig.2) says he reached land near the middle of West Spitzbergen's north coast, at Verlegen Hook ($80^\circ 04'\text{N}$, $16^\circ.3\text{E}$) and then went nearly west to Amsterdam Island ($78^\circ 45'$, $10^\circ.8\text{E}$) before there turning roughly south to Kings Bay. This makes sense, because the flyers' prime concern — after hundreds of miles of horrid tension as they expected at any moment to find themselves limping along on two motors — would be to get over firm land as quickly as feasible, and mid-Spitzbergen reached further north than AI (see Figs.1&2). Fortunately consistent with this priority is another: avoid coming down on ocean ice — an especially scary prospect north of Amsterdam Island for reasons well known to Byrd: see fn 16, where we see he knew that being on the ice just north of mid-Spitzbergen was much safer. The Byrd *NYT* correspondent's description of the Verlegen Hook leg (radioed 5/9 20:30 GCT) appeared in the first sentence of the first page of the *NYT*'s top headline story of the day (5/11), yet it has since been Memory-Holed (a striking witness to political power's ability to suppress history). I will quote this and the following two sentences exactly & without excision (but with emph added): “The successful trip of Commander Byrd was made on a bee-line from Amsterdam Island to the Pole, thence a bee-line to Verlegen Hook, New Friezeland, thence *west* to Amsterdam Island and home. He did not follow the identical course on the return because he wanted to be sure to hit Spitzbergen, but the navigation was perfect. He saw not a single sign of life after entering the icepack, which begins immediately north of Amsterdam Island and apparently touches Verlegen, reaching much further southward than usual.” Note especially the close-up Verlegen detail (not an idle tourist's-remark but the observation of one who knew that if he came down on the ice, this was the path to life-saving land: fn 16) — hardly likely to be observed had Byrd taken the later official report's homeward diagonal-route to AI [☉12], which never brings Byrd closer than 40 mi of Verlegen. (See Fig.1's alleged diagonal leg, which swerves off the previous due-south path: starting from $80^\circ.9\text{N}$, 15°E , the diagonal strut aims straight towards AI.) Note also that his long mid-May *NYT* 4-part story substitutes nearby Grey Hook ($14^\circ 1/2\text{E}$ longitude) — hitherto totally unmentioned — for what his previous story had called Verlegen Hook ($16^\circ.3\text{E}$), a spot never again spoken of by Byrd. But the new&improved May version still lacks the yet-to-be-added diagonal-leg time-saver. (The Fig.1 diagonal embellishment debuted in June: TA 8 = G 156, where the original 5/11 ice-touching-land feature has now been permanently Disremembered. Also at Byrd 1928 p.200.) And this edition states (*NYT* 5/17:4:3=4, emph added): “When we reached the edge of the ice pack I noted that *westward* it was possible to walk to land from the polar ice pack. I judged the strip of ice that led to land to be four or five miles wide.” This would be impossible by the Fig.1 diagonal path. (There is no land at all just west of AI, where Byrd said the pack began: see above here, or at D 6 = G 81.) If he has confused east & west, then he is just repeating W.Bird's 5/11 Verlegen report (quoted above in this note), but with such precise numerical detail that close-up observation is undeniable. (So, by *either* interpretation, he did not go straight to AI — as is shown in the NGS official map's return leg: Fig.1.) The extra distance involved in the Verlegen Hook-dogleg (as against a

both of his sun-compasses by $4^\circ = 16^{\text{m}}$ to account for his 4° shift of meridian-path. His first full newspaper account says (*NYT* 5/17:1:3=4) that the sun-compasses were changed several times during the trip due to cold — but he makes no mention of the crucial 16^{m} correction. He claims (*idem*) that the sun-compasses were set on 15°E time, but leaves it unclear when this setting occurred. (Note the odd coincidence at ☉7.) This hints at the possibility that Byrd actually intended returning on the outward path but drifted somewhat to the east, net.

¹⁰¹*NYT* 1926/5/11:2:3=4; re-issued at p.781 of *Aviation* 20.21:780; 1926/5/24.

direct return from the Pole, an ideal case which even Byrd did not claim occurred) would add 42 nmi to the trip; and that can explain most of the 3/4 hour gap (§J4) to be accounted for [⊙12].

J7 In concluding this section, I must re-emphasize that (§J2) at the farthest outward point of their 1926 flight, Byrd & Bennett probably saw farther north than anyone in history previously had, including Peary (1909) and Amundsen-Ellsworth (1925).

K Temporal Bookends

Having already found (§§C15-C16) the time of the *Jo Ford's* Farthest North, we now try to determine the times of departure and return.

K1 Let us start with a stark bottom line (§K3): all newsmen (including Byrd's own *NYT* man) reported that the *Jo Ford* took off ordmag 10^m later than Byrd's expedition said — and reported that it returned 10^m earlier than Byrd's expedition said. (His ship's log backed his times: fn 102. No other record does.) The takeoff-time (00:40 GCT), assumed by Byrd during his in-flight dead reckoning calculations (Fig.3), disagrees by at least 10^m with observers at Kings Bay.¹⁰² Byrd's own *NYT* correspondent, W.Bird, has it 00:50 GCT (*NYT* 5/9:1:1 "Byrd rose at midnight from a short nap . . . and within 50 minutes had disappeared over the sun-gilded mountains"; also 5/10:1:6-7 says Byrd "started at 12:50 o'clock this morning, Greenwich Civil Time"), and the Oslo reporter Odd Arnesen makes it 00:55 GCT (1:55 CET, *Aftenposten* 5/10:4:6). See Rawlins 1994 fn 7.

K2 If the ship's clock was in error, then this might infect Byrd's sun-compass. (A sun-compass uses a 24^h clock.) Sun-compass slowness-error would, as he northed, throw him an increasingly great distance to the right of his intended poleward path; this effect would, at 7:07, permit him to be a bit further north (than §E14 has him) and still be on his 7:07 GCT Sumner line. (See §I6.) However: [i] No such rightward steering error is reflected in the much-more-steering-sensitive 4:39 GCT sextant shot. [ii] Byrd's chronometer records (D 366 = G 94) indicate no serious problems. These record three chronometers' rates almost daily from 1926 April 9 (only 4^d out of NYC, where he presumably checked time solidly) through 5/8, though it is unclear what he was checking them against. (See fn 102. He had 4 chronometers: TA 1 = G 149.)

K3 Note: the log of Byrd's ship has the *Jo Ford's* total flight-time as 15^h52^m (fn 102, using the log's time of return, not landing), very close to the 15^h51^m figure that appeared in

¹⁰² But the log of Byrd's ship, *Chantier*, claims takeoff was at 1:27:40 (the most precise record available, whether or not the most accurate), and that the plane was seen (not first heard: see below) returning at 17:20, landing at 17:30; the mean of the last two figures is 17:25. Byrd's typescript says takeoff was 00:37 (TA 3 = G 151); return, at 16:34 (TA 8 = G 156). I suspect that both these times were simply the *Chantier* log's times minus the 51^m difference between 1926/5/9 Kings Bay Apparent Time and GCT. That is: 1:28 – 51^m = 00:37 and 17:25 – 51^m = 16:34. (See §K3. This ignores the 1^m change in the Equation of Time, between 4/29 & 5/9.) In any case, *Chantier* Cap't Michael Brennan's adjusted figures were transmitted to Byrd on 6/10: G 52 n.16. The last re-settings of the ship's clock noted in the *Chantier* log were 1926 April 28&29, when the clock was set ahead 5^m (28th) then back 5^m (29th). (Byrd set his eventual flight-chronometer ahead by 2^m between April 28 & 29: D 336 = G 94.) Since the ship was steering from Trondheim, Norway, northward virtually along the 10°E meridian (Local Apparent Time 43^m ahead of GCT on 4/29), the net clock-resetting change for arrival at Kings Bay (11° .9 E) should have been an advance of roughly 8^m. This suggests that the 5^m clock-reset on 4/29 could have been in the wrong direction. (Perhaps a sign error. Or possibly just a random error of imprecision due to the sailors' unfamiliarity with such gradual noon solar culmination as occurs in very high latitudes.) Adding this 8^m to the 1^m change of the Eq.Time between 4/29 and 5/9, we have a possible explanation of most of the c.10^m difference between Byrd's takeoff time and that of the reporters Bird & Arnesen, who were in regular contact with their respective nations, which kept zone time, from which GCT can easily be found simply by adding or subtracting whole hours: New York Daylight Saving Time (NYC) was 4^h behind GCT; Central European Time was 1^h ahead of GCT.

the 5/10 *NYT* headline. But all reporters at Kings Bay (from *NYT*, Oslo, Milan, & Moscow) recorded the total as 15^h1/2. (Arnesen radioed Oslo at 17:25 CET = 16:25 GCT that the *Jo Ford* was visible over Cross Fjord, just to the NW of Kings Bay. See Rawlins 1994 fn 10.) And the same total was announced by Pres. Coolidge at the NGS medal ceremony (*NGM* 50:377): "fifteen hours and thirty minutes". Byrd's own intensely loyal Kings Bay assistant Pete Demas (initially confusing ship's time and GCT, and thus thinking he was getting an extra hour for Byrd) says the *Jo Ford's* motor was first heard at 17:10, 10^m after the Norwegian ship *Heimdahl* rang two bells. (Demas letter of 1972/10/23 to Balchen, copy to DR: p.4. Later changed to 15^m on p.10 of 1975 ms in the Demas papers at the US National Archives, but unaltered in the diagram on p.4 of the same ms!) This is 17:10 Kings Bay Apparent Time (on 11° .9 E, when Equation of Time = 4^m) or 16:19 GCT — which is in almost perfect accord with reporters' estimates (§C14) and with the *Jo Ford's* barograph (§K5). So it seems that several pseudo-discrepancies (see fn 102 and, e.g., G 52 n.16) regarding the moment of return, are just due to confusion over what was being recorded: 1st hearing, 1st sighting, 1st flyover, or — after several circlings — final set-down.

K4 Speaking merely differentially: the barograph record of the flight shows that the plane first rose sharply at a time 15^h.6 before it rapidly descended back into Kings Bay. But meteorologist Haines (G 158) notes a slowness of the barograph machine's clock (15^m/day), which he calls "erratic". (Molett 1998 p.61 rejects the idea of cold-caused *Jo Ford* chronometer-error, evidently unaware that Byrd's own meteorologist testified to such. And see *DIO* 2.2 fn 35. Also from G 49 — which betrays obvious innocence of Byrd's 1926 method of finding longitude — one would not be aware that Byrd carried instruments not called timepieces but which nonetheless depended upon internal chronometers: the barograph and both suncompasses. Byrd had to re-set the latter occasionally: fn 100.) This would (maybe) add 10^m, making a total of about 15^h3/4. However, this 10^m is rather cancelled by the triple circling-time of the returning airplane over Kings Bay (not to mention some heading-time at start). Arnesen reports (*Aftenposten*, 1926/5/10 story, transmitted to DR by Erland Østgaard) that the plane landed 10^m after it was first seen. (And Demas says it was heard c.10^m before seen.) The plane was first spotted at an enormous height over Kings Bay, and took a quarter-hour to come down (§K5). Dovetailing all this information, we may estimate that the total time during which the Fokker was actually proceeding to or from its turnback point was (contra log: fn 102) indeed about 15^h.5.¹⁰³

K5 When Kings Bay first saw the plane, Arnesen (quoted at Montague 1971 p.12) put its altitude at over 1 1/2 kilometers, which corresponds well with the brief height-peak on the flight-barograph (G 158), ending 1^h4 before the descent for landing. W.Bird (*NYT* 5/10 p.1) reported that the *Jo Ford* circled thrice before setting down. C.Tomaselli (Milan's *Corriere della Sera* 5/11:3:3): at 16:25 GCT, the Fokker was seen widely circling "at a very great height". The barograph (⊙5) shows a height-peak at about 16:20-25. Fortunately, Byrd's own account tells us that this peak was at the true time of return, when he swooped to a great height (*NYT* 5/17:4:3=4&5=6, emph added): "I climbed the plane *gradually* as we neared Kings Bay, until we were nearly a mile high. . . . We circled over, and as we came

¹⁰³ One might subtract 0^h.2 for the claimed 13^m circling the Pole; but, given that an engine was leaking — 765 frozen, corrugated, effectively-lifeless miles from base — this part of the story is too incredible to be taken seriously. It was probably added in order to anticipate attacks on steering-precision by taking in enough of the surrounding region that a little *en route* mis-aim wouldn't matter. Yet another now-long-forgotten lethal item in this sensitive connection was buried on *NYT* 5/9:20:1, towards the end of W.Bird's page-one detailed pre-flight interview of Byrd, appearing while the *Jo Ford* was flying poleward: "At the Pole, Commander Byrd would have to use up at least 100 miles in making a wide circle to insure that his plane had flown around the Pole. His instruments cannot locate the Pole with exactitude. There is a margin of error of about twenty miles, and the flier will have to make a big loop to be able to show that he circumnavigated the Pole." Unless the *Jo Ford's* speed between 9:02 and 9:15 jumped up to 100 mi/13^m = 462 mph, Byrd failed to prove his case — by his own prescribed criterion of success. But note also that such high standards reflect the earnestness of Byrd's original genuine intent to succeed.

down for the landing we could see the people rushing from the village The landing field had people running all over it, so we had to wave them away and ascend for another try. This time space was cleared for us. Bennett made the landing, and it was a perfect one.” (Byrd’s height is now in good agreement with the barograph, which indicates¹⁰⁴ height c.4700 ft.) But Byrd does not explain why he wasted time with a high swoop over Kings Bay — *the maximum climb-rate and maximum height during the entire flight*. Note that Byrd says above that he climbed “gradually”. But the burst upward beginning very suddenly at 16:05 GCT (when Kings Bay is just a few minutes away) takes the plane up 1000 ft in about 5^m! — the steepest climb of the whole day, even including the takeoff-climb; and ascent is then continued until the plane’s height has almost doubled (from its half-mile height at 16:05) to a peak of nearly a mile by c.16:25. Recall that he (ultimately: ☉7) claims he was only about half as high when at “the Pole”! — which was by far the most important place for a discoverer-explorer to achieve great height: ☉7. (Was he more anxious to discover new land around Spitzbergen than beyond the North Pole?!) Nor does he adequately account for the long wide-circling-delay in attempting a landing. An alternate explanation is obvious, especially in light of the flyers’ all-day seclusion after the flight: fn 43. Isn’t it common sense to make a little extra time for rehearsal, before premiering act-one of the thespian performance of a lifetime? (Note Peary’s like 1909/9 hesitations: Rawlins 1973 pp.152-153, 163, 284.) A.Lebedenko (*Pravda*), in his 1930 book, *To the Pole by Air* recreated¹⁰⁵ the scene: “Above our heads in a bright sunny sky, the black bird was flying in wide circles. . . . after several minutes the airplane landed safely All that was alive in Spitzbergen crowded around the plane. . . .”

L How an OSU-Promoted Expert Works Backwards to an Answer

L1 Since the 1996/5/4 DR scientific report, the only published attempt to overturn it has been a strange effort by looong-retired military flyer Wm. Molett who claims extensive polar navigation experience, which he believes makes him such an authority on navigational math that he has the ability to stomp anyone who intrudes onto his turf. Acootly frustrated at finding not the slightest error in DR’s math, he rages anyway that its use of Byrd’s takeoff time is “utter nonsense” (see ironies at, e.g., ☉6 & ☉18!) and that DR’s analyses use “highfalutin language — all smoke and no fire” (*Mercator’s World* 1998 Mar-Apr). *Merky World*-judiciousness (see ☉18) in 1997 apparently invented a cartoon “Cartocontroversy” section — where printing trite, sputtering, and baseless insults wouldn’t seem quite so out of place. (Unfortunately, *Merky World* ran Molett’s article after the Byrd claim was no longer a real controversy — except in a purely psychological sense.) This department was then in 1998 quickly uninvented! — before a Pickering equal-space response could go to press. Having gone to so much trouble to cater to National Geographic’s religious attachment to Byrd, *Merky World* is now about to learn that it has called Byrd-defenders Newsom & Portney *and Byrd himself* purveyors of Utter Nonsense — since all three men have done just the same thing as DR: computed Byrd’s distance from his takeoff time. (Full bizarre details at ☉6.) Molett’s attacks on DR’s skills (in line with NGS’ own ☉19 tactic, intended to divert attention from the ghastly evidential status of NGS’ Byrd N.Pole myth) are being promoted by Goerler, obviously encouraged by OSU (☉18), the National Geographic Society (☉19), and (some of) the Byrd family, all of whom had been treated with pacifist courtesy, sympathy, appreciation, and kindness in the original 1996/5/4 version of the present report: copy in OSU Archives (as also copies of Molett 1996 and Newsom 1997 reports). The main thrust of the 1996 version of DR’s report was the establishment of Byrd’s heroism for genuinely trying to reach the Pole, as well as his skill in keeping to his

¹⁰⁴Via Haines’ formula: ☉7.

¹⁰⁵Photocopies sent by B.Krutsikh, Deputy Director, Arctic & Antarctic Inst, St.Petersburg. Translation by Linda Olsen.

meridian. But since Byrd-adulators are trying to dispute and evade the secondary part of the DR report — the proof that he faked the northernmost leg of the trip — that has become the main subject of the present version of DR’s analyses. Numerous remnants of DR’s original gentle edition survive here, e.g., §§A7-A10 and §§B2-B8, as well as additional recognitions of opponents’ contributions. DR’s conciliatory effort was obviously naive. (And it has been a 100.00% flop in the teaching-by-good-example dep’t.) One simply cannot expect calm rationality to prevail in a community that routinely kowtows to rich & unprincipled forces, and which thinks that preserving the reputation of US science is best accomplished by: [a] slandering critics, [b] adding fear-ridden institutional coverup to already-sufficiently-disgraceful hoaxes, & [c] running a 9-decade sociological experiment (with utter and purely tactical cynicism) to prove that it’s easy to find Experts who can be induced to support absolutely ANYthing (☉5) — no matter that the competency of their productions mark them as the Special Children of the science-history community. Note that Molett’s attacks on DR were previously received here with jolly tolerance, and we encouraged admiration for his intermittent valid observations. (See *DIO* 2.2 fnn 46 & 98, also here at fn 47.) But, when Molett’s least creditable fantasies are prominently flogged by an academic institution, and when his prime argument (and his main utility to Byrd’s defenders) is not logical but instead simply trust-me-I’m-THE-expert — well, I hope readers will understand the thoroughness of *DIO*’s exposures here. (DR’s original 1996 scientific report simply provided analyses; neither there nor here have I asked anyone to accept my judgements just because I am a specialist in some of the relevant technical areas: ☉13. I.e., slandering DR is irrelevant. Not that I have the slightest expectation that this factual detail will stop a continuing sewer of largely sub-rosa vitriol.)

L2 During *Merky World*’s editing of Molett’s 1996 paper, the following genuflection found its way into his analysis at Molett 1998 p.59: “the National Geographic Society . . . examiners spent five days [§M1] meticulously combing through Byrd’s navigation records and confirmed that he did, in fact, reach the Pole.” It’s always fun to watch a politically savvy comer-magazine suck up to a much-richer china-paper companion. And the ultra-cleverness of exploiting zany Molett *publicly* to puff up NGS will be especially appreciated there after we here expose what Molett *privately* thinks of National Geographic expertise: in his 1994/1/15 letter to DR [see fn 116], when asking DR to compute sph trig work for him, he comments: “I need [these] sun altitudes . . . in order to make monkeys out of [National Geographic’s] Navigation Foundation.”

L3 Molett’s initial attack on DR was in a 1996 manuscript which is the premier item¹⁰⁶ cited by OSU’s Goerler at G 57 n.19, justifying the relegation of all the DR 1996/5/4 report’s diary-discoveries to a two-sentence (ludicrously incomplete) summary in a footnote. (The Molett and Newsom reports are also briefly summarized. But neither contains a single discovery. Entering a case with one’s mind already rigidly made up, is no way to make discoveries. Which is why no one in the Cook-Byrd camp ever does so — with the sometimes delightful and rule-proving exception of Ted Heckathorn: see, e.g., *DIO* 2.2.)

L4 This Molett paper, like Molett 1998, portrays DR’s analysis as requiring the impossible by the odd expedient of comparing it to points in Byrd’s already-suspect typescript (which Molett repeatedly confuses with the diary! — hilarious, undeniable & undeniable details in Pickering’s letter at *Mercator’s World* 1998 Sept-Oct pp.8-10), in particular the alleged hourly drift-indicator (d-i) figures. Catch? Simple: the raw data (d-i timings — see, e.g., §E10) of this hourly d-i dead reckoning have never been found.¹⁰⁷ I.e., in a textbook

¹⁰⁶The other “expert” there cited (G.Newsom) has neither published nor replied to correspondence. In any case, his attitude is kindly-optimistic agnosticism rather than abusive certainty.

¹⁰⁷To the contrary: the diary has completely different dead reckoning data (Fig.3), rough, unreliable (mere airspeed), last-minute — and superfluous if Byrd had actually taken the hourly d-i data Molett trusts as real even in the absence of the supporting data. Note that, happily, there is ONE published Byrd dr speed-estimate where the original diary record *does* survive. It is from the 1927 flight — and it shows that Byrd’s public report exaggerated the airspeed: fn 199. So Byrd’s tendency to stretch speeds

example of circularity, Molett is simply assuming the Byrd story's official (*unverified*) d-i details and then showing that the key diary (*verifiable*) sextant sight (Fig.5) doesn't agree — when this disagreement *is* the very scandal which he is trying to bury. (Irony: see fn 107 for the reliability of the only diary-verifiable dr speed-datum Byrd published.) Will criminal corporations' lawyers henceforth go into court and ask that we not trust internal documents proving they lied — because the documents disagree with the lies? (The heart of Molett's attack is disbelief that Byrd could have gone until 7:07 without knowing his groundspeed via d-i data. While getting worked up over this point, Molett not only ignores the absence of such data from Byrd's diary but also forgets NGS's own denigration of d-i speed-estimates' accuracy: ☉6.)

L5 G 57 n.19 cites Molett & OSU astronomer [☉13] Gerald Newsom as if they were experts on this case. (In their private reports, Molett spells “Coriolis” as “Coriolus” 75% of the time, and Newsom spells “sextant” as “sextent” 23 times in succession — and gives the wrong sign to all 21 of his solar hour-angles.) Both attack DR's 1996 report on the long-secret 1926 May diary's erased figures by drawing upon the smoothed post-flight 1926 June Byrd typescript. Question: do Goerler, Molett, & Newsom prefer to believe [i] tobacco execs' longtime public protestations that nicotine isn't addictive? or [ii] their private communications showing otherwise? Should we pass off the latter as innocent “mistakes” *because* they were suppressed or erased? (See §N6.) Alternate analogy: a fraud case in court, where the chief evidence of fraud is newly-recovered, long-secreted private records — but the defense lawyer asks that the private accounting records be disbelieved and the official report be accepted, when the whole issue is (§L4) the former's disagreement with the latter.

L6 Question in passing: how is Molett's&Newsom's alleged scientific expertise relevant here? The “defense” isn't a DR navigational-math error but merely: we choose not to believe the diary sextant figures!

L7 Another question: if Byrd was allegedly doing unreliable navigational work, then: how can one accept his navigation-based claim?! [See ☉13.] Implicit Molett-Newsom theory: Byrd realized his sextant observations were “mistakes” only AFTER his 7:07:10 sextant-shot computation (correct math, visible right on Fig.5) of this sunshot had put him WAY farther south than he'd expected to be (from his rough dead reckoning). Then, carrying the Defense theory to its ill-logic conclusion, Byrd suddenly & permanently stopped writing his data & math on the diary page displaying the shot's time & solar declination (both still unerased to this day), and — even while continuing to enter all his other extant 1926/5/9 writings into the now-selectively-shunned diary — henceforth **ignored the enormous amount of blank space still left on page 11** (Fig.5), not to mention many other entirely blank pages in this weighty diary (see, e.g., §O15), and, from this point on, switched over to writing his data upon unbound, unpaginated pieces of paper (refreshingly novel idea in an intermittently windy airplane), the originals of which are now largely¹⁰⁸ if not entirely missing, the data allegedly later-copied into a separate neatly typed report (TA).

is not just a theory of skeptics. (Note similarity to Peary in 1906: *DIO I.1* †4 §B3.) Irony: since, for the cover of its book on the 1925-1927 diary, OSU didn't want to display bungled 1925 sextant data or altered 1926 sextant data (the embarrassing state of both were discoveries by OSU-loathed DR), OSU turned to numbers it felt it could trust: data from the genuine-achievement 1927 trip. Which is how OSU Press ended up putting *trans-Atlantic*-flight data (from D 16 = G 115) on the cover of a *North Pole*-title book. And if that isn't weird enough for you, check out the result: this hardback Byrd-apology-effort ends up putting smack on its dustjacket's front cover the original datum (89 mph airspeed) of *the* most unquestionably (fn 199) later-altered numerical entry of Byrd's entire career.

¹⁰⁸ See Molett 1998 p.60 (emph added): Byrd “did his celestial [math] on [his] charts, in his diary, or anywhere else he found a place to write. He later combined all of this *scattered* detail into a single report.” Molett (*Mercator's World* 1998 Sept-Oct p.10, emph added): “One has to go to the official report [TA] to find celestial figures that Byrd *copied from* his scribbblings and charts.” But the only non-diary 1926 “scribbblings” — e.g., the chart (Fig.8) and loose sheet (Fig.6) — that have turned up hardly help Byrd's case! See §F.

L8 The Molett paper's most specific argument-alibi is that the 7:07:10 observation in the diary is merely an erased mis-observation. Of course, this narrow ad-hoc hypothesis fails to explain the other erased observation — or the erasure of the revealing Fig.3 words (§D7): “How long were we gone before we turned around?”¹⁰⁹ And, if Byrd erased the 7:07:10 solar altitude as an error (atypical: Byrd did not erase errors; he wrote-over them),¹¹⁰ then: why did he not enter there the correct data? Incredibly, Molett states¹¹¹ in shameless [☉14] *Mercator's World* that he did so! — a flatout invention,¹¹² as bold as Byrd's. (Alert *Merky World* was clinging to this whopper even after *DIO* exposed it; Pickering's 1998/6/10 attempt to enlighten *MW* on its blunder brought this 6/11 reply from *MW* Assoc Ed Cheri Brooks: “As far as the unerased sextant reading goes, were there not two readings on the same page in the journal, one erased and one unerased?”) The cause of Molett's fantasy is obvious: for Byrd's defenders to be right in discounting the erased datum, the “corrected” reading *would indeed have to be on the page* (see §L11) — so Molett's mind just: made-it-happen! (An oddity, which may be a partly a fluke: no one has yet found 18° 18' 18" — or any of the typescript's other alleged raw sextant altitudes — in Byrd's handwriting.)¹¹³ Note that there is no place in the Byrd diary where he strikes out material without entering the corrected data on the same page (see, e.g., Figs.3-5) — just as you or I would behave. (See fn 115.)

L9 It is self-evident that such thorough erasing (as we find at Figs.3-5) takes alot longer than just making a big “X” through these data. So, obviously, the erasures didn't occur during the frantic hours of the flight. But would one just leave bad calculations there while working on “right” ones? No: the (hypothetical) bad stuff would have been crossed out as soon as found faulty, were the “mistakes” innocent ones. (One other comment: if erasure is held to indicate “mistake”, then what about 9:15 GCT on Fig.3? — it is not erased, and this particular “mistake” proves that Byrd lied about his Pole-arrival time: §C11. I.e., as is always the case with anti-Occam *ad hoc* alibiing, it covers only the piece of evidence it was aimed at, but leaves other data shivering naked in the cold gaze of reason, thus requiring that further and disjointed patches be added to the larger alibi-zanyquilt which such cover-stories inevitably become. See *DIO* 2.3 †8 §C20, 4.3 †15 §F5.)

L10 A further point: all the handwritten times for the diary's 2 sextant readings — chronometer time, mean time, and apparent time — are unerased on the two otherwise-thoroughly-erased sextant-data pages (Figs.4&5). (Also the solar Declination atop Fig.5 is unerased for the 7:07 observation.) Rather as if someone had contemplated eventually entering a “corrected” version there, until realizing that the erasures were not quite invisible. Question: **What other explanation is reasonable?** Indeed, Byrd did ultimately “correct” the 7:07:10 shot — by a huge amount — but thought it safer to try this, twice (inconsistently! — see §F5) on fresh, separate sheets of paper.

¹⁰⁹ G 56: “No one knows who erased the question.” Hmmm. Is Goerler implying that two *different* invisible polar-geists are at work here? (similarly at ☉1) — one deputed to erase embarrassing sextant data (we know it was Byrd: G 57 n.19), the other to erase embarrassing questions (we don't know if it was Byrd: G 56)! (Similarly at G 3: “No one will ever know who did the erasures”! — yet G 57 n.19 turns right around and promotes as serious scholarship the *ad hoc* Molett-Newsom alibi that erasing the diary sextant data shows that Byrd regarded these as “mistakes”. To borrow the refined term of OSU-puffed Molett (§L1): can't these monkeys even get their alibis *straight*? See ☉3.) Well, the diary was in Byrd's exclusive possession for decades, and the erased material was upsetting primarily to Byrd. So it hardly takes a detective to figure out who wielded the eraser.

¹¹⁰ See, e.g., D 6 (G 81; see §O3), Fig.4 (D 8 = G 82; see §E4), Fig.5 (D 11 = G 84), D 19, D 328, D 336 (G 88), D 338 (G 90), Fig.3 (d 3 = G 96; see fn 32).

¹¹¹ Molett 1998 pp.60 & 63.

¹¹² As fictional as the press-conference and Univ Md professorship Molett 1998 p.59 claims for DR. Never held either in my life.

¹¹³ The two documents discussed at §F come close to 18° 18' 18", but one (Fig.8) has only the claimed true altitude (with r&p already removed: §F4) 18° 15' 32" (same true altitude at TA 6 = G 154), and the other (Fig.6) has 18° 19' 18" (§F5).

L11 Let us now take a deep breath of nitrous oxide and adopt the “Mistake” theory, in order to see in detail the jollies it leads into.

[A] Byrd had to do a remarkable amount of labor in just 1 timesecond: (1) Make sextant observation of solar altitude h . (2) Write it down. (3) Write down chronometer time. (4) Correct latter (fn 64) to get GCT. (5) Correct for equation of time to find GACT. (6) Find in his nautical tables¹¹⁴ the r&p corresponding to the observed h . (7) Find in his tables the solar declination δ for his GCT. (8) Subtract δ from h to estimate Hinks-line’s distance from Pole (the quantity the typescript calls “Alt. Diff”). (9) Discover his “mistake”. (10) Make the “real” sextant observation. *All this in ONE second of time.*¹¹⁵ (“Genius” is too pale a compliment.) For, remember: the “real” solar altitude in the official report (18° 18’ 18”) is given for the EXACT SAME TIME, 7:07:10 GCT. (Do **not** miss fn 115.)

[B] Byrd then set aside several precious minutes to accomplish something worthless (which only proves how clever he had been to save alot of time by compacting really important activity into 1^h): though he had an expanse of white page staring him in the face (Fig.5, a huge blank area with or without erasures), beneath the needed 7:07 time calculations (two) and solar declination *already set up atop this page* (Fig.5), he bailed out of page D 11 and *took the time to needlessly re-copy these already-written data onto different paper* — evidently upon those Molett-alleged (since-evaporated) loose slips of scribble-sheets (fn 108) — looking back-and-forth between the two documents as he made the needless transposition and re-checked to make sure there was no copying error, all the while risking freezing his bare hand as he effected the move. (He “constantly” rotated 3 sets of thick mittens: Byrd 1928 p.192 and photo opp. p.183.)

[C] Another defense-unconsidered point: at 7:07, Byrd likely already knew (ere shooting the Sun) that he should find h to be very near the integral value 19°, if he was on-track at 80 mph. (See §O6; also §E3.) So, if the 19° 25’ 30” diary shot (Fig.5) was a “mistake”, he’d immediately — before even starting steps (2)-(7) in above scenario [A] — have sensed the incompatibility and (as the apologists insist) re-shot to obtain the “good” observation, 18° 18’ 18” (you know, the one he tucked away somewhere, so we still can’t locate it), *and so he would not have computed the 19° 25’ 30” sight out fully*. But he did: Fig.5.

L12 Taking leave of planet Molett-Newsom’s pleasant N₂O atmosphere, we now return to the solid ground of Earth for a realistic look at some hitherto-unappreciated fast shuffles that reside in apologia for Byrd. To start with, there’s an error in the very title of Molett’s 1996 attack on DR: “Analysis of Admiral Byrd’s Erased Sextant Reading”. Singular. Molett is unaware that there are in the diary *two* such erased readings (4:39 GCT & 7:07 GCT) — and no others from 1926. Notably, Molett’s paper disputes none of DR’s math. (E.g., it does not dispute¹¹⁶ that the 7:07:10 GCT solar altitude 19° 25’ 30” puts Byrd

¹¹⁴The standard tables of the Sun’s position turned out by the US Naval Obs & the RGO. (Today, the senior author of these tables is E. M. Standish of Cal Tech & DIO.)

¹¹⁵ At TA 2 (G 150), Byrd said it took over 100 secs to compute&draw a Hinks line. Commenting via 1999/12/16 e-mail on Portney’s 12/15 e-mail fantasies, Keith Pickering stacks up the anomalies here (which JP’s reply had 0 solutions for) illustrating the dead-end which kidding apologia have to end up at (note dead-pan version at ☉13 item [b]): “why does not one of the sextant readings in the official report appear in the diary? Did Byrd keep 2 diaries, one with the true readings and one reserved for the erroneous ones? But if that is the case, how would Byrd know in advance in which diary to record a particular reading? And if Byrd knew in advance that a particular reading was erroneous, why record it at all? But if Byrd did know (before writing it down) that a reading was erroneous, how long did it take him to determine that? More than one second, certainly.” (Contra the defense-argument of a quick&erased “mistake”: note in Fig.5 that Byrd took the care and time to catch both his math slips there [one erased much later, one never erased]; and, as always, he just rapidly scrawled over both, instead of wasting navigating time with the very thorough erasures that one now finds in the journal.)

¹¹⁶ Molett evidently derives psychic re-assurance that he’s right, by repeatedly denigrating DR’s abilities. E.g., Molett 1996 ms p.3: “Rawlins seems to believe confusing the issue makes his analysis scientific. For example he constantly uses nautical miles then converts them to statute miles or vice versa. Even kilometers are mentioned. This practice makes for difficult reading, not evidence of

at 85° 16’ N, not at the “official” 1926 sight’s much higher indicated latitude: 87° 42’ N.) Neither Molett paper computes a single instance of the standard spherical trig calculations which are the basis of Byrd-era celestial navigation. Indeed, the 1996 Molett paper’s sole navigational calculation (p.4) regarding Byrd’s geographical position is simple subtraction: 85° 16’ N (Byrd’s diary-sextant-shot-established 7:07 GCT latitude: §E14) minus 79° 48’ N (Byrd’s typescript figure for Amst. I’s latitude: fn 134) = 306 nmi — an elementary-school calculation which is bungled by 22 nmi. Correct difference = 328 nmi. This error has an entertaining effect upon his subsequent superior remarks regarding what DR’s analysis requires of Byrd’s speed: dividing the false figure 306 nmi by 5^h45^m (7:07 GCT minus 1:22 GCT), Molett announces his result: 53 knots. (The correct answer: 328 nmi/5h.75 = 57 knots. But DR nowhere says this is Byrd’s speed, either, since it depends upon Byrd’s crossing Amsterdam I. at 1:22, a datum which is not in the diary.) Molett’s fallacious 1996 arithmetic soon found its way into print; see p.11 of the abstract of this unfortunate paper printed (entirely unchecked) by Byrd-worshipper Brian Shoemaker’s loyal *Polar Times* 2.8 (1996 Fall-Winter pp.10-11), where its (*later-altered*) 5^h3/4 basis is again explicit: “nearly six hours . . . only 53 knots”. Same 53 knot figure appears at p.62 of Molett’s eventual article in *Mercator’s World* (1998 March-April pp.58-63), but — having evidently since been apprised¹¹⁷ of his arithmetic error, he (without explanation) arbitrarily shortens [☉15] the interval by 1^h (computing speed using 2:22 as starting-time instead of 1:22)¹¹⁸ in order to transform 53 knots into a “correct” answer! I.e., even while Molett scoffs [☉16] at DR’s proof that Byrd was working backwards to an answer, Molett is slyly doing it himself.

M Superswift Flying & Certifying

M1 Further on the theme of the ethical effect of backwardly defending scientific folly: the National Geographic Society gave Byrd its gold medal (from the hand of Pres. Coolidge) on 1926/6/23 — five days *before* NGS completed its variously panic-speed-bungled [☉17] five-day “investigation” of his data on 1926/6/28. The Society was so embarrassed at these reverse-time dates that, when it published the report, it *cut out* both of the two places where the latter date appeared (a censorship first exposed in Rawlins 1973 p.268,¹¹⁹ along

scientific knowledge. Byrd said [fn 67 here] all distance or ground speed figures he used were nautical.” (In fact, the diary distances and speeds are entirely in statute miles. See fn 139. Comparing diary to TA obviously requires some conversions. As for metric units: when Arnesen reports a height at §K5, it is in the metric system because that is Norway’s standard.) *Merky World* eliminated this passage (see similar cleanup at ☉18) which might have red-flagged hypothetical competent *Merky W* referees onto Molett’s mental agility. (But *Merky W* itself either didn’t learn from the flag or was blinded by other priorities. But: it least *it* dropped a flag. . . .) It’s odd that Molett would pretend not to respect DR’s expertise. Molett wrote DR 1994/1/15, apologizing for an erroneous 1993/10/23 public criticism of a DR navigational statement — *and o-by-the-way requesting DR to do 31 sph trig celestial calculations for him*. (More of this memorable letter: above at §L1.) His thankyou letter 1994/2/21.

¹¹⁷ DR told Goerler of Molett’s error on 1997/2/20. Public thanks for this assistance are naturally out of the question when the NGS-Byrd cult’s desperate defense is centered on attempting (☉19) to portray DR as unreliable — as if: DR is the problem, and when the Forces of Right finally neutralize him, all will be well. A fantasy that’s as flattering as it is demented.

¹¹⁸ *OLD* basis (Molett 1996 ms p.4): 306 nmi from 1:22 to 7:07 (5h3/4) equals 53 knots (error noted here at §L12). However, 306 should’ve been 328 (since it was [§L12] 79° 48’ N to 85° 16’ N); thus, the obscurantist-attraction to — *NEW* basis (Molett 1998 p.62): 251 nmi (81° 05’ N to 85° 16’ N) from 2:22 to 7:07 (4h3/4) does indeed equal 53 knots. So there.

¹¹⁹ Curiously, Goerler’s book (which at G 50-52 skips mention of the NGS report’s 1926/6/28-23 reverse-time paradox — of which I happen to know Goerler is *fully* aware, since he mentioned it to me and Ted Heckathorn at OSU on 1996/4/12) also clips out one of these two mentions of the 1926/6/28 date. The letter photographically reproduced at G 142 has been cropped, eliminating the top, which reads: “Washington, D.C., June 28, 1926”. (The OSU archives possesses the full page [a xerox of the entire OSU photocopy of TA is in DR’s files], so this is definitely OSU’s cropping.

from its base, though it seems this was more due to MacMillan's caution than Byrd's: G 36-37) was co-funded by National Geographic. (See Rawlins 1973 p.259. The major Byrd report on it appeared in 1925/11 at *NGM* 48:518.) Conflicts-of-interest rarely get this cozy, clumsy, and insensitive (see below at §T14): [a] A National Geographic-sponsored fake attainer, fronting for a previous National Geographic-sponsored fake attainer. (Byrd was such a captive of NGS that he says virtually the only thing he did during the flight other than navigate and pilot was at the turnaround: "I went back into the cabin, stood at attention and saluted for Admiral Peary" [*NYT* 5/16:4:4=5]¹²¹ and his "gallant, indomitable spirit" [⊙17].) [b] Judgement here rendered by a committee where a 2/3 majority was comprised of (fn 147) an NGS employee and a secretly-paid approver of the prior NGS N.Pole fraud (1909). (I think that Barbara Rawlins' seemingly sardonic remark is in fact quite literally correct: if the NGS committee had opened Byrd's records on 1926/6/23 and found nothing but a note saying: "I never got beyond 88°N, ha ha" — the note could have been deepsixed and Byrd simply told that for the sake of Peary, NGS, and US science, it would be best to save the laugh for the next millennium. Which is almost what happened anyway.) [c] Grosvenor at the bemedalling: "We have assembled to welcome home and felicitate a member of our society, whose arctic explorations began under its auspices"¹²².

M3 In 1926 May, before reaching the US, Byrd was in London but turned down an invitation to speak to a Royal Geographical Society dinner. Rawlins 1973 p.265: "it is hard to imagine a man whose veracity is in question, but who has genuinely succeeded and has proof of his success, passing up an opportunity to put his claim beyond doubt [by having RGS be its arbiter]. Why should he instead pick ["at his request": *NYT* 6/24:1:8] an organization whose recently demonstrated attitude towards skepticism was to kick a practitioner of it [Amundsen] out of its lecture series?"¹²³ Instead of arranging quick medals and massive parades, NGS should have instantly backed off when Byrd refused RGS examination and put off all celebration until long after Byrd's return — or it should have asked that he mail or radio¹²⁴ his data well ahead¹²⁵ of arrival in the US. Indeed, it was QUITE NATURALLY assumed that he would have done so; a 1926/6/17 radio to Byrd from Ass't Sec'y of the Navy T.Douglas Robinson (NARA RG 80 29455-95:25, copy to SecNavy) says: "Understand you mailed certain papers from London¹²⁶ to [SecNavy] for transmittal to [NGS]. None so far received. If forwarded as stated, from where, when and via what vessel were they mailed[?] Were they registered[?] Expedite." It is the failure of such simple caution that reveals what NGS' prime purpose was, right along, namely: taking no chances on the possibility that lengthy, careful expert checking might upset its Byrd-to-the-Pole plan to save its own face in connection with its equally overhasty approval of Peary's by-now-frighteningly-vulnerable 1909 N.Pole claim.

¹²¹See also *NYT* 6/23:24:3 & 6/26:3:3. And below at ⊙17.

¹²²*NYT* 6/24:1:8.

¹²³For details of the ejection and persecution of Amundsen (*who genuinely did what Cook, Peary, & Byrd merely faked*), see Rawlins 1973 p.252. Note also that England's variously flawed but ultimately heroic S.Pole martyr Rob't Scott *died* to do honestly (1912) what these three US explorers only pretended (in the Arctic) to do, namely, attain a geographical pole of the Earth. Detailed vindication of Scott's oft-maligned navigation at *DIO* 2.2. (Scott and Byrd have both been described as martinets, & each went into exploring partly because he had no future in the ships sector of his respective navy.)

¹²⁴Byrd was in regular contact with Washington by wireless and the Navy specifically requested (NARA RG 80 29455 95:20; 1926/6/17) that he report daily his position and his estimated date of arrival.

¹²⁵Also, if Pathé movies of the flight got to the US ahead of Byrd (as suggested by the report at *NYT* 6/23:1:1), then the TA report could have, too. A 1926/6/21 wireless from the Ass't Sec'y of the Navy to Byrd (NARA RG 80 29455 95:28, copy to Chief Naval Operations) said that his train ("Congressional Limited") would arrive in Washington at 19:45 EST and "on arrival Washington you will proceed direct auditorium to see movie operator."

¹²⁶Byrd had started a 5^d stay (NARA RG 80 29455-95:8; Byrd to SecNavy 1926/5/24) in England on Thursday, 1926/5/27 (⊙8).

M4 But, rather than carefully scrutinizing Byrd's records, Grosvenor One chose to match Byrd's ability to bluff the world. (And NGS was still huffing & bluffing decades later. Even after 40 years, National Geographic kept hiding Byrd 1926 documents it was known to possess: fn 189). GG 1 stood up in front of the President and stated, regarding Byrd's TA report (*NYT* 6/24:1:8): "These records, in the opinion of the committee, substantiate in every particular the claims of our member that he attained the north apex of the globe by airplane on May 9, 1926, the first to reach the North Pole by aerial navigation." Later (as noted at Rawlins 1973 p.267), he used virtually *the same words* to describe what the NGS trio had actually required *nearly the following week* to check out (*National Geographic Mag* 50:377, 1926/9). The official NGS report repeats (nearly verbatim) the redundant boast and (to impress all with the meticulousness of the examination — which in fact was riddled with errors: various noted at ⊙17 and in Fig.1's expanded caption) specifies the impressive amount of time required by its scrupulousness (emph added): "these records substantiate in every particular the claim of Commander Byrd that on May 9, 1926, he reached the North Pole by airplane, thus being the first person to reach the North Pole by aerial navigation. . . . [the NGS trio] verified all of Commander Byrd's computations, devoting *five consecutive days to the work*; they have also critically examined the sextant used by Commander Byrd. Their examination began at 10 A.M. on June 23 and was completed at 5 P.M. on June 28." (Quoted from *NYT* 6/30:5:2. Or see G 141.) The identical text also appears at *National Geographic Mag* 50:384 (1926/9) — but with the final sentence and the report's 6/28 date both neatly excised, out of embarrassment at its awful potential revelation that: those five days *followed* the medal ceremony they should have preceded. . . .

M5 A final and hitherto-unnoticed slip by those certifying the 1926 hoax: the above-cited time period is actually *six* consecutive days: Wed (6/23), Thu (6/24), Fri (6/25), Sat (6/26), Sun (6/27), Mon (6/28). The trio's report provides no dates other than 6/28 (eliminated at G 142, as we note in fn 119). It is hard to believe that a trio that had actually spent 6 serious days at work on their Byrd project would tell Grosvenor (late on 6/28) that it was merely 5 days. And the NGS report was definitely written up on 6/28. (The date is handwritten: G 141.) Sooo — on exactly what day DID National Geographic's presumably-numerate trio actually start its exam of Byrd's records?¹²⁷

N Summing up the 1926 Evidence

N1 In brief: Byrd's diary and his typescript describe two quite different¹²⁸ trips. Indeed, there are actually FOUR distinct northward trips, because the diary has three separate & contradictory sections: the sextant-observations (where there are no dr data) are for a 70 mph celestially-navigated trip (§I4); the radioed distances are for an 80 mph dr trip (§O); while the last-minute dr data (where there are no sextant shots) are for an 85 mph trip (§C6). But the later neat typescript claims that the mean northward speed was over 90 mph (8^h25^m for the 765 mi from Kings Bay to NP is 91 mph: §C3). (If figured from AI, the

¹²⁷Did the NGS trio actually start work on 1926/6/24 (not 6/23), as the 5-consecutive days slip appears to reveal? Well, Secretary of the Navy Curtis Wilbur's letter (G 148) transmitting Byrd's report (TA) to National Geographic is signed by Wilbur on 1926/6/23. If NGS' threesome really dove into Byrd's report at 10 AM on the very same day, then we have here a degree of efficiency and celerity which are not evident elsewhere in these hitherto hyperdawdling proceedings: Wilbur got to his office; connected with the report (TA); composed and signed the 6/23 letter; placed it and TA in an envelope; got hold of a Navy courier; the courier travelled to National Geographic; he found Grosvenor; all three experts were assembled at NGS — and *all this activity* was completed between Wilbur's 6/23 arrival at his office and 10 AM of the same day! Most impressive. Especially for Grosvenor, who (if one accepts his own dates) couldn't count days correctly if it required more than one hand's worth of fingers.

¹²⁸Rob't Bryce has found similar problems in the tales of the first N.Pole-faker, Doc Cook: two different trip-schedules, in *both* his McK & N.Pole diaries (see *DIO* 7.3 ¶8 fnn 2-3) — which was presumably the reason his daughter Helene Cook Vetter kept them hidden throughout her life.

speed is 92 mph: fn 139.) Note: the typescript trip does not agree with ANY of the three disparate diary trips. Another Byrd multiple-contradiction. The main other one: see §D6 for the triple-troubles on Byrd's 9:15 GCT activities, the final crusher in the *NYT* 1996/5/9 p.1 story so loathed by the Byrd clique. Such conflicts write *finis* to the Byrd claim.

N2 In reaction to this evidential array, Byrd's impenetrable defenders operate by using the later-produced typescript as if it is just as valid a document as the on-site diary! (Must the Believers now launch a fine-tooth search for textual hints that mayhap the *Jo Ford* carried a typewriter?) For a *professional archivist* (OSU's R.Goerler) to be encouraging such a view represents Ohio State University's attainment of a genuine First, which should somewhat compensate that institution for losing the North Pole.

N3 Yet another First: Ohio State now proudly houses the private papers of both Doc Cook and Adm. Byrd — that is, a *majority* of the three¹²⁹ explorers who faked firstness at the North Pole. (OSU's Byrd Center takes gross-hoaxer Cook deadpan-seriously.)¹³⁰

N4 To return to the real world: if you are caught keeping two sets of fiscal books, you go to jail. Not even the wildest defense lawyer would try alibiing the accused by treating the differences between the two documents as exculpatory or mysterious (or reflecting on the prosecutor [⊙18]) — when the whole point of the indictment *is* the discrepancies.

N5 Question: did any other explorer in history leave us manuscript astronomical observations for position which (grossly to his disadvantage) invariably differed from his published observations?

N6 Briefly summing up Byrd's 1926 primary record: back&forth diary-entries (§O15), mysterious erasures (fn 109), fabrications (§F), contradictions with two versions (takeoff time: §G7), three versions (Pole-arrival time & 7:07 sextant altitude: §D6 & §G7, resp), four versions (speed: §N1). I.e., a polar-class blizzard of tampering-indicia, any single snowflake of which suffices for the 1926 claim's uncertifiability. Do the Defenders seriously suppose and contend (⊙13) that a claim founded upon such a record can ever be restored? — and that scientists will accept as mere coincidence [see ⊙2 & ⊙13] that **both** of Byrd's two erased & long-hidden northward sextant shots on his 1926/5/9 meridian-path indisputably (§§E15&L5) put him ordmag 100 mi short of the poleward distance he later claimed for those two shots' given times. Note: these shots are in different halves of the sky (one north, the other south), so that a constant sextant-error [an apology suggested in Portney 1973 p.212] would cause disparate indications. Instead, both shots are extremely consistent (§I4) with a much slower speed than Byrd alleged and both put him far south of where his 1926 report (TA) claimed he was, but about where Balchen had already in 1958 said Byrd had to be (§I5), on the basis of Balchen's intimate knowledge of the *Jo Ford's* speed capabilities.

N7 Two ironic conclusions are inevitable from the foregoing.

[a] In spite of his 1971 exaggeration (fn 13), Balchen's disbelief in Byrd's 1926 success has here been utterly vindicated.

¹²⁹Frederick Cook 1908/4/21, Rob't Peary 1909/4/6, Richard Byrd 1926/5/9.

¹³⁰ Cook was so technically-inept that he cannot at all be put in the same class with Peary and Byrd, who knew navigational math and unquestionably got most of the way to the Pole — while unmathematical Cook was never within 500 miles of it. The Byrd Center has run straight-faced academic conferences on the subject of Cook, this while OSU receives (from the Cook Society) 5-figure sums annually for the upkeep of the Cook papers. Byrd's dear daughter Bolling (one of my favorite people — & a DR relative via Pocahontas & the Bollings of Virginia) has even joined the Cook Society board and was recently on hand personally to help the Society re-dedicate the marker at Cook's birthplace. When *DIO* 7.2-3 published Rob't Bryce's recovery of the photo that proved (see *NYT* 1998/11/26 p.1, explicitly based on *DIO* 7.2-3) the enormous fraudulence of Cook's 1906 claim to have climbed Mt. McKinley, Ohio State's Goerler congratulated the Cook Society journal's repulsively *ad hominem* reply, calling it "a particularly excellent issue". As one of Byrd's most prominent old friends and admirers puts it (in disgust at the association of the Byrd Center with Cook): regarding the conclusive evidence against their respective heroes' N.Pole claims, both the Cook and Byrd camps are now in the same boat. [See the just comment of Roberts 2001 p.129. Note: since the original 1982 edition of his book on hoaxes, Roberts has written for NGS, but has never let this corrupt his frank view of its hoaxers.]



Figure 10: Two Genuine North Pole Discoverers.

Umberto Nobile (right) and Dennis Rawlins (left) meet in Rome (1975/6/15). The photo is by librarian Gertrude Stolp Nobile, Nobile's wife. Nobile designed the dirigible *Norge* ("Norway") and helped to pilot it over the North Pole on 1926/5/12, accompanied not only by explorers R.Amundsen & L.Ellsworth and crew, but also by Titina, Nobile's fox terrier: the first female to reach the spot. (So the North Pole was attained simultaneously by both genders.) After her death, Titina was mounted by a taxidermist and kept with the collection of Nobile-memorabilia visible behind us. Though the image was underexposed, Titina's left eye and sharp white-marked nose (pointing leftward) are discernable in the photo's lower left corner, just peeking over the right shoulder of DR (who has never been nearer the Pole than Frederick Cook got).

[b] Whereas Byrd was long regarded as first to both Poles by air, it now turns out (fn 6) that the best candidate for that honor is the exposé of Byrd's 1926 fake, Bernt Balchen! He flew Byrd to the South Pole on 1929/11/29, and himself flew over the North Pole on 1949/5/23. Balchen suffered needlessly at the hands of the Byrd political machine, for defending the truth of the Amundsen-Wisting achievement of being first to not only both¹³¹ but EACH Pole. We trust that history will now undo the lengthy usurpations by Peary, Byrd, and the unspeakably selfish Grosvenors — and at last do full justice to Umberto Nobile, to Lincoln Ellsworth, to Oskar Wisting, to Bernt Balchen, to Ralph Plaisted, to Wally Herbert.

And, above all, to Roald Amundsen — the incomparable polar hero for all time.

¹³¹ As pointed out in an earlier *DIO* by Cambridge's Bob Headland (1994), the Amundsen-Wisting priority in being first to have seen both poles has never even been controversial.

O Appendix 1. Reconstructing Actual DeadReckoning & 9:15 Stress

We next induce in detail the remarkably simple navigational structure underlying Byrd's sparse northward non-sexant diary entries. (This reconstruction was effected subsequent to the 1996 report.)

O1 The diary provides the texts of several "radios" (wireless messages) that Byrd asks Bennett to send. Some of them give distances, but what has hitherto frustrated any attempt at extracting information from them is their entire lack of correlative time-data.

O2 The entries¹³² are: [a] radio that the *Jo Ford (JF)* is passing Amsterdam Island (AI); [b] radio that *JF* is 85 mi (badly-written) north of AI; [c] radio that *JF* is 240 mi north of Spitzbergen; [d] Byrd's panic at (initially & wrongly) concluding Bennett had been aiming 5° to the right of northward (§O10); [e] radio that *JF* is 230 mi from Pole (§O13).

O3 The initial clue that starts unravelling this section of the diary is something tiny — which I and others (e.g., G 55&77) have missed up to now: superficially, the first post-AI radio message (§O2[b]) in the diary looks like it claims 85 mi north of AI. But close examination (at D 6 = G 81) shows that the "5" has been written-over with a distinctly thicker "0" — which is easily missed because Byrd's script zeros were smaller than his fives, and much of this zero melds with the five's loop.

O4 Next we ask where Byrd's final¹³³ distance-message (§O2) puts him. Since 230 mi = 200 nmi = 3° 20', then the latitude claimed is $L = 86° 40' N$. Amsterdam Island's latitude¹³⁴ is 79° 45' N ± 2'. Thus the indicated distance north of AI is 478 ± 2 mi. But the fact that our subtraction's 230 mi element is obviously rounded (probably to the nearest 10 mi) tells us that Byrd was radioing that his distance north of AI was about 480 mi.

O5 So the three radio-message northward distances (§O2: 80 mph, 240 mph, 480 mph) all bear the common factor 80 mi. Since Byrd's own typescript report claims he was estimating dr distances every hour (TA 4-7 = G 152-155), an attractively simple hypothesis suggests itself: during the northward trip, Byrd was merely multiplying 80 mph times the number of hours he'd flown north of AI — i.e., he was using an equation familiar to all of us since grade-school:

$$\text{distance} = \text{time} \times \text{assumed speed.} \quad (3)$$

O6 Recall that, despite the typescript's drift-indicator data, Byrd's actual (diary) dead reckoning (dr) indeed *was* — as we've already seen at §C6 (from Fig.3) — just to compute his distance via eq. 3. (Dead reckoning was also Byrd's navigation in 1927 & 1929.)¹³⁵ Because the speed Byrd uses in his Fig.3 dr math (§C6) is 85 mph, it is not immediately

¹³²The first is on D 5 = G 80; the rest, on D 6 = G 81.

¹³³It is odd enough that there are no sextant data at all from the return trip, an omission Byrd excused by saying that his sextant got broken near the Pole. (Though, see fn 33.) Question: why were no radio-messages sent during the return, giving position? (See D 14 = G 86.) Byrd does not say that his drift-indicator was broken. (To the contrary: TA 8 = G 156 says he was using it hourly, just as during the northward leg.) This blank makes sense if a troubled Byrd was now leaving open as many story-options as possible. Which he did just as long as he could: §O8.

¹³⁴The typescript puts the north coast of Amsterdam Island at 79° 48' N (TA 3 = G 151) and uses that figure as the basis for his report's cumulative d-i distances. But no d-i data are in the contemporary diary, which does not specify (D 5-6 = G 80-81) the part of the island that was used during the actual flight. (No diary dr numbers refer to AI at all.) His diary drawing (D 6 = G 80) shows use of AI's west edge (79° 45' N) for aiming, so that is presumably his in-flight AI reference-point. Further, Byrd complains (TA 3 = G 151) that the chart, H.O.2560, which he took on the airplane, slightly mismaps Amsterdam Island. (See also comment at TA 4 = G 152 on the lengths of degrees on H.O.2560.) Thus, the slight (2 mi) difference between 478 mi and the §O5 theory's 480 mi is insignificant.

¹³⁵See D 16-21 = G 115-117 (no sextant readings: fn 143) and §C6, resp.

obvious that previously (*up to* the turnaround point), he was figuring (for his crude eq. 3 dr calculations) that the *JF* was going 80 mph. But, remember, Bennett's 85 mph estimate (§C3) was not transmitted to Byrd until the northward trip was finished (see Fig.3). Also, Byrd is known to have been planning¹³⁶ on traveling 80 mph for the first hours of his flight. (See also fn 88; and, contrarily, ☉4.) Further, the intelligent Byrd flight-plan independently induced here (§B8) requires arrival at the Pole at roughly Amsterdam Island Apparent Noon. A plane starting at 1926/5/9 00:40 GCT = 00:44 GACT and traveling 765 mi (Kings Bay to N.Pole) at 80 mph will arrive at the Pole at 11:02 Amsterdam Island Apparent Time (AIAT), whereas assuming 85 mph predicts arrival at 10:28 AIAT — almost 60% further from 11° E Apparent Noon. I.e., compared to 85 mph, a speed of 80 mph fits well over half again better with the §H5 theory that Byrd was neatly planning to sextant-establish a Summer line perpendicular to his meridian-path. Also: we've proposed that Byrd may have shot the Sun with his sextant pre-set, perhaps to whole degrees (§E3 and §L11 item [C]). So it's neat that, for an 80 mph flight north from KB at 00:40 along 11° 04' E, apparent *h* (Hinks-computed) was near 19° at 7:07 GCT. (Barely less: 18° 57' & climbing slowly.)

O7 According to the simple §O5 theory leading to eq. 3, the 80 mi message (item [b] at §O2) was obviously sent at hour 1 (past AI); the 240 mi message, at hour 3 (past AI); the 480 mi message, at hour 6 (past AI).

Our next inductive step will check the 80-mph-speed assumption.

O8 The Byrd typescript says that he passed AI at 1:22. So hour 6 past AI (§O7) is 7:22 GCT, which is¹³⁷ just 15^m after the 7:07 observation that was Byrd's first big shock of the flight; and this Shock One soon makes itself felt (as we will verify at §O10) in the diary's startled reactions to the 7:07 sextant shot (D 6 = G 81):

"You are keeping to the right 5° too much."

"You *must* not persist in keeping too far to the right." (Byrd emph via *triple*-underlining.)

O9 The calmer entry directly below this (evidently transmitted a few minutes later): "Radio that we are 230 mile [sic] from the Pole."

O10 As we saw in §H, if Byrd wished at this time to convince¹³⁸ himself that his groundspeed was nearly his presumed 80 mph airspeed, then he must be FAR to the right of his intended 11° E meridian. At the time of the 2nd sextant shot (7:07 GCT), he was 5^h 3/4 past AI, which (at 80 mph) translates into 460 mi = 400 nmi = 6° 2/3 of latitude — putting him at about latitude $L = 86° .4 N$, or almost 70 nmi north of where the 7:07 sextant sight put him (85° 16' N: §E14) if he's on his intended 11° E meridian. Thus, to justify his distance schedule, he must put himself off his meridian by nearly 70 nmi-tan 27° (see analogously at §H2) or roughly 35 nmi — which for a 400 nmi trip north of AI works out to a misaim to the right of about $\arcsin(35 \text{ nmi}/400 \text{ nmi}) = 5°$. Which nicely matches the 5° diary alarm quoted at §O8. This match indicates that the 5° note (§O8) was written after 7:07 GCT.

O11 But 85 mph for the 5^h 3/4 from AI to the 7:07 GCT shot is 489 mi, or less than 220 mi from the Pole. Considering that computing the 7:07 shot would take several minutes, any message sent after completing this would (if Byrd dr-computed with 85 mph) have claimed he was about 200 mi from the Pole, not 230 mi. Thus we eliminate the 85 mph hypothesis, and conclude that 80 mph was indeed the basis of Byrd's eq. 3 dr-math.

O12 But one mustn't let the foregoing detailed questions (about Byrd's exact speed) overshadow the larger point here: Byrd's reports pretend that he used entirely d-i-based

¹³⁶See D 339 (G 90). (I owe the noting of this to K.Pickering.) Byrd originally contemplated (*idem*) upping the speed to 90 mph in the 3rd hour out; but §O11 eliminates any speed near that value for the actual 1926/5/9 northward trip.

¹³⁷This assumes that the AI-passing time 1:22 GCT (and thus 7:22) is fairly accurate. There are several lines of evidence indicating that it is not. (See, e.g., ☉7.) But if it is seriously off, then Byrd is lying — so we can just forget all the fine analysis and go home early.

¹³⁸His hope would be buttressed by the fact that he had been a little worried about a rightward tendency earlier in the flight. During the first hour north of AI, Byrd tells Bennett (D 6 = G81): "you are steering too much to the right. Set compass few degrees to left."

precise groundspeed distance-estimations which showed an average¹³⁹ northward speed of about 92 mph = 80 knots — even while his diary instead consistently shows crude linear airspeed dr by simple multiplication of hours times a presumed speed of 80 mph = 70 knots. (Similarly near turnaround, but then using 85 mph = 74 knots: §C6.) And recall that his sextant shots prove an actual northward speed of (§I1) about 70 mph (barely 60 knots).

O13 On the same diary page (D 6 = G 81) with the other radios we've been examining (throughout §O) The final message reads:

"Radio / nothing but ice everywhere — no sign of life motors going fine."

O14 So we assume that it was after¹⁴⁰ this that Byrd was hit by Shock Two: the horrible sighting of that fateful leak in his right engine.

O15 Byrd's entries have reached nearly the bottom of the diary page (D 6 = G 81) and he now wishes to discuss the crisis with Bennett via notes. So where does he write them? Though the next page (D 7) was blank (and is still blank), Byrd leaps past it *to the diary's back* (Fig.3). Why? (Similarly: fn 59.) After writing (Fig.3) the distance to go to the Pole, he allegedly (G 78) leaps out of the midst of this page and returns to the front of the diary, starting some "at-the-Pole" notes for Bennett on D 14 = G 86. (However, due to the non-continuous nature of the entries — plus the fact that this is the last page of his non-rear-of-diary 1926/5/9 entries — we can't even tell whether or not this entire page was added after the flight. After all, a faker who erase-deletes certain inconvenient diary-passages would probably not be above adding convenient ones. See also ☉9.) But then, if one accepts D 14 as real, an inexplicably flighty Byrd goes *again* to the diary's rear, back onto that **still incomplete** rear-page (Fig.3) he'd just jumped out of — and all the while he's leaving not only D 7 but D 10, 12, & 13 **blank**.¹⁴¹ A summing-up of this jumping-bean scenario: Byrd bolts from diary-front to back, then leaves the rear-page incomplete (at a point c. 1/3 of the way from its bottom) to return to the front (D 14) — followed by a switch right back into the same incomplete rear-page he'd just bailed out of. And this is not just my reading of Byrd's jumpiness: *all this bizarre oscillation is (very tacitly) acknowledged by Goerler*. (See his identical ordering of entries at G 78-79.) Elementary consideration: wouldn't an honest account simply provide events in the actual order of occurrence? — which is how most of us keep diaries. Since it is obvious that, during those hideously tense post-leak moments, Byrd started thinking damn-fast about what his tale was going to be, it is provocative that at this very moment he suddenly goes to the rear of the diary —

¹³⁹ TA 4-6 (G 152-154) hourly d-i-determined alleged distances in nmi: 77, 77.5, 82.5, 78.5, 80.5, 81, 81.5. Average speed = 558.5 nmi/7 hrs = 79.8 knots = 91.8 mph. The return-trip d-i-based speeds in nmi (TA 8 = G 156): 91, 89, 93.5, 92.5, 92, 94.5. (As noted at fn 116, all miles written in the diary during the 1926 flight are statute.)

¹⁴⁰ But it is a rather striking coincidence that Byrd's last transmitted message (D 6 = G 81), before a motor went drippy, uniquely commented upon what good shape the motors were in. He then shifts (§O15) to a back diary page (Fig.3) to tell Bennett privately that a motor is leaking. Later yet, we find (D 14 = G 86) a radio-message (with nothing after it on the page) mentioning reaching the Pole and having a leaky engine. But I am not aware that anyone in Spitzbergen knew about the leak until the plane returned to Kings Bay. See §H5.

¹⁴¹ As one sees in Fig.3's faithful depiction of d 3 (G 96), much of the scrawled math (and the following "20 miles to go to Pole") was done in such frantic haste that (even though the diary pages are lined), the handwriting is at a slant which is uncharacteristically severe. (No other in-flight Byrd writing was so. See ☉9. Note the *NYT* 6/26:3:3 report that Byrd told the public when about 20^m short of the Pole, he wrote a message saying: "We ought to be there pretty soon." That is not precisely what Fig.3 says.) The leaving-key-pages-blank-at-least-for-now ploy (akin to ☉8 & fn 133) is reminiscent of Peary's suspect 1909 diary: see Rawlins 1973 p.230. These pages just might be seen as useful for future-filling-in of parts of a presently-in-flux hypothetical story. (Since comparing initial stories to final ones is among the best methods for detecting lies, it is indicative that the truth-loving legal branch of the US gov't has helpfully arranged for us the liar-protector "Miranda rule", by which all criminal suspects are *legally required* to be warned against providing an initial story at all! See *DIO* 7.3 ‡9 fn 15.)

writing (for the 1st time on the trip) upon an *unnumbered*¹⁴² page, that might later be torn out (this would raise far less suspicion than removing D 7, which was part of a continuous, preprinted-pag numbered record) in case a judge demanded to see the diary. (Of course, in 1926, no one at amiable National Geographic was capable of committing such an offense.)

O16 Byrd then goes back towards home and simultaneously goes back again to the diary's end (Fig.3) — to start his desperate scribbled-arithmetic, to come up with the airspeed-based estimates which we have already covered in §C6.

P Appendix 2. Restoring the Diary's Flawed 1925 Sextant Sights

Byrd did some sextant work¹⁴³ in-preparation for & during his 1925 Greenland expedition. They all appear to be just practice shots; in any case, none were taken in previously unknown regions. The unravelling of them was much more difficult than for the 1926 data, due to several scribal errors; but it proved possible to correct all of these and ultimately produce a firm solution for each of the four 1925 diary shots.

P1 At D 336 (G 88), we find two sights taken with an ordinary sextant. (No artificial horizon, as for the 1926 sights. So dip must be applied.) The first is a lunar sight for 1925/5/6 18:24 GCT: lower limb apparent altitude 25°30'30". Byrd finds: semidiameter = 15'54"; augmentation for topocentricity = 7"; dip correction (for 20 ft) = -4'23", index correction = -1'30". The sum is 10'08". Adding this to 25°30'30", he has the apparent altitude of the lunar center = 25°40'38". He then adds r&p = 50'33" (from Bowditch's Table 24), which produces the geocentric true altitude of the Moon's center: 26°31'11". All of this difficult math is accurately¹⁴⁴ carried out. But the simple part of the problem is not: Byrd writes 18:24 GCT when he means¹⁴⁵ 18:24 GMT or 6:24 GCT. Correcting for this (and trusting that the chronometer error is not large), we find that the Summer line thus determined passes through Connecticut (roughly through Hartford & along the CT River) and crosses Long Island & sea lanes thereabouts. Not knowing Byrd's location on this date, I cannot check the line's accuracy.

P2 But we can check the next sight on this page, since it is known (G 29-30) that Byrd was at sea, east of Maine, when he made it. He even provides an approximate longitude (perhaps referring to the AST time zone): 60°W. For 1925/6/21 local apparent noon, he finds the apparent altitude of the Sun's lower limb: 40°04'00". This is a misread by 3 in the tens place, since the actual altitude was 70°04'. From Bowditch Table 46, he finds the correction (sum of SD, dip, and r&p — the last being for an altitude off by 30°) for a 40° lower-limb solar observation made at height 20 ft: +10'37". This he adjusts (roughly correctly) by -16" for annual variation in solar SD (since the Earth was near aphelion). Adding +3' index correction, he has 13'21" to add to 40°04', yielding 40°17'21". Adding the complement of this (ZD = 49°42'39") to the near-solstitial solar declination 23°26'48", he makes an addition-error of 6°, ending up with deduced latitude $L = 79°09'27''N$ — instead of the result actually implied by his false observation, nearer

¹⁴² The pages of Peary's 1909 diary were, exceptionally, also unnumbered. Curiously, one of the very fishiest aspects of the 1909 diary (which could very well be the original, though see Rawlins 1973 p.155) has hitherto been unnoted: *the extensive caretted-in passages* (some crucial). None of Peary's other diaries exhibit even *a tenth* as much inserted material — which is obviously suspect of being added later.

¹⁴³ The 1927 section of the diary (the *America* flight) contains no Byrd sextant data. And none were taken on the 1929 flight to the South Pole: §C6.

¹⁴⁴ Of course, technically, one should apply r&p at the start not end of the process. Still, the effect of this slip is small. (Bartlett's similar error at Peary *North Pole* 1910 p.359 could have been much more serious but for a lucky fluke of cancellation. Cook *My Attainment* 1911 p.302 has the right order, despite his bungled r&p [mis-plagiarized from Peary *op cit* p.362].)

¹⁴⁵ The *h* is quite wrong for 18:24 GCT; but that is the time for which his lunar semi-diameter is chosen. (See fn 53 discussion of astronomical ephemerides' upsetting 1925 switch of tabular epoch.)

73°N. (I.e., the observation “proves” Byrd was north of latitude 73°N, when he was actually about 1800 nmi further south. Which shows how easy it is to get sextant data that prove you’re in the Arctic even when you aren’t.) At the time, Byrd’s actual latitude was $L = 43^{\circ}09'N$ (or less, if the sight was not quite at apparent noon), as a correct reduction will show. So Byrd was rounding Labrador, *en route* to Etah, Greenland.

P3 The 2 other 1925 sights (D 335 = G 87) are both¹⁴⁶ cited to 1925/7/27, 6:06:20 GCT. Sun at altitude 16°07', Moon at altitude 3°53' — both already reduced to geocentric true altitude of center. (Thus the work betrays no information as to the type of sextant used.) The problem is that, if taken at face value, these data place Byrd out in the Arctic Ocean at 82°46'N, 154°36'W (near where the *Norge* would be on 1926/5/13 — about 40% of the way from the N.Pole to Pt.Barrow, AK). This is about 1200 nmi away from where Byrd really was at that time — a situation which is due to several Byrd slips, some of which are of the same type we have encountered previously: [a] His lunar observation errs in the degrees tens-place altitude, which (after accounting for this error’s 8' effect on r&p) means that the correct geocentric true altitude was 14°01'. [b] He confuses GCT & GMT again (recall fn 145). [c] He confuses this with his zone time, AST (Atlantic Standard Time, 4^h W of Greenwich). [d] It also seems probable that he observed at 6 PM mean time but thought it was 6 PM apparent time. I.e., the observations were actually made (according to his chronometer) at 6:00 PM Atlantic ST = 18:00 AST = 22:00 GCT, which Byrd treats as 6:00 GACT = 6:06:20 GCT. Net time error: almost -16^h. These four errors would seriously affect steering by the Moon’s azimuth, since Byrd has the lunar-center’s geoc. rt.asc (from the *AENA*, for 6:06:20 GCT) as 13^h00^m55^s — whereas the correct rt.asc for the actual time of the observations (22:00 GCT) was nearly 13^h33^m, a distinctly nontrivial error of 8° (lunar motion during the 16^h time-error). His deduced Sumner lines are intended for the type of Hinks-Littlehales grid-navigation math (apt for the polar-regions) which he used in the 1926 flight (where we also found a tens-place error: §E4). Both lines are nontrivially misplaced, due to the time-error & the lunar altitude error. The solar-based Sumner line is off by 9' or 9 nmi (virtually the shift in solar declination corresponding to the time-error). The lunar-based Sumner line is off by 13°, an error which is easily traced to the sextant-reading error of 10°, added to 3° of declination motion by the Moon during the 16^h time-lapse. From these two altitudes (corrected), we find Byrd’s location to have been: 70°45'N, 52°23'W. This is almost exactly at Umanak, Greenland. (The Umanak native settlement was at 70°41'N, 52°09'W, at the south tip of tiny Umanak Island, whose north tip is at 70°44'N, 52°09'W. If Byrd’s chronometer had an error of merely +1^m, the solution would become: 70°45'N, 52°08'W.) And he was indeed at that very outpost in late July of 1925. (See Hoyt 1968 p.82. Also G 33.) This shows that [i] Byrd’s reductions of Sun & Moon to geocentric place were both done quite accurately, based upon essentially (setting aside the 10° lunar altitude slip) scrupulous observations, & [ii] his chronometer must have been pretty steady. Commendable on both points. And, as for the other 1925 data, we find that: while he is occasionally careless with the easy arithmetic, he has done well by the hard parts of the work.

Q Appendix 3. Ohio State University: Neutral? Or for the Byrds?

Q1 The diary was edited by OSU archivist Raimund Goerler (of the Byrd Polar Research Center Archival Program: G 5) and published by OSU Press in 1998. OSU houses the Byrd records under the solicitous eye of the Byrd family. The Byrd Polar Research Center, established by the family, is on the OSU campus. DR urged that the diary data and all vying analyses be reviewed before publication by competent neutral scientists (e.g., National

¹⁴⁶This fiction is for navigation-math practice-shot convenience, since the GCT is chosen to force the GACT to be exactly 6:00. The work is explicitly labelled “Example” at the outset. It wasn’t used for navigating the ship, which was in-harbor.

Academy of Sciences — specifically proposed to the then-Director of OSU’s Byrd Center, Ken Jezek, on 1996/8/20, shortly after receiving Molett’s ravings as a Goerler 1996/8/6 enclosure); but this was never done. So I here repeat this advice, now as **a challenge to NGS (and the Byrd Center):**

Submit the issue to your own Washington neighbor, the National Academy of Sciences, and see if the Academy will conclude that Byrd deserves recognition as first air-attainer of the North Pole.

(In the 1991/8/13 *Washington Post*, I put the same challenge to NGS on its Peary 1909 N.Pole claim [repeated at *DIO* 7.1 ¶4 fn 21, adding the following emph]: “have Admiral Peary’s claim [and NGS’ 1989-1990 NavFou Report] evaluated by the National Academy of Sciences, just as papers are routinely refereed every day in US science. **I am willing to abide by the Academy’s evaluation. Is National Geographic?**” NGS, unwilling to commit its legends to automatic suicide, has chosen to pass up this opportunity at independent confirmation of its expertise.) For months after the 1996/5/9 *NYT* splash, OSU juggled options for seeking experts for external review. (Goerler to Molett 1996/8/6: “This controversy, I suspect will continue. We are still discussing the formation of a panel to review the matter.”) The original Byrd Center idea was to have “at least six scholars” review the diary: *Columbus Dispatch* 1996/5/11. But since no fair national-search process could possibly find six experts all willing to support Byrd’s claim, OSU then shifted to mumbling (to DR only when pointedly asked) about getting the opinion of an on-campus trio.¹⁴⁷ But the Byrd cult obviously couldn’t find six or even three informed scholars insane enough to give it the desired no-doubt&thumbs-up verdict. (On 1999/11/5, one [fn 210] of the contemplated OSU trio told DR in so many words that there was a pro-Byrd bias at OSU during these proceedings, and indicated he had not been shown the two key fabrication-proving documents — our Figs.6&8 — right in OSU’s archives under Goerler’s care, which were spotlighted in the 1996 DR report to OSU — and are analysed here at §O.) The national-six plan shrunk to on-campus-three and eventually to one, in effect: OSU’s own Pangloss (fn 66), Gerry Newsom. The Byrd faction finally just retreated into trying to maintain radio silence about its review and publication plans, intending (in G 57 n.19) to go with the old-unreliable hey-experts-disagree ploy [⊙13], instead of creditably admitting (as it had at first) [⊙19] the awful but utterly undeniable truth: the very man for whom Ohio State University’s exploring center is named perpetrated [⊙20] a sensational hoax-theft upon the world exploring community.

Q2 While Goerler’s book on *the diary* goes on&on in his main text (G 52-54) about old secondary and-or obsolete *non*-diary-based criticisms of Byrd, raising and then immediately countering each one successively (as neutral a format as any other catechism’s), Goerler, himself of the Byrd Polar Research Center Archival Program (§Q1), tersely relegates the most damning evidence against Byrd — the recent *diary*-based contradictions — to a mere footnote on G 57, where he just says that DR found “erased sextant readings that differ from those in the official report”. **But the book does not even state which data are involved** — nor that the sextant contradiction is ordmag a hundred miles. A hundred miles *south*. Goerler just couldn’t find room in his book for this information, even while spending pages upon now-minor secondary pro&con arguments (e.g., §S here) on the 1926 claim. Both

¹⁴⁷Gee, what an original number! The earlier trio (1926, NGS) was bursting with revolutionaries & whistleblowers: mathematician Henry Avers (32nd degree Mason), pliable & buyable Hugh Mitchell (fn 27), and A.Bumstead (Grosvenor’s *employee*, whose sun-compass was being made famous by Byrd). Of the modern OSU trio, one is not much involved. Another (incredibly) is the very same person who dropped-the-ball with the initial (pre-DR) 1996 investigation of the diary (reporting [fn 4] to the Byrd group that the sextant shots [Figs.4&5] looked safe: mere practice calculations) — no chance of personal non-neutrality here. The third is Gerry Newsom, whose imperviousity to indicting evidence is so lovably otherworldly (§F3) that I actually joked to him (1998/3/9): if I ever commit murder, I want an all-Gerry jury. (To be fair to Newsom: I believe even he is more agnostic than confident of Byrd’s success.)

versions of the key 7:07 solar altitude appear in the Goerler book (compare G 154 to Fig.5 or G 84) — but without a flag anywhere to guide the reader to their hugely disparate values, as well as the other remarkable discrepancies¹⁴⁸ that earlier had gotten OSU's temporary integrity onto the front pages of the world's newspapers. Positively monkish humility. (Before the diary-vs-TA conflicts were found, OSU press had turned down the book as of no public interest — Byrd himself at D 109 called it “the most uninteresting [diary] ever written, I guess”, yet another (⊙12) passage slightly misrendered in the book's transcription, in this case to Byrd's disadvantage: G 69.) Nor do we find at G 57 (or anywhere else in the book) any recognition that the Greenwich Civil Time for the 2nd erased diary sextant sunshot is *identical* (to the time-second) with that given for the publicly-reported sunshot (typescript) — 7:07:10 Greenwich Civil Time for both hugely discrepant versions (§E8).

Q3 Goerler omits three key sources outside the diary (two of them in the very collection for which he is archivist! — fn 65) which contain crucial contradictions that were exposed in DR's 1996/5/4 report to OSU. The computations on two of these documents are presented here in Figs.6&8. They prove (see §F) that Byrd twice fabricated the raw time for the 7:07 sextant shot. The 3rd critical record omitted by Goerler is Byrd's 1926/5/12 radio to SecNavy (fn 43), establishing the tripleness of his contradiction regarding the plane's Pole-arrival time. All three of the current Byrd-defenders appearing in the book (Goerler and the two men cited at G 57 n.19) deal with this triple-contradiction (§D6) by simply not mentioning its existence.¹⁴⁹

Q4 OSU has published Byrd's diary under the amusing title *To the Pole*. Goerler claims that this title is neutral, not implying that Byrd succeeded. (I leave this one to OSU's English Dep't.) But another of this *diary*-book's jokes on the reader is so cute that it achieves yet another indubitable First for OSU: in order to defend Byrd's detailed claim (TA) that his celestial navigation guided him to the Pole, Goerler and his hardy band of apologists have to ask (G 56-57 n.19) you to *toss out ALL* the scientific (celestial)¹⁵⁰ navigation observations and calculations IN THIS VERY DIARY. Understand: **THEY** are all merely “mistakes”.¹⁵¹ So, ah, tell me again: why is this (otherwise mundane)¹⁵² diary worth publication by an academic press?

¹⁴⁸See, e.g., §D6, §G7, §N1.

¹⁴⁹The nearest (to indicating there's a problem) is Molett at ⊙18; but since he there refers only to differences of a few seconds, Molett 1998 is simply overlooking the Byrd 13^m discrepancy which he in 1996 tried (⊙18) to blame on DR. It is hard to tell whether Molett had (when he wrote his article) seen the diary's turnaround page (Fig.3) or just never knew of it until Pickering's letter arrived at *Merky World*.

¹⁵⁰G 57 n.19 brings in a Goerler colleague for alleged astronomical expertise — who says to ignore the diary's astronomical data, thereby making himself no longer a relevant expert. (See ⊙13.)

¹⁵¹A demonstrably false wish-fantasy: §G6.

¹⁵²§Q2.

R Addendum 1: American-Experience, PBS, and Fraud

R1 On 1999/2/8, the Public Broadcasting System ran a docudrama entitled “Alone on the Ice”, an hour-long American Experience television program on Byrd, done by Nancy Porter Productions: co-producers: Nancy Porter and Beth Tierney. It faithfully followed the Goerler line that, though there were some doubts about whether Byrd's 1926 story was a lie, nonetheless, the diary “doesn't resolve this question”. Well, since that statement is itself a lie, I phoned Nancy Porter Productions (NPP) the next day to try to find out whose lie it was. Beth Tierney came on the phone and, when asked what was the basis of the program's statement, she replied that she and Nancy Porter had sought the neutral advice of an aeronautics expert at M.I.T. — who didn't think much of the diary evidence. But when I asked who could have made such a strange¹⁵³ statement, she said that NPP had sort-of a policy of not giving out consultants' names without permission. So I asked her to seek such and get back to me. When she didn't, I phoned again (1999/2/11) — and found that the formerly-sorta rule about anonymity had evolved into grumpy rigidity. (I am told by a top journalist that no such rule even exists in journalism, since this is not a case of anyone being in danger.) I again asked that permission be sought so that I might speak to this mysterious expert.

R2 Over a month later, just back from a long trip to the southern hemisphere, I phoned Tierney again (1999/4/1) and persisted, noting that she had not put anything on my answering machine in the interim. She said that NPP had tried to contact its mystery-man, but: he had never responded — so she couldn't give me the name. She made no offer to keep trying. Her attitude was not one of warmth. (This isn't not the first time I've experienced the perversity of an official getting huffy at someone for committing the uppity offense of not accepting that official's falsehood.)

R3 But it gets even weirder. The expert in question was John Hansman. And there was no legitimate excuse for NPP to keep his name from DR *since he was cited in the list of consultants that were visually displayed at the end of the program*. Tierney never mentioned to me the input of NGS-kisser Portney, though he was in fact consulted by NPP but was never named publicly — a highly peculiar omission, considering that the program's comments on the diary were far closer to Portney's line than to the (rather contrary) opinion of Hansman. Hansman was the only listed expert not interviewed on the air. And Portney's name was not mentioned anywhere, though he was a major force behind the scenes doing National Geographic's desperate work, by convincing several non-scientific journalists that mere-data-alterations don't really mean anything! Meanwhile, NGS-flack (§S12) Portney launched a private *ad hominem* attack on DR, to convince as many persons as possible that DR is an unreliable scholar who should be entirely ignored in favor of magnificently expert Jos. Portney. And Portney also complimented suppressor Rai Goerler, a willingly gullible victim of this pitch, for doing a “scholarly job”. Since fumbler Portney cannot deal with the deadly array of evidence against the 1926 claim and so [a] uses an I'm-the-real-expert pose as his major argument, and [b] has succeeded in thus fooling Goerler and Nancy Porter Productions, DR is here showing in detail the true worth¹⁵⁴ of the pathetic pretenses to plausibility & reliability staged by Portney and his NGS inspirers.

R4 When I contacted Hansman (1999/4/13), I learned that far from having told NPP that my analysis was no good, he mildly considered himself a fellow-skeptic about the flight! This suggests that Tierney had been feeding to DR arguments of Portney's invention — but putting them into Hansman's mouth (presumably because Portney preferred staying in the

¹⁵³See also fn 188.

¹⁵⁴Why do so many of those who are losing debates keep making the same mistake? Instead of honorably admitting the obvious truth, they irrelevantly try to save face by attacking the messenger's reliability (§L1) and posing as being more trustworthy — an argument which invites exchanges that end up with everyone tattered, thus ultimately returning the debate to the raw evidence which should have been the sole criterion in the first place. . . .

background, to hide NGS' connection to the program's result). As for the Molett-Portney ultra-folly of trusting the typescript more than the diary (!): Hansman found that particular notion vastly amusing — adding that Byrd had had over a month to prepare the typescript before NGS saw it, so that the report hardly was worth anything as evidence (given the ease with which sextant observations can be cooked up).

R5 And what of Tierney's alleged effort to reach Hansman? I was shocked to find that he had not heard from anyone at Nancy Porter Productions for ordmag a year — which meant that I had been solemnly, deliberately, and systematically misinformed by Nancy Porter Productions.

R6 WHAT is going on at PBS' "American Experience"?! PBS has received funding from National Geographic. But that does not (seem to) explain why one of PBS' now-mainstay commentators, Pulitzer Prize winner David McCullough, fell 100% for a crank Little-Brown hagio-paranoid book (H.Eames' *Winner Lose All* 1973) defending notorious hoaxer Frederick Cook (McCullough's now-hilarious-in-retrospect review can be found at *NYTimes Book Review* 1973/6/24 p.6)¹⁵⁵ — putting it on *NYTBR*'s exclusive New&Recommended list for 3 weeks (!) — and (presumably accidentally) warming National Geographic's chest cavity by simultaneously faking the nonexistence of DR's solid book, published the very same day, on the very same Peary-Cook controversy. . . . Are these the sorts of favors (to mega-publishers) that are required to advance up-the-ladder in the semi-showbiz world of the "Public" Broadcasting System? If so, then no wonder public versions of history pay so little attention to evidence — the weight of which is of vanishing moment when careerist climbers focus 100% on catering to major powers.

S Addendum 2: Portney's Complaints

S1 No pro-Byrd author suffers the slightest objection to Goerler's even hands. This includes J.Portney's embarrassingly NGS-kissing papers, published in a military-oriented journal (*Navigation*) whose historical standards may be gauged by its unashamed printing of Molett's apologia for Peary's Bunyanesque 1909 speeds, and which has I believe never published a skeptical paper on military heroes Peary and Byrd. (But has repeatedly published defenses of each.)

S2 In order to stimulate his increasingly lonely self-pleasuring fantasy that Byrd succeeded, Portney complains that skeptics overestimate the Byrd sextant's accuracy (§S5) and do not accept (§S11) the thoroughness of Byrd's NGS expert judges.

S3 Roughly 30% of the 1973 Portney *Navigation* paper's printed text (excluding space for figures) is just the extensive 1926 National Geographic report (though his p.209 deems it merely "inferential" & uncorroborated), plus navigator Pres. Coolidge's NGS-medal-presentation speech!¹⁵⁶ Both texts reprinted in entirety, pp.211-212. With the NGS conclusion

¹⁵⁵See *DIO* 2.2 fn 22 for details of the 1973 double reviewing disaster at *NYT*, and especially on the honesty-quotient of an incomparable farce (bringing to life the hilarious reviewing contrempts in the 1962 film *Only Two Can Play*) perpetrated in the 1973/7/29 *NYT* by lordly then-regular *NYT* reviewer Christopher Lehmann-Haupt, who is as adept with the dustjacket review as his hero Cook was with the gundeck-sight. (When DR pointed out [1973/12/9] that *Winner* had cribbed references [see, e.g., Rawlins 1993 CookSymp n.2 & *DIO* 9.2 §§D11&K27], Lehmann-Haupt [e.g., 1974/1/14] dismissively kept backing the "convincing" horse he came in with. Gosh, I can't imagine why dishonest scholarship disturbed Lehmann-Haupt so little. To *NYT*'s credit: since 2000 Spring, Lehmann-Haupt is aptly gone to the *NYT*'s obit desk.) *NYTBR* has a long-notorious weakness for giving space primarily to the heavyfreight publishing firms (whose ad-dollars pay the bills), so that small houses (the unis & the boonies) are always lucky to get a look-in at all, much less a friendly one. (See *NYTBR* Editor J.Leonard's confessions at *National Review* 1973/7/6 p.735.)

¹⁵⁶Which contains such navigational judgements as, at p.212: "We, his countrymen, were particularly proud. This man . . . added luster to the brilliant history of the American Navy." But: is this another gene record? There may be more words here than Coolidge spoke throughout the rest of his life.

also fully quoted early on at p.209, just in case you don't get as far as p.211. As in the culminating Peary debate of 1988-1993,¹⁵⁷ it appears that NGS got-lucky, and a defense-lawyer-consultant type just up and decided on his own to attack the Montague 1971 book. Portney is a Navy man (recall Byrd's perhaps humorous remark at *NYT* 5/17:4:5=6, "I don't see how the navy can avoid some responsibility for our flight") who at least used to understand the fragility¹⁵⁸ of the Byrd claim.

S4 Goerler unqualifiedly promotes (G 54) the Portney 1973 paper (the Byrd Center even considered naming him as an official reviewer of the case, which hints at how neutral a verdict OSU wanted) though it is now inevitably obsolete, in light of the subsequent revelations of the very diary Goerler has published!

S5 Portney's 52 nmi Byrd-uncertainty-envelope¹⁵⁹ around the Pole was openly based upon just trustingly spinning a large standard deviation for distance and aim, adopting the claim (§E12) in Byrd's now-diary-contradicted (§E11) typescript that the plane was not wind-retarded on the outward journey, combined with Portney's opinion that the 1926 sextant was poor. Though there is no certainty on the point, Byrd's sextant work seems to have been more accurate than Balchen or Portney — or I — previously thought: §12. Portney 1973 pp.210&213 grades the sextant as "crude" partly from Balchen's memory, partly by mis-citing a remark by Byrd's 1912 Annapolis classmate, the great navigation expert Philip V. H. Weems. The method Weems referred to is far more sensitive to errors than Byrd's, and Portney should have known that — especially since he tends to boast of his navigational background (while scoffing at others') as if it were itself an argument in favor of Byrd. (See discussion of same folly at §L1.)

S6 After all, if the sextant was indeed unreliable, then this would only raise the question of how Byrd's TA shots seemed to fit so remarkably well to a straight-line trip — and to d-i data which Portney more justifiably questions the accuracy of. (See discussion at §G2. Goerler concludes his book along the same apologetic line at G 132, quoting A.Lindsay's opinion that Byrd "was not a scientist and never claimed to be one". If meant as an alibi for Byrd's holey 1926 record, this makes an odd comparison to Byrd's scientific pretensions in his 1926 report, not to mention Goerler's reference to Byrd at G 21 as: "an expert in navigation".)

S7 If Portney is right that Byrd's sextant was so bad that a 67' discrepancy (over a degree!) between two simultaneous 7:07 shots is nothing remarkable, then: [i] Byrd's 1'' precision was thousands of times better than his accuracy! [ii] Byrd had to have been kidding when he said at his alleged Pole (TA 8 = G 156): "I took these [4 sights] very carefully and they averaged 17°-15'-14 and the altitude [h] seemed to remain constant."¹⁶⁰

S8 Goerler summarizes at G 54: Portney in 1973 "concluded that Byrd was likely to have come within at least fifty nautical miles of the North Pole". But the reader is not told:

[1] The main point (especially emphasized in the p.208 abstract by Portney) is that

(Legend has it that he was once challenged by a man who told him: "I'll bet I can make you speak 3 words." To which Coolidge's reply was: "You lose.")

¹⁵⁷See §B1; also *DIO* 1.1 ¶4, *DIO* 2.1 p.2, *DIO* 2.2, *DIO* 2.3 ¶8 §B, *Science* 260:1587 (1993/6/11).

¹⁵⁸Back in 1973, Portney showed a commendable attitude of appreciation for the work of those he disagreed with, and he could mildly demur from one of the 1926 NGS trio's opinions at, e.g., p.214. Like Molett, he has flown lots in the arctic — though that biographical fact can't make true the false elementary-school arithmetic in both men's writings on Byrd.

¹⁵⁹Two-sigma, with sigma figured (but see §S9) as 26 nmi. See Figs.12&13 (where 26 nmi is inadvertently drawn as 27 nmi): 2:26 nmi = 52 nmi.

¹⁶⁰Incredibly, Byrd is here implying that he could sense just from the solar *h*'s alleged constancy that he was near the Pole. However, had he been 20 mi off the Pole but still on the Sumner line indicated by his "observations", then *h* would have changed by just 1' in Byrd's 13^m of circling at "the Pole". Meanwhile, loyal Portney is saying that Byrd's accuracy was ordmag 100 times worse than this, if a 67' discrepancy is the kind of ho-hum which JP ludicrously alleges. Again, remember: Byrd's sextant-vs-dr match is almost entirely ordmag 1'! (Even the sole exception was still pretty small, the curious 7:07 instance: fn 67.)

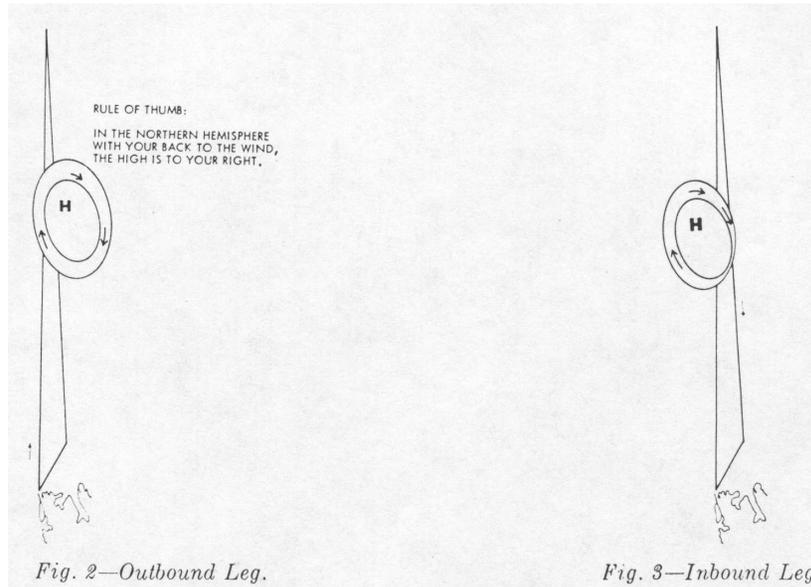


Figure 11: Portney's Smart Tornado (Dorothy & Toto Should Have Been So Lucky).

The matching Figs.2&3 from Portney 1973 (p.209), conjuring-up an anti-cyclone that helps Byrd's speed in *both* directions. See discussion at §S8. There being no independent evidence for this uniquely felicitous arrangement, one sees that its prime recommendation is: there's no other way to get an 85 mph airplane to cover 765 mi (each way) in less than 18 hrs without help from some awfully well-educated and well-intentioned weather — a godly-toggly (fn 163) wind-which thinks and asks: now, *which* way do I blow, in order to aid this mythic hero? Even the hardest skeptic cannot deny Portney's proof that Byrd discovered the Good Which of the North.

the Fokker-speed problem is much eased if Byrd fell *short* by about that much.¹⁶¹ For a similarly relayed misimpression, see ☉6.

[2] The contention by Portney that the Fokker probably got even within 52 nmi of the Pole depends upon making up for much of the plane's speed-limitation through a neat meteorological script.¹⁶² His Fig.5 shows that to get Byrd to the Pole, we need — as Liljequist *loc cit* previously noted — a 5 knot *northward*-wind-boost when Byrd is going *north* (this is implicit in the 85 mph-vs-91 mph discrepancy noted above at §C3 & fn 22) as well as a 25 knot *southward*-wind-boost when he's going *south*. So Portney 1973 actually proposes (complete with illustrations! — his Figs.2&3, reproduced here at Fig.11) a neatly placed, felicitously-west-moving, anticyclone-ex-machina, crossing the flight path with such just-right timing that it blows Byrd poleward while he's headed north,

¹⁶¹ However, as a resourcefully flexible apologist, Portney has since fallen in love with bewildered Demas' §K3 confusion about time-zones, so Portney hopes now to find enough time to get Byrd to the Pole, which will of course require Byrd to have not only a north wind behind him on the return but the well-timed *reverse going north*, since the attested airspeed of Fig.3 is inadequate to do the distance in the 8^h25^m of TA: see §S8 item [2].

¹⁶² Portney's new Demas-delusion time-schedule has lessened JP's need for the following, but the fact that he tried putting it over in the 1st place tells us how frantically and malleably one-sided Portney's approach is.

then blows towards home while he's headed there! [A *priori*-Portney 2000 pp.20&21 even goes a-wooing: "We call upon you [the reader] to use your analytical skills to help explain [Byrd's] early return".] If you think this a joke, I'll protest only that I'm not the joker, and I recommend that the reader not miss Fig.11 here.¹⁶³ (Note: Molett's 1996 ms p.2 buys into this weather-miracle in detail, though not crediting Portney's *unquestioned* originality-of-concept.)

S9 Portney's MPP (most probable position) is not found via the Bowditch §2906 procedure cited at Portney 1973 p.218. Portney's curious p.218 non-elliptic computation ignores his own crucial aiming-error-induced ± 11 nmi uncertainty-band, shown as the graphical basis (Portney 1973 p.216 Fig.7: our Fig.12) of Portney's proposed short&to-the-left 52 nmi Byrd miss of the Pole (2 times a 26 nmi σ), advertised as a 95%-probability solution.

S10 There are some extremely elementary problems with Portney's error-zone illustrations (our Figs.12&13):

[a] The 11 nmi error-region width is computed at p.217 as 625 nmi times $1^\circ.4/57^\circ.4$, when it ought to be 615 nmi times $1^\circ.41/57^\circ.3$; either way, it equals 15 nmi, not 11 nmi.

[b] An error seen from the slightest comparative glance at Portney 1973 Figs.5&7:¹⁶⁴ the Byrd-at-N.Pole solar azimuth shown on JP's central, *thrice-published* graphic (1973 Fig.7 [or 1992 Fig.2] & [27^y later!] 2000 p.27) is 135° , though near the Pole it was in fact c. 148° for 9:02:30 GCT = 9:06 GACT (= $136^\circ/2$ GACT), since adding longitude $11^\circ 04'E$ to the GACT yields $147^\circ.6$. So Portney has confused Greenwich App. Time with Local Mean Time [bungle twice repeated graphically at Portney 2000 pp.20&22]: using $11^\circ 04'W$ longitude as if 0° longitude! — plus taking mean time as apparent time. (Latter error also infected NGS chart's NP Summer-line: see ☉17 and-or caption to Fig.1. As for local-vs-Greenwich time — same confusion ruins the same official chart's 7:07 Summer line: see *idem*.) By accident, there is cancellation of the effect of these two Portney errors ([a] arithmetic & [b] orientation) on the deduced Byrd distance from the Pole; thus, the short-left corner of his 2σ parallelogram of error still comes out 52 nmi from the Pole.

[c] The probability of being in the intersection-area of two 2σ uncertainty-bands is not 95% but 90%. (I.e., the product of the two bands' 95%. Note: the very same error was made by NGS' 1989-1990 NavFou defenses of Peary's Pole claim — and was 1st pointed out by K.Pickering, now DIO's Editor. The NavFou deserves commendation for acknowledging the initial misimpression.) Correcting for this, we find that the equivalent of 2σ odds occurs within about a mile of the $89^\circ N$ circle. Note: Portney 1992 p.257 has finally caught & corrected error [a]; however, incredibly, he retains¹⁶⁵ the original 1973 diagram and the shortfall-distance-estimates based on it! — merely tiny-print correcting "11 NM" to read "15 NM", while keeping both the scale and the number of 1 nmi-wide bands *the same as before*. We reproduce both versions here at Figs.12&13 so that readers can verify this astonishingly bold sleight for themselves. [Portney 2000 p.27 at last corrected this beaut, but] the flagrant 11° azimuth error (item [b]) continues to elude his notice.¹⁶⁶

S11 But Portney 1973 p.213 at least sets a strict archival standard for scholars doing primary historical research: "It makes no difference that the [Byrd flight] charts were later misplaced: they once existed and were found to be accurate." (NGS — sole 1st-hand certifier of the Byrd N.Pole claim — has never produced records of these exams.)

¹⁶³ These winds are even more obliging than was intended by Byrd's own already sufficiently convenient scenario: §E12. Evidently, the 1926 NGS gold medal should have been adorned with Byrd's two toggling-on-cue wind-gods: Auster on the front-half, Boreas on the back-half.

¹⁶⁴ Here: Fig.1 vs Fig.12 or Fig.13.

¹⁶⁵ I thank Tracy Lener of St.Louis University for assistance.

¹⁶⁶ Similarly, his 1992 bibliography continues his 1973 bibliography's attribution of *Skyward* to "William E. Byrd". But he adds a new error at Portney 1992 p.259: "Ships generally keep their time consistent with their zone time." This is modern practice (e.g., Bowditch 1958 p.459). But it was not yet standard in Byrd's day. See, e.g., Harold Jacoby *Navigation* 1918 p.94. And see Byrd's probable arithmetic here at fn 102.

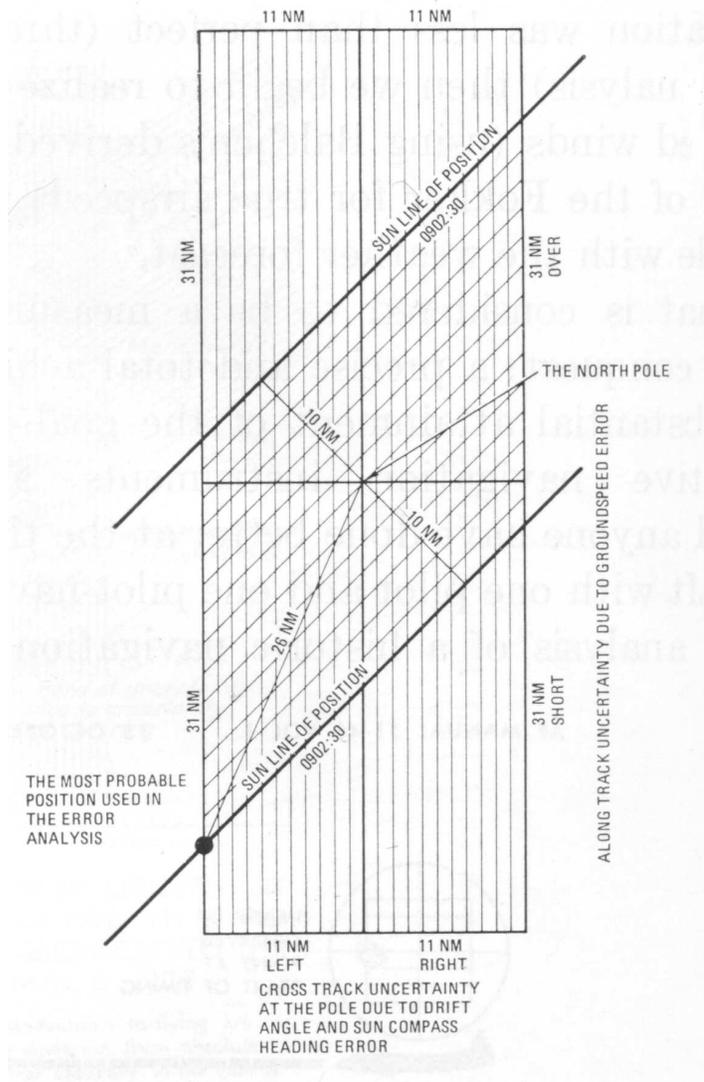


Figure 12: Portney's 1973 Diagram of Byrd's Purported Path & Error Zone.

The above Portney 1973 (p.216) diagram's solar azimuth is off by over 10° , as may readily be seen by comparison of the solar azimuth (at the Pole) to that in NGS' official chart of the flight (Fig.1), which is almost accurate on this line (see §§A3&⊙17). This error is due to Portney's confused taking of Greenwich Mean Time as local apparent time (math at §S10 [b]), the very same error National Geographic made for one *en route* observation in its own diagram. (See ⊙17 or Fig.1's caption regarding the 7:07 Sumner line & solar bearing.) This is the sort of bungling one rarely sees outside the work of astrologers. And the width of Portney's diagram is too slim by more than 25%, due to a grade-school math blunder. See caption to Fig.13.

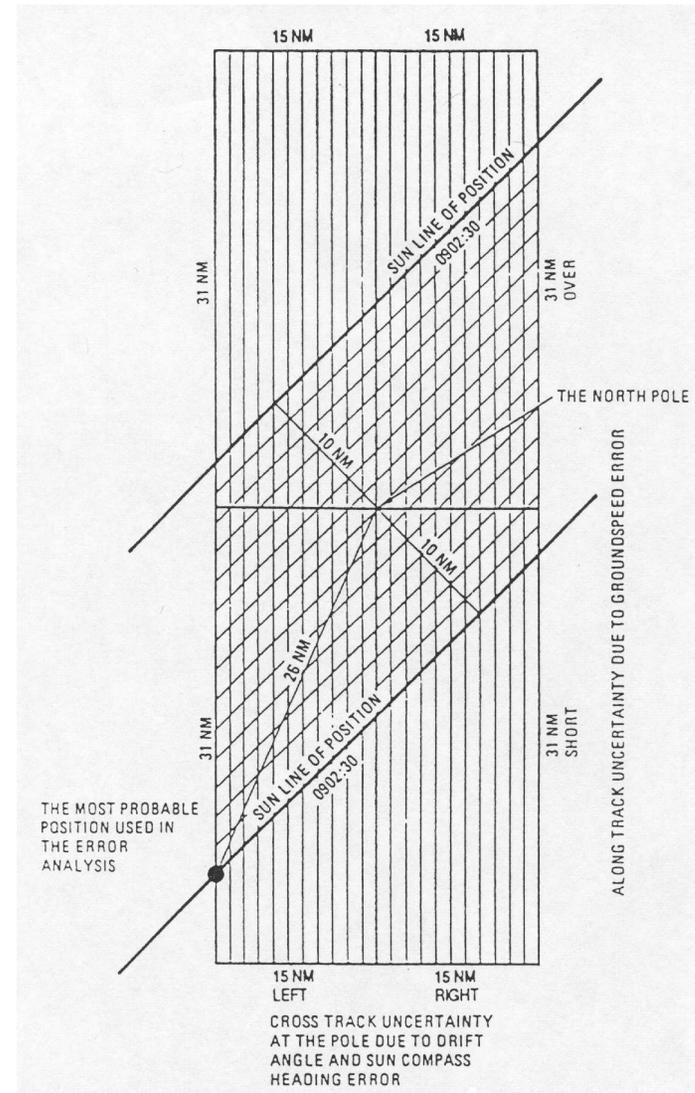


Figure 13: Portney's 1992 Diagram of Byrd's Purported Path & Error Zone.

This Portney 1992 (p.259) illustration is absolutely identical to Fig.12 (from his 1973 article) — with one sly exception: the width of the error zone is here labelled as "15 NM", instead of the 1973 version's miscalculated "11 NM" (Fig.12), where "NM" refers to nautical miles. (The print is small in both originals, so the contradiction is easy to miss visually.) Simple proportions (or counting of parallel vertical lines here: 1 nmi apart) shows that the 1992 version retains its 11 nmi width, in spite of the altered label! See §S10 [a] for further details (including source of the 11 nmi miscomputation). Additionally, the "26 NM" line is drawn as 27 nmi long.

S12 Portney affects an almost childlike reverence for NGS. On 1976/4/9, Balchen's close friend Evelyn Isakson sent DR some Portney material he'd circulated in preprint form but which had not made it into the *Navigational* piece. It reads like awe at the temple, as Portney describes a visit to the National Geographic Society: "I spoke with the museum curator and librarian and was able to photograph the famous Bumstead sun compass which helped Byrd establish his true heading. The library contained early editions of [*National Geographic* on the 1926] polar flight [plus] a comprehensive news clipping file on Byrd. It was interesting to read the newspaper accounts on Montague's book and Balchen's claim from all quarters of the English-speaking world. Many of the letters to the editor were extremely critical of the book and Balchen regarding the accusations [against] Byrd's polar conquest [emph added]." Portney 1973 p.213 provides similar burden-of-proof-inversion reasoning that is unadulterated Grosvenorbabble (§T3 vs fn 171): "In summary it appears that no new facts have been established to dispute Byrd's claim".¹⁶⁷ (And, now that new facts *have* been established, NGS is burying its putative brains in the sand: ☺19.)

T Addendum 3: It Ain't Over 'Til the FatCat Sings

T1 National Geographic has shown how a typical obese institution deals with the ultimate disaster: upon the appearance of flat-out disproof of a claim you have prominently honored & even deceived (§M1) to promote, you simply walk away from the evidence. I.e., if We don't acknowledge the proof against Our myth, then that proof doesn't exist. (When less wealthy folk pretend that reality can be altered by ignoring it, we do not accuse them of excessive sanity.) And the behavior of most of NGS' fellow media explains why the Society has been getting away with such arrogance. One of the sociological and journalistic peculiarities of dealing with fraud-controversies in science-history is that the "science press" (fn 84) has a predictable pattern of ensuring & bolstering a perverse asymmetry: *the more dishonest an institution is, the less it will be criticized for dishonesty*. This, because the lapdog "science press" is in general so lazy and osculatory (and too often plain innumerate) that it would — rather than itself examining evidence — prefer to draw its wisdom from Authoritative institutional press-releases. Thus, a DIO Law has repeatedly impressed itself upon us: public consensus (formed by the media-Fourth-Estate) will generally not accept that a major institution has done wrong unless that institution *itself* confesses. (**Thus, the press will publish news of a science institution's dishonest behavior only if that institution is honest.**) And the chances that the National Geographic Society's Gil Grosvenor 2 will ever own that his grandfather and Byrd in 1926 symbiotically established one of the great exploring hoaxes of all time, are about on the order of the likelihood that Orthodox Jewry will recognize that Moses personally chiseled the 10 Commandments up on Mt. Sinai,¹⁶⁸ even though both propositions are obviously true.

T2 The National Geographic Society certified the Byrd 1926 fabrications for most of this century — and (despite decades of numerous competent parties' reasonable doubts) treated his attainment of the Pole as fact, in myriad of its publications.¹⁶⁹ (E.g., the 1928 May *National Geographic Magazine* celebrated the non-event's 2nd anniversary by commissioning religious art: boasting atop its cover's list of contents: "Special Color Supplement, 'Commander Byrd at the North Pole' By N. C. Wyeth".) Thus, as anyone reading DR's original 1996/5/4 report to OSU on Byrd (copy Byrd Center archives) will see, DR had bent over backwards in it to be kind¹⁷⁰ to NGS and (§L1) to Byrd, since, when a controversy is over, it is unpleasant to tromp on a defeated foe. (Even Gen. Sherman

¹⁶⁷ See similarly by NGS' NavFou at *NGM 177.1:44-61* (1990/1) p.47.

¹⁶⁸ Moses' isolation on Mt. Sinai was high-security — and backed by the threat of death against anyone attempting to breach that security: Exodus 19.12&23, 24.2, Deuteronomy 5.5.

¹⁶⁹ Commendable exception: map & text at *NGM 154.3:298-325* (1978/9) pp.302-303.

¹⁷⁰ See, e.g., here at §D9 and fn 27. Both items were in the 1996/5/4 DR report.

agreed: *DIO 4.3* ¶15 fn 13.) Unfortunately, NGS has reacted to that kindness by spitting on it. Instead of dealing with evidence, NGS instead immediately launched (through, e.g., *Baltimore Sun*, *Polar Times*, and its friends' gossip) a slanderous attack on DR. Since NGS has chosen this road (without reckoning the cost of war to all parties: fn 154), I have already (p.6) responded above by providing an idea of the actual status of NGS in the academic community. Next up: details of NGS' disgusting reaction to its 1996 Byrd disaster.

T3 Its 1st response on hearing Byrd's real data had survived wasn't yet personal:

[a] "We haven't seen the latest evidence, *but we'd be glad to look at it*. But the National Geographic Society accepts the claim of Admiral Byrd for lack¹⁷¹ of *definitive* evidence to the contrary." (*Columbus Dispatch* 1996/5/11. Emph added. Similar upside-down [§S12] science quoted in *Polar Times*, loc.cit.) This weird response (inept to the point of self-contradiction!) was blithely or deliberately oblivious to the fact that, as an Ohio State Univ scholar privately put it: the most critical new evidence possible *had* just appeared.

[b] Soon after, in a newspaper (*Baltimore Sun*) with close contacts to National Geographic's then-President (Reg Murphy, publisher of the *Sun* until c.1990), NGS' paid consultant (lawyer Doug Davies) prominently said no one should take the new findings seriously since DR is totally unreliable (*Baltimore Sun* 1996/5/15 p.1 story). A reporter who had never worked at the *Sun* before (specially imported: 1st day on the job) tried to cover DR with abuse in a front page story¹⁷² (pretty prominent for a 1st-day-on-the-job piece) — and, while he was (during a break in the interview) conferring 1996/5/13 by phone with his desk (thinking DR couldn't hear), the *Sun* understandably tried to keep the cast of the article from the subject. *Sun* desk, when asking its reporter for guidance about the direction of the article, said that it of course "might be hard to say this with him there." Just another heartwarming chapter in ethical journalism. . . .

T4 But NGS evidently soon learned through channels that things were rather graver than its automatic slanderous optimism had deluded it into initially supposing. So, even while its newspaper friends assisted in NGS' personal vendetta (against doubters of its mythology), NGS now pretended to genial, above-the-fray boredom. Its next offerings (both *Philadelphia Inquirer* 1996/6/2): "We haven't seen the latest evidence. And *we'd be happy to take a look at it*." (NGS spokesman Halsey Spruance. Emph added.) But NGS p.r. director M.Jacobsen simultaneously affected a yawn: after 70 years, "A lot of people don't care as much as they had." (This is the same NGS that spent about 1/4 million dollars in 1989, trying to kill off DR for questioning NGS' other sacred polar myth, Peary's — which was then *eighty* years old. Now, NGS is uninterested. Or perhaps has finally learned a lesson — but hasn't the character to acknowledge the fact. Or the instructors.)

T5 So, the very institution that gave its gold medal to Byrd's 1926 claim continued to play games: *Cleveland Plain Dealer* (*Sunday* section 1996/8/11 p.12): "Until we see [the latest evidence] and analyze it or have it analysed independently, we will accept the . . . claim" — but the article also reports: "National Geographic has no plans to review the journal." In sum: we at NGS will accept the 1926 claim until we see new evidence, but — we don't plan to see new evidence. . . .

T6 All scientists watching this comedy of hyper-unfalsifiability were amused to note that — as NGS crouched low, month after month — the Geographic wasn't (officially) asking the Byrd Center (or DR) for copies of the data or even of the DR report.

¹⁷¹ A perfect example of reverse evaluation-technique. In real science, a claim is accepted on the basis of proofs in its favor, not upon the lack of absolute disproof — though in the amazing case of Byrd-1926 we now, exceptionally, do possess absolute disproof.

¹⁷² Quoting *ad hominem* junk from D.Davies before DD had even seen DR's Byrd report! — despite DR's specific pre-publication what's-the-hurry protest to the reporter on that point. This was not the *Sun*'s first prominent attack on DR. (Even in the 1991 *Sun* obit for NGS' T.Davies!) But the 1996 story admitted expert opinion backs DR. [And later *Sun* stories on DR (1998-2001) were helpful.] However, sub-sophomoric-crude GG2-instigated slander of DR is now prominent on the internet. (Recall *Sweet Smell of Success's* JJ.) What's the richest mud-mine in the universe? A controversy's last ditch.]

T7 Finally, the *Washington Post* contacted NGS 1996/10/23 and asked some uncomfortably elementary questions: [a] Is NGS (now *five months* after the *NYT* 1996/5/9 front-page story) YET ready to acknowledge that it has seen the evidence or the DR report? [b] If not, why not? [c] And if it says it hasn't seen any of the new evidence, then why is NGS still sticking with the 1926 N.Pole claim? Reply: since Joe Judge died, no one here at NGS is equipped to look into the matter! (But, since Judge had been pushed out of NGS in 1991, right after his Peary-case attacks on DR had led to disaster, his lamentable 1995 death seems questionably relevant. There is also here an implicit admission that the 1926 claim's official *certifying* institution is pleading non-competence at *certification*.)

T8 So, hmm, what became of NGS' alleged desire to look at evidence (NGS damage-control chapters 1&2, above: §§T3&T4), & its bold talk (above: §T5) about having the case "analysed independently"? — back in those sunny moments ere NGS realized where competent analysis was going to lead. (NGS has danced the pugnacious→cowering transformation before: *DIO* 2.2 §fn 110.)

T9 So, NGS has made the conscious decision not to (on the record, at least) look at the fatal evidence for one of *its own* MOST favorite all-time myths (evidence prominently produced by its LEAST favorite scholar), an ostrich-act which is, however, itself a piece of evidence — evidence proving beyond doubt that (before this decision was made) NGS did (off the record) gain access to the evidence, and realized it was the absolute end of the road for the Byrd 1926 claim.

T10 NGS' tactics would then naturally be: [a] Delay public acceptance (to soften the blow to its own prestige — standard p.r. priorities) by encouraging the continuation of a phony "controversy" (which is just what has happened). [b] Ensure that any public questioning of Byrd's 1926 flight be credited to anyone but DR. (This because: DR being a central critic of NGS' Peary myth, any concession that he is right about anything is intolerable to G.Grosvenor 2.)

T11 The Byrd 1926 controversy is finished for serious scholars. (The defenses shamelessly concocted by Portney¹⁷³ and Newsom are about on the level of the Flat Earth Society attempting to deal with a pseudo-paradox such as the mysterious existence of world time zones.) But the fallout is not over — simply because the Byrd 1926 claim's prime institutional defender for most of this century (recall that the American Geographical Society did not honor it) has — instead of responding openly to incoming evidence — chosen to create a classic model of non-scientific reaction. (As noted, NGS head G.Grosvenor 2 has publicly attacked DR [§T12] — without even asking to examine the evidence!) Thus, in the linked arts of sinuosity and bad-losership, the National Geographic Society continues to create the loftiest professional standards — by which envious lesser aspirants must measure their own amateurish attainments.

T12 NGS high-scholarship dep't. Focussing on the evidence-discoverer rather than the evidence, Halsey Spruance, "a spokesperson for" the National Geographic Society, emptily comments:¹⁷⁴ "Rawlins goes to great lengths to debunk the extraordinary accomplishments of famous polar explorers." By contrast, National Geographic's then-Chairman Gil-Grosvenor-the-2nd says:¹⁷⁵ "Rawlins goes to great lengths to debunk the extraordinary accomplishments of famous polar explorers." Incidentally, if brave G.Grosvenor 2 (who at *idem* tries to treat a single past error of an evidence-finder as if it invalidates all that finder's other work)¹⁷⁶ can stop hiding behind his mounds of wealth, while his family unproudly

¹⁷³See, e.g., §S, ☉4, ☉5, ☉6, and §R.

¹⁷⁴*Philadelphia Inquirer* 1996/6/2 p.K4.

¹⁷⁵*Polar Times* 2.8 (1996 Fall-Winter) p.13. Gil Grosvenor the 1st's writings were so fully ghostwritten that NGS legend reports that when G1, kid (Mel G), and ghost-writer were spotted together in NGS' halls, an insider commented: verily, there go they — Father, Son, and Holy Ghost.

¹⁷⁶Should such a criterion be applied to the goofy errors of the NavFou, to toss out all its work? Of course not. (See, e.g., ☉12. The only reason for mentioning them here at all is to remind ourselves that none of us is infallible, especially DR.)

stoops to hire snoops¹⁷⁷ and prossy-proxies (e.g., NavFou) to fight his supposedly-pride-inspired battles for him, I'll be glad to meet him in open 1-on-1 debate. In the meantime, I look forward to *National Geographic's* publication of an article appropriate to GG2's grasp of science: "Isaac Newton Once Made a Mistake. Is the Law of Gravity Still Valid?"

T13 NGS has deliberately chosen (in response to temperately-expressed hard-evidence findings: fn 170) to ignore scientific rationality on Byrd in favor of encouraging (§T11) a process of pretense and slander — i.e., to artificially prolong a now-pointless controversy that is pre-programmed to have all the dignity of a frat-house food-fight (and will needlessly embarrass the genuinely skilled and honest people who do the real work that has made

¹⁷⁷See Rawlins 1973 p.253. A more recent weird episode: The Amer Astron Soc's Historical Astronomy Division has long been dominated by DR's most dedicated academic enemy, astro-historian Owen Gingerich, who persists to this day [2000&2001] in his obsessive behind-the-back 1/4-century compulsion to defile the refereeing privilege by using it to make secret and/or anonymous libels against DR's sanity (textual details at *DIO* 4.3 ¶15 §§H4-H6) — and even honesty. (As a fanatical alibi-hallucinator for a notorious scientific cheat, OG is an odd one to attack anyone else's mental health or integrity.) To chair the 10:15-to-11:45 EST section of its 1994/1/12 meeting (Crystal City Marriott) Gingerich's HAD appointed Brad Schaefer, who wrote DR (93/9/14) *suggesting* (though not formally inviting) a DR talk, an HAD first (& last). Being the 5th of 7 scheduled speakers, DR arrived late c.11 AM. Getting into a Marriott elevator, I found only one other person in it: none other than Barbara McConnell, who's not an astronomical historian — but was chief of National Geographic's Peary-defense dep't. Clearly, she'd intended to sneak unobserved into the dark auditorium, observe DR's imminent talk (*DIO* 2.1 ¶4) and then exit just as quietly, skipping the post-DR talks — which is exactly what she did. (Hmmm. Who told BM that DR was on the program?) Gingerich's clandestine anti-DR maneuvering goes back to the dispute over whether the Ancient Star Catalog was observed at Rhodos (latitude 36°N) by Hipparchos or at Alexandria (latitude 31° .2 N) by the notorious plagiarist Claudius Ptolemy, "the Greatest Astronomer of Antiquity" according to Gingerich (*Science* 1976/8/6, echoing Neugebauer *HAMA* p.931 *verbatim*). A key reason why most astronomers side with Hipparchos: the southernmost Catalog stars are just above his horizon, but way above Ptolemy's. (Over 6°, a gap far greater than that found in any of the other large naked-eye star collections: Hipparchos, Ulugh Beg, Tycho, Hevelius.) Yet certain extremely dense atmosphere enthusiasts claim this was just due to a conveniently-dirty ancient atmosphere. Since 1991/1/14, I've known that Schaefer would eventually use this notion to launch (in Gingerich's *JHA*) an attack upon my 1982 *P.A.S.P.* pro-Hipparchos paper, since Gingerich's clandestine 1977 libel had failed to suppress it. (His entire secret referee report to *P.A.S.P.* is laid out at *DIO* 4.3 *loc cit.*) [Schaefer's huge assault finally went off at *JHA* 32.1:1-42; 2001/2. (OG's *JHA* has now multi-fallaciously attacked the original 15pp 1982 paper in 3 lead *JHA* articles, totalling over 100pp!)] Overturning Schaefer's verdict is too easy to be even called a challenge. The mountain of problems fatally gutting this latest *JHA*-salvation of Ptolemy includes ones previously noted at *DIO* 2.3 ¶8 §C14 & *DIO* 4.1 ¶3 fn 5. A sampling of new lethal items follows here. [a] Shevchenko (*JHA* 21:187; 1990) has already roughly identified about a half of the zodiac where Catalog stars lack the usual excess of 40' fractional endings in longitude, or 00' endings for Hipparchos' epoch. But at that epoch, for Gem-to-Sgr (roughly Shevchenko's range), we instead find a huge excess (1000-to-1 vs chance) of 00' endings in *polar* longitudes, which *don't change additively with precession*; so this statistical excess (like that also at *DIO* 4.1 ¶3 §§E4-E6) cannot be alibied-away by dreaming that Ptolemy just happened to use 40'-ending fundamental stars (the basis of *JHA* Assoc. Ed. James Evans' hilarious 1987&1998 weasel-ex-RubeGoldbergMachina fantasies: *DIO* 2.3 ¶8 fn 14). I.e., there is no such excess for Ptolemy. [b] Schaefer's wishful assumption of 0.23 mags/atm opacity conflicts with, e.g., Hipparchos' observation of difficult γ Ara (*Commentary* 3.2.6) and Eudoxos' sighting (Strabo 2.5.14) of extremely low α Car from sea-level Knidos (36°40'.3 N). [c] Hipparchos' only extant star collection (his *Commentary*, never even cited in Schaefer's megapaper) has a southern horizon identical to the Catalog's! Had Ptolemy observed the Catalog, there'd be a 5° gap between its south bound and the *Commentary's*. There is no gap. (Incredibly, this simple joint KP-DR discovery has never previously been explicitly stated by either side of the lengthy Catalog debate.) I.e., the entire Catalog-authorship "controversy" has been needless all along. A common apology-pretzel attempts parallel irony: both OG (*DIO* 2.3 ¶8 §C31) and professedly-independent Schaefer [2001 pp.31-32] claim that even if Ptolemy stole much of the Catalog, that wouldn't prove him dishonest! Question: if the evidence of Catalog plagiarism doesn't matter, then: why has Gingerich [1987-2001] **wasted over ONE HUNDRED *JHA* pages** upon vain attempts to overturn that ho-hum evidence?

National Geographic a justly beloved institution). So I think it's worth coming right out and saying what's really been going on here all along. Otherwise, onlookers may not understand WHY the Grosvenors have gotten ever caught-in-wringer deeper into covering up for Peary & Byrd: *the stakes are huge for NGS*. (This factor has artificially protracted both controversies to the point where apologia have long since become too weird to be taken seriously [see §T11 & Fig.11], thus the unavoidability of here exploring politico-economic forces, which are now *all* that keep the two NGS N.Pole myths even as viable as a status of "controversial".) The subterranean fear — which has invisibly dominated this controversy for decades — is that the public will come to realize the panoramic magnitude of what NGS has done: *to the finest of all polar explorers* (Amundsen), also to the many decent & productive people at NGS (who do not deserve association with an odd family's embarrassing behavior), and to the world reputation of US science's integrity (§T14). (Most observers — myself included — would have preferred to pass over all this in silence, in return for having the truth of the first four North Pole claims established. Thus the friendliness of the 1996 version of the present report. But the rich, pushy, hitherto-immune Grosvenors wouldn't have it. So . . . we're back to recalling *DIO 1.2 §A3's* pseudo-oldProverb: "Archons who won't tolerate mild criticism always get their way.")

T14 Peary's 1909 N.Pole claim was itself a tale of navigation-claims more ridiculous¹⁷⁸ than Byrd's and speeds more incredible than Cook's. NGS' certification of Peary's 1909 N.Pole fable succeeded primarily because the 1909 mid-Oct revelations of Cook's fakery (a 1908 hoax unleashed in part¹⁷⁹ by Cook's direct 1906/12/15 behind-the-scenes witnessing that National Geographic Society was bemeddalling Peary for his fake 1906/4/21 Farthest without¹⁸⁰ substantial checking of data or instrument) created a situation where the American exploring community could not encourage the world to believe that US claimants were **entirely** liars. Rawlins 1973 (p.188) on the 1909 Oct political situation: "Cook was probably a goner. If Peary [fell] too, the drop in contributions the [National Geographic] Society could expect for future expeditions would be catastrophic." But, in the panic of the hour, unethical providence ruled (*DIO 9.1 §1 fn 10*) and no one cared to reck the cost of *explicitly* (Rawlins 1973 p.221) putting US science's prestige behind a lie. So it is no excuse that those responsible in 1909 did not anticipate that approving Peary's fake would later require yet a **third** US North Pole fraud to cover for *it*. In sum: disgrace#1 (Cook's gross 1908 fraud) required disgrace#2 (rubberstamping Peary's lesser 1909 exaggeration) — which required disgrace#3 (Byrd's 1926 certification, to silence Amundsen's potential challenge to Peary: §M2). (Note: the ultimately successful two impostures — Peary's & Byrd's — occurred when US explorers had strictly US judges, an arrangement which did not happen by accident.¹⁸¹ Rawlins 1973 p.170.) **That is the triple-hoax [⊙21] legacy of the insecure geographical community's century of fearfully & greedily hocking its integrity** — by silence if not outright cooperation (e.g., Rawlins 1973 pp.289-290), in return for fiscal crumbs¹⁸² tossed it by National Geographic's ultimately (§M2 & ⊙21) somewhat fraud-based mega-wealth.

T15 Lesson: Keep ignoring a history and its like will keep right on happening.

¹⁷⁸ See Rawlins 1972 n.26, Rawlins 1973 Chaps.8-11, *DIO 1.1 §4*, *DIO 2.2*, *DIO 2.3* & *7.1 §4*, Bryce 1997 pp.420 & 517f.

¹⁷⁹ Cook had already faked an exploring claim (Mt. McKinley 1906/9/16); but he now (1906/12/15) learned firsthand that polar claims were getting gold medals & national glamour without verification.

¹⁸⁰ See fn 191 and Rawlins 1973 p.82 & Rawlins 1991 §B4.

¹⁸¹ Likewise, it's not accidental that US athletes' drug-test-bannings are generally controlled by all-US committees, who systematically suppress US celeb-athletes' positive drug-test results — on the convenient (and very US-ish) excuse that, after-all, publication might lead to libel litigation. [Lending credence to these suspicions: *Int Herald Trib* 2001/7/12 p.18.]

¹⁸² See Shackleton: p.6. [Note: Portney 2000 defends *both* NGS' independently-melting polar-hoaxes: Peary (pp.11f, speculating compass-methods despite Peary's [JP-uncited] lack of 1909 compass data) & Byrd (pp.19f). How many scientists are genuinely *that* misguided-squared?]

⊙ End-Notes:

⊙1 [Note to "Preface: DIO, Byrd, & the "North Pole" Diary".]

In Which the Byrd Center Does Some Erasing of Its Own . . .

Actually, the Byrd 1925-1927 diary had been discovered in 1985 by OSU's Peter Anderson. (Newspaper articles at the time explicitly noted that it included the 1926 entries. For details & Anderson's account, see Dave Lore's revealing piece in the *Columbus Dispatch* 1996/5/10 pp.B1-B2.) But the diary was then conveniently "lost" — à la the RGO-Neptune file.¹⁸³ A quietly skeptical Lore quotes Anderson's explanation (*op cit*, emph added): "Former Byrd Center staff member Peter Anderson, who negotiated the deal [with the Byrd family, which brought the Byrd papers to OSU], *certainly knew the diary was Byrd's North Pole flight log*. He talked freely¹⁸⁴ about the diary during 1985, and the volume was mentioned in several newspaper articles. 'I had read it,' Anderson said yesterday [1996/5/9]. '**I just didn't know the importance of it.**' It was only lost, Anderson said, after it was *turned over to students* who were organizing the Byrd collection." So one wonders just how "coincidental" it really was that the Byrd 1926 diary should re-surface (1996 Jan, according to *Columbus Dispatch* 1996/5/10&15) only *just* after representatives of the University of Cambridge and of *DIO* had jointly doubted the Byrd 1926 claim in public in *DIO 4.3*, mailed out at the very end of 1995 Dec. (Note: Anderson's involvement is entirely skipped over by Goerler at G 5.) In any case, two facts are beyond question:

[1] Goerler's 1996 statements, before data-alterations were found in the diary, show he believed the contents of the diary would help Byrd's claim. (He may still: fn 4.)

[2] It turns out that many of the xeroxes & faxes of the diary's 1926 entries that were sent to DR in 1996 Spring were actually made not from originals but *from a set of old photocopies*. Using a mirror, one may read the print which was stamped upon the back of all these key photocopies — because the stamp's ink seeped through to the paper's front. The stamp consistently says (all words capitalized): "From The P. J. Anderson Papers"! A photocopy of the entire diary has long existed in this state. Same for the barograph, both "flight-chart" photos, the Hinks-line curvature correctors, part of the *Chantier* log, the NGS report, and all versions of the typescript. Wherever these latter documents are reproduced in the Goerler book, OSU Press has attempted total obscuration of the Anderson stamp, by artful whiteout and-or peculiarly light reproduction. So, we now know of even *more* erasures here, in a case with rather too many erasures already. (Yes, this index-less book is *not* the proudest production in US archival history.) Nonetheless, close scrutiny will just bring out traces of the Anderson stamp on some of the book's reproductions, where it appears as a faint fragment-blemish, matching perfectly the position and shape of the much darker and occasionally quite legible Anderson stamp-print on the photocopies in my possession. Look very closely at, e.g., G 144 (lines 8-9), 147 (§1 lines 2-3), G 151 (§10 lines 6&7), G 158 ([barograph] between curved lines for 13:51 & 14:52). Question in passing: **where are the originals** of the barograph, NGS report, typescripts, etc? — which presumably would have been used for the book's illustrations (instead of the Anderson copies), *if available*. Other documents (even those of rather trivial evidential import) are uniformly cited to a folder in the Byrd Papers, in the manner "BP, folder *xxxx*"; but, notably, nothing is said (besides the false implication at G 161: "reports . . . and records . . . Other primary sources") regarding the whereabouts of the NGS report, typescript, & barograph (which comprise Goerler's

¹⁸³ See, e.g., *DIO 7.1 §5 §A* vs *DIO 9.1 §1 §§L8-L9*.

¹⁸⁴ But did other polar societies and scholars ever see it? How long was it available before disappearing again for another decade?

extensive Appendix B), ship's log, or "flight-chart". (The 1926 flight's certifier, National Geographic, has always claimed it can't find any unpublished material in its archives: fn 22 & 189.) For the last item, we have only xeroxes OF photos OF the original. Embarrassment over not being able (or allowed) to specify the originals' location may also explain why at G 142-145 the handwritten initials of NGS' trio have been shaved off all four pages' left sides. Possible reason: on the 2nd & 4th pages of the NGS report (G 143&145), Anderson's photocopy had accidentally clipped off the left edge of the initials of Bumstead & Avers; thus, reproduction of these important but mutilated manuscript initials would reveal that OSU's "archival" book was often using mere photocopies. (And: I don't recall noticing Anderson's name even mentioned in Goerler's book.) This might then raise the question of how Anderson's diary photocopies were simultaneously "lost" for the same decade as the diary, etc. Summing up what the foregoing tells us: First, we now know that students did not "lose" the diary before a set of photocopies was made of it. Second, must we now believe that this set of diary photocopies was ALSO "lost" for a decade? — synchronistically along with the original diary? I.e., are we supposed to believe that Anderson lost his own "The-P.-J.-Anderson-Papers"?! Which then got re-"found" a decade later, *just* when the diary did? (Yet another parallel with the Neptune affair: fn 183.) Given this revelation: should the Byrd Center's lost-for-a-decade tale now be taken any more seriously than Byrd's N.Pole fable? How credible is it that the holiest document in the entire Byrd archives would be turned over to a bunch of youngsters and "lost" for over 10⁹? — even aside from the fact that a photocopy existed all along. Tangled-web: how did a whole collection get "lost" but only one item was later "found"? If lost together, they'd be found together. If lost separately, then we must posit (as in fn 109) *two distinct sets* of OSU student-morons. And professional librarian B.Rawlins asks: is it normal archival practice at reputable universities to have an archivist make a photocopy of such irreplaceable material **just for his private papers?** — and STAMPED as such. (Why the *private* stamp for what were — for a few 1985 days — actually *publicly* known records?) If the purpose of photocopying was to protect the papers against loss, then: why the ten years of silence after the original's Lamentable-Loss-to-Student-Carelessness? Why was no other photocopy-set ever made in the interim? It looks alot like an "intervention" occurred in 1985. (Which, for another coincidence, was about the same time National Geographic was wrestling with the Peary family over the sticky matter of releasing the Peary papers.) The entire polar community deserves to know the truth: who did the intervening? (See similarly at ☉3.) Bottom-line: Ohio State Univ — eminent home of *BOTH* of history's clumsiest N.Pole-fakers' papers (§N3) — has some de-smell Explaining to do.

☉2 [Note to §A1 & §N6.]

The Undropped Flag-Blizzard.

The significance of Byrd's un-dropped dozens of flags was 1st raised at Rawlins 1973 pp.263-264. Molett (1998 p.63) & Goerler (G 54 n.17) alibi that Byrd feared flags dropped on the Pole mightn't be seen even if he'd succeeded. (Note also ☉12 here.) But, then: why did he bring them?! (If non-visibility genuinely concerned Byrd, then he could have dropped just one spectacular flag, and no one could complain if it *weren't* seen. Catch: what if it *were* seen [by some sharp *Norge* eye, 3^d later] somewhere around 87-88°N . . . ?) Since loyalists don't do justice to the contexts & impact of Rawlins 1973 p.264 (though Goerler, to his credit, cites the page), let us quote from it here: "anything dropped onto the ice at the turnaround point of the [*Jo Ford*] just might be spotted a few days later by one of the [*Norge*] dirigible's observers, thereby verifying that point's approximate latitude. (Drift is only a few miles a day in the central Arctic Ocean.) In the context of this [then] unique [N.Pole] opportunity, it is therefore particularly distressing to learn of a correspondingly unique though long-forgotten aspect of Byrd's expedition, namely that it was the first and probably the only one in exploring history to claim a Polar record without leaving a national flag at the [allegedly] conquered goal. . . . [Yet] Byrd had had the patriotic concern to stuff the [plane] with a record number of Star Spangled Banners for dropping upon the Pole:

'a hundred small and several large American flags.' [NYT 5/11:1:7=8] But somehow, the flyers had been too-busy¹⁸⁵ to remember to let fall this red, white, and blue blizzard — and the banners all ended up right back in Spitzbergen. Byrd later explained his failure to beflag the Pole somewhat differently: 'Peary had done that.' [NYT 5/16:4:4=5] Byrd did, however, possess sufficient presence of mind to remember to take still photos and movies of the monotonously typical Arctic Ocean ice below, during the [claimed] 13 minutes spent circling the Pole.¹⁸⁶ It is odd that the publicity-savvy Byrd would, [during] all this time, not have thought of the spectacle United States audiences would be most eager and proud to see on film: the American ensign being laid upon the North Pole of the Earth. Byrd's book *Skyward* [ghosted by Fitzhugh Green: see fn 18] omitted all mention of the flags in the fusilage — while spending an entire paragraph on a ukelele 'secreted aboard by the notorious "Ukelele Ike" Kontor' [Byrd 1928 p.182]." Goerler suggests (☉12 or G 53-54 n.17) that the flags were preserved as souvenirs for backers! (Just imagine what observers would have thought if Scott found nothing of Amundsen's at the S.Pole in 1912, but Amundsen said: well, actually, we took our tent and flags home with us — to give to backers as souvenirs. . . ?!) The souvenir-alibi is contradicted by the jist of Byrd's Peary-did-it comment (above) and of his own reporter's NYT 5/11:1:7=8 story: "Commander Byrd established an exploring record by not dropping flags. When he reached the Pole he was too busy taking observations and worrying about the leaking oil tank to think about flags, although he had a hundred small and several large American flags stored in the plane." (Again: Byrd says he remembered to take movies.) My summation-thought on this matter is similar to what might be said here regarding a dozen other pieces of evidence that undercut Byrd's fraud — and it is advice which I have offered, almost always vainly, to a wide spectrum of cranks (especially the Cook kooks): whereas the evidence in question (the flag-omission, in this case) may not be absolute proof that Byrd hoaxed (not as powerful as the lethal time & sextant contradictions here at §§D6 & E8, resp) nonetheless: if I were just coming into the Byrd 1926 affair, hoping to defend Byrd's claim — and then learned of this, well, I might not instantly lose all belief in him, BUT, I wouldn't spend alot of my time defending such a shaky contention. I'd find something more likely-productive to do with my life. And, in a case with this many independent indicia — ranging all the way from fuzzy hints to a cluster of secure locks — one has to wonder if a creditable theory can suffice any longer to explain the behavior of Byrd's microscopic band of non-family defenders. One can safely anticipate that devoted Byrdites' predictably inevitable never-say-die attacks on this report will follow the diversionary example Goerler has set — and will thus cavil disproportionately at such periphery as, e.g., the undropped flags and §J6's Verlegen leg — because the lethal central scientific contradictions summed up at §N1 are too unambiguous to be sanely opposed. Byrd-defenders may also be tempted to comment on the length of this report (with more text than Goerler's book, this has itself become a book: DR's 3rd) as if that proves something helpful to the Defense cause. E.g., if Byrd's guilt is so obvious, why does it take so much space to prove it? (Obvious non-cult answer: the arsenal of evidences of that guilt is so immense that it takes awhile to detail the major items.) Those who make it a religious practice to defend suspicious claims seem to be missing something between the ears (even aside from the usual content): an elementary alarm-system mental Bell, which I'd have thought came bundled — standard — with every human. A normal person should have the built-in common-sense awareness to intuit that, if you spend years defending a claim that has lots of suspicious aspects — even though final proof may not

¹⁸⁵ So said Byrd's *NYTimes* reporter at *idem* (quoted here below) — a passage which Byrd's imaginative current defenders of course ignore.

¹⁸⁶ See *idem* & Byrd 1928 p.197. Filming unmentioned at Fig.3. On the improbable but awful chance that the *Norge* inadvertently photographed Byrd's "Pole" ice far south of the actual Pole, the ever-cautious Byrd swiftly laid the groundwork for explaining how it might be OK to say his 88°N ice was at-the-Pole: Byrd at NYT 5/17:4:3=4: "movie camera . . . film [sometimes] jammed . . . I wonder how those pictures are coming out, if at all? I took a movie of Bennett at the wheel . . ."

yet be known (& mayn't ever be known) — you are taking an awful risk. For, solid proof MAY later arise. (Look at what Byrd's 1"-precision slip-up [§G6] will do to his loyalists, none of whom noticed it, just as none recked what would happen to their own credibility if Byrd left such simple giveaway proofs of his fakery.) And it may doom your work, perhaps your life's main work. That is why an investigator should never treat incoming evidence with a loyal, stubborn, reluctant-to-admit-error attitude, asking merely: have I been utterly & unequivally *proven* wrong? He should rather consider, with genuinely, conscientiously humble open-mindedness (*DIO* 8 ¶5 §B4), whether this evidence simply *favours* his side or not. For, such indications (or just mere hints, even if far short of decisiveness) are the clue to *his future* on the issue (☉21) — that is, to whether eventually-appearing stronger evidence will vindicate or overturn his belief. Just having the care to face such realities at *present* can save **the rest of one's life** from grief: disappointment, unpleasant surprise, and (most important of all) wasting years — even decades — of frustration, chasing a chimera of vindication that can never come. Applying the foregoing to the present case: the new Byrd diary evidence was not released into a vacuum — it fell upon a controversy where there already existed three competing theories: [a] Byrd didn't try, [b] Byrd tried but fell short, or [c] Byrd succeeded. A wise scholar doesn't just stick to the theory he came in with. (In my case, a very tentative [a], which I of course jettisoned in light of the new data. I was disappointed to see Newsom's 1997 ms [p.1] attempt to treat DR's responsiveness to new data as a negative [Portney does likewise] — rather than seeing it rightly as just an expression of integrity, as well as a decent attempt to set a good scientific example, an example which has obviously made no impression whatever upon Newsom's own record-setting invincibility to new data: §F3.) The intelligent investigator simply asks (regardless of previous commitment): which theory is most consistent with the new diary evidence? Anyone who can't see that theory [b] is now preferable is either a hater or a worshipper-cheerleader on the subject of Byrd — but he is not a philosopher of science.

☉3 [Note to §A7 & fn 109.]

NP Fictional Productions.

Byrd's 1925-1927 diary was the most precious real documentary record of his life, proving that (whatever his shortcomings) he was in truth a hero. How *could* he destroy it? Pseudo-neutral archivist Goerler argues (G 57-58, emph added): "the very existence of the diary supports Byrd. If it had contained evidence that he knew *at the time of the flight* that he had not reached the North Pole, he would, if deceitful enough, have destroyed it. Instead, Byrd published two references to the diary after the flight [*National Geographic* 1927 & Byrd 1928]. *Repeatedly* announcing the existence of the diary hardly seems like the actions of a person who believes that it contains something that needs to be hidden. Even Byrd's erasures *can be read without assistance*, further evidence that he made no effort to conceal." Comments: [a] In a context in which a claim is being torpedoed by hard-number discrepancies, fabricated raw data, and faked overprecision, it is courtroom-level artful to resort to soft evidential arguments. [b] Byrd's erasures were unquestionably a determined effort at censorship (at least until he realized that totally safe obscuration was hopeless), a serious effort at destroying key raw data, the ultimate crime in scientific reportage. (This destruction is consistent with one striking aspect of TD, the only version of the official report distributed in 1926: *it contains no raw data whatever*. See §G6. The nearest is an average of the alleged raw "Pole" sextant data: fn 68.) Contra Goerler's ridiculous claim of easy legibility, some of the erased raw data are now extremely difficult to read.¹⁸⁷ Which is the real reason why Goerler spent months trying to get X-ray analysis of these pages. Also, G 3: "I had the diary photographed by means of ultraviolet light to make certain that all erasures had been detected and read properly." The original papers of Molett and

¹⁸⁷The "5" of the "45" arcseconds on D 11 (Fig.3) is virtually invisible in the book's reproduction at G 84-85; consultation of notes taken during my 1996/4/12 examination of the original diary provided the only way I could recover this datum for use in our Fig.5. See §G6.

Newsom both initially misread digits on D 11 (Fig.5 or G 84-85), where reside the most fatal data-discrepancies — and thus the most determined attempt at erasure): Molett's 1996 paper p.1 believed 19°22'54" (the true solar altitude in Fig.5) was 19°22'44" (Byrd's pre-strikeout version of the datum), while Newsom's 1997 paper p.8 initially saw 19°25'30" (the apparent solar altitude in Fig.5) as looking like 17°25'30", though Newsom creditably admits (before his p.8 is done) that DR's interpretation was right. There's now no dispute over any of the key readings. However, Goerler's book continues its pretense that the sextant data are unimportant by providing none of the data in the text, even while he does provide [at G 77-79] clear text for the verbal radio messages. He just states at G 77: "Navigational calculations are not included in the transcribed text but appear in the [often-faint] illustrations." I.e., the most important empirical data in the book are not included in the text! [c] Goerler's argument (that the mere existence of the diary is pro-Byrd) exemplifies the use of a classic HIWTYL Gambit: Heads-I-Win-Tails-You-Lose. (See similar alibiing for Dr. Cook at *DIO* 7.3 ¶9 fn 17.) If a faking explorer destroys his original records, his defenders will try arguing (option 1) that not enough evidence exists to justify doubt. (This OSU-desperation alibi was actually tried out on DR.)¹⁸⁸ But if he instead leaves-behind records proving his fakery, defenders switch instead to the (option 2) alibi that if the data proved anything unfavorable he would have destroyed them. (Note: had Byrd not erased the incriminating data, the very same advocates who now use his erasures to justify scrapping these data, would instead be arguing that non-erasure proves they must be innocent errors — or he would have tried to erase them!) Maximal irony here: in all of serious exploring history, the 1926 Byrd case is THE least-appropriate possible instance for trotting out the HIWTYL-Gambit, for: **Byrd DID [1] try to destroy — by eraser — specifically the indicting parts (only) of the 1926 diary BUT ALSO, due to eraser-inadequacy (and-or irrevocable seepage of lead into paper), [2] he left the damning data anyway.** I.e., the Byrd 1926 situation provides us the almost-uniquely felicitous and ironic evidential situation where *both* HIWTYL-Gambit-exits [1]&[2] are closed. [d] As for Byrd's twice (identical wording, so basically once) later mentioning the diary, this may well have been based upon careless remarks to his 1927 *National Geographic* ghostwriter. (He and the Grosvenors all used *NatGeogr* ghosts. See fnn 18&175. Speculation: did mention of the diary lead to questions by AGS? — and thus to the abortive effort to erase data? See §G6.) [e] For the (essentially successful) 1927 *America* flight, Byrd directly quotes *numbers* from the diary. E.g., compare D 16 (G 115) top line to Byrd 1928 p.252. (Note his exaggeration: ☉6.) But, since he cited 1927 data, then there's an obvious question here: how come he never published any 1926 raw numbers from the same diary? [f] There is an oddly complicated phraseology in Goerler's above-quoted advocacy: he wonders why Byrd would preserve the diary if it showed that he knew he'd failed "*at the time of the flight*". Why didn't Goerler just say far more briefly: "if its data showed he'd failed"? Fact: in 1996, Goerler repeatedly stated (☉19) that his disagreement with DR was over whether Byrd *knew at the time* that he had failed. The appearance of similar language in his 1998 book (see also G 56) suggests that he still privately disbelieves Byrd's success, but (except for this slip) isn't letting on publicly — even while he is deeply involved in a propaganda campaign that is attempting to fool the world into accepting that the diary-vs-official-report data-discrepancies *do not at all affect* the status of Byrd's 1926 claim. I.e., Goerler says (to the public) not merely that the claim is still viable. He nowhere in his book admits or even implies that the finding of

¹⁸⁸By Beth Tierney (1999/2/9), defending Nancy Porter Productions' (at-best innumerate) decision to commit PBS's "American Experience" (1999/2/8) to the position (§R) that the Byrd 1926 diary proves nothing at all! (See also Newsom at ☉13.) Even if one were so dim as to believe that nothing could be gleaned from the rich evidential lode Byrd left us in the diary, would it not occur to someone at NPP (or at "American Experience", or at PBS) to ask: [a] Just *whose* responsibility & loss would it be if enough evidence didn't survive to evaluate Byrd's claim? [b] Does anyone (but National Geographic) accept a claim for no reason but (NGS thinks): it can't be positively *dis-proved*? (Recall ☉2.) Every scientist knows that the burden of proof (for any scientific claim) is upon the claimant, not the skeptic.

suppressed southward-placing sextant data has even *slightly* decreased the probability that we should continue to accept Byrd's claim. (This spectacle is on the level of the Leakeys hanging out with a cult of creationists to announce that creationism is still defensible — and that the Leakeys' finding of Australopithecus proves nothing about evolution. Or of Bessel saying that his discovery of parallax is worthless — and joining a colony of reactionary geocentrists. See *DIO 1.1* ¶7 §F2.) To watch Goerler believing that he must say that his own greatest discovery has no scientific value is reminiscent of the Galileo case — and raises the same question: what forces, inducements (or, at the least, misleading advice) must be brought to bear on a person, to cause such behavior? Note that in the Goerler book: [a] Peter Anderson's actual 1985 discovery (⊙1) of the diary never happened. And [b] Goerler's 1996 re-release of it doesn't change the 1926 claim's status. Orwell could write a book about these sorts of mentalities. Come to think of it, he did.

⊙4 [Note to §C4.]

Drift-Indicator: Aim vs Distance.

The d-i method of finding distance is very nicely illustrated at Portney 1973 p.215 (his Fig.6). See also TA 1 (G 149), Byrd 1928 pp.190-191 & 247-248. Molett is now irrevocably committed to his main argument (Molett 1998 p.62) against accepting the 7:07 sextant shot: he cannot acknowledge that Byrd could have gone so many miles, up to 7:07 GCT, without knowing his speed via the d-i data we see in the typescript. But this argument is a feeble reed to begin with. And it goes right into the wastebasket when we find that, unfortunately for Molett, these alleged d-i data are [a] absent-from the diary (also: the diary never once mentions either a barometric reading or the plane's height — the very basis of d-i math), and [b] outright-contradicted by the diary: §C4 & Fig.3. (See also the irony of disappearing typescript height-data: ⊙7.) Molett (1996 ms p.2) argues that, since DR "concedes" Byrd flew straight north by d-i, this implies that DR must additionally agree that Byrd also found distance (and thus speed) by d-i. This contention is so obviously false that one wonders how it could have been published anywhere. Comments: [i] The "concession" on steering (actually just a delighted, frank appreciation of Byrd's accomplishment) was based upon the very diary sextant data (§E5) which Molett rejects (§§L4-L8). [ii] While *aiming* by d-i was not at all subject to barometer-uncertainty, *distance-gauging* was (by contrast) severely so. As even NGS agreed. See G 143 for the NGS trio's explicit statement: the d-i will indeed *steer* one accurately, but the d-i based "estimates of speed may be subject to large errors". [iii] Byrd's diary dr was not d-i but strictly Fig.3 airspeed arithmetic: §C6. Faced with this contradiction, increasingly indiscriminate alibi-fount J.Portney has tossed up the notion that Byrd computed in Fig.3, by the 85 mph constant speed assumption of §C3, merely in order to find windspeed by deduction. But, if Byrd allegedly knew his groundspeed via d-i, then subtracting 85 mph from this would have instantly found what he wanted: no need to do Fig.3's multiplications: §C6. And Byrd's computed post-9:15 conclusion [Fig.3: "20 miles to go"] shows he was using *non-drift-indicator* data, since his typescript d-i figures put him well past the Pole at that time. (See §C11 or TA 7 = G 155.) [iv] The difference of actual 70 mph vs Byrd's assumed 80 mph is under 15%, but Byrd was (since d-i-based distance depends on height) uncertain by over 15% if he could so casually alter¹⁸⁹ 3000 ft (TA) to 2000-3000 ft (TD): fn 200 ! Thus Molett's already ludicrous objection to accepting the 7:07 sextant shot is directly undercut by Byrd himself. I.e., even if Byrd had been making occasional d-i estimates, these mightn't (as we also see from §E10) have been very obviously inconsistent with the plane's actual 70 mph speed. But, again, keep in mind: no raw data survive for these alleged d-i distances (fn 107). Now, since it is hard to find anything that can boost a Byrd-Defense argument, I'm happy to point out something the

¹⁸⁹ The alteration was not previously known because NGS refused to produce the original TA version of the typescript: NGS to DR, 1968/7/23, claiming that NGS did not have it — this despite the US National Archives' 1968/6/19 letter to DR stating that NGS had just told the National Archives that NGS did indeed have "some charts and the 'original' copy of the Navigation Report." [See fn 22.]

Defenders themselves haven't noticed, even though it might seem to be in their own favor: the atm pressure probably increased going north, and this introduced a systematic factor that would have led to Byrd's putative d-i measures being underestimates. So, at groundspeed 70 mph, he might have gotten 65 mph from d-i timings, which would indeed have alarmed him. If it happened. But there's no evidence that it did. Thus, such considerations merely add to the already powerful¹⁹⁰ evidence showing that there were no d-i distance-estimates at all beyond Spitzbergen, and that Byrd's d-i work rightly concentrated on aiming, since distance was (§B8) so much more reliably gauged by sextant — and without cumulative error. Byrd (*NYT* 5/16:4:2=3): "I could not let up for a single minute. I was putting every ounce of my mind, energy, and knowledge into making our course so straight that we could not deviate half a mile from the imaginary line leading us on toward the goal." Credible and admirable. (Even if the precision is enthusiastically overstated.) But he goes on at *idem* to allege even more ("continuous checking of drift, speed, and course" see also *NYT* 5/15:3:6=7). Anyway, TA conflicts with the 85 mph speed by not only suppressing this figure (prominent in his diary calculations: Fig.3) but by alleging a mean northward speed of over 90 mph (§N1). In light of this and our §O, the reader can make up his own mind on the believability of all of his post-diary claims — in the light of TA typescript d-i figures & sextant data that all consistently have him at the Pole at least 13^m before his handwritten diary (Fig.3) places him 20 miles short of it: §§D6&N1.

⊙5 [Note to §C5.]

Byrd's Scientific Equipment and National Geographic's Exam.

Reproduction of the barograph record is at G 158. The barograph was found (by the photocopying person cited in ⊙1) in Byrd's papers, along with Byrd's copy of the NGS report. I am not certain whether Byrd's NGS judges saw the barograph record. It is obvious that they didn't see the diary, despite their claim (G 141) that they "examined the original records of Commander Byrd". Note: [a] the diary proves (§G7) the original-record "flight-chart" was a post-flight fabrication, and [b] NGS pleads temporary amnesia (Rawlins 1973 p.270) about the location of its records of this perfunctory examination. (Though it can't find its records — when skeptics ask for them at any rate [fn 189] — National Geographic can always find go-along Experts who'll sign onto just about anything they're asked to. But: will observers ever wise up and refuse to accept claims where the records are hidden for decades? Apparently not: see Portney at §S11.) Thus, we are justified in being skeptical of NGS' claim (§G6) that it also examined Byrd's sextant. Keith Pickering asks: how could a meaningful exam occur when, by Byrd's own account (⊙8 & Byrd 1928 p.202), the sextant was broken during the trip? (Not that a mere broken sextant ever stopped a medal-wielding¹⁹¹ Grosvenor One whenever he got within pinning-distance of any popular explorer who might juice his magazine's circulation.) Further: Byrd's 1st written story (with a classic generous-hero-touch) concludes, "I was anxious to do all I could to help¹⁹² Captain Amundsen and Mr. Ellsworth. I . . . gave them my sextant . . ." (In this final sentence¹⁹³ of his long *NYT* story, Byrd goes on to cite lots of other equipment he gave them. He modestly doesn't mention the shaft.) So — how did the NGS committee examine the broken *Jo Ford*-flight sextant, which may have been given away? If we assume that Byrd gave Amundsen a different sextant (a reasonable thought, since a broken sextant wouldn't be of much use), then that only re-enforces another suspicion: is it credible that Byrd, one of

¹⁹⁰ See §C4 and Fig.3. Question: How could Byrd (for at least the first 6^h north of AI, that is: until well after 7 AM GCT) have stuck with his 80 mph dr (eq. 3, §O5, fn 136), if he really was taking frequent d-i data showing a very different speed? — *either* the actual 70 mph (§I) *or* the TA-claimed 92 mph (fn 139).

¹⁹¹ See *DIO 1.1* ¶4 §B3 on the damaged state of Peary's sextant for his claimed 1906/4/21 "Farthest North", which (fn 180) got him an NGS gold medal on 1906/12/15.

¹⁹² Try re-reading this, in light of what Byrd had just done to the life's dream of Amundsen: §A5.

¹⁹³ *NYTimes* 1926/5/17:4:5=6.

the most cautious of explorers, wouldn't have brought this 2nd sextant along on the 1926/5/9 flight, as a backup spare? (He had a spare of every other compact navigational instrument And he took a spare sextant in 1927. See ☉8. All of this just casts further suspicion upon an already suspiciously-convenient claim: that "the" sextant fell *just* after his turnaround. If he really had a 2nd sextant, then: why wasn't it used during the return?) B.Rawlins wonders aloud whether — assuming the NGS' alleged sextant-exam occurred at all — it was anything more than gee-that-sure-is-a-sextant. One is reminded of one of NGS' least glorious moments, when it came out at a 1910 Congressional hearing on Peary's claim that the equally *pro-forma* 1909/11/1 NGS exam of his sextant occurred indoors in the baggage room of a railway station, where no celestial observations with it could be attempted. See Peary's reluctant testimony at Rawlins 1973 p.179. Not that this caused NGS' 1909 hero any problems, since the bill to promote Peary as Attainer was pushed by "a paid lobby that . . . haunted the Capitol . . . in the interest of the bill" (*idem* p.239; also pp.213-223, and Bryce 1997 p.522). One can be reasonably sure that the 1926 trio didn't shoot any celestial object with the allegedly-examined Byrd sextant, either — or the NGS committee would have been directly confronted with the overprecision of the raw sextant observations alleged in the TA report: §G6. (Note that none of the NGS trio were navigators or explorers. We recall that Peary's lobby went to alot of trouble to rig his congressional exam similarly. See Rawlins 1973 p.215.) Anyway, whatever sextant it supposedly examined wasn't cited in the Byrd 6/22 or SecNavy 6/23 cover letters (G 147-148) when TA was sent NGS (allegedly opened at NGS, 6/23 10 AM: ☉17, fn 127, and §M5); so it is obvious that, whatever the instrument, it physically accompanied Byrd on his hours-long NYC-Wash train-trip¹⁹⁴ and thus could not have reached the NGS trio until several hours after the NYC tickertape parade had already made Byrd an unassailable national Hero. Byrd arrived in Washington at 19:30 or (fn 125) at 19:45 EST, only "a comparatively few minutes" (Rawlins 1973 p.268) before the NGS medal-presentation — minutes which Byrd expended in his hotel, not at NGS (where the only committee he planned to meet that evening anyway was the movie operators: fn 125), as noted in the *NYT* 6/24:1:8 story, a story that mentions "records" having already been received&approved, but makes no such remark on instruments. (The Byrd 1926 sun-compass is on display at NGS Explorers Hall. But neither his nor Peary's "North Pole" sextants are now uniquely identifiable: the non-extant sextants.)¹⁹⁵

☉6 [Note to §C9.]

OSU Expert vs OSU Expert.

Byrd-defender Molett 1998 p.61 denies Byrd's use of starting-time 00:40 in dead reckoning math, saying that DR's assertion that Byrd computed speeds using his takeoff time is "utter nonsense".¹⁹⁶ When Molett's error was pointed out by K.Pickering, Molett (at p.10 of the KP-vs-WM exchange in *Mercator's World* 1998 Sept-Oct) kept insisting anyway that "Byrd never used the takeoff time at any time in his dead reckoning." But Byrd's estimated takeoff time (00:40 GCT) is the only Spitzbergen-area time-datum that appears in any explicit (though see §O) dr calculation in the diary: twice (written as 12:40) at d 3, our Fig.3. And Goerler does not let his readers in on the differently entertaining fact

¹⁹⁴ Originally planned as a quick plane hop (*NYT* 6/22:22:8) but eventually done by train, apparently due to weather (NARA RG 80 29455 95:17).

¹⁹⁵ Another odd coincidence that is worth reflection: both Peary and Byrd were able navigators by sextant; but each man, once he had faked a sextant-guided trip to the N.Pole, never made another (surviving) sextant observation for the rest of his life. See fn 4.

¹⁹⁶ Which creates yet another hilarious pretzel in the Byrd-defense effort, since this attack also inadvertently brands as Ut-Nonsense the G 54-cited Byrd-alibis of Portney 1973 p.213 Fig.4, which likewise computes speeds from the takeoff time: "NAUTICAL MILES MADE GOOD FROM KING'S BAY TO THE NORTH POLE". Oddball note: Portney has lately begun privately attacking DR for using the takeoff time, so his slavishly-indiscriminate hope to denigrate NGS' top hate-object has inadvertently led this peculiar once-President (1989-1990) of the Inst. of Navigation to self-classify *his own* Goerler-cited (G 160) 1973 paper as "utter nonsense".

that the foregoing puts Molett at unresolvable odds with G.Newsom, the other defender-against-sextant-data adduced by Goerler at G 57 n.19 — since Newsom's *entire* basis for his OSU-loyal conclusion (*idem*) for Byrd getting within "tens of miles" of the Pole is¹⁹⁷ the very Fig.3 diary data & takeoff-time-based (§C6) math which Molett keeps on rejecting¹⁹⁸ — in favor of the typescript d-i distances, which Molett (§L4 & fn 107) takes as firm, in spite of the NGS sub-committee's clear statement (☉4) that they are unreliable.¹⁹⁹ Meanwhile, the crude figures Newsom has gossiped say (even if one can accept them — thereby ignoring the quadruple-contradiction they're gummed up in: §N1) that Byrd only went (see Fig.3) 722.5 mi + 20 mi = 742.5 mi north of Kings Bay, though the full Kings Bay-Pole distance is 768 mi (fn 24) north of there. I.e., when the reader is told at G 57 n.19 that Newsom-Goerler think perhaps Byrd got within "tens of miles" of the goal, no warning is imparted that this isn't a statistical matter, a symmetric Gaussian standard-deviation envelope, but rather it's just a simple arithmetical *shortfall*: over 25 mi short in this case.

☉7 [Note to §C16.]

Unsteady Heights & Byrd'sSuppressions of Them.

Byrd makes his turnaround-height only 3000 ft (TA 8 = G 156) — and even this he later reduced to merely 2000-3000 ft.²⁰⁰ But his actual turnaround height can be estimated from the barograph record (G 158), on which Byrd's meteorologist Haines recorded Kings Bay sea-level $P_o = 30'' .5$ (1033 mb), $T_o = 14^\circ\text{F}$ (-10°C), and wrote (right on the barograph's zigzag pencil-record of the flight) the height corresponding to each major extremum, figures which show that he effectively used a formula $z = 60600' \log[30'' .5/P]$ (where primes signify feet; double-primes, inches of Hg). He used the same formula to compute the $z = f(P)$ relations listed on the narrow strip of barograph paper shown at the bottom of G 158, a strip which Haines evidently used physically (like a ruler)²⁰¹ for the values he wrote on the barograph record. Haines seems to have estimated (G 158) 4300 ft for the peak height near 9:30 GCT (the figure we adopted in the 1st edition of this report), but that value does not agree²⁰² with his formula.²⁰³ But sea-level pressure P_o could have been

¹⁹⁷ See pp.6&10 of the Newsom 1997 paper cited at G 57 n.19, or see below at ☉13.

¹⁹⁸ See above in this section, likewise on p.3 of the 1996 ms he sent to OSU!

¹⁹⁹ Typical of everything in TA, Byrd's d-i values are remarkably smooth: the biggest of all the variations in successive hourly speeds, N or S, is just 6%. See fn 139. By contrast, the only two values in his real 1927 log's groundspeed-data column differ by more, 94 mph for the 1st hour vs. a doubtful 102 mph for the 2nd hour (☉12). And Byrd stretched the top-line diary airspeed 89 mph (D 16 = G 115), reporting it as 100 mph at Byrd 1928 p.252, to match his estimated mean-speed figure (also upped) on the same page: the very sort of smoothing-alteration which Molett denies Byrd would bother with. (See Molett's full Why?-wail at ☉16.) The educationally ironic outcome of the 1927 fudged-datum is detailed in fn 107.

²⁰⁰ TA 8 had 3000 ft (G 156), while TC 5 makes it (handwritten): "Altitudes [plural] between 2000 and 3000 feet." Typed final edition (TD 6): "Altitude [singular] between 2000 and 3000 feet."

²⁰¹ The other (wider) strip at the bottom of G 158 is founded upon a similar formula (explicitly for 10°F): $z = 58140' \log[30'' .5/P]$. He didn't use it, though its underlying T_o ($10^\circ\text{F} = -12^\circ\text{C}$) is much nearer 14°F than is the 30°F basis of the strip he did use. (Formula#2 [wide strip at G 158] gives $z \approx 3900'$, as does Babinet's formula [Bowditch 1926 p.32].) But we here adopt Haines' chosen formula, in order to err on the generous side with respect to Byrd's turnaround height and thus distance seen. Accuracy in the figures Haines was entering (onto the barograph record) seems to have declined as he went along, evidently from slight but systematic vertical displacement of his little narrow scale-strip: this caused some of his height-estimates to be roughly 5% on the high side.

²⁰² Errors' cause suggested at fn 201. For turnback-pt $P = 26'' .1$, Haines' formula gives $z = 4100'$.

²⁰³ Byrd's starting sea-level temperature was 14°F (TA 3 = G 151; from Haines: G 158) or $-10^\circ\text{C} = 263^\circ\text{K}$. Note that the standard atmospheric vertical gradient ($-6^\circ .5\text{C/km}$) predicts -2°F at 4100 ft (for 14°F ground temperature), so that Byrd's 0°F "Pole" temperature at this height comes out virtually the same as that implicit at 4100 ft in the column of air over Spitzbergen: ☉7. If Byrd's first rough draft of his report admitted he was about 4000 ft up at turnaround, then this match might not be accidental. (At *NYT* 5/16:1:1=2, he mentions being at 2000 ft *en route* north, with $T = 8^\circ\text{F}$ [miscalling it 22°

nearer 31'' at the turnback point, so the barograph readings in that vicinity perhaps signified heights approximately 10% greater than our figures — and thus a peak-height-view about 5% bigger than that estimated at §J1.) Byrd's much-too-low alleged height at "the Pole" was forced upon him by two considerations (besides the simple fact that he was saying 2000 ft right after the flight: see caption to Fig.2): [a] His peak height near the turnaround (as shown by the barograph, c.9:30 GCT [§C16]) occurs long after his alleged 13 minutes at the Pole. (Between 9:02 and 9:15 AM GCT, the barograph shows — contra the TC 5 plural-heights version [fn 200] — very little change of height, ranging less than 200 ft: nothing like 1000 ft.) [b] He understandably figured that if land existed just beyond the Pole, he'd best say he could have been low enough to make it credible that, when he looked in that direction, he might easily have missed the discovery. But the lower bracket-value he inserted (post-NGS-exam: fn 200) into the final edition, 2000 ft, is contradicted by the barograph, which shows nothing below 3000 ft in the period 9:02-9:15 GCT. (This is why we can at the very least indict Byrd for *uncertainty of height* — which exposes the "utter nonsense" [⊙6] of his alleged close agreement of d-i & sextant data: §G2.) And there is an even more obvious illogicality revealed by the barograph: are we to believe Byrd's implicit claim that the post-9:30 height-peak (a climb of over 400 ft above the height of 9:02-9:15) was done only after he'd left the Pole? (Note: his 2000 ft "Pole" height lower-limit is merely half his actual probable height at the turnaround — and at the return to Kings Bay, for that matter: §K5.) Wouldn't he want to see as far as possible BEYOND his farthest point from land (especially since, according to Byrd, the Pole itself had earlier been visited by Peary)? Clearly, the best place to do so was at the Pole, not many miles south of it. Further on Byrd's heights: we find that already by his 2nd version of the official typescript (TB 3), Byrd has eliminated (fn 206) both of the original version's paragraphs²⁰⁴ which had mentioned the *Jo Ford's* height at Danes Island (c.1:17 GCT) and AI (1:22 GCT) as being 2000 ft. And we know he had the right height here because he had just compared it to the Hoel peninsula peak (of the same height) while flying past it, as he says,²⁰⁵ "Twenty-six miles [north of] Cape Mitre we passed . . . a high jagged [Hoel peninsula] peak about the height our plane was flying, 2,000 feet. A line drawn from this peak tangent to Amsterdam Island's westernmost point lies in the true north direction and I determined to take full advantage of this fact to line up the plane on the true north and get the error or variation of the compass from the north". He certainly performed this lineup: it's in the diary, with his drawing at D 5 = G 80 showing the line from Hoel peak to the west tip of AI. The catch is that this line actually aims about 4° west of north — about parallel to the 15°E meridian, as is easily seen from, e.g., the Norsk Polar Inst's 1:1,000,000 map, "Svalbard". (Does this relate to the question of what meridian the sun-compass was initially set for? See fn 100.) Which creates an obvious problem for Byrd's claim of perfect steering, since such a mis-aim would have put him about 50 mi to the left of the Pole even if he had gone the full outward distance. Another problem here: we've already seen (fn 204&205) that Byrd originally gave his height as 2000 ft for Hoel, Danes Island, & AI — sites he came to at about 5^m intervals. The trouble is that the barograph (G 158) shows that in the time interval 1:10-1:20 GCT, the plane's height stayed at about 1500 ft, and it only got back up to a steady 2000 ft after that time. But if the plane passed AI at 1:22 GCT, then it passed Hoel at c.1:12 and came to Danes Island at c.1:17 — each at a barograph-indicated height that contradicts Byrd's TA report. Which is presumably why he swiftly deleted (fn 204) both of the early 2000 ft paragraphs in TA, one for Danes Island, the other for Amst I: paragraphs

below freezing]. This is within 1°F of what the same Spitzbergen air-column would imply.) In the final edition of his typescript, he added after "Zero Fahrenheit" the parenthetical comment (TC 5 or TD 6): "(Warmer than we expected to find it.)" Note also that neither of his typescript reports' claimed temperature readings (and there were only two) are recorded in the diary's 1926 data — nor are any other temperatures.

²⁰⁴ TA 3 = G 151.

²⁰⁵ *NYT* 5/15:3:6=7.

8&9, resp.²⁰⁶ What innocent reason can be found for the paragraphs' elimination?) It is more reasonable to assume (from the barograph evidence) that he reached the Hoel-DI-AI region no earlier than 1:20 (probably somewhat later), which puts him ordmag 10^m behind the TA schedule. (These deductions are independent of any assumption about starting time, since [G 158] the barograph is figured from the TA 3 estimate, 00:37 GCT.) This indicates a speed of roughly 1 nmi/min (60 knots), not very different from that (70 mph) independently induced at §I4. (The alternate fn 88 theory is less attractive.) Note that the excisions cited here (fn 206) left extremely peculiar incongruities in the final edition of Byrd's report (TD), the one that finally went out (to, e.g., the AGS) months later. E.g., at TD 2 we read: "Wind continued from East", though TD has made no prior mention of E wind. (Lost with TA ¶8 [G 151] deletion.) Also, the key AI-time 1:22 GCT (nowhere found in the diary, an unwitnessed datum which defenders call DR utt-nonsensical for distrusting: ⊙6) has now been eliminated from the report's text! (Gone when TA ¶9 [G 151] suppressed.) This, though all the later hourly d-i alleged times are given (2:22, 3:22, 4:22, 5:22, 6:22, 7:22, 8:22). The reason for such clumsy pruning: as we see (from his softening of the Pole-height precision: fn 200), Byrd belatedly realized that his alterations of the times of actual events had created height-conflicts with the barograph, so he hid the discrepancies by deleting whole paragraphs from his typescript report before distributing it to academic societies. (The barograph at G 158 shows his 1:22 GCT height was more like 1500 ft; and the 2000 ft time was nearer 1:30, long after Hoel at that height, at c.1:12 by TA's schedule.) Result: not a single firm airplane-height appears in the final (TD) report! (Similar shearing at §G6.) I stress again that this is a report which Byrdites defend by insisting upon the reality of d-i data *which depend on height* (and aren't in the diary — any more than are height-data). See ⊙4. (A personal note: the *Jo Ford's* barograph machine was made by Baltimore's Friez instrument company, whose owner was for years my nextdoor neighbor in Ruxton, MD. The same company was later involved in making the A-Bomb's trigger.)

⊙8 [Note to §D8.]

Steering-Mysteries & Open Options.

For Byrd's claim that he knew the trip's compass variations, see *London Times* 5/28:16:4 or *NYT* 5/28:1:2&2:6. (And *NYT* 5/17:1:3=4 alleges that the already very well established §D8 increase of compass variation with northing "would interest the scientific world, for [magnetic] variations in the polar sea are largely unknown". See also *NYT* 5/16:1:1=2 & 5/17:4:3=4.) Note that the latter (*NYT*) story is verbally almost identical to the former (a *London Times* interview upon Byrd's arrival in England). Except at the start. The *Times* version opens as follows: "Byrd, who has just flown over the North Polar basin, and believes he is the first man to reach the Pole by air, arrived . . ." But this was reworked by the *NYT* to read: "Byrd, the first man to fly to the North Pole, arrived . . ." Even though the *NYT* column-headline here is "BYRD EXPLAINS HOW HE LOCATED THE POLE", this news story contains no claim by Byrd that he used his sextant to navigate the flight. (I believe this is also true of other stories directly from Byrd in 1926 May, after a brief mention by W.Bird at *NYT* 5/10:1:5=6, where it is unclear whether the information comes from Bennett or Byrd. See also apparent implication at 5/11:1:7=8, "taking observations" at the Pole.) It is remarkable that, despite much discussion about using the drift-indicator, sun-compass, and magnetic compass (e.g., *NYT* 5/15:3:5=6, 5/16:1:1=2), the only explicit mention of the sextant in his *own, written* initial lengthy and detailed four-part *NYT* 5/14-5/17 account is when it falls and is damaged (5/17:4:3=4). At the very least, it seems that Byrd was avoiding committing himself to a firm story on how many — if any — sextant shots he was going to claim, and for what times. My own reading of this extensive story: he was considering claiming his distance estimation was mostly (if not entirely) done by d-i, a resort necessitated by his "broken" sextant. Again (⊙5): is it credible that Byrd took

²⁰⁶ See TA 3 = G 151. (Besides 2000 ft, what other factor do these paragraphs have in common? Perhaps also the de-fogged description of the pack-ice edge: fn 16.)

along two or more of every unfixed instrument, *except* the all-important sextant? He had two sun-compasses, 3 magnetic compasses, and at least that many time-pieces. See fn 48 & *NYT* 5/17:1:3=4. For the 1927 *America* flight, he packed a spare sextant (Byrd 1928 p.227). His language as he approached and reached his farthest point (*NYT* 5/16:4:4=5): “As I navigated” and “I took my calculations and found that we were at the Pole!” Such vague descriptions could refer to either d-i or sextant determinations of distance. Or the truth: eq. 3 (§O5). (And: “took my calculations”?? Very peculiar language, which navigators might suspect happened when “took my sextant sights” got a last-minute re-write.) Byrd’s long 5/14-17 account especially emphasizes the aiming part of navigation by sun-compass & drift-indicator, but is terse about how distance was gauged. More (also fnn 133&141) of Byrd’s keeping-options-open. Along the same verbo-frugal line: note too that this same huge 4-part story gives only three times: takeoff after 00:30 GCT (*NYT* 5/15:3:2=3), the “Pole” at 9:04 GCT (*NYT* 5/16:4:4=5), land re-sighted “about”²⁰⁷ 14:30 GCT (*NYT* 5/17:4:3=4). All either rough or later-altered. Instead of firm, detailed times of crossing d-i-basis-pt AI (fn 88), of taking sights, of attaining record latitudes 88°N and 89°N, and of his return moment, he instead fills space with talk of “good old Floyd” (*NYT* 5/16:4:2=3), “blessed old Navy” (*NYT* 5/16:4:4=5), a sewable Peary coin he thought about while “circling the Pole” (☉17) though he hasn’t yet spun the part saying how long he spun over it. By the time he got to England, he had (*Times* 5/28:16:4) changed the Pole-arrival time to 9:02, firmed the takeoff time to 00:37 and claimed he “remained at the Pole for 14 minutes”. Next day, the last datum was adjusted (*NYT* 5/29:1:7, Special Cable to *NYT*): “his nineteen-minute flight above that desolate spot”. Later it settled down to reside permanently at 13^m: §D6.

☉9 [Note to §F5.]

The Loose Sheet of Figures: Mid-Fabrication in Mid-Ocean.

Ted Leckathorn was the person who asked DR to come on out to OSU to look at the Byrd diary. DR & Ted first saw the diary on 1996/4/12. A few minutes after the discovery of the 7:07 discrepancy there, when Ted saw Byrd’s loose-sheet calculation (Fig.6), he instantly and rightly recalled Peary’s similarly suspicious penchant (Rawlins 1973 p.218) for keeping data on loose pieces of paper, a habit contrary to normal scientific procedure, since it allows opportunity for interpolations and deletions. (See fn 66 and discussion at §O15.) Fig.6 was written on a piece of blank paper, 10 cm by 15 cm: metric measure. (The 21 sec correction is badly obscured on my photocopies of Fig.6, but can be induced from the lines above & below it. And it’s known from §E1 — and is given explicitly in the identical time-calculation of Fig.8.) The writing on the original shows a slight counterclockwise slant (not shown in the Fig.6 rendition), presumably due in part to the lack of lines on the paper used. (The much-more-pronounced slants of several scribbles on Fig.3 are striking precisely because [fn 141] they are on the pre-lined pages [fn 4] of the diary.) The metric dimensions of the loose sheet (Fig.6) suggest a possible Norse origin. The calculation on it had to be written between Byrd’s Spitzbergen flight and his arrival in NYC, but the only stop *en route* was London, England, then a non-metric nation. So why was Byrd fine-tuning his faked data on paper manufactured in continental Europe? Vying possibilities: [a] He had bought the paper (perhaps in Trondheim: fn 102) before arriving at Kings Bay. [b] Fokker was Dutch; perhaps Byrd got a Holland-made pad from him. [c] Montague 1971 p.15 relays Norseman Balchen’s memory of giving Byrd a few celestial-math examples, which may relate since these could have been written upon the start of a borrowed Norwegian pad: during the May-June trip back from Kings Bay to the US on the *Chantier*, “Byrd asked [Balchen] to do some computations of different positions along the route from Spitzbergen to the Pole.” Balchen (*idem*): “four or five different problems and I worked them out and and gave them to him . . . I don’t know what he did with them. I can’t say he put them on his charts. I gave

²⁰⁷The barograph (G 158) shows that the *Jo Ford* rose sharply (starting from c.2900 ft) at about 14:30, peaking c.14:45 GCT at a height of roughly 4200 ft (by the Haines formula of ☉7), the greatest height the plane achieved throughout the trip (until over Kings Bay, just before landing: §K5).

them to him and that’s all I know about them. . . . I do know something about the charts he turned in to the Navy [6/22] and which were later used by the National Geographic Society’s committee of experts to ‘verify’ his flight. The committee said these charts contained ten positions, given hourly and also when sextant observations were made.²⁰⁸ When I saw the charts on the *Chantier* they contained just a couple of scribbled notes.”

☉10 [Note to §G4.]

Speculative Reconstructions of Some Marvellously Agreeable Fakes.

I will here propose speculations which produce exactly Byrd’s laughably over-precise 8:18 & 8:38 “dr” latitudes, merely by assuming in each case a single digital bungle of the sort we’ve found him prone to (fn 55). Hypothesis: Byrd was so determined (§G2) to force his dr estimates to fit sextant readings that he simply *confused* a dr latitude with a sextant latitude (see fn 76) in each instance. The simple trig of the Hinks navigational scheme (§E14) allows one to find co-latitude swiftly by dividing the *AltDiff* ($h - \delta$) by $\cos A$ (where approx azimuth = GACT & longitude). For the 8:38:25 GCT shot, GACT is 8:42:02 GACT, so according to Byrd’s own adopted figures (TA 7 = G 155), azimuth $A = 15\text{--}8^{\text{h}}42^{\text{m}}02^{\text{s}} + 11^{\circ}04' = 141^{\circ}34'30''$. But it seems that the “42” was read as “24” — thus instead he got azimuth $A = 15\text{--}8^{\text{h}}24^{\text{m}}02^{\text{s}} + 11^{\circ}04' = 137^{\circ}04'30''$. And this yields (by eq. 2 [§E14]) latitude $L = 90^{\circ} + 23'/\cos 137^{\circ}04'30'' = 89^{\circ}28'35''$, exactly Byrd’s “dr” latitude (TA 6 = G 154). The 8:18:26 “dr” position at TA 6 (G 154), 89°01'40"N, is also ludicrously over-precise, so we will try the same theory — but here the GACT will be fabricated from the *AltDiff* (42'30'') instead of the reverse. (When a concocted observation is being fine-tuned, either direction suffices.) To put ourselves 58 1/3 nmi from the Pole (i.e., at $L = 89^{\circ}01'40''\text{N}$), we use eq. 2 (§E14): subtract the rounded AI longitude 11° from the supplement of $\arccos(42'30''/58'20'')$ and divide the result by $15^{\circ}/\text{hr}$, which yields GACT = $8^{\text{h}}23^{\text{m}}04^{\text{s}}$. Byrd’s value (TA 6 = G 154): GACT = $8^{\text{h}}22^{\text{m}}04^{\text{s}}$. Except for a whole timesec discrepancy, the results match impressively. (Note that fits to 5 and 6 places are being generated out of inputs expressed only to 2 or 3 places.) Lending further credence to the theory: both of these mis-labelled “dr” latitude data ($89^{\circ}01'40''\text{N}$ & $89^{\circ}28'35''$) were deleted from future versions of the typescript: see fn 76. Note that the 7:07 sextant shot was that observed just prior to the pair discussed above. Are there signs of confusion for this shot, too? Well, via eq. 2, the faked 7:07 altitude of Fig.7 puts the plane within 1' of the regular 7:22 d-i latitude, $87^{\circ}45'\text{N}$ (TA 6 = G 154). See fn 67. (And, if one reduces the actual diary altitude in Fig.5 by 1° to $18^{\circ}25'30''$, and then computes latitude by eq. 2 [ignoring eq.time], the result is $87^{\circ}25'\text{N}$, Byrd’s claimed 7:07 dr latitude: *idem*.)

☉11 [Note to §J5.]

The “Splendid Check”.

Byrd alleges he had a “splendid check” on his navigation at 11 AM GACT when he asked Bennett to aim the plane at the Sun and found “the shadow [of the sun-compass shadow pin, perpendicular to the instrument’s clock-face — thus parallel to the Earth’s pole when the instrument is correctly oriented] exactly bisected by the line on the [bi-directional] hand of the clock.” (See TA 8 = G 156; also Fig.3, where Byrd tells Bennett: “Head the plane right at the sun”. Note that Byrd’s first published version at *NYT* 5/17:4:3=4 describes this check as occurring without even aiming the plane. The final version says the airplane was aimed, but that cannot aim the sun-compass unless the latter’s orientation is specified with respect to the plane’s fore-aft axis. In this case: the sun-compass-N-S-axis was made parallel to the airplane-fore-aft-axis.) Why has no one previously questioned this procedure? (Was the whole odd item just a Byrd prank on the NGS trio, to see who was snoozing? If so, it worked.) NGS’ 3some (including the designer of the sun-compass Byrd carried!) accepted that Byrd’s “splendid check” was “verifying his position as being on that [15°E] meridian.” (See G 145.) But Byrd steered along his chosen meridian by lining up the d-i-determined

²⁰⁸This is a roughly accurate if imperfect version of the NGS report: G 142.

ground-velocity vector with the N-S axis of the sun-compass, which works as follows (Byrd 1928 p.190; note verbal similarity to foregoing): “the shadow of the sun, when it bisects the hand of the 24-hour clock, indicates the direction after the instrument has been set” to one’s known latitude & the correct app. time for one’s known longitude. I.e., the shadow-hand-bisect was supposed to hold not just for 15°E-apparent-noon, but *throughout the whole trip*. There was nothing special or “splendid” about any one moment of this laborious & delicate life-preserving continuum. (Indeed, apparent noon [or midnight] is the time when the Sun’s position is least informative on longitude, for a north-south-flight navigator who knows his approximate distance travelled.) The hitherto-missed point here is simple. Byrd was at this moment merely aiming his sun-compass’ N-S axis at the Sun (otherwise he wouldn’t have found the pin-shadow lying on the clock-hand’s line), but he doesn’t seem to have realized that, at 15°E apparent noon, the hand (of a sun-compass set to 15°E apparent time) will of course *automatically* be on the sun-compass’ N-S axis (since the very definition of apparent noon, north of the Tropic of Cancer, is the Sun’s transit due south). Needless to say, if (by aiming the sun-compass’ N-S axis at the Sun when the clock-hand is along that axis) you put simultaneously into the same plane, [a] the shadow-pin, [b] the sun-compass clock hand, and [c] a visible Sun — then, the Sun’s shadow *has* to fall on the hand. (More simply phrased: Byrd aimed the clock-hand at the Sun — and then affected surprise at finding that the clock-hand pointed at the Sun!) Thus, the Splendid Check was circular and told Byrd nothing of use to him. Basic fact: the sun-compass could not place the airplane unless it was building upon a continuous series of careful observations from a known point, which is why Byrd was (⊙4) frantically, doggedly keeping at the d-i steering from the moment he left sight of Spitzbergen until he re-sighted it a half-day later. The sun-compass could not by itself determine position (see fn 99); rather it *used* a determined position to aim towards another position. (When awake, the NGS trio realized this requirement. G 143: “these instruments [sun-compass & d-i] . . . used almost continuously”.) This constantly-scrupulous aiming, not a single measurement, is why Byrd had confidence that he was near his intended path. See §D8. (His final, detailed version of the trip skips the Spendid Check: Byrd 1928 pp.199-200; though it’s at *idem* p.203 in the NGS report).

⊙12 [Note to §J6.]

Verlegen Hook, Misreadings, & Ungenerous Apologists.

If one accepts Byrd’s final, altered version of the return trip, in which he disremembers hitting mid-Spitzbergen (at Verlegen Hook) and instead swings diagonally to the right towards Amsterdam Island long before reaching Spitzbergen (see official chart [our Fig.1] at *NGM 50:377* [1926 Sept] p.386 or Byrd 1928 opp. p.200), then this (vs. the Verlegen Hook path: Fig.2) saves Byrd 30 nmi. (Returning directly from Pole to AI, an ideal path which even Byrd did not claim occurred, would merely save him 12 nmi more, vs. the official Fig.1 path.) This possibility might encourage an advocate to propose an average speed of only about 80 knots (92 mph) for the plane’s return trip — indicating that the north wind’s speed had dropped by half, to c.7 mph, implying the possibility that, if this drop (obligingly) occurred right after 7:07, then the northward groundspeed could have been as high as 85 – 7 = 78 mph (68 knots) for the 2^h.4 from 7:07 to 9:30 GCT, which would carry the *Jo Ford* to about 88°N. Advocates who wish to add to this the equally convenient theory (above) that Byrd might (at 7:07) have been, say, 20 nmi east of 11°E (which permits 40+nmi more northing while still on the 7:07 Sumner line) can imagine the *Jo Ford* up to c.88°3/4 — just barely far enough to glimpse the North Pole, 75 nmi distant. Perhaps the kindest way to respond to such enthusiasm is simply to observe that one can’t positively prove that it *didn’t* happen that way. One other point: Byrd makes the seemingly plausible implication that the plane was flying faster on the return partly because it was carrying less fuel. In *NYT 5/16:4:4=5*, he wrote: “we were making knots over the ground with an increasingly lighter load as the gasoline was burned up.” (Similarly at TA 8 = G 156.) However, for maximum miles/gallon, an airplane should go slower not faster as it progressively lightens. Consultation with *DIO*’s Keith Pickering and Gerry Gregorek

(recent head of Ohio State University’s Aeronautics & Aerospace Engineering Dep’t) has confirmed this. (Note Byrd’s own comment: 1928 p.252.) D 16-21 (G 115-117) shows that, during the *America*’s 1927 trans-Atlantic trip, the airspeed steadily declined — by roughly a quarter. Note: Byrd in 1927 records no firm groundspeed figure beyond the 1st hour’s 94 mph: D 16-21; later values of 102 mph & 103 mph are both tentative. (The “103 m.p.h.” figure is not even entered into the groundspeed data column.) And this is for a case where [a] Weather prevented sextant gauging of distance at that time (though much later on the flight the sky cleared enough that Byrd’s failure to take sextant shots of stars mystified Balchen: Montague 1971 p.125), and [b] precise 1927 steering was much less vital than in 1926 (Europe being rather bigger than Spitzbergen), so that Byrd could have afforded to spend time using the d-i for distance as well. Regarding the 1927 flight’s declining speed: again, gradual slowing of motors (Balchen *Come North With Me* 1958 p.105) was normal strategy for maximizing mi/gal. But icing may also have played a rôle in the trend; see, e.g., D 18 (G 116): “ice began to form . . . temp dangerous”. One of Goerler’s several diary misreadings (see too §Q2 & Pickering’s upcoming review of Goerler 1998) renders this as “terribly dangerous” at G 113. (I see that mere consultation of *Skyward* would’ve enlightened Goerler, since Byrd 1928 p.258 reads, “One notation in the log stated: ‘Ice is forming on the plane.’ We were at a dangerous temperature.”) This is one of dozens of places where the book’s quality suffers from interested parties’ determination to skimp on meaningful scholarly refereeing, in favor of slipping into print a version pleasing to the Byrd cult. Three more Goerler misreadings (of D 5 = G 80) are at G 77: for “Now we” read “Now ice but” (§B3); for “head on” read “head as”; for “a match to strike” read “a match box to strike”. In his Byrd Center talk of 1996/4/12, Goerler originally misread “sun” in “Then I want the sun” at D 14 = G 86 (making it “Then I want the time”) and was corrected there by DR; at G 78, he now quietly accepts this nontrivial correction — but doesn’t say so. Despite Goerler’s polite but unspecific compliment to DR at G 5, OSU has helped launch Molett’s slander (⊙14 & ⊙18), and Goerler’s loyalist book follows (e.g., G 52-53 n.17 & G 56-57 n.19) the National Geographic-Byrd family-Molett party-line that DR has contributed *precisely nothing* of firm value to the entire Byrd N.P. debate. (At G 53, archivist Goerler refuses, like Newsom’s 1997 ms p.1, even to credit DR’s key *pro*-Byrd new evidence: the archival proof given at Rawlins 1994 §A4 — pre-cited at *DIO 1.1* p.2, 1991/1/14 — that Balchen had invented most of the details of the famous Bennett confession at Montague 1971 p.48. This item is cited in DR’s 1996/5/4 report to OSU — but it has never been cited by Byrdites because it shows that DR is balanced and will produce new evidence regardless of party. Wrong message. Especially for last-ditch *ad hominem* desperados.) To avoid admitting the plain implications of the DR 1973 discovery (⊙2) that Byrd didn’t drop his flags, Goerler actually resorts to accusing Byrd of deceiving his backers! (G 54 n.17: the flags “may have been left behind to lighten the load, something Byrd would not have wanted to reveal when he presented the flags to benefactors.” Considering the trifling weight involved, we have a parallel to one of Cook’s ploys: his alleged leaving of some of his 1908 N.Pole “data” in Greenland, though data-records are a featherweight addition to an explorer’s gear, as Peary rightly pointed out.) This ridiculously desperate extreme is merely one of the letting-’em-have-it-with-the-bottom-of-both-barrels dodges used in the book to deny all credit to a particularly feared skeptic — an embarrassingly transparent cult approach to him, the discreditable *ad hominem* cause of which is discussed (in a similar context) at *DIO 1.2* §H2. See also *DIO 1.1* ¶6 fn 4 & *DIO 6* ¶1 §J. *DIO*’s contrasting attitude (fn 176) of appreciating even its most vicious enemies’ positive contributions (a generosity which ought to be unrestrainedly natural to fair-minded scholars, and is thus repeatedly evident in the current report) is noted at *DIO 6* ¶3 §E7, which adds that political cliques not only cannot appreciate *any* of their opponents’ work — they additionally cannot appreciate enemies’ appreciations. (Or even assistances. See, e.g., fn 117.) Pathetic. See similarly at *DIO 4.3* ¶15 §F2.

⊙13 [Note to §§L5-L7, N6, & Q1.]

Velikovsky Lives! — How Cults Eliminate Contradictions.

OSU's Newsom is not a positional astronomer. DR happens to be one — though, *that is no reason to take my word for anything, either*. (Criticisms here of my detractors are not intended to cause automatic rejection of their arguments, but rather merely to discourage automatic acceptance: see *DIO* 1 †1 §C12 & †10.) The contrast would not even be mentioned here, but for the Defense approach, which is not primarily logical but rather an artful-if-it-weren't-so-laughable numerical 2-to-1 pretense at G 57 n.19 that a majority of qualified Experts believe in Byrd's 1926 claim, as if DR is the only scientist who thinks the diary data-discrepancies are meaningful — ignoring, e.g., the known-to-OSU opinions of specialists from CalTech and the University of Cambridge (§E15 & *DIO* 7.1 back cover, resp) — going for the old courtroom-lawyer version of Newton's 3rd Law: for every expert there is an equal and opposite expert. (See fn 82.) And Newsom's creditably-unenthusiastic (but upside-down-agnostic: see conclusion of this note) support for Byrd has nothing to do with astronomy. (Despite attempted but honestly abandoned cavilling, his celestial calculations all end up agreeing with DR's, a fact one won't learn from Goerler's book.) His (sort-of) positive verdict is instead based on a grade-school formula: eq. 3 (§O5). He just amiably accepts (⊙6) the crude airspeed-arithmetic of d 3 (Fig.3) as evidence for Byrd, regardless of its obvious scientific inadequacy and its (nowhere cited) irreconcilability with the diary sextant data and the typescript Pole-arrival-time! (See §N1.) I.e., Goerler is gravely waving a university-professional-expertise wand — and then using it for mere elementary-school Fig.3 arithmetic. And, naturally, without telling us this. (Details here at ⊙6.) Non-specialist Goerler at G 57 n.19 makes an amusingly amateurish effort to sum up Newsom's argument: "the erasures were the work of a navigator who, although tired, was still alert enough to realize that he had made an error in his calculations. According to Professor Newsom, Byrd got within 'tens of miles' of the North Pole and may have reached it. See Gerald Newsom to Rai Goerler, July 9 [sic], 1997". ("See" this 1997/7/7 report? When DR and *Science's* Eliot Marshall asked to see this *cited* document, Goerler & Newsom both refused to send it! DR luckily obtained a copy elsewhere.) Comments: [a] Defenders claim no error in the Byrd diary 1926 *calculations* (which essentially agree with DR's) but rather in the Byrd *observations*; so Goerler's footnote misunderstands the basis for his deliberately tucking the lethal evidence against Byrd entirely into this same tiny-type footnote — where Goerler adds yet another jawdropper, this time the ultimate: Molett "believes . . . *there is no discrepancy between the official report and the diary*." (Emph added. See here at §N6.) OSU archivist Goerler's approach to archival evidence is ingenious, rigorously logical, and unquestionably a pioneering contribution to his profession: hey, look, if we toss out every archival discrepancy as a "mistake", then — there is no discrepancy! But even defter is Goerler's omitting to inform his readers that: when one drops all the raw 1926 diary data that are absent from and/or contradicted by the TA report, *there isn't a single raw number left*. (The common time, 7:07:10 GCT, isn't raw: §F. And it is particularly indicting for its very sameness: §E8 & §L11.) So diary-editor archivist Goerler's first-expert Molett has simply *eliminated all the archival-manuscript data and replaced them with the typescript data*. (Sometimes without even saying so: §L4.) In the 1950s, W.Albright & M.Gardner characterized the output of the psycho catastrophist historian Dr. Immanuel Velikovsky as resembling that of "the professor who identified 'Moses' with 'Middlebury' by dropping the '-oses' and adding '-iddlebury'." But even Albright-Gardner didn't envision that someday scholarship promoted by Ohio State University and *Merky World* would create an intellectual equivalent to identifying "Moses" with "Fiddlebury" just by dropping the "Moses" and adding "Fiddlebury". [b] Note the delicate tightrope Defenders must walk — averring that Byrd's data were so grossly unreliable that the erased 7:07 datum's huge disagreement (over a degree: §E8) with his official story is a believable accident — *this even while* saying at the same time that his navigation was reliable enough to hit the Pole within a modest fraction of a degree. (See §L7.) The concluding paragraph of Newsom's

1997/7/7 paper is so lovably, circularly head-scratching (see Dave Barry at *DIO* 2.3 †8 §C25) that I cannot resist quoting from it here (emph added): "Why are Byrd's [official TA] sextant observations . . . not recorded in his diary? Why are the two sextant readings in his diary not given in his [TA] report? . . . Assuming Byrd's [TA] report . . . is not concocted, then why do the . . . two in the diary seem to be the only two that were [DR: see §N6] *erroneous*? We do not understand the answers to these questions. Unless and until we do, I see no way to determine with certainty if Byrd made it to the Pole." (See fn 115.) Therefore (?!), Newsom just accepts the (TA-contradicting: §D6) diary dr scribbblings of Fig.3.

⊙14 [Note to §L8.]

In Which Goerler Forgets the Day He Was Famous.

Goerler's book & Molett's article both appeared the same month. But there's more OSU-*Merky World* echo-synchronicity (similarly at ⊙2 & fn 66). Molett 1998's astoundingly funny²⁰⁹ map (our Fig.9) of Byrd's 1926 path is just a photocopy of a reproduction by Goerler (G 56) from Byrd 1928. Worse: at the time these 2 strange publications appeared, DR's report had been published nowhere but in the *NYTimes'* 1996/5/9 p.1 summation; yet, while promoting attacks on DR's work, neither OSU's book nor *Merky World* tells the reader of the existence of this prominent *NYT* article on said work, impeccably written by the *NYT's* top science writer, John Wilford, which put Goerler onto national media later that day. Right after the book's publication (*caused by Wilford's careful article*), DR asked Goerler about this seemingly inexcusable omission; Goerler had his excuse all ready: "There are alot of newspaper stories." He *actually* said this (1998/3/7), and said it to the very scholar whose discoveries caused this story, thus triggering OSU Press' reversal of its former hilariously mal-timed turndown of Goerler's diary→book proposal (rejected 1996/4/4 for lack of "financial subvention", only 8^d before DR found the diary data discrepancies that changed everything about Goerler's publication-prospects). In 1996, Goerler's disgust for newspapers hadn't yet made itself felt: he used 1996/5/9's big press coverage as an argument for publication, and in fact made his case right in the press (*Columbus Dispatch* 1996/5/15). From these data, I leave it to others to judge Goerler's trustworthiness, especially since the book's jacket was not above exploiting the event (a blurb which looks less like refereeing than something written to urge publication, back before OSU Press accepted the book): "The subject . . . has made national headlines in the recent past." (In Goerler's hand on the flyleaf of my copy of the book: "Many thanks for helping this into print.") National Geographic, OSU, the Byrd family, & Molett all hate the epochal *NYT* article (even though none can find a digit out of place anywhere in it). But does that justify cohesive non-citation of the excellent story that caused the book's publication? (*NB*: The *NYTimes* newspaper story was much, much more reliable than the reports of the Byrd Center's favorite scientist-experts: Bossler [fn 4], Portney, Newsom.) As to whether Goerler's omission was accidental: there is almost nothing in *To the Pole* that would upset National Geographic, from whom Goerler actually contemplated (openly) seeking "financial subvention" for the diary's publication! When DR on 1996/4/12 (in the presence of Goerler & Ted Heckathorn) made the diary-discrepancy discoveries that finally led to Goerler's success in getting published, DR told Goerler (p.8) that he and Ted would keep silent and leave it up to OSU to publish the truth; DR would simply write a report for OSU, which is what OSU sent to the *NYTimes*, leading to the 1996/5/9 story. This caused a letter from OSU's President to Goerler, in delight that the event had caused more public credit to OSU's integrity than any in OSU history. Yet, in a peculiar display of this putative integrity (not to mention gratitude): from that point on, OSU dealt stealthily with DR, not making any effort at keeping him informed about the reviewing process or the efforts of its internal OSU trio²¹⁰ or even the names of its members, until after publication of the book (which, we repeat, didn't cite the *NYTimes* story launching it or the OSU President's letter).

²⁰⁹See fn 119 & ⊙18.

²¹⁰Newsom, Bossler, and one scientist who (§Q1) has wisely sidestepped serious involvement.

©15 [Note to §L12.]

Another OSU-Promoted Expert Wrestles with Numbers.

Even while manipulating his speed-arithmetic's time-interval, Molett manages to mix up Byrd's pure dead reckoning estimate of 380 nmi with an intended 397 nmi distance (expressed allegedly-dyslexically as 379 nmi in Molett 1998 p.62, then "corrected" in 1998 Sept-Oct *Mercator's World*), which he divides by the 4h45m between 2:22 & 7:07 — and finds not 80 knots (or the 84 knots his own final numbers indicate)²¹¹ but 83 knots. He slips this incomparable revision right past the scientific watchdogs (©18) at *Merky World*: 1998 Sept-Oct p.10. The 83 knot figure is not only false, not only miscalculated — it is also unambiguously contradicted by the very typescript dead-reckoned hourly distances which Molett elsewhere²¹² flays DR for not accepting! They are found here at fn 139; in nmi: 77.5, 82.5, 78.5, 80.5, 81 (average = 80 nmi). Not one of these dr claims is as high as 83. And averaging them per hour to get 83 knots requires a special brand of math. (See fn 139&211.) Molett insists on the 83 knot figure anyway. (Do **NOT** miss his letter; again: *Mercator's World* 1998 Sept-Oct p.10.) The accurate math of this matter follows. Byrd's dr latitudes for 2:22 & 7:07 are 81°05'N (TA 4 = G 152) and 87°25'N (TA 6 = G 154), resp, a difference of 6°20' or 380 nmi in 4^h3/4, which indicates a speed of exactly 80 knots. While saying (1998 p.62) that this distance is based on the 7:07 *drift indicator*-based latitude, Molett is instead using a figure 17' greater, namely the (Rawlins-Standish computed) *sextant-based* latitude $L = 87^{\circ}42'N$ given by the typescript's oblique 7:07 altitude, $h = 18^{\circ}18'18''$ (TA 6 = G 154). (See fn 56 and §§E9&E15; this L is not in Byrd's reports.) Subtracting 82°05'N from 87°42'N produces Molett's 397 nmi, which leads Molett to insist (contra Byrd) that 397 nmi/(4^h3/4) \approx 83 knots is the correct speed. (The disagreement between the typescript's sextant and d-i positions at TA 6 = G 154 is discussed above at §E10, math which has been in Molett's hands since 1996.) All this culminates in the concluding paragraph of Molett's letter (which *Merky World* refereed with the same care as the original Molett article): "From 1:22 to 7:07 Byrd flew 397 nmi at an average groundspeed of 83 knots. The final two numbers in the [Molett 1998 p.62] distance figure were mistakenly transposed from 397 to 379 during the editing process." So who is responsible for the several gaffes in this allegedly de-gaffing paragraph? The Molett-OSU-*Merky World* comedy gets only funnier with each attempt to undo prior jests: [a] 397 nmi in 5^h3/4 is 69 knots, not 83 knots. Molett has simply forgotten that an earlier de-gaffing sleight (§L12) had slipped in 2:22 for 1:22. [b] By accepting that Byrd's 7:07 position was at 87°42'N, Molett has Byrd going from there to the Pole (138 nmi) in 1^h55^m, which is merely 72 knots — a startling comedown from 84 knots (fn 211), producing a negative 12 knot speed-discontinuity (at 7:07) which Molett does not explain, presumably because he is not even aware of it.

©16 [Note to §L12.]

TimeFudge-Emulation & Byrd at 1 Mile/Hour.

Molett 1996 ms p.5: "At one place in Byrd's diary he writes 21 seconds but in his final report he writes 20 seconds. Rawlins considers this evidence of cooking (faking) the figures. The one second would make only about 2 feet of difference. Would he fake one second? Why?" Why? Simple: Byrd was smoothing his report (see similarly at fn 67&199), to excise inconsistencies that might cock the eyebrow of a careful reviewer. (He boasted at TA 1 = G 149 that he was "certain of the time" and that the chronometer [Hamilton #5098] was "21 seconds fast" — he certainly didn't want NGS to think he had slipped and used 20". (Since Byrd was unusually prone to arithmetic errors [see, e.g., §P] it's quite possible that he, proud of being known as a Great Navigator, was ashamed of this propensity & so took too many pains here to expunge all symptoms of his problem.) Byrd, finding that his diary had inadvertently used 21^s at D 8 (Fig.4) and 20^s at D 11 (Fig.5), determined to keep the chronometer-correction constant at 21^s throughout his final math. (The last figure in his

²¹¹ Dividing 397 nmi by 4h45m yields 83.6 knots.

²¹²See, e.g., Molett 1998 p.61.

pre-flight tests was 21^s 1/2: D 366 = G 94.) Perhaps because he didn't wish to get anywhere near revealing his inconsistency, he ended up giving just post-correction times in the official typescript. (Though, the 21^s correction is on the "flight-chart": Fig.8.) Since these aren't raw data, the NGS sub-committee should certainly have asked to see the diary, which might have exposed Byrd's manipulations way back in 1926. See Rawlins 1973 pp.269-270 and *DIO* 2.2 fn 35; also Royal Geogr Soc chief Hinks in 1926 July at *Geogr J* 68:58, p.62: "It is particularly desirable that every figure of the observations made on any [exploration] flight be published".) Molett 1998 p.61 raves that it's "utter nonsense" even to consider the possibility that Byrd's chronometers were off, since the explorer stated at Byrd 1928 p.191 (or *NGM* 50:369) [quoted almost accurately by Molett] that "we carried two chronometers that I had kept in my room for weeks. I knew their error to within a second." But Molett leaves out Byrd's very next sentence: "There seems to be a tendency for chronometers to slow up when exposed to the cold." (Byrd of course adds that he attempted to account for the effect: "With this in mind we had taken their cold-weather error." However, nothing at D 366 = G 94 indicates that his occasional re-settings of chronometers had anything to do with applying a temperature-correction.) So *Merky World*'s wide-awake editorial process permitted Molett to use the 1st part of the quote to portray as "utter nonsense" DR's suggestion that Byrd's chronometers might all have been c.10^m slow, even though the 2nd part of the same quote lends credence to this very suggestion. (Even aside from the omission, there's a simple question of decency: why would *Merky World* throw such apoplectic insults at an author's merely-tentative suggestion, when Molett's "evidence" against it is nothing but the word of a party whose word is the very subject of the debate? Note: a careful reader of DR's investigations will notice that the assumption of this 10^m error actually gets Byrd farther north than without it, as Molett should know: see above at §I4, an analysis which he has possessed since 1996.) As for Molett's remark that the airplane only went 2 feet in 1 second: this translates into 1.2 knots! [For similar 60-factor confusion, see Portney 2000 p.13's claim that 1'/hr = 1"/sec.] Understand: *this* 1996 Molett analysis is the 1st-cited document upon which *Ohio State University* founded its decision to attempt escaping from DR's verdict on Byrd & put the strongest case against Byrd (diary data discrepancies) into a terse small-print footnote at G 57, pretending it proved nothing, not even telling the reader which of its own pages to compare, to appreciate the discrepancies.

©17 [Note to §M1 & §M4.]

Can Leashed Judges Leash a Rampaging Approval-Orgy?

The NGS report charts Byrd's alleged sextant-based Sumner lines in a diagram (reproduced here as our Fig.1) in the 1926 Sept *National Geographic* (*NGM* 50:386). Several remarkable errors in this chart will be noted here. (More in p.11's comments on Fig.1.) E.g., it is obvious from the merest glance (at Fig.1) that the key 7:07 Sumner line has been drawn at the wrong angle: an azimuth of 107° (off by 11°). The elementary cause: NGS' sub-committee accidentally computed the 7:07 GCT solar azimuth for the Greenwich meridian instead of Byrd's 11°E meridian. (Another pseudo-prestigious Byrd-defender's like [repeated] slips at §S10 [b]. Byrd's "flight-chart" has both lines correctly oriented. But one wonders: would any of NGS' True-Believer "examiners" have noticed if they weren't? As in 1909, the explorer was clearly smarter than the NGS trio he faced — and smarter yet [see ©19] than certain modern defenders, whom he continues to fool to this day.) Smaller but similar glitch for the Pole's Sumner line: the equation of time has been ignored. These are the kinds of goofs that can occur when one is rushing to minimize the temporal extent of the folly of giving someone a (pre-engraved and bejewelled: *NYT* 6/22:22:8) Hubbard²¹³ gold medal 5 days before completing the (alleged) verification of the (alleged) feat the medal was (allegedly [vs §M2]) given for. As noted at Rawlins 1973 p.268, the Byrd-Bennett tickertape parade in NYC started before noon, NY Daylight Saving Time, BARELY before

²¹³The medal is named for the 1st in the 5-generation family line running the "National" Geographic Soc (©21).

his “data” were (reportedly [vs §M5]) first seen by NGS’ sub-committee in Washington, DC. This temporal-narrowness was probably not accidental: Balchen reports²¹⁴ that the *Chantier* arrived off NYC two days early and that Cap’t Brennan was door-slamming-angry at the (seeming) folly of just sitting offshore for that period. [*Skyward* p.208 slips: “We arrived at NY on the morning of June 22, 1926.”] By standard coincidence, the relevant portion of the *Chantier*’s log is missing at OSU — but, from the National Archives, Alison Wilson recovered pre-arrival US Navy radios in which it is explicitly stated [NARA RG 80 29455-95:18] as late as 1926/6/15 that the *Chantier* will arrive at NYC on about 1926/6/21. So Byrd’s “arrival” the night of 6/22 looks deliberately tardy. And understandably so. Note: according to NARA RG 80 29455-95:14 (1926/5/14 wire to SecNavy from Southern Society & Virginians of NYC), the original plan for the NYC parade was for 6/22, not 6/23 — another indication that Byrd had to slow his approach to the US in order to minimize the time NGS’ committee would be able to use in checking his math. In any case, even if we trust honest NGS’ claim²¹⁵ that the full expert-trio was entirely assembled & rapidly checking calculations on 6/23 at 10 AM EST sharp, Byrd’s neat arrival-timing had arranged it that: this UltraRespectable (all-US) trio had only **minutes** to decide whether to announce cautiously the *possibility* that Byrd hadn’t *necessarily* made it (thus fleets of escort airplanes should return to base; blue-blood committees already in place in NYC should go home; the cream of the corporate United States [Rockefellers, Fords, etc] might have wasted huge sums of their money on an incompetent or a fake), and so — on a we’re-not-yet-completely-sure vote — **recommend postponement** of a massive, *already-well-underway* celebration for the brother of the powerful Governor of Virginia.²¹⁶ NGS-appointed committees are famous for that sort of impertinence. . . . Peary-adulator W.Hobbs rightly called the 1909 Danish welcome of Cook an “almost insane orgy of hasty approval” (Rawlins 1973 p.85), but he had no comment when the same process led to Byrd’s 1926 certification as hero. *Could* the NGS trio have called off the ceremony?²¹⁷ Prior to the Byrd 1926 parade, “a fleet of boats and a squadron of airplanes” (NYT 6/23:1:1) saluted the returning hero at 9 AM EST (10 AM NYDST), an hour *before* the reported time of NGS’ data-opening (NYT 6/24:3:1), “The day’s ceremonies began . . . when the [mayoral ship] *Macom*, with representatives of the city, State and nation, came abreast of the *Chantier* The *Macom* whistled furiously . . . in honor of the famous ship and the gallant company aboard her. . . . Byrd, Bennett . . . and others of the Byrd expedition were taken aboard the *Macom*, which proceeded to Pier A, the Battery, followed by a welcoming fleet of small boats, colors flying, whistles shrieking. Overhead three squadrons from the Scout Division of the Atlantic fleet flew this way and that, describing arabesques in the leaden sky in honor of Byrd and his party.” Understand: as this massive celebratory machinery was being *irrevocably unleashed*, no institution had yet seen a digit of Byrd’s data! But National Geographic had already seen the only evidence *it* was interested in, which instantly convinced NGS that Byrd was gold-medal stuff. This evidence had first been published as a subheadline on page one of the *NYTimes* (5/10:5=6), reading: “PEARY’S OBSERVATIONS ARE CONFIRMED”. This was followed by text claiming that Byrd “had . . . [verified] Admiral Peary’s observations completely.” Byrd’s written story (NYT 5/16:4:4=5) describes arriving at the Pole: “I went back into the cabin, stood at attention and saluted for Admiral Peary.” Byrd 1928 p.198 expanded: “we saluted the gallant, indomitable spirit of Peary and verified his report in every detail.” (Actually, all that Byrd could “verify” [at a great height] was that the Pole

²¹⁴Balchen-Ford 1958 (fn 18) p.55; Montague 1971 p.27; Balchen unpubl. NARA autobio p.46.

²¹⁵It’s implicit but unstated (G 141, NYT 1926/6/30:5:2) that all three were on hand at 10 AM on 6/23.

²¹⁶Virginians were naturally working closely with the Mayor of NYC, arranging the welcoming ceremony: see NARA RG 80 29455-95:14.

²¹⁷In 1909, Cook also delayed off-shore-incommunicado in his ship (Cook 1911 p.478) 1 day ere NYC’s 10/15 Freedom of the City ceremony for him. The Barrill&Eskimo testimonies against him were being published (10/13&14, resp); but NYC pols’ fête-machinery was so advanced that con-man Cook could suggest postponement, without concern it would actually occur. See Bryce 1997 pp.434f.

was over an ocean, a fact which [if it vindicated anyone’s priority] would make F.Cook the Pole’s discoverer.) Byrd’s concluding installment of his big 4-part *NYTimes* story, page one, sentence one (NYT 5/17:1:3=4), “As we circled over the Pole I thought of a little coin I carried pinned to my shirt that had been pinned to Peary’s shirt when he reached the Pole. For the second time that little coin had reached the top of the world. I had also carried it with me in the Arctic last year.” After such publicly-sworn allegiance to the Peary Pole myth, there were no other data that NGS and the rest of the US establishment (§T14) needed to see. That is why, to repeat: when the lionizing, military-escorting, and committee-mobbing of Byrd was launched at 9 AM EST in NYC on 6/23, no one had yet bothered to check a single datum of his flight. Not his alleged sextant shots. Not his dr claims. Not his time of passing Amsterdam Island, etc — not any of these 6-weeks-after-flight detailed data (which his defenders now try to put over on readers as more trustworthy than his contemporaneous diary). At this point on 6/23 in Washington: even if the NGS threesome *had* set aside tar&feather nightmares and dared to peep a wee question about Byrd’s report, the outnumbered & outgunned trio stood less of a chance than astrologer Hypatia at the mercy of St. Cyril’s monks (Gibbon Chap.47). Far away in NYC, Byrd watched a twin to the Fokker fly over and (6/24:3:1) took time out to arm-hook Senator T.Walsh (“presiding officer” of the 1924 NYC national convention of the Democratic Party, whose VA guv was Byrd’s brother, Harry, also present: NYT 6/24:1:6); and Byrd detailed to Walsh all about how that 3rd motor of a trimotor could keep one not-to-worry flying even with two motors. (Get it?)²¹⁸ When photographers asked Byrd to hold poses, he protested that: he was, after all, not an actor (NYC 6/24:3:2). Following his long Broadway parade, Byrd endured a speech (planned for 11 AM EST or local noon: NYT 6/23:3:3) by Mayor & apt-songwriter Jimmy “Will-You-Love-Me-in-December-As-You-Did-in-May” Walker (NYT 6/24:3:1), whose Byrd-welcoming committee was an army of over 300 eminent US citizens, every one of them listed at NYT 6/21:6:1-2 (followed by dozens more in NY Gov. Al Smith’s own committee). At City Hall at 13:30 [NARA RG 80 29455-95:26], the only talk of fakery (NYT 6/24:3:1) centered around the locals’ attempts to sing “Carry Me Back to Ole Virginia” with the right accent. Byrd was then fêted at a breezefag luncheon at the Advertising Club, where Virginia Senator Claude Swanson (a close ally of his eventual successor, then-Gov. Harry Byrd) made a speech, probably along the lines of: “the dash and daring only found in the soul of an American” — though probably not in those exact words, which were how he in 1909 greeted word of Cook’s prior fraudulent “discovery” of the Pole (Walter Lord *The Good Years* 1960 p.248).

©18 [Note to §N4.]

In Which Byrd’s Defenders Juice Him Up to 130 Miles/Hour.

Molett’s inventiveness reaches an inevitable apogee when attempting to deal with the lethal 13^m discrepancy in Byrd’s two public-record Pole-arrival times (9:15 vs. 9:02): he simply blames it on DR! Molett’s 1996 ms, p.4: “Rawlins lost 13 minutes somewhere”. Nose-holding *Merky World* dropped this revealing passage, but didn’t learn enough from it to re-consider publishing the paper (same strangely unbalanced editorial discrimination at fn 116), instead suppress-compressing consideration of all such data-contradictions into just (Molett 1998 p.60): “Occasionally, one can find small differences between the figures in Byrd’s diary and those in his report, but none have any real significance. . . . I attach little importance to a ten-second error here or a one-mile difference there.” Most of us would consider thirteen minutes (§D6) more serious than ten seconds. And 67 nmi (§E8) alot more than 1 nmi. But, then, we are not imbued with *Mercator*’s otherWorldliness. So: just how smart and neutral is this *Merky World*? — which attempts so venomously if insubstantially to portray DR as an incompetent? We had a hint at ©15. But it gets even more interesting

²¹⁸Consistent with the same reassurance-that-the-leak-didn’t-bother-us motif: Byrd claimed (NYT 5/16:4:4=5) that right after finding the oil-leak he throttled the suspect engine to test being able to continue north. Air historian Russell E. Jones has doubts about the believability of this.

upon closer examination. We note that *Merky World* got Ohio State University to vet Molett 1998, a referee-choice about on the level (as regards both integrity & smarts) of selecting a Clear in the Church of Scientology to review a life of L. Ron Hubbard. But maybe we should be grateful, since fun always sneaks through when journals put politics above quality-control: the map of the Arctic at Molett 1998 p.61 is taken (©14) straight out of OSU's book (G 54), which had borrowed it, without the slightest checking, from Byrd's *Skyward* (1928). Among other delicious features, the map repeatedly credits fake Farthest North records while suppressing real ones (fn 119). But we mustn't let perplexity, at the map's slavish political correctness, blind us to this creation's highly original contributions to geography. (The P.C. here is rather National Geographesque. But it must be said that NGS itself [a highly able mapmaker] would never directly turn out anything this awful, especially a map that mis-spells the name of one of NGS' favorite explorers, D. MacMillan.) It moves Kings Bay all the way to Grumantbyen, more than 100 km south of the Kings Bay we used to know; and Grey Hook's name is printed as "Green Pt." (*Skyward's* verdant ghost at work?) — while being kicked west & somewhat north, all the way across Wood Fjord to Welcome Point. The map bears (in its lower-left region) an even more astonishing scale, 500 mi = 6° of latitude, which shows that *Mercator's* world was about 1.2 times wider and thus 1.7 times more voluminous in 1928 than it is now, 7 decades later — revealing for the 1st time the sensational geophysical discovery (which even Dr. Velikovsky missed) that the Earth is shrinking. Indeed, at the rate discovered, the Earth will completely disappear before the year 2100 AD! Further on this amazing path: the Kings Bay colony's retreat into Icefjord forces Byrd into a lengthy curved initial path to get-out & later back-into the town's fresh location way up the fjord. Thus, combining its revolutionary World-size with Kings Bay's also-hitherto-unheralded escape to (a fjord in) the southern half of Spitzbergen, p.61's illustration in the historic 1998 March-April issue of controversy-arbiter *Mercator's World* (detector & trasher of incompetents & other purveyors of "utter nonsense": ©6) has Byrd going a total of over 2000 mi in his 15^h1/2, which whisks his 85 mph plane up to a mean speed of 130 mph! (See fn 163 under: bemedalling the wind-gods.) Though there's a crowded field lining up for the Byrd-Speed-Defender-Fishstory Derby, the joint (©14) Byrd-Goerler-*Merky World* map-entry emerges as undisputed Winner — barely nosing out Goerler's previous, seemingly invincible 120 mph record (§H5).

©19 [Note to §Q1.]

Odd Hominems: the Fruits & the Fates.

See quotes from Goerler, *Columbus Dispatch* 1996/5/11, *Baltimore Sun* 5/15 p.A7. Byrd's failure was so obvious from the conflicts between diary and transcript that Goerler never even thought of denying it. But, then, National Geographic immediately (before checking anything) announced it was sticking by the Byrd 1926 claim. NGS refused to examine (on-the-record) the new diary evidence — while NGS chief G.Grosvenor 2 instead just slung an *ad hominem* attack against DR's person instead of his evidence. (The attitude at OSU then began its evolution from rational openness to kook elusiveness.) This attack was printed in the same *Polar Times* 1996 Fall-Winter issue which 1st published the ravings & mismath of Molett's 1996 paper. The same issue also got more broadly into the *ad hominem* pool by publishing a slew of praises of Byrd as one-who-couldn't-lie. (And the Byrd faithful damn Balchen as just a jealous grouch.) Question: do those who go this route ever wonder where it takes a controversy? I.e., should *DIO* publish 1st-hand testimonials (which we possess: letters to DR of 1974/8/20 & 1977/1/3 — neither from Balchen, by the way) which portray Byrd's integrity in a less worshipful light? (It was in the hope of steering clear of this sort of discussion that DR wrote his original 1996/5/4 report on the diary as amiably as possible.) Odd hominem Grosvenor 2 (who sees admission of error more in the context of a sports-level macho-contest than a matter of science & truth) evidently doesn't even realize that his tactic — besides risking a demeaning war of slander — accomplishes nothing beyond revealing: [a] unseemly pique, & [b] the hopelessness of the evidential situation for the Byrd N.Pole myth. (When you have the hard evidence on your side, you don't

immediately resort to talking personalities & hoping for a phony array of Experts to fool the public, J.Cochran-OJ-style. Of course, one must always leave open the possibility that the dominant factors in GG2's campaign are that: [i] he talks only to those who agree with him, and [ii] he is getting bad advice from careerist sycophants who tell him what they sense he wants to hear.) I.e., NGS knows it now cannot find any group of neutral, well-credentialed scholars who can be counted upon to defend the Byrd N.Pole claim. (If still alive, not even its original 1926 trio would do so by now — a point which ameliorates our criticism of NGS at ©17.) For this reason, I wonder if GG2 will try to save his family's peeling face by starting up increased promotion of its hoary Peary myth henceforth, since — except for Crocker Land (fn 66) — Peary's fraudulence is slightly less immediately obvious than the N.Pole claims of Cook & Byrd. In doing so, NGS will inadvertently tip off how scared it is of the smoking guns now publicly available on Byrd. (And, on Peary, NGS will religiously avoid putting the word "Land" anywhere near the word "Crocker".) NGS' later comment on the new Byrd evidence is even weirder: no one here at NGS is equipped to look into the Byrd matter (§T7). I.e., the 1926 claim's sole major certifying institution is pleading non-competence at certification! (Again, recall [p.5] that the American Geographical Society has never honored Byrd's 1926 flight. Or Peary's 1909 claim.) Note: one of the most reliable marks of a crank is the stubborn inability to admit even the most utterly undeniable mistake. As one watches the formerly NGS-scorned (Rawlins 1973 p.253) chief of the cranially-impenetrable Cook Society, now defending the equally-openminded NGS' co-favorite explorer, Byrd (who had the scientific & political sense to side with Peary *against* Cook), it seems as if it was always just right & natural (if not absolutely inevitable) that these two irresistible farces would ultimately semi-fuse into an immovable blobject.²¹⁹

©20 [Note to §Q1.]

FlameKeepers vs Scholars: Why Are Outsiders Making the Analytic Finds?

The Byrd Center wouldn't exist if Byrd hadn't hoaxed, since the 1926 lie is what made him famous, an ironic point first privately realized (right away, in 1996) by the franker among the many excellent scientists at the Center — scientists who are in no way responsible for the unscientific behavior of certain administrators and publishers. Note: before and at the 1997 Byrd Center conference (which I attended uninvited), DR urged the Center to start — before the Byrd generation is gone — inviting to future conferences the families of Balchen & Finn Ronne (both of whom suffered injustices from Byrd's power), perhaps also putting portraits of these fine fellow explorers beside Byrd's on its walls (a painting of Hubert Wilkins [the first man to fly in the Antarctica] is already there), with the hope of finally winding down the bad feelings that have existed for too long between the factions. (E.g., I'd like to have seen Bolling Byrd & Finn Ronne's widow, Jackie [first woman to winter in Antarctica], become friends.) No one at OSU ever mentioned the idea to me again. (The Center instead just continued to not invite DR to any of its conferences.) Should a prominent polar center be acting so clannishly un-universal? Which brings me to a related point: since Goerler and the similarly skulking Byrd Center rulership seem cultishly unable (©12) to acknowledge the value of any of DR's discoveries on Byrd (or even [©14] to try maintaining communication with DR during OSU's vaporously insubstantial refereeing process for *To the Pole*: §Q1),

²¹⁹ As noted at ©21, both NGS and CookSoc behave as if enough money can buy truth — or, more accurately put: Victory in public truth-perception. Neither organization has stopped to consider the fantastic nature of their implicit model: for coverups to be effective in the world of science, the coveruppers must pitch a perfect game every time out; but not even the hardest cynic seriously believes that the scientific world is that corrupt. There will always be a few people who are not buyable. Killing them off is a more Herculean task than killing the multi-headed Hydra — and it never, never, never ends. I.e., it's going to require perpetual expensive (and risky) eternal-flame infusions of inducements to weak-minded intellectual prostitutes who will either mangle science themselves, or find & promote loonies who'll do it for them — most of whom will sell out because they haven't the talent to survive honestly in the academic community. (Whether this analysis relates to certain humorous features of the current report's revelations, I will leave the reader to decide.)

I'm finally forced publicly to pose a question I'd never wanted to ask at all (even privately), namely, WHAT does it say about the IQ of Ohio State University's Archives, Mapping Dep't, & Byrd Center rulership, that: all had access to the Byrd diary for months (years in some cases) before DR, yet not one of OSU's experts was able even to realize the fact (§E8) that the sextant shot at D 11 was given for *exactly* the same time (1926/5/9 7:07:10 GCT) as a sextant shot in Byrd's (physically adjacent) official report, though recording a wildly different solar altitude, putting him over 100 miles further south than later claimed for that time? Nor did any of these munificently OSU-salaried purported experts note that the diary (Fig.3) has Byrd still *en route* to the Pole at 9:15 GCT, though (§D6) his official report (all four versions of it — each in OSU's Archives) has him departing then. (For the pinnacle of perverse irony: these are the *very same* crimson-mugged genii who are now ever-so-neutrally attempting to pretend that unpaid-investigator DR has found nothing of any real value. . . .) Like, how much scientific intelligence would it take to wonder why sextant data were *erased*?! (Erasers are verboten even in freshman physics labs.) Final thought-out-loud on the Byrd Center: does anyone REALLY think that the decade-long "loss" of the diary 1985-1996 had no relation to the problem posed by the 1985 discovery of these erasures? (No outside expert ever saw the diary in 1985, and none of the newspaper stories at the time mentioned the erasures.) AND there are now not one but *two* mentally-impenetrable polar cults lamprey-attached to OSU, which reminds us (while on the subject of longtime-cult-possessed-yet-still-unexamined expedition records) of the Frederick A. Cook Society, another OSU-affiliated family-founded institution (fn 130), whose top scholars are a p.r. man & a lawyer, and which *exists* to worship sainted-faker Cook. (The best scholar on the Cook side, Ted Heckathorn, isn't a CookSoc member. His key finds on Peary were those of an outsider to the geographical establishment.) CookSoc has possessed & fondled his diaries for years. Yet ALL the time-table contradictions, plus his self-indicting Mt. McKinley photo (see *NYTimes* 1998/11/26 p.1 story on Bryce's DIO 7.2 revelation of the photo) ended up being revealed not by its own *hugely* remunerated officers but by "off-campus" talent (paid but a pittance by CookSoc): the extremely able researcher & analyst, Robert Bryce (a Head Librarian at Montgomery College). See Bryce 1997 & DIO 7.2-3. A rather more jocular treatment is found at DIO 7.3 ¶9. To re-emphasize: long before Bryce's book & DIO 7.2-3's revelations appeared (1997), CookSoc's officers & cult had access to Cook's diaries and photos. Yet no one among the devoted worshippers seems to have studied these documents of *their own hero* with enough intelligence to discern the contradictions Bryce & DIO discovered. (Are we here dealing with dedicated scholars or careerists? And: how did Ohio State University end up deeply involved with cults?) Do the trustees governing the relevant institutions' heavy funding learn anything from these embarrassing contrasts, or from their cornered fundees' vicious penchant for *ad hominem* resorts (©19 & fn 130) in the face of overwhelming evidence against their heroes?

©21 [Note to §T14.]

Nuts, Sluts, Grosvenors, & Clones.

The promoters of each of the 3 links in the connected chain (§M2) of US North Pole hoaxes, continue to con nontrivial (if ever-shrinking) fractions of the public into sharing their faith in their man's exploring fantasy. The prime weapon is simply: money. Once a hoax's acceptance generates funds beyond a certain critical mass, it is unkillable, since enough lucre can make any person or institution Respectable, regardless of how that money was obtained. (Successful pols, celebs, & other questionable characters must master exactly the same investment-gamble: [1] push-past-the-initial-hump & then [2] stabilize the money-cycle: do incoming funds exceed outgoing expenses for: [a] standard promotional publicity, and [b] silencing criticism of, & possible penalties for, how the enabling funding was originally obtained?) National Geographic wealth may have stemmed from questionable activity even before its Peary hoax: regarding NGS mentor Alexander Graham Bell, see L. Coe *The Telephone & Its Several Inventors* 1975 (McFarland&Co) esp. pp.69, 186, 198-213. NGS was founded by G.G.Hubbard in 1888. GGH's son-in-law was Bell, whose

son-in-law was GG1 = G.Grosvenor One, who sired M.B.Grosvenor, father of NGS' current richest officer, G.Grosvenor 2. How many "National" institutions or "societies" are run by 5 generations of the same consistently ethical family? CookSoc chief Russ Gibbons (once a victim of NGS arrogance: Rawlins 1973 p.253) now differs hardly at all (©19) from NGS' Gil Grosvenor 2. Each acts as if squandering enough money can protect his myth from the awful evidence against it. Both have gotten ever deeper into supporting what have turned out to be sensational lies. And each has been reduced to merely staging a well-cast pretense that a "controversy continues". (Also the pathetic policy of the *Journal for the History of Astronomy* as evidence against its poster-astronomer C.Ptolemy continues to avalanche: DIO 11.3.) And so a pseudo-controversy goes on being staged long after every independent expert knows that the issue has been settled with cast-iron definitiveness. GG&RG have each by now wasted hundreds of thousands of dollars upon his special mission. Neither would dare meet DR in a 1-on-1 debate.²²⁰ Each (esp. GG) instead hides behind the squid-ink "research" of the kind of scholar who figures that (DIO 4.3 ¶15 §A3) the side with the most money is the one that's right. Sacred-tenet-obsessed paymaster-archons typically can't even tell who'll be a non-embarrassing hired gun and who won't, unaware that in science it's hard to find&entice capable "consultants" or dupes, since the very best scientists are so self-confident and established that they aren't for sale. Those who dwell in perpetual intellectual and moral twilight²²¹ cannot of course even begin to understand this. But when one is so detached from the real world — even while perversely supposing that distorting reality is practical-realistic — a tragedy of eternal frustration is predetermined. (Analogously, see DIO 7.1 News Notes fn 3.) *Truth can be rented but not bought.* Oh, yes, public perception of truth can indeed be warped for years on end by steady lavish fiscal injections. (Are the Grosvenors proud to have proved that?) But, no, reality can't be bought off permanently. [See DIO 6 ¶1 p.4's hush-slush lullaby.] Nor can one forever evade evidence by flight: the wise scientific historian will not petulently regard unexpected evidence, contrary to his pet theories, as uppity botherations — but will humbly&responsively welcome the news as a potential gift-warning (©2): *a possible harbinger of lots more of same that may yet come.* And he keeps ever in mind Tycho's motto (DIO 3):

Neither wealth nor power, but knowledge, alone, endures.

Notes added 2003/12/16: [A] Byrd made off&on diary entries (G 60-79) from 1926/4/5 (N.Y.) until his flight's 1926/5/9 return leg. Then, starting with 18^h of tense, story-juggling Kings Bay seclusion (fn 43), *he never wrote another 1926 word in this diary.* See fn 66. (Peary stopped diary-entries from 1909/4/6 "Pole" until 4/9: Rawlins 1973 pp.230&284.) [B] Top NGS-apologist Jos. Portney will not accept our suggestion of written debate.

Conventions in the Foregoing Report

Numerical rendering of dates is astronomical (see DIO 8 ¶5 §Q2): year/month/day.

For newspaper citations, we use year/month/day:page:column, with equal sign for double columns. Yearless dates are 1926 (unless another year is an obvious antecedent).

E.g., *NYT* 5/17:1:3=4 stands for *New York Times* 1926 May 17 page 1 double-column 3-4.

Both types of miles are used. (See below at Abbreviations and-or above at fn 24.)

²²⁰Nor, needless to say, is the captive *Journal for the History of Astronomy's* Editor-for-Life (math&integrity-challenged M. Hoskin) willing to debate DR. Indeed, the *JHA's* equally cowering-even-while-sniping #2 Editor has already run away from such a direct proposal: DIO 9.1 News Notes.

²²¹See fn 219. Though NGS continues to try fogging the public mind to prevent general realization of the Society's past rôle in promoting fake polar claims, I think that the Grosvenors see themselves (with some justice) as positive contributors to society. Additionally, it should never be forgotten that *National Geographic* is one of the few major magazines that has consistently refused to run take tobacco ads. How many other big journals besides *NGM & Reader's Digest* can boast that they have held to principle on this for most of a century? [And Dave Roberts 2001 p.154 makes an appropriately perceptive & generous point: National Geographic's "reputation for veracity in matters where it does not nurse a vested interest" is a key reason why it could maintain the Peary myth as long as it did.]

References

- (A far fuller bibliography is appended to the 2000/1 *Polar Record* version of this report.)
 Bowditch = *American Practical Navigator* (annual).
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Abbreviations

- AENA = *American Ephemeris & Nautical Almanac* (annual, US Gov't Printing Office).
 AI = Amsterdam Island.
 d-i = drift-indicator.
 dr = dead reckoning.
 fn = footnote; fnn = footnotes.
 G = page in Goerler 1998.
 GACT = Greenwich Apparent Civil Time.
 GCT = Greenwich Civil Time (midnight epoch).
 GMT = Greenwich Mean Time (noon epoch).
 HIWTYL = heads-I-win-tails-you-lose.
JF or *Jo Ford* = the Fokker trimotor, *Josephine Ford*.
 KB = Kings Bay, Spitzbergen.
 mi = statute mile [diary] = 5280 ft. See fn 116.
 nmi = international nautical mile [typescript] = 1852 meters. See fn 24.
 NARA = US National Archives (see References).
NGM = *National Geographic Magazine*; *NGS* = National Geographic Society.
 NPP = Nancy Porter Productions.
NYT = *New York Times*; *NYTBR* = *New York Times Book Review*.
 r&p = refraction & parallax.
 RG = Record Group (at NARA).
 RGO = Royal Greenwich Observatory (England).
 rt.asc = right ascension.
 SD = semi-diameter.
 TA, TB, TC, TD = Byrd 1926 report. (See References [under [at Byrd] & fn 22.)
 ZD = zenith-distance.

²²² An admirably super-complete index to *Cook & Peary* is available from Bryce; telephone 301-353-7855, fax 353-7859.

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Contributors should send (expendable photocopies of) papers to one of the following: Robert Headland [polar research & exploration], Scott Polar Research Institute, University of Cambridge, Lensfield Road, Cambridge, England CB2 1ER.

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

Keith Pickering [navigation, exploration, computers, photography, science ethics], 10085 County Road 24, Watertown, MN 55388.

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

Hugh Thurston [early astronomy, pure math, WW2 cryptography, skepticism] (Univ. Brit. Columbia, Prof. Emer. Math), Unit 3 12951, 17th Ave, S.Surrey, B.C., Canada V4A 8T7.

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

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- Publisher & journal cited (1996 May 9) in *New York Times* p.1 analysis of his discovery of data exploding Richard Byrd's 1926 North Pole fraud. [*DIO* vol.4.] Full report co-published by University of Cambridge (2000) and *DIO* [vol.10], triggering *History Channel* 2000&2001 recognition of Amundsen's double pole-priority. New photographic proof ending Mt.McKinley fake [*DIO* vol.7]: cited basis of 1998/11/26 *New York Times* p.1 announcement. *DIO* [vol.4] identification of prominent astronomer who stole Royal Greenwich Observatory file on Neptune case: vindicated 1998. [*DIO* vol.9.] *Nature* 2000/11/16 cover article pyramid-orientation theory: *DIO*-corrected-recomputed, *Nature* 2001/8/16.
- 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).
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