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DIO

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Dr.Frederick Cook's True 1908 Path Recovered

Cookite Plagiarism

Snow Job & the 7 Dwarfs

The Great Comet of 1618 Long-Ignored Galileo Episode

ERatosthenes' AIR ERRor

BuriedGlory—AccurateAncient Astronomical Achievements: Badmathed by *JHA* and *Isis*

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An Irreplaceable Loss:

Longtime *DIO* Editor Keith Pickering has died, in an automobile crash 2017/11/5. Editor until 2014, his rôle in the history of *DIO* is unique in that, as well as designing some of the key mechanics of the journal, he shared in refereeing articles for it, and contributed perceptive ones to it — including the multifaceted paper (www.dioi.org/vols/wc0.pdf, *DIO 12* ‡1 2002) that established beyond doubt Hipparchos' authorship of history's pioneering 1025-star Ancient Star Catalog. His mathematical skills easily exceeded those of all current historians of science put together, as his various research projects demonstrated.

His initial book was devoted to the 1^{st} surface conquest of the North Pole in 1968, by fellow-Minnesotan Ralph Plaisted's expedition. Keith's last years produced several papers on cosmology (a new career at 60^{y}!) followed by *The Lost Island of Columbus: Solving the Mystery of Guanahani*, http://columbuslandfall.com/ccnav/author.shtml, a typically well-reasoned book on his prime life's-project: where on 1492/10/12 did Columbus 1^{st} set foot on the New World?

His dedication of the book "For DR / who set me on the road" is a valued honor that reflects his pride in helping *DIO* thrive, as well as recalling that his 1^{st} paper on the Columbus mystery appeared in *DIO 4.1* ‡2, www.dioi.org/vols/w41.pdf, in 1994, 23^{y} ago.

Both as analytic genius and cheerful friend, we of *DIO* will miss him — to the end of our own lives.

‡1 "A Hack Job": The Enduring Perils of Copyism

by

Robert M. Bryce

A How the mighty have fallen

In 2005 yet another attempt to resuscitate Frederick Cook appeared under the imprint of the once-respected independent publisher W.W. Norton Co., whose proud motto had been "books not for the single season, but for the years." As a case study in how the print publication industry is foundering, Norton's publication of *True North* by Bruce Henderson would do nicely. It speaks volumes about the sorry state of Editing, Proof-reading, and Fact Checking in a time of struggle, transition and consolidation in the face of new technology in which several mightier than Norton have already fallen.

B A penchant for plagiarism

B1 This is all the more disheartening because Henderson's previous polar potboiler, *Fatal North*, had already firmly established his propensity for plagiarism and reader deception. That book was not simply an unnecessary retelling of Charles Francis Hall's *Polaris* expedition of 1871-72, already told so masterfully in Chauncey Loomis's *Weird and Tragic Shores* (NY, Knopf, 1971), which acted as Henderson's blueprint. In addition, a significant portion of this retelling had been copied from *Arctic Experiences*, containing Capt. George E. Tyson's *Wonderful Drift on the Ice-Floe, a History of the Polaris Expedition, the Cruise of the Tigress, and Rescue of the Polaris Survivors, to which is added a General Arctic Chronology*, Tyson's account of his experiences as a member of Hall's expedition, published by Harper and Brothers in 1874, edited by E.V.Blake. *Fatal North* also contained a considerable amount of quoted dialog between the expedition members of which there is no record in Tyson's book or any other source. Henderson couldn't copy this, so he simply made it up.

B2 In fact, although Henderson adopted all of the trappings of a legitimate scholarly effort in *Fatal North*, an examination of the three books will quickly demonstrate that, beyond his fictional dialogs, Henderson's talent for either original research or prose is limited indeed. Even his copying without attribution was done in so crude a manner that no responsible secondary school teacher would have permitted it in a student paper. Take this passage on a single incident as copied from Tyson's account:

p.223 of Tyson:

"I have been thinking of home and family all day. I have been away many Thanksgivings before, but always with a sound keel under my feet, some clean, dry, decent clothes to put on, and without a thought of what I should have for dinner; for there was sure to be plenty, and good too. Never did I expect to spend a Thanksgiving without even a plank between me and the waters of Baffin Bay, and making my home with Esquimaux; but I have this to cheer me — that all my loved ones are in safety and comfort, if God has spared their lives; and as they do not know of my perilous situation, they will not have that to mar their enjoyment of the day."

p.166 Henderson:

"Tyson thought of home and family all day long. He had been away at sea on many Thanksgivings before but always with a sound keel under his feet, clean and dry clothes, and no thought of what he would have for dinner, for it would doubtless be (on a ship?) turkey with all the trimmings aplenty and delicious. Never did he expect to spend a Thanksgiving without even a plank between him and the waters of Baffin Bay, making his home in an igloo with Eskimos on an ice floe. But he had this to cheer him: his loved ones were together in safety and comfort, and they knew nothing about his perilous situation."

C Encore!

C1 Likewise, Henderson tries to pass off *True North* as an unbiased study of the Polar Controversy, backed by research in the original sources. It is nothing of the kind. Like *Fatal North*, Henderson again merely picks and chooses what suits his bias for Frederick Cook from easily obtained secondary sources without acknowledging this is what he has done, although he lists the original sources in his bibliography.

C2 Nevertheless, on its jacket, Norton's copy writers tell us "Bruce Henderson has crafted a gripping account of the claims and counterclaims, and presents fascinating scientific and even psychological evidence to put the harrowing details of polar exploration in a new context." In reality, *True North* had about as much "craft" to it as *Fatal North*.

C3 Henderson's blueprint this time was the now-obsolete first biography of Cook, Andrew Freeman's *The Case for Doctor Cook* (NY, Coward-McCann, 1961). *True North* repeats the substance of Freeman's portrayal of Cook a a naïve, helpless and even hapless outsider cheated of his great achievements and victimized by the big power and monied establishment represented by the Peary Arctic Club, which bankrolled the efforts of Cook's eventual polar rival, Robert E. Peary. But not only does Henderson adopt this characterization pioneered by Freeman, and taken up by all of Cook's subsequent partisans; as he did with Tyson's, he often appropriates Freeman's exact text, with only the slightest of paraphrasing.

C4 To cite just one example, compare this paragraph of Freeman's text on his p.17 and one from Henderson's p.31, line for line:

Freeman: "The great blizzard of 1888 forced him to suspend milk deliveries and medical studies. Not a wheel turned on streets, roads, or railroad tracks."

Henderson: "During a massive blizzard in 1888, New York City came to a standstill, leaving Frederick unable to make milk deliveries or attend class."

Freeman: "There was a dearth of all necessities, principally coal. To replenish his mother's supply, he put sledge runners on an eighteen-foot boat Theodore had built to use at the beach during the summer and hitched two of his horses to it."

Henderson: "To replenish the family's coal supply he rigged up sledge runners on an eighteen-foot boat built by one of his brothers for summertime at the beach, and hitched two horses to it."

Freeman: "As he drove home from the coal yard, he was offered premium prices for deliveries."

Henderson: "On the way back from the coal yard, he picked up other customers willing to pay a premium for coal deliveries."

Freeman: "Night and day for a week or more thereafter he and Will were in the coal business. Before the boat-sledge was retired, a picture of it was made by a photographer for one of Frank Leslie's magazines, which reproduced it as an exhibit of man's resourcefulness during the blizzard."

Henderson: "He was in the coal business round the clock for a week, and before the specially outfitted boat was retired, a photographer took a picture of him standing with his innovation. The image ran in a magazine as an example of individual resourcefulness during the storm."

C5 Henderson might try to defend himself by saying he is only repeating facts, but because these "facts" and their specific sequencing are original to Andrew Freeman's book, being the result of personal interviews Freeman conducted with Cook during the 1930s (no one knows whether this story is really true), Henderson's use of them nearly verbatim without citing their source is the very definition of plagiarism.

D Curiosities of a copyist

D1 Other examples of similar direct paraphrasing could be cited that occur in Henderson's book: many more from Freeman, some from John Edward Weems's *Peary, the Explorer and the Man*, and some from my own book, *Cook & Peary*. Additionally, scores of facts 1st published in *C&P*, still available nowhere else but the original documents, are reused in the same contexts, in the same sequence and in similar language unique to my book, making clear that it, and not the original documents, was the source of these facts.

D2 The trouble with copying, beyond its ethical considerations, is that when something is copied without examining its sources, the author has no way of judging its accuracy. In other words, copied material in the hands of an uniformed author is only as accurate as the material being copied, at best. He has no real basis by which to distinguish what is actually true or false. Because of this, Henderson has inevitably copied others' mistakes, "facts" that newer scholarship has supplanted and material that the copied writer simply made up, whole.

D3 The point is not that these errors matter in the larger scheme of things, but that they show the methods by which Henderson assembled his text from others' without examining their sources. Almost all of his quoted references taken from original documentation are copied from already published quoted references to those same documents in others' books. A comparison of these common quotations shows that Henderson's quotes use the exact same text as the other writer published, even when inaccurately transcribed by the first author, that he uses the same edits done to the original text by the author he copied, which do not appear in the originals (see Weems's note on this, on his p.viii), and he uses ellipsis marks to omit the exact same text omitted by others, or he cites less of the quotation than appeared in the secondary work used, but never more. All of these characteristics of Henderson's quotes are dead giveaways proving that Henderson did not use the original sources he cites, but instead used the secondary sources that originally cited them. Yet he cites his sources as if he did use the originals. [Like pseudo-scholarship detected at ‡3 fn 2 & ‡5 §B8.] In doing so, Henderson has attempted to deceive his readers as to the basis of the authority of his text, and ultimately the conclusions he draws. His citations (§C1) of "original sources," therefore, are mere window dressing, not the actual authority of his text. To anyone familiar with his sources, it is self-evident that Henderson was never even in the same building with 95% of the "sources" he quotes. Such a willful deception of his readers condemns *True North*'s scholarly merits even if it were not defective in other ways. In a book whose title's first word is "True," that's not a small matter.

E An inability to distinguish fact from fiction

E1 Ironically, the few previously unpublished materials that Henderson does use introduce many completely false statements into Henderson's narrative. Most of these come from a single source: Cook's unpublished memoirs. They were written in the mid-1930's, as much as sixty years after the events they describe. Yet they are full of quoted dialog. Think about it: can you now recall, word-for-word, conversations you had even a year ago, much less twenty or sixty years ago? Cook couldn't either, and such material is no more valid than the fictional dialogs Henderson made up to fill out *Fatal North*. An author can't put words into the mouths of historical characters and call it non-fiction. Likewise, an author can't rely on after-the-fact memoirs that contain many "facts" that can easily be shown to be Cook's own self-serving inventions when compared with contemporary primary sources associated with the events he is describing. To cite just one infamous example from Cook's memoirs used by Henderson, consider Cook's account of his alleged diagnosis of pernicious anemia in Robert E. Peary in 1901.

E2 As I show in my book (p.788), pernicious anemia is impossible to diagnose in a patient, even with all of today's medical knowledge, as far in advance of its once-fatal manifestations as Cook claimed to have done in his memoirs. The account of Cook's diagnosis of the disease 19 years before it killed Peary, and Peary's refusal of the correct treatment Cook prescribed to Peary (although then totally unsuspected), is simply a fantasy concocted by Cook in 1935 to lend an ironic twist to Peary's fate. By then, Peary had already died of pernicious anemia (in 1920) and the treatment of the disease had been described by its discoverers, for which they won the Nobel Prize in 1934. Cook's "diagnosis" is a favorite fable of the Frederick A. Cook Society, Cook's booster club, and is endlessly repeated by it as fact in its propaganda, which goes so far as to say Cook should have gotten the 1934 Nobel Prize instead!

E3 Even given this, Henderson never fathoms the difference between simple anemia, which can be caused by any number of underlying conditions, and pernicious anemia. Pernicious anemia is a specific autoimmune endocrine disorder that results in an inability of the stomach to produce the intrinsic factor necessary to metabolize vitamin B12. It is not a blood condition, *per se*. And it is definitely not a "Polar malady" as Henderson says on his p.277. Anyone can develop PA, but it is most likely inherited. The fact that *True North*'s index lists only "anemia" to cover all of his references to both simple and pernicious anemia, shows Henderson didn't know the difference. It is dangerous to the credibility of copyists to try to make pronouncements on complex subjects they haven't taken the proper time to study, like medical pathology or the Polar Controversy.

E4 Since Cook's memoirs are corrupt, I made very little use of them in *Cook and Peary*, and when I did, I always cautioned the reader that they were being used only because there was no other account. This is how a responsible author uses the material at his disposal. He evaluates all sources, compares them, rejects after-the-fact accounts that conflict with other sound primary evidence or known facts (in this example, medical reality), and synthesizes his account accurately. Then he cites exactly what he has used, its actual source, and when necessary, cautions the reader when he doubts its authenticity. A scholar does not sit down with half a dozen published books and booster club publications and assemble a new text from them, just trusting them to be correct, or picking and choosing passages from them that suit his agenda. There may be other names for such a writer, but none of them would be "scholar."

E5 And, of course, a scholar never quotes as if he has used the originals when he has merely lifted them from a previous writer's finished pages. Henderson, however, has done this repeatedly, and because even the best copyist makes mistakes, he has inadvertently made an enormous number of errors through copying mistakes or because he lacks knowledge of the underlying topics, like pernicious anemia, or even elementary polar conditions in general. My point, again, is larger than the fact that Henderson made these mistakes. Every book has mistakes, including my own. The point is, a huge number of mistakes and obviously ignorant statements undermine the authority for whatever conclusions an author may eventually draw.

E6 Worse yet, some of Henderson's citations are pure fabrications, because they are also due to copying others' citations rather than consulting the original sources. (For a detailed example of how this happened, read the ones appended to the end of this review: §L.)

E7 If an author is going to rely on being a copyist, he needs to know enough about his subject to at least be able to recognize which is the most reliable secondary source from which to copy. Henderson would have done well to have stuck to my book, it being the most recent and based on a massive number of primary sources, many never before cited, and all precisely documented in its more than 2,400 endnotes. Generally, where he did, he did well, but, alas, *Cook and Peary* also has a few errors in the text, and Henderson relied on so many of its facts that he managed to copy at least two of its mistakes into his own text: L.L.Dyche didn't teach at Kansas State University (Henderson's p.99, copied from my p.114) but at the University of Kansas; the ship the *Portia* ran over in 1894 was the *Dora M. French*, not the *Dora N. French* (his p.106, copied from my p.128). Sorry.

F Even copyists need knowledge

F1 Copying from other non-fiction titles is one thing, but when an author indiscriminately copies from fantasy sources without knowing any better, this really condemns all pretensions to original scholarship or subject expertise. Quoting extensively from Bradley Robinson's *Dark Companion* (NY, McBride, 1947), whose style is more like a Rover Boys novel than a work of non-fiction, and which is filled with totally invented dialog and "facts" that appear nowhere else and can be easily demonstrated to be the inventions of either Robinson or his subject, Matt Henson, when compared with known primary sources, is damaging to Henderson's or any other author's credibility. To be able to tell truth from fantasy, you must have a decent grounding in a subject won by many hours of study, and the first step in that process requires at least reading the easily available published accounts of those who participated in the events under study.

F2 That Henderson thinks Langdon Gibson's first name was "Longdon" (copied from Robinson, who bizarrely thought "Longdon" and Gibson were two entirely different men!), and that he reports John Verhoeff was from St.Louis, rather than Louisville, shows Henderson has never read any of the primary published books on Peary's North Greenland Expedition by Peary, his wife, or Eivind Astrup, much less ever looked at the extensive original documentation of that expedition at the National Archives II or Bowdoin College. Even Andrew Freeman got those two facts correct. But when Henderson starts incorporating details from childrens' books that have no pretension to being truthful (apparently children are in even less need of Truth than adults), like J.Alvin Kugelmass's *Roald Amundsen, a saga of the polar seas* (NY, J.Messner, 1955), and doesn't realize that anything is wrong, a knowledgeable reader has no alternative but the dismissal of Henderson's whole text as having doubtful authority, at best, and his conclusions as having no credibility whatsoever.

F3 The Belgian Antarctic Expedition is one of the best documented of all polar expeditions by its participants. Of the nineteen who sailed on it, five left published accounts, so there is not much question over the basic facts. Yet here are some of the fictitious "facts" Henderson copied from Kugelmass's book for children (recommended for "Grade 7 and up" and panned by the *New York Times Book Review* in 1955): There was no "French sailor" named Ernest Poulson in the crew at all; they were all either Norwegian or Belgian. (There was a French cook, but he left the ship in South America before the *Belgica* sailed for Antarctica). Had Henderson even opened Cook's own account of the expedition, *Through the First Antarctic Night* (NY, Doubleday-Doran, 1900), he would have seen pictures of every sailor on board (opposite p.401), and this person is not among them. Not only did the non-existent "Poulson" never fall on his own knife and die, no sailor went mad and threw himself from the yardarms, either. These are all Kugelmass's inventions, copied by Henderson. Two people died on the expedition: one, a sailor named August Wiencke, fell overboard in a storm on the way to Antarctica, and Emile Danco, the magnetician, died of a congenital heart ailment during the winter there. All of this proves Henderson didn't even bother to read the one easily available source in English detailing this expedition — Cook's own — although he is ostensibly the subject of Henderson's book. I suppose that is another advantage of being a copyist; you not only don't have to bother with looking at original sources, you don't have to read much of anything. But the disadvantage is that you have to trust much. Because of this, copyists who pretend to be scholars always get caught out.

Figure 1: Roald Amundsen with Order of Leopold, earned on the *Belgica* Expedition, early in his career, on his way to becoming the ultimate polar explorer, whose feats included genuine first attainment of the South Pole 1911/12/14 and genuine first attainment of the North Pole 1926/5/12.

G Boosterism and bias

G1 Beyond mistakes, there is the matter of intent. It is very clear that the intention of Henderson's book, following Freeman's lead, is always to maximize Cook and minimize Peary. One of the many examples of this that could be cited is Henderson's repetition of another favorite fable propagated by Cook's boosters: that Cook's services to the Belgian expedition were thought to be so exceptional that he was the only non-Belgian awarded the Order of Leopold after the expedition returned from the Antarctic. (Henderson's p.132, copied from Freeman p.58). In fact, the other three members of the scientific staff, Arctowski (Polish), Dobrowski (Polish), and Racovitza (Romanian), plus all of the officers, got the same award as Cook. In proof that both Freeman and Henderson are wrong, Fig.1 shows Roald Amundsen, the second mate, who was Norwegian, wearing his Order of Leopold.
G2 Even more remarkable, but characteristic of the way Henderson's book was assembled, Cook's memoirs (now among Cook's papers held at the Library of Congress), which Henderson heavily relied upon for favorable but fabled "facts," states on p.17 of Chapter 14: "King Leopold honored the officers and the scientific directors of the *Belgica*. Amundsen, the doctor and the foreign workers all got the same rewards. We were knighted as Chevaliers of the Order of Leopold, an honor of great distinction for which we were grateful." Here he had Cook's own contradiction of it, yet Henderson copies Freeman's incorrect statement instead.

H Uninformed and out of date

H1 Henderson's *True North* is, quite simply, uninformed; and it was out of date on the day it was released, since it failed to account for or counter any of the already published documentation that proves that Cook's two biggest geographical claims were both hoaxes. Instead, it quotes freely from those self-justifications written by Cook himself and his apologists' baseless theories and arguments as "evidence" that he did climb Mt.McKinley in 1906 and reach the North Pole in 1908, just as he said. Nothing could be more out of date than to quote Cook's 1911 book, *My Attainment of the Pole*, in his defense. My own book spent many pages pointing out its provable lies and exaggerations and citing the many improbabilities it contains. And nothing could be more uninformed than disregarding the only scholarly examination of Cook's personal papers (my own), available since 1997, which revealed doctored diaries and faked photographs showing that Cook's claims to attaining the Pole and summiting Mount McKinley were knowing frauds, especially when Henderson has manifestly made no effort to examine these materials for himself.
H2 Even so, Henderson's retelling of Cook's northern journey of 1908 on pp.228-29 adds more fabulous new details that Cook never thought of: Henderson says Cook used his collapsible boat to get back to land on his return from "the Pole." He also says he used it repeatedly to try to reach his caches on Axel Heiberg Land, but failed. Cook by his own account never used the boat until he reached Jones Sound, far south of either of these locations (‡4 Fig.2). He couldn't, simply because, again by his own account, he still had as many as ten dogs with him up until then. And he did not winter upon reaching Cape Sparbo, as Henderson would have it. He went far past that cape, seeking to reach a whaler in Lancaster Sound so that he could emulate Nansen's famous chance-rescue 1895 encounter with Frederick Jackson in Franz Josef Land. Since there was no whaler in sight, he then doubled back to Cape Sparbo, which he noted was teeming with game when he passed it. And he did not live in the "ruins of an old ice cave" as Henderson puts it. He reconstructed a perfectly standard Inuit winter stone igloo from the ruins of an old one, and enjoyed a very comfortable winter, by Arctic standards, shooting the abundant musk oxen there at will with the 120 rounds of ammunition he still carried with him. After the sun set, he spent the winter there perfecting the details of his fictitious attainment of the Pole in his notebooks. Cook's narrative is not at all confusing on these points, except his experiences that winter, but that point is clear from his original notebooks. His so-called "stone-age winter" is simply yet another of the favorite fables of the Frederick A. Cook Society, disprovable from Cook's own hand.
H3 Using disproved "findings" to bolster Cook's case does nothing for Henderson's credibility, either. Cook's long journey through the Sverdrup Islands (Heiberg & Ringnes), where the ice did not drift, even in summer, proves nothing about him as an "ice traveler" or his sledging ability to reach the North Pole over constantly shifting pack ice. But it does prove that he lied about his return route.

I Polar precedents

I1 As I demonstrate in detail in Chapter 29 of my book, Cook's "original descriptions" of conditions in the Arctic were solidly based on conventional scientific beliefs of his time, some of them now proven false, and therefore his "findings" are now inaccurate as well. His knowledge of an unknown westerly drift does not require attainment of the Pole for him to have observed it. Cook's description of Bradley Land does not even remotely resemble an ice island, and it doesn't exist anyway (even though he published two photographs of it). Therefore, Cook's published narrative is neither credible nor consistent in itself, as my analysis in *Cook & Peary* shows; and, when compared to his original notebooks, it is very inconsistent with them, even as to several versions of where he claimed to have been on certain days during his journey, and even variant on what day he claimed to have discovered "Bradley Land" and reached the North Pole. It is thus condemned as an out-and-out fake.
I2 As Capt. Thos.Hall remarked (‡4 §D1) on such conflicts as Cook's notebooks contain:

> Did all these various writings agree with themselves . . . it would not prove their statements to be true, because they might, nevertheless, be fabrications; but as they contradict each other in every particular, it proves falsehood

> absolutely. If one is true, the other speaks falsehood. If the other is true, the one speaks falsehood. There is no authority for believing either; and if the author cannot be believed in what he sets out to prove, the author is not entitled to be believed in anything he may say at any time. Truth is a uniform thing.

I3 Likewise, the "findings" Henderson lists about Mt.McKinley prove nothing about Cook's claim to have climbed it. The estimate of its elevation Cook gave was nearly identical to the one already determined by triangulation previous to his "climb." That McKinley has twin peaks is obvious to anyone approaching from the southeast, and was first reported by Edward Brooks in his official report of his travels in its vicinity in 1902. Even Cook made note of this fact in 1903, and is quoted as doing so by Henderson, himself, on his p.150, before Cook attempted any climb at all. Also, the upper slopes are easy to observe from 25 miles off at the snout of the Muldrow Glacier, even without binoculars, and Cook had this very view through binoculars in 1903. When an explorer has circumnavigated the entire mountain, as Cook did in 1903, all the time looking up at the summit, it is not evidence of his having climbed it for him to state what could be seen looking down from the summit. But, in fact, Cook badly exaggerated how far he could see from the summit, and he failed to make any description at all of the dozens of then unknown glaciers and peaks he could have seen had he ever been there. That's because the closest he ever got to the summit in 1906 was about 15 miles as the crow flies, and he never in 1906 climbed higher than 5,338 feet (Fake Peak: *DIO 7.3* ‡9 fnn 1& 7) of the 20,310 feet [latest estimate] above sea-level required to reach McKinley's summit.

J Who found the "findings"?

J1 And whose "findings" are these anyway? They are no more than the pet arguments of Cook's boosters, which are endlessly repeated by them and have been copied by Henderson (again, nearly word for word) from the publications of the Frederick A. Cook Society.
J2 Upon finishing Henderson's book I realized that he acknowledges absolutely no one as an aid to his writing it: no librarian, no archivist, no editor. This is singular among all previous books ever written on this subject. Given all of the above, however, it is also self-explanatory: copyists don't require scholarly assistance and don't want to acknowledge their real sources, anyway. And I also noted that his bibliography, small as it is, is nevertheless padded. For instance, Henderson doesn't even have an index entry for Ernest Shackleton in his book, let alone a single word about his *Endurance* expedition, yet he cites his account of it, *South*. And Henderson makes absolutely no mention of Joseph Bailey, Cook's mail-fraud trial lawyer, yet cites Sam Hanna Acheson's biography of him. Many of the entries seem once again to have simply been copied for show, this time from the extensive bibliography that accompanied Frederick Pohl's introduction to Cook's posthumous book, *Return from the Pole* (NY, Pelligrini, 1951).
J3 Finally, Henderson gives no space to Cook's baffling mentality as one of the world's greatest fabulists, even though it lies at the heart of the Polar Controversy. The argument over what Cook really was, and not what he claimed to have done, is what actually drove the Polar Controversy. But to Henderson, like the Frederick A. Cook Society, it is all so simple: The naïve Underdog shorn of his laurels by the all-powerful Establishment. To blandly accept this cartoon-like characterization of such a complex person as Frederick A. Cook is to throw away the opportunity to examine the most interesting part of the Polar Controversy and the larger lessons it has to teach about human belief and how history is made.
J4 Had Henderson written a partly original book, or one with even a single original thought in it, or a single piece of original evidence that added to the history of the events it discussed, I would have been pleased to have noted it here. Alas, there are none. Instead, in reading it, I had the distinct, and well-justified, feeling that I had read all of this before somewhere: in Freeman or in Weems, and, indeed, I had no trouble at all in recognizing that I had written some of it myself. Because of the way it was assembled, then, not written, *True North* contains many accounts and assertions already proven untrue.

K You can't fool all of the people all of the time.

A book like *True North* is still possible only because the Polar Controversy is an extremely complex subject filled with more details and subtleties than most people can or want to absorb (or afford to publish), and because, as Dr.Cook knew, the big lie once spoken will always find someone with a reason to give it credence. Even so, among a number of submissions from readers whom Henderson succeeded in deceiving, one review posted by an intermittently perceptive Amazon.com reader shows that all of this was not lost on him:

> Evident in his depictions of Cook versus Peary, Henderson's motive is to prove that Cook was indeed cheated out of a victory that was rightfully his. Through Henderson's descriptions, Peary is shown to be an egotistical and hard-hearted man concerned only with fame, with a boisterous attitude and little respect for other people. In opposition, Cook is portrayed as being very humble and quiet, an inventive man who is content to share victory. When the events of the contested pole discovery come about, Henderson details how Cook was thwarted his due by Peary's sabotage, and raises suspicion for Peary's claim by pointing out that Peary would not hand over his own notes for inspection before Cook released a statement, insinuating that Peary was getting information from Cook to use in his own dubious notes. As told by Henderson, Cook's evidence, though he produced no notes as proof and with only a diary and the statements of him and his Eskimo companions to back him up, is still more credible than Peary and the incomplete notes he supplies. It is even insinuated that Peary was responsible for Cook later going to prison for mail fraud because the judge trying the case was a friend of the family. Henderson finishes up his assessment by listing all of the ways in which Cook was right or credible in both his pole and Mt.McKinley claims. So, despite Henderson never explicitly stating to support Cook, it comes through in his presentation of facts and their evident bias. Whether or not the facts are true as stated, Henderson clearly wants us to see things a certain way.
>
> Henderson's source usage raises concerns over his presentation of facts and how they support his central purpose. *True North* is rich in detail and follows the separate and intertwining paths of Cook and Peary closely, even to minute detail. Yet the background provided, including an array of personal stories and emotions too intimate to be part of common knowledge, is given no footnoted documentation, which calls into question the validity of the information, its truthfulness, and whether or not Henderson is being true to the facts and portraying them accurately. A reader would have a difficult time verifying many of the things said and claimed to have happened by Henderson. Henderson does provide a selection of source notes at the end of the book, which serve the purpose of explaining where some of the specific personal statements come from. These are actually very informative and valuable to the credibility of the story because they are all primary sources, sources that come direct from people involved or in the time — they are the words of Cook, of Peary, of people witness to the events in question. There is included a bibliography at the back, but without the aid of footnotes, one cannot tell if the books listed at the end are indeed used and where.

Another Amazon reviewer was more blunt in summing up the truth about *True North*: "In short, this is a hack job," he wrote.

L Beating a dead horse, or how Henderson invented citations in *True North*

L1 On Freeman's p.231, he cites Representative Roberts's comment on Peary's notebook: "If the members of the committee care to, I would like to have the book examined particularly with reference to its condition and state. It shows no finger marks or rough usage; a very cleanly kept book." On Henderson's p.275, this quotation is abbreviated to "shows no finger marks or rough usage; a very clean kept book," which is a slightly inaccurate copy of the portion used. But leave that aside.

L2 Freeman cites this quote as coming from "Extension of Remarks," House of Representatives, January 25, 1916, 64th Congress, 1st Session, Vol.53, No.44, Part 14, *Appendix* 1-13, p.275. Henderson cites it as *Congressional Record*, 61st Cong., 3rd sess., vol.46, "Extension of Remarks," Rep. Henry Helgesen, p.275. But the citation from the *Congressional Record* Freeman cites is actually that of a speech given by Representative Bruce Macon of Arkansas.

L3 How this happened is clear to those who actually have an acquaintance with the publications cited and the works Henderson copied his citations from, in this case Freeman's p.300. The *Congressional Record*, and the *Appendix to the Congressional Record* are entirely separate publications, with separate paginations. Macon's speech was actually given in 1911 on the floor of Congress, whereas Helgesen's "Extension of Remarks" was merely entered into the *Appendix*, never spoken. Freeman's citation of each of these is correct, but Henderson's of Helgesen, which appeared in the *Appendix to the Congressional Record* in 1916, is cited as appearing in the *Congressional Record* in 1911. Unfortunately for Henderson, when Freeman cites Roberts's remarks on Peary's notebook, he is citing Helgesen's speech a second time, so he just refers the reader to "Helgesen's 'Extension of Remarks,' as above, page 275." When Henderson looked "above" he accidentally copied Freeman's citation for Macon's 1911 speech, thus creating a unique fabricated reference because Henderson is not so good a copier. It is very clear that Henderson never read either speech in its original, but merely copied the identical excerpts from Freeman, then miscopied his citation of the latter, thus inventing a completely false citation in the process.

L4 Another example: On p.245 of Freeman's book, he quotes a passage from Judge Killits's sentencing speech to Dr.Cook. He gives as his reference on his p.303 "The excerpts from Killits' charge are from the court record." In 1973, Hugh Eames, another copyist, copied Freeman's quotation into his book, *Winner Lose All* (NY, Little Brown, 1973), and stated as his reference: "Court Record 2273, Fort Worth, Texas." Eames at least had obtained portions of the transcript of the trial (but not this speech) so he knew the number of the court record. Henderson cites portions of Freeman's quote from the speech on his p.287 and gives as his reference "U.S. District Court record 2273, Fort Worth, Tex."

L5 It is possible that the judge's speech was part of the court record when Freeman was working on his book in the 1930s, but it is no longer part of it today. I went to Fort Worth in November 1991 and spent a week there going through every page of the 12,000-page court record of Cook's trial now at the Southwest Branch of the National Archives. The judge's sentencing speech was not in that record. After I returned and made note of Freeman's and Eames's citations, I wrote to Margaret Schmidt-Hacker, archivist at the Southwest Branch, asking her to check again for this speech. After she conducted her search, she wrote to me assuring me that this speech was not among the records of the trial or any associated material (see my note fn75, p.1,065 in *Cook & Peary*). Henderson simply copied it from Eames, who had assumed its presence in the court record that he learned of by reading Freeman, but without seeing it for himself.

‡2 "Ignored" no more.: "Frederick A. Cook reconsidered" considered

An editorial review and documentation of the proceedings of the conference held at Byrd Polar Research Center, Ohio State University October 22-23, 1993.

by

Robert M. Bryce

Editor's note: This paper was originally written in late 2000, after the proceedings in question had been released by The Byrd Polar Research Center. After writing it the author was able to obtain the videotaped record of the conference. The printed proceedings of a conference are never identical to the actual oral presentations; however, the author felt the differences between the two were so unusually out of the norm, that this paper needed to take that into detailed consideration, not only to weigh the conference's impact upon its stated purpose (§A1), but also to give the reader a feel for what an attendee at the conference actually saw and heard, which would be crucial to give that question a fair answer. Alas, other projects intervened and this paper was only taken up again in 2014. Much had changed after *Cook and Peary, the Polar Controversy, Resolved* was published, and was indeed highly influential in resolving the Polar Controversy, so much so that there was little notice given it, even by the National Geographic Society, on its centennial in 2009, and the publication of a transcription of Cook's polar notebook in 2013 settled the question of Cook's claim in the negative, once and for all. Since 2000 some of the presenters at the conference and all of the then-sitting officers of the Frederick A. Cook Society have passed away. Indeed, the Frederick A. Cook Society went bankrupt and since 2009 has not had even enough funds to publish its once annual publication, *Polar Priorities*, or even a newsletter. The present paper is a reworking of the original that expands its purpose of answering a narrow question and is now aimed at giving a more complete picture of the conference by an active participant (from the audience, and who afterward contributed a paper that the Frederick A. Cook Society published separately) so that those having only the printed proceedings to rely on will have a better idea of the actual conference as the participants witnessed it. There is no disagreement with Dennis Rawlins's opinion expressed in his summary remarks at its end (§N4), that the Frederick A. Cook Society showed great openness in the organization of this conference and assembled an impressive list of speakers, and that it gained greatly in prestige as a result. Unfortunately, its failure to bring out the proceedings in a timely and professional manner limited those gains largely to those in attendance, and its failure to follow its own example of objectivity in the discussion of Cook's claims in the wake of the publication of *Cook & Peary* condemned it to its previous status as no more than a narrowly-focused booster club, and led directly to its insolvency and dissolution.

A Much hyped; poorly typed.

A1 One of the favorite lines of attack by the Frederick A. Cook Society (FACS) illustrating the supposed-bias in the writing of my book, *Cook & Peary, the Polar Controversy, Resolved*, was my failure to acknowledge the expert opinions of those who participated in the "watershed" symposium held at the Byrd Polar Research Center (BPRC) in October 1993 under the title "Frederick A. Cook Reconsidered: Discovering the Man and His Explorations." From that time until the publication of my book in 1997, the symposium was never available except in an archival videotape format [although I was chided for being "uninterested" in the videotapes of the conference, the reader is told in the introduction to the proceedings now in hand that "the tapes proved difficult for those concerned with historic and geographic research"], even though FACS had announced the printed proceedings would be forthcoming as early as 1995. While browsing a catalog from a polar book dealer in September 2000, I came upon a notation offering for sale "The long-awaited proceedings of the Cook Symposium held at Ohio State." Naturally, I hastened to acquire a copy.

A2 Although I was never officially on the mailing list of FACS, over the years I have had access to their publications, if sometimes belatedly. I asked myself, why had the issuance of this supposed ur-document in Cook rehabilitation not been page-one headlines in *Polar Priorities*? It seemed as if it had rather slipped in the side door, being announced only in *FACS Membership News* for December 1998 and never mentioned again. It never appeared on the list of publications "Available from the Society," which were listed on the inside front cover of each *Polar Priorities*. A complementary copy was not even sent to each of the presenters, which is customary. When my purchased copy arrived, I began to realize a possible reason for the lack of fanfare.

A3 After all the society's promises of publication by an "academic press," the finished report was issued as one in a series of Byrd Polar Research Center Reports (Report #18). In this format it could not possibly attract much attention. Immediately, one must ask, did it require five years to bring this "unique conference" out in such a humble format by the very institution that co-sponsored the symposium? Why, if an academic press was the announced vehicle by which its revelations were to be given to a waiting world, was it not issued by the Ohio State University Press, whose offices are just across the street from BPRC? After reading Report #18, the answer to the last is clear. It falls short of any academic press's minimum standards. In the end it was only published "through a contribution from the Society." [*FACS Membership News* vol.5, #3, p.2.] In other words, it was, in effect, self-published.

A4 An answer to the lapse of five years between conference and publication is offered in the Introduction to Report #18 itself: "The publication of the Proceedings of this unique conference were delayed for almost four [*sic*] years because of circumstances involving the transmission of final manuscripts by several of the prestigious participants, whose work literally took them to the ends of the earth in that time frame." Apparently, at least one never submitted a final paper, as shall be seen, so if that were the real reason, the proceedings would still not be published. But even if true, this hardly justifies the state of the finished product, which can be described in a single word — amateur — sadly, a credit to no one who did submit a paper. If anything, the stated reason for delay should have given the editors more time to perfect the printed proceedings. In the December 1998 announcement "checking copy" is also mentioned as a delaying factor. But this can hardly be the case.

B Editorial atrocities

B1 More inexplicable than the long delay in its appearance, is its actual appearance. How were these proceedings allowed to proceed to press in their present editorial condition? They meet not even the most minimal standards to be expected of a serious academic undertaking, and should be a frank embarrassment to all involved in their production. To the casual reader, such a state of affairs necessarily must promote the feeling that the contents were not at all valued, and therefore should not be taken seriously, because the editor apparently did not think them worthy of even routine editorial care. This is most curious for a symposium to which such rhetorical importance had been attached before it could be read by all.

B2 Among the editorial oversights: nearly a hundred typographical errors in 135 numbered pages, 12 of which are blank. [By way of comparison, 39 typographical errors have been found in *Cook & Peary*'s 1,133 pages of text.] Some of these make unintentionally amusing reading. We learn that "The stories handed clown state that Cook and his two boys did not go more than a few days' travel into the Arctic Ocean." [p.107] And "[Cook] fell to the same temperature he had on McKinley." [p.109] Many of the typographical errors are undoubtedly due to text recognition software used to convert typed copy into

electronic text. As any good editor knows, this often results in substitutions of letters that inadvertently spell legitimate words, though not the ones intended. Report #18 is riddled with these, especially the substitution of "t" for "f". Fail becomes tail, far becomes tar, fact becomes tact, four becomes tour, and so on. These, of course, will not be caught by spelling correction programs. They must be weeded out by good old-fashioned proofreading. It seems obvious from the frequency of such errors that precious little time was expended by the editor on that activity. But these are the least of the problems, which only add to the inevitable impression that this is not a serious enterprise.

B3 Some of the editor's thoughts are not exactly in Standard English. In a note in the acknowledgments, we are told "the resources [*sic*] in the photo section are labeled with each image." Looking at the table of contents, the photo section is listed as Appendix A on p.137. But Report #18 has no appendices and ends on p.136. Once the photo section has been located (between pp.68-69) it becomes apparent that the editor's statement is not only grammatically inept, it is also untrue. Of the 23 images that are not direct reprints of the symposium's handouts, only nine are labeled with the "resources" from which they came, and some of these are identifiable as to source only by virtue of the fact that they are reproductions of periodicals and newspapers that bear their own title and date. Worse than that, some of the "labels" are incorrect. For instance, one photo is captioned: "More than two years after he left for the Arctic, Cook is welcomed in Copenhagen in September 1909." However, the image is actually one of Cook's arrival in New York later that month. He is wearing the famous wreath of white tea-roses given him on that occasion, and a member of the U.S. Army's 47th Regiment Band can be seen standing beside him. But the biggest gaff is the image above this one, labeled "Dr.Cook's route to the North Pole as shown in his book." If this were true, the Polar Controversy might have been resolved long ago. The map reproduced clearly shows Meighen Island, which Cook swore he never saw or visited. The map is not from Dr.Cook's book at all, but was made in the 1930s by long-time Cook advocate Ted Leitzell. Leitzell's name, in fact, appears in its lower left-hand corner. This is a typical example of how little FACS people know about the story they so vigorously "research" and defend.

B4 And it only gets worse, with errors that severely compromise understanding and documentation of the proceedings. We are told in the Introduction that the proceedings will include "standard reference citations and bibliographies," but in Report #18 these, and much more besides, are anything but standard. • There are numerous transposed and erroneous dates (1813 for 1913, 1080 for 1908); there is even an April 141. • The end notes have no consistent style, the same work being cited in more than one paper inconsistently. • In two papers, items referred to in the text do not appear in the end notes. • Two papers have no end notes at all, even though there are citations to such given in their texts. • One paper has internal references to diagrams, but no relevant diagrams are reproduced in the paper or in the photo section. • The internal references in one paper do not match Report #18's pagination, and the original formatting of this paper has been incompletely converted to another system, leaving blind references to non-existent subsection headings. • Internal footnotes are left in ordinary-sized print and are often left hanging in the middle of the text. • In one case, the abstract has nothing whatever to do with the "paper" that follows. • Warren B. Cook (then President of the FACS) is listed as one of the "hosts" on the back cover. But Mr.Cook was ill at the time and missed the entire conference.

B5 The responsibility for these editorial atrocities falls mainly on the stated editor, who was none other than Russell W. Gibbons. [Now deceased, identified as "currently the Executive Director of the Frederick A. Cook Society founded in 1940." Actually, the Society was not founded in 1940, but 1957; it was incorporated only in 1973. The organization founded in 1940 was called The Cook Arctic Club, which quickly dissolved after Cook's death later that year.] This goes a long way toward explaining the above state of affairs. Anyone who reads the publications Gibbons regularly "edited" knows they can expect anything or nothing in the way of editing, since he had even been known to misspell

"Frederick" in the banner of "Frederick A. Cook Society" publications, and he couldn't count the number of stars on Cook's 1897 flag. But some share of the blame must fall on Lynn Everett, Editor of Publications at the BPRC at the time, whose "patience and attention to detail," Gibbons says in his Acknowledgments, was supposed to bring "uniformity to many of the papers."

B6 As much as all this makes Report #18 hard to read, its ultimate contribution to history can be evaluated only by getting on to the substance of the papers constituting the proceedings of the symposium that Gibbons hoped would change minds about Cook's supposed attainment of the pole. He later claimed that its proceedings would show that *Cook & Peary* had not actually resolved the Polar Controversy after all, because it "ignored" vital evidence by the expert presenters at the conference and was, therefore, biased and selective in the evidence it presented. Many of the issues raised in the papers reproduced in Report #18 have already been covered in detail in previous publications, e.g., www.dioi.org/vols/w73.pdf, & www.dioi.org/vols/w93.pdf, but still we need to focus on that last claim: **is there really anything in its proceedings that should have been mentioned in *Cook & Peary* that was not, that would have made a difference if it had been noted there**?

A brief summary of each paper follows, seeking to answer that question.

B7 To answer this question fairly, however, and also give a better idea of what an attendee experienced at the actual conference, the printed proceedings alone can't be relied upon to give an answer. That is because the proceedings' printed papers differ, sometimes markedly, from what participants at the conference actually heard, and those oral presentations, of course, would have been, before their publication, the only basis on which anyone who attended the conference could arrive at such an answer. Therefore, beyond reviewing the papers as printed, the video tapes made of the conference were also reviewed in relation to the printed proceedings. Unfortunately, these videos do not represent the entire proceedings, leaving out the moderator's introductions of most of the speakers, and possibly some of the after-presentation question sessions, because there are no questions after some of the key speakers' presentations, for instance, that of Wally Herbert. However, the following comments rely only on what exists on the video record and not on any memory of the conference as, of necessity, I did when I wrote my previous *DIO* papers [internet sources at §B6 above]. Direct conflict, if any, between this paper and the earlier ones, then, should be settled in favor of the current paper, for that reason. In this review the comments on the video tape record have been placed at the end of the comments upon the printed proceedings that follow here.

C "Frederick A. Cook, M.D., the Physician: Pioneering Polar Medicine & Beyond," by Ralph M. Myerson, MD (died 2010)

C1 Dr.Myerson reprises the explorer's life with an emphasis on Cook's medical insights, especially in relation to his medical work on polar expeditions. In so doing, he gets a few facts wrong: Cook was the 5th of 6 children, not 4th of 5; the Cook brothers put runners on a boat, not a wagon to deliver coal during the Blizzard of '88; Cook moved to W. 55th St. after his wife died, not before; Cook's "medical prowess" was not "put to the test" by Peary's 1891 accident, because his broken leg did not even need to be set; Peary's *Northward Over the "Great Ice"* was published in 1898, not "1893"; Cook baked his Antarctic patients in front of a large stove, not a "bonfire"; you wouldn't eat "fresh walrus" in Antarctica because none live there; Cook did not receive "the gold medal of the Order of Leopold" but he did receive the white enameled cross of that order, and a silver medal from the Royal Belgian Geographical Society; it was a medical impossibility for Cook to have made a diagnosis of pernicious anemia in Peary in 1901, which is when Cook says he examined Peary, not "1904" (‡1 §E2); Cook took no gum drops with him on his polar journey, though he did say he took Nabisco cookies; Cook did not winter in a "cave," but a standard Inuit igloo; Cook died on August 5, 1940, not "August 2"; and there is a lasting memorial to him as an explorer in Buls Bay, Antarctica, etc; etc. In the notes, Cook's 1897 paper (*Medical Record of New York*, vol.51 [June 12]: 833-36) is unattributed. And the citations of *Hampton*'s and Peary's book are inaccurate. Every single one of the facts that Dr.Myerson presents, correctly or incorrectly in his paper, are to be found correctly stated in *Cook & Peary*. Therefore (in reference to §B6's bold-faced question), there is nothing in his paper that could change anything in that book, though the converse is certainly not true.

C2 In the oral presentation, Myerson made much of Cook's prescription of the exact cure for the illness that proved fatal to Peary, pernicious anemia, for which its discoverers received the Nobel Prize in 1934. This caused quite a buzz in the audience, the implication being Cook was cheated out of the prize, himself. But as thoroughly explained in *Cook and Peary* [pp.787-788; 1082, note 49] such a diagnosis is medically impossible even today, so many years in advance of the onset of the characteristic symptoms of pernicious anemia, and Cook's tale of his diagnosis only appeared in 1935, after Minot and Murphy had received their recognition as its discoverers by the Nobel Prize committee. Myerson does not mention the Nobel Prize in his printed paper.

C3 There was a question period following Dr.Myerson's paper. I posed a couple of questions and there was one by a grandnephew of Cook, another Dr.Frederick Cook, who was also a psychiatrist/physician (now deceased). While interesting, none were given conclusive answers. After this paper, further questions were deferred to the time remaining before lunch.

C4 Dr.Myerson's conclusion on Cook's controversial claims? "In 1906, [Dr.Cook] claimed to have made a successful ascent to the summit [of Mt.McKinley]. . . . He claimed to have reached the Pole on April 21, 1908." Not exactly a solid endorsement from the Vice-President of the Frederick A. Cook Society, but these are, at least, accurate statements.

D "Dagtikorssuqq and Inhghuit: Cook and the Polar Eskimos," by Rolf Gilberg

D1 Dr.Gilberg's paper deals with the relative relations of Cook and Peary to the Inuit, which concludes that Cook was the more "progressive" in his, setting an example that is still worthy of imitation today. He comes to no conclusions, either negatively or positively, about Cook's controversial claims, but does conclude, "Cook seems to be the better anthropologist of the two." Again (with reference to §B6's question), there are no relevant factual statements in the paper that were not covered in *Cook & Peary*.

D2 Dr.Gilberg shortened his paper by about 50% in his reading of it before the conference, though what he said appears, for the most part, in the published paper. Afterward, he showed a number of color slides taken during his stays with the Polar Inuit. None of the comments he made in relation to these appear in the proceedings, but none had any bearing upon evidence for or against Cook's claim.

E "Liars and Gentlemen: Cook, Rasmussen, Freuchen and the Polar Eskimos," by Kenn Harper

E1 I have already expressed my admiration for this paper [*DIO 9.3* ‡4 §C4], which came to many of the conclusions I had already come to in my then-existing manuscript of *Cook & Peary*, of which I brought a copy to this conference. It's surprising that in his printed paper Mr.Harper failed to correct the erroneous authorship of the maxim that forms his title, because I cited it in the question period and sent him the exact reference soon after the conference. [The quip, "Cook was a gentleman & a liar, Peary was neither", was coined by Senator Chauncey Depew, not the usually-cited Peter Freuchen; it was quoted in the *New York Times* March 31, 1910.] There are a few other factual errors, such as the statement that there is "no record" of Matt Henson being present at the 1909 interrogation of Cook's Inuit companions. Henson explicitly states in the July 17, 1910, *Boston American* that he conducted this interrogation for Peary: "I obtained for Peary the details of Cook's performance, which he afterward offered as his own investigation, that Cook was not once out of sight of land." Harper also states that "Theon Wright reports, on no quoted authority, that 'the Eskimo word for the North Pole is Tigi-su which means Big Nail' ". It may not be a reported authority, but Wright's "authority" is Dr.Cook, himself. On p.272 of *My Attainment of the Pole*, he uses the phrase "Tigishu-conitu" and translates it as "The Pole is near." Once again (see §B6 boldprint), there is absolutely nothing factual in this paper that does not appear in substantial detail in my book, and my conclusions are coincident to a very high degree with it on the questions Harper's paper seeks to answer speculatively.

E2 Harper forms no definite conclusions on whether Cook reached the pole, although he tends to imply that his claim's rejection was partially manipulated by the press, with Peter Freuchen as the ringleader. That is a long way from unequivocal support, or even saying Cook's claim had any true merit. He does say he believes that "no one will ever know the truth of the Eskimo story of Dr.Cook's attempt on the Pole," thus striking a blow to the great store put by FACS in the "First Eskimo Testimony" (www.dioi.org/vols/w93.pdf, ‡4 §G).

E3 Harper read his paper almost exactly as it appears in the proceedings. At the end of this paper there was a general question session for the three morning session presenters. It is unfortunate that the proceedings make no attempt to record these questions and answers, because they were in some ways as interesting as the actual papers and demonstrated that the audience contained several persons as well versed in Cookiana as any of the presenters. This is not the place to make up for this shortcoming, but here is a summary of the more important points raised. I pointed out Chauncey Depew as the originator of the epigram that forms the title of the paper; I had all the references at hand because Harper's title had tipped me off in advance. I also mentioned that in the fall of 1909, Roald Amundsen had stated in the newspapers in the wake of the controversy that the correct term used by the Inuit to describe the North Pole was "The Big Navel," not "Big Nail," as Harper says in his paper [this was not part of the video record]. Mr.Harper asked me to send him the references, and I did so in the first case. In the second, I did not use the citation in my book and was unable to find it among my notes, and so was unable to send him the exact date of the paper in which Amundsen's comment appeared.

E4 The next questioner brought up issues concerned with the Danes' rôle in the case. He wanted to know where Cook had met Rasmussen in Greenland [see *Cook and Peary* p.346] and why the Danes did not comply with Cook's request to fetch back his Inuit companions to confirm his claim [see *Cook and Peary*, p.371]. Harper did not know the answer to the first question and thought the Danes might have been intimidated by Peary's aggressive anti-Cook stance. At this point Dr.Gilberg arose to "defend his country." He explained how the Danes' claims in Greenland did not extend beyond North Star Bay, and that the early American and British expeditions into the areas north of there might result in a conflict. He said that while Rasmussen lived he was "in charge" of the Polar Eskimos, but beyond that the Danes thought it better to keep a "low profile" in this territorial "no-man's land."

E5 Ted Heckathorn correctly pointed out that Cook had left written instructions on how his property was to be divided among the Eskimos, and that it was Peary who aborted this plan upon Peary's return to Etah in August 1909. A question was raised about Cook's alleged attempt to "steal" Thomas Bridges's dictionary of the Yahgan language. Sheldon Cook-Dorough was asked to address this issue by Dr.Myerson. Sheldon deferred to me as probably knowing more about the subject than himself, but I had left to meet with the Ohio State University Press to discuss possible publication of *Cook & Peary*, and was no longer in the room. Sheldon, however, did an admirable job, in his typical lawyerly fashion, at reciting a richly detailed account of the chronology of this incident, which I could not have done without notes. I don't believe he got a detail wrong, but he did not know of some additional details I had discovered in my research. After the conference, FACS published my detailed analysis of this same question as a separate paper. [*Dr.Cook and the Yahgan Dictionary*. Special Supplement *Polar Priorities* v.14, 1994, 12 pp.]

E6 The next questioner wanted to know if Cook could navigate, and what methods did he use to keep his course. The panel members all looked stumped, and finally one of the moderators from BPRP suggested that the experts on that subject would speak in the afternoon, and the questioner should pose his question to them.

E7 The next questioner wanted to know what became of the two Eskimo children Cook had brought to America and exhibited at some of his lectures. Kenn Harper was only able to give a partial answer, but Dr.Gilberg correctly stated that the two had been brought from Labrador by permission of their parents, had appeared at lectures with Dr.Cook and had been returned in good health to their parents. [See *Cook and Peary* pp.104-106; 110-111.]

E8 Finally, I pointed out the inconsistency of the remarks attributed to Rasmussen quoted in Harper's paper with the several statements by Freuchen that Rasmussen was terrible at math and never learned navigational calculations [see *Cook and Peary* p.911]. Harper confirmed that Rasmussen was a poor navigator and left this analysis to others.

F "Oceanographic Currents in the Arctic Ocean: Did Cook Discover an Unknown Drift?" By Brian Shoemaker

F1 I have also previously referred to this paper and how it supported conclusions already independently drawn about where Cook actually went on his polar journey. [See www.dioi.org/vols/w73.pdf, ‡7 §§G-H & ‡9 §D.] I agree that the answer to the question posed in the title is YES, but disagree on the implications of that answer to the larger question of Cook's claimed polar attainment. Despite his title, however, in the question period following Dennis Rawlins's paper (see §G5 below) Shoemaker stated flatly that Cook didn't understand what he had observed in relation to the Beaufort Gyre. Capt. Shoemaker was the first of the presenters at the symposium to say something definite about Cook's North Pole claim in his printed paper: "The Environmental evidence does support Cooks [*sic*] claim to have traveled from Svartevoeg to the North Pole." [It should be noted that throughout his oral remarks and printed paper, Shoemaker uses Svartevoeg for Cape Stallworthy (as did Dr.Cook), although these are two distinctly different places, and all evidence indicates that Cook left Axel Heiberg Island from Cape Thomas Hubbard, the westernmost of the two capes that form the island's northern terminus, not Cape Stallworthy, which is the eastern cape. Sheldon Cook-Dorough also repeats this error in his paper.] He comes to this conclusion even while noting that the rate of Cook's drift data (derived from Cook's "field notes" published in *My Attainment of the Pole*) is much greater than shown in the modern studies on which Shoemaker based his paper, and while saying "it is amazing that Cook did not realize that there was no drift east or west from Svartevoeg north to the 84th parallel."

F2 This is not so amazing if one accepts the solid evidence that Cook had no navigational skill (see Dennis Rawlins's paper, §G below) and therefore could not determine his drift. These inconsistencies with modern data are less remarkable when it is realized that Cook said he intentionally set out to the west to find his "Magnetic Meridian," as explained in *Cook & Peary*, and that Shoemaker's entire analysis depends on, as he says in his abstract, "if the assumption is granted that he was in the proximity of the Pole on April 21, 1908." That's a lot of assumption to be granted, given the revelations presented in full in *Cook & Peary* as gleaned from Cook's original field notebook that contains his actual field notes [see *Cook & Peary*, pp.969-975]. Cook did discover an unknown drift, but it was not because he went to the pole. His notebook indicates, rather, that he discovered it on an approximately 100-mile journey to the northwest that ended when he confronted the shear zone that Shoemaker describes in his paper. The value of the printed paper is undermined by the editor's failure to reproduce the slides referred to in the text, by which Captain

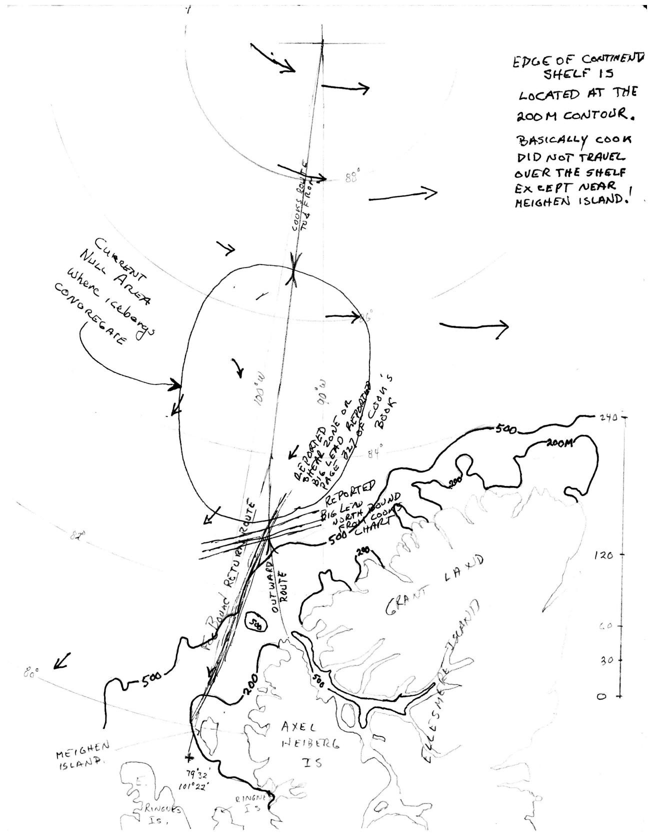

Figure 1: Captain Brian Shoemaker's Map Sent to the Author

Shoemaker illustrated his talk at the symposium, and by the fact that none of the references cited in the text are printed with the paper. However, the map he used in his talk proved erroneous (see §F4 below). The sketch map he sent me at my December 17, 1993 request is reproduced here as Fig.1, but it differs from the map Shoemaker used at the conference.

F3 Captain Shoemaker started out his oral remarks by saying he was "ambivalent" on the subject of who first reached the North Pole and that "I haven't formed an opinion on whether Cook went to the pole or not." However, at the end of his oral presentation he unequivocally stated "I believe he went to the North Pole," and made additional remarks calling for another expedition to follow Cook's route, implying that such an expedition would restore Cook's claim by confirming his observations. Shoemaker's talk only roughly followed the outline of his published paper, most of his remarks being made in explaining the slides he had prepared illustrating its various points. He also misidentified Joe Fletcher as the first person "to stand at the pole" after Cook and Peary.

F4 Shoemaker's answers to questions concluding his paper provided this additional information: he differentiated between the shear zone caused by the Beaufort Gyre and the "Big Lead" described by Peary (Fig.1). it was demonstrated that his map of Cook's *claimed* route was in error in showing Cook going east of Hassel Sound rather than down it

(Cook 1911 p.285). Shoemaker blamed this on copying the map from Theon Wright's book (NY, *The Big Nail*, John Day, 1970), but claimed he had checked the navigational positions shown on the map against Cook's "field notes." This is dubious, and it appears that he relied more upon Wright's plots than Cook's. When I asked him if Cook could not have gone just beyond the shear zone (as Cook's field notebook indicates) instead of all the way to the pole, and still have discovered this oceanographic feature, Shoemaker stated Cook must have gone at least as far as 85° north to have done so. He said that if he got that far, if it had been him, he would have gone on to the pole. This is not logically sound, because reaching the pole from 85° would involve a straight line journey of 300 more nautical miles, not to mention the return journey of over 500 more. A journey from 81°22′ (‡3 §C1) to 85° does not guarantee that reaching the North Pole was a cinch. Nansen, Cagni, and Peary all claimed to have been north of 86° when they achieved their respective Farthest Norths, yet none of them reached the North Pole. Shackleton claimed to have been within a hundred miles of the South Pole in 1909, but did not attain it. When asked how Cook made the fixes shown on Shoemaker's map, the presenter answered "by sextant," then merely cited the equipment Cook said he took with him as evidence. And when asked "how did he steer?" Shoemaker said "by eyeball." But when pressed just how: "By compass?" Shoemaker said "we can't get into that in a few minutes," and added something that sounded flippant, but all of his words weren't understandable on the audio of the tape. In answering this question Shoemaker said that he had relied on Wright's map for the coordinates and that they being "generally plotted correctly" he didn't change them. He said he was more interested in the return route, to which he applied his own drift data. This moved Cook's route 3° west of the route marked on the map Cook published. Wright had shown Cook's route as Cook had himself reported it, so the justification of moving the return track's plots and not the outward bound one seems unjustified, and designed to fit Shoemaker's hypotheses. Wright's authority was Cook himself for both plots.

G "Cook as Nondiscoverer: Demolishing the Mythical Attainments of 1906&1908," by Dennis Rawlins [Paper's full text is ‡3 below.]

G1 Dennis Rawlins wastes little time getting to "the fun parts" of Cook's career, and presents a blizzard of references and well documented notes in refutation of Cook's most controversial claims, including attention to Cook's oft-neglected *New York Herald* serialization of his North Pole tale (Cook 1909). In most cases Rawlins's remarks hit the mark, though, once again, this paper is not entirely free from error. The date of Cook and Barrill's departure from "his group" for their "climb" of McKinley was in late August, not September 9, 1906; that's when the two set off with John Dokkin up Ruth Glacier (‡3 fn 15; *C&P* p.809); the Fake Peak is 16.9 naut.mi from McKinley's summit, not "a little over 15" nmi; Henry Helgesen did not write the criticisms of Cook's claims that appeared in the *Congressional Record*, it was E.C.Rost, Cook's paid congressional lobbyist (Rawlins 1973 p.247; & ‡3 fn 47 below), who researched & wrote them, but this was not generally known until *Cook & Peary* published the true authorship; the photo referred to in end-note 27 (‡3 fn 27) does not show the Fake Peak "prominent on the left side of this photo"; and August Loose did not make his navigational calculations in New York City, but at the Hotel Grammatan in Bronxville, NY. There are other mistakes, but most of them are editorial, Gibbons having left numerous baffling internal references from Rawlins's original format that can't be traced.[1]

G2 Rawlins spends considerable space on Mt.McKinley in his written paper, making original contributions to the record from the suppressed minutes of the Explorers Club committee that sat in October 1909 to decide on the legitimacy of Cook's climb. I had

[1]FACS ignored DR's request to photo-reproduce *My Attainment* 1911→1912→1913 editions' evolution of faked data for 1908/4/8&14 pp.257&274. But all appear in ‡3 Fig.1 (§§C7-C8) here.

already included excerpts in my manuscript from a different set of the same minutes found in Peary's papers at the National Archives in 1991, but Rawlins's references to them were four years in advance of my publication date. He also poses a number of questions about Cook's movements in the Arctic in 1908, such as why he would go west across Ellesmere Land, instead of to the north tip of it, to make his attempt, a much shorter route to the Pole. Rawlins suggested [‡3 §C3] that it makes sense for a navigationally illiterate explorer to avoid traveling due north far out of sight of land (there's only sea-ice between Ellesmere & the N.Pole), instead heading northwest (as he did in 1908: ‡4 §N5) to try reaching Peary's reported Crocker Land, where he might move northward on *terra firma*. Such questions are all answered in *Cook & Peary*. In fact, to this point, not a single presenter would have made a mistake or posed an unanswered question in their papers that could not have been corrected or answered from the pages of the manuscript I had even then in my hotel room in Columbus. Rather than the symposium making a difference in my book, my book would have made a big difference in this symposium — just the opposite of Gibbons's §B6 argument.

G3 Despite these few flaws, for the most part Rawlins's paper is devastating in laying bare not only the logical and scientific weaknesses of Cook's claims, but the fallacies and follies of his believers that still keep them alive. His paper (‡3 §C4) contains a perfect answer to Captain Shoemaker's positive contentions based on conflicting evidence in the only paper so far to support Cook's claim overtly. "Some Cook defenders regard his report of the direction of ice-drift north of Axel Heiberg Land as evidence for his attainment of the Pole. But such information is not astonishingly specific. . . . if . . . Cook is vindicated because he reported a rough direction for drift. . . . then why isn't he disconfirmed when he reports in detail — *and photographs* [www.dioi.org/vols/w93.pdf, *DIO 9.2* ‡4 Fig.5] — a wholly mythical land? [Bradley Land]. . . . a neutral investigator will not [a] treat vague alleged positive evidences as vindicating Cook, while [b] treating all negative evidences as mere Problems or Paradoxes that prove nothing but the need for increased effort at dedicated Re-explaining."

G4 I disagree with none of Rawlins's points, but I have a more sympathetic view toward Cook's personality; though, in this particular instance, Rawlins's oral presentation was more restrained than his Annapolis paper read in 1991 at the U.S.Naval Institute, Annapolis. For the most part it followed the points of the printed paper but was more conversational in style. His conclusion on Cook's claim: "Frederick Cook is generally viewed as either an unjustly persecuted hero or a gentlemanly hoaxer. I regard him as a justly persecuted hoaxer. . . . Cook and his Occam-defying believers have traditionally attributed [his woes] to a complex web of bribery&conspiracy by the Peary Arctic Club. . . . But there is a much simpler theory, which easily explains an otherwise ultra-complex saga. This elementary theory is that: Cook was a liar — even though Peary said he was."

G5 A question period followed this paper. I asked if it was plausible that Cook might have attempted following his "Magnetic Meridian" in lieu of navigational ability. Rawlins logically rejected this because Cook reported and tried to present incorrect standard navigational data later [‡3 Fig.2], and Rawlins was of the opinion that it would be difficult to get back to land using such a method. The next question asked if the navigtional positions Cook presented corresponded to his narrative. During this discussion Rawlins pointed out (as in his OSU-printed paper) that in his long *N.Y. Herald* narrative, no times were given for Cook's alleged longitudinal observations, although their times would have been necessary when his secret temporary navigational computer (‡3 §C15), August Loose, reduced the data to verify Cook's alleged positions, as Cook says [‡3 §C17] he asked Loose to do **from the *New York Herald* narrative**, not his private records.

G6 Another questioner opined that the Magnetic Meridian method "wouldn't work" and Rawlins explained why (similar to the explanation given me two months later by Keith Pickering [see his letter at *Cook & Peary*, p.1095, note 42]), namely, the instability created by the divergence of the lines of constant magnetic declination [compass variation], and when Rawlins added that it would work [coming back to the magnetic pole] (contradicting his earlier careless mis-statement: §G5), the questioner agreed. Pickering's letter also emphasized the extra complication introduced by the lines' nontrivial curvature [DR: which would swing a "Magnetic Meridian" follower way to the right of the Pole]. Ted Heckathorn asked if Rawlins had seen the original observation sheets among Cook's papers at the Library of Congress, and said that there were also tables of refraction, lunar tables, and potential lines of magnetic declination besides, in his papers. Rawlins said that there may be tables, but there were [no sph trig calculations], which he assumed would've been presented by FACS by now if there were any [‡3 fn 7]. He added that he had seen copies of the alleged "original" [non-trig] observation sheets. There was also a question for Captain Shoemaker. He was asked when Cook observed the Beaufort Gyre did he realize what he was observing. Shoemaker replied that Cook had not, nor did he believe Cook ever knew it was there (thus contradicting the answer he seemed to have given to the question posed in the subtitle of his paper), but he (Shoemaker) had "interpolated this from what happened to him."

H "Ice Islands from the Ellesmere Breakoff: Was Cook's 'Bradley Land' a Sighting?" by Joseph O. Fletcher (died 2008)

H1 Report #18 hits the nadir of editorial duplicity in the "paper" that stands exactly at its center. The abstract concludes by saying that "Cook's account should be examined in light of our modern understanding of oceanic and atmospheric circulations." What follows makes no attempt to do this, though Fletcher made a somewhat similar remark in the summary session of the conference (see §N3 below). What follows has absolutely nothing to do with the abstract. Incredibly, what follows is a literal reprint of not quite the first three pages of Fletcher's article, "Three months on an Arctic Ice Island," which appeared in the April 1953 *National Geographic Magazine* [pp.489-91]. There it just ends, with no point or conclusion. It not only has nothing to do with the abstract (other than they both happen to mention ice islands) but contains no mention or allusion to Dr.Cook whatsoever.

H2 In the Acknowledgments, Russell Gibbons expresses his appreciation to the National Geographic Society (surely this was a unique event) for permission to use material from this article. But even armed with permission, the copy takes a beating at his editorial hands. He was so phenomenally careless that he left in the already totally irrelevant text three internal references to other pages in the original article (not printed) and to a chart (printed in the photo section without anything to relate it back to the reference), which says it is on "page 493" in a report containing 136 pages, p.493 being where it appeared in the April 1953 *National Geographic*.

H3 How can this bizarre arrangement be explained? Fletcher did not present a formal paper, he simply narrated some silent film footage of his experiences setting up a base on the T-3 ice island in 1952. And in doing so, he never took any stand on Cook's claims one way or the other. Perhaps he was unable to submit anything formal because of the extemporaneous nature of his presentation, or due to ill health. But whatever the reason, nothing can justify presenting three pages of a popular magazine article and calling it an academic paper bearing on the subject at hand, much less an important contribution to a conference on Dr.Cook held 40 years after it was published. Trying to represent it as such, even by implication, should have been totally unacceptable academically. How BPRC's editor charged with bringing "uniformity to many of the papers" allowed this to pass is inexplicable on the basis of any professional criterion. Nothing could better exemplify the kind of pseudo-serious organization FACS was than this, and how little BPRC cared about what anyone thinks of its association with FACS. Needless to say, nothing in Joe Fletcher's non-existent "paper" could have any influence on the conclusions of *Cook & Peary*.

H4 Fletcher started his presentation by apparently denying his introduction as "the first man to stand at the pole after Cook and Peary," although this can't be certain due to the fact that the introduction was not preserved. In any case he says, "not exactly," followed by laughter from the audience, and goes on, "I think that was made up." [The first men to stand near the pole after 1909 were the Russian scientists who landed there by airplane in 1937: see Christopher Pala, "Unlikely Heroes: The story of the first men who stood at the North Pole," *Polar Record*, v.35, Issue 195 (October 1999) pp.337-342.] He said he was not a student or advocate, but rather an admirer of Cook and Peary "who accomplished epic journeys." He had no prepared remarks, but instead wanted to "take you to the locale" by showing a film of the early days of establishing a scientific base on the ice island T-3. His remarks during the showing of the film were very interesting in many ways, but during his narration he only mentioned Cook once in connection with his claim to have traveled over ice similar to that of an ice island for "days." Fletcher's remarks on ice islands in relation to Cook made after the film implied that he did not have a clear idea of exactly where Cook reported his "glacial island" and the extent of his route on which he had traveled over such ice. For instance, he indicated the area in which ice islands congregate, but this was not the area Cook postulated his sighting of the Glacial Island. In *Cook & Peary* it was demonstrated why Cook chose this particular locale [see *Cook & Peary*, pp.885-887]. Fletcher said he thought eventually there might be some evidence uncovered bearing on Cook through the study of the breakup and circulation of ice islands.

H5 In the question period that followed, Fletcher was asked if an ice island could be mistaken for Bradley Land. He said that it would be clearly discernible from ordinary pack ice, but as far as being mistaken for land, that might have been due to mirage. He was asked the dimensions of T-3: he said that it was 165 feet thick on average, but only rose about ten feet above the surface of the pack; it was seven miles long and about four wide. It broke up only in the 1980s. Dennis Rawlins pointed out that "Bradley Land" had been photographed and was described as more than ten times as high as an ice island is thick [actually Cook said it had an elevation 180 times ten feet]. Finally, Bill Molett asked if Fletcher, based on his experience with the pack surrounding T-3, thought it was possible that Peary could have made 26 miles a day over the ordinary pack. Fletcher demurred, saying he had never driven a dog sledge and left that to other experts.

I "Admiral Peary and Doctor Cook: New Insights into an Old Controversy," by Ted Heckathorn

I1 I have already [www.dioi.org/vols/w93.pdf, ‡4 §§C5&K] dealt with aspects of this paper at length. Apparently my letter to Heckathorn[2] about the 17 errors I detected in his oral paper was not in vain after all. In his final paper he corrected or dispensed with eight of them. Some he clings to, however, others he modifies. Most interesting is that he now claims that the disputed daughter I said John Verhoeff didn't have was from a union with an Inuit. He merely cites "Peary papers" as his corroboration. He may have an actual reference, but as in scientific experiments, research that cannot be independently corroborated is worthless. It might be true that Verhoeff left an Inuit daughter, but to say merely that somewhere in the 235 cubic feet of manuscript materials that make up the Peary papers reference to Verhoeff's "daughter" exists, meets no academic standard of evidence, even for a self-proclaimed "polar historian." [Heckathorn is the only presenter allowed such a blanket citation of this kind, and the only one without an abstract; so much for "uniformity" and "standard reference citations."] And it certainly is at odds with what Heckathorn cited as evidence in his 1993 letter to me. In that letter he claimed "Peary refers to Verhoeff's daughter in *Northward, Over the "Great Ice,"* and makes a more detailed reference in his diary." [Letter TH to RMB, November 4, 1993]. Peary might have written about such a thing in his diary, but not for publication. He would never have brought up the subject of illegitimate Eskimo offspring, especially since he himself had sired one by that time, and his mentions of Verhoeff in the book are, anyway, all laudatory, as one is always supposed to speak well of the dead. Heckathorn should produce the passages he refers to in context. Now that he has committed this statement to print, he has an obligation to document such an important unknown fact or admit his error.

[2] Idaho telephone 208-552-4718.

I2 As usual, even though I helped him fix some major gaffs, this "polar historian" can't keep all his facts straight. He says Peary could not secure leave from his Naval duties between 1895-1898, but his service record shows that from May 2 to October 30, 1896, Peary was on leave, and again starting on May 25, 1897, for five years [*Appendix to the Congressional record*, January 25, 1916, p.316]. During these two leaves he went to retrieve the largest of the Cape York meteorites; Heckathorn says that J.Gordon Hayes was unaware of Peary's deception concerning Peary Channel, but the Reverend Hayes discusses it in detail on pp.40-43 of the book Heckathorn cites; he deplores Sverdrup's deviation from "the normal practice of international cooperation in polar exploration," when this was actually one of the most nationalistic and personal battlegrounds of the Imperialistic era that characterized the late 19^{th} Century, with precious little co-operation of any kind; Mrs.Peary was stranded in the Arctic on the 1900 relief expedition, not that of 1899; Herbert Bridgman was not the "executive officer" of the Peary Arctic Club, he was its secretary; Peary's supply ship was the *Erik*, not the "Eric"; Dr.Cook fled the country in November of 1909, not December; after a 1915 citation, Heckathorn says "meanwhile in Greenland, Danish explorers inadvertently uncovered the Peary 'Channel hoax'," when it was actually uncovered and widely published as early as 1908, thus discounting much of the paper's misguided thesis. Apparently, Heckathorn remains blissfully innocent of the Eskimos' evaluation of Dr.Cook. The embarrassing moment where the Eskimos called Cook "shag la-hutte" (a huge liar) is now enshrined in print on p.77 of Report #18. (*DIO 9.2* ‡4 §L17.)

I3 In conclusion, Heckathorn says: "An objective examination should have been done decades ago when additional data and key witnesses were available. Some important items are now missing and all of the key witnesses are dead. Our major advantage today is that both the Cook and Peary Papers are open and available to provide revealing documents and personal insights about the rivals. Perhaps this symposium will stimulate further interest in finding the remaining missing pieces of evidence to resolve Dr.Cook's North Pole claim." That is what Heckathorn said in 1993, but at the time he spoke those words, I had already made just such a study, discovered the missing pieces and by using them had come to just such a resolution. But when my book appeared in 1997, he would not admit that the missing pieces I had found were of any importance to the resolution, and conveniently shifted his emphasis to the importance of "field study" over the documentary evidence he had — just four years earlier — claimed would hold the resolution to Dr.Cook's claims. That's because my book didn't resolve Cook's claim in Cook's favor. Heckathorn's paper purported to offer some of this new evidence, but none of it was new to me, even then (unless Verhoeff actually did have a daughter!). It was already all written down correctly and fully documented in my finished manuscript.

I4 Curiously, in his paper, Heckathorn never comes right out and takes a stand for Cook, although he flatly states Peary's claim is "discredited." But, of course, he strongly implies Cook was done in by his beloved "McKinley-gate" conspiracy [www.dioi.org/vols/w93.pdf, ‡4 §K11], which has already been discussed ad-nauseum.

I5 The question session did not add much. Heckathorn confirmed there was no mention of Crocker Land in the diary of 1906 expedition doctor Louis Wolf, as I believed after reading it twice; Dr.Cook's grandnephew opined that Peary's behavior might have at its root mal-absorption that affected his nervous system. Bill Molett objected to Heckathorn's characterization of the Peary Channel as a hoax; Dennis Rawlins added some detailed evidence on Peary's faking of non-existent far-north "Crocker Land." [Original charge of fraud based on various documents cited at Rawlins 1973 pp.71-77. Indicting diary-page later recovered by National Archives: photo at www.dioi.org/cot.htm#barr.]

J "A Russian View of the Cook-Peary Dispute, 1909-1993," by Vladislav S. Koryakin

J1 As far as my book is concerned, this paper doesn't count any more than Joe Fletcher's in relation to the question (§B6) at hand. That is because it was not part of the actual symposium. Dr.Koryakin was unable to get out of Russia and made no appearance in Ohio in October 1993. Therefore, no one heard his paper, it was not caught on tape, was not available until OSU's 1998 publication, and it can have no relevance to Gibbons's claims about the merits of the symposium as I experienced it in relation to my conclusions. But it wouldn't matter if it had been presented. The paper details the rise in estimation of Cook's claim relative to Peary's in the Soviet Union/Russia. It assembles third-hand opinions to bolster its case, and it trots out much of the same evidentiary line taken by such Cook partisans as Sheldon Cook-Dorough and Silvio Zavatti, who have all been discussed. [E.g., www.dioi.org/vols/w93.pdf, ‡4 §R.] It is, in short, a very good example of accepting opinion as if it were fact, the notion that Truth is somehow a popularity contest, and an example of how advocates select evidence that fits the desired end and reject that which does not, solely on that basis. One example of this last should suffice.

J2 Koryakin says: "In general, these facts set forth in Dr.Cook's book form a system of proofs of his correctness because the conditions described by Dr.Cook are completely explained by the natural process In [*sic*] that part of the Arctic. . . . In his notes, one cannot find *many* [emphasis added] facts contradicting the modern information concerning the part of the Arctic Ocean through which he traveled. There are inaccuracies and certain subjectivism in Dr.Cook's descriptions but there is no question of 'deliberate lie' or 'falsification.' " "In general" a few selected facts (some of which he admits are actually inaccurate) can never "completely explain" anything. This brings to mind Dennis Rawlins's comments about Bradley Land already quoted (§G3) and makes us look forward to the discovery of Cook's fake photo of his "Glacial Island" recounted in the next paper by Wally Herbert. Each of these is a gross example of "deliberate lie" or "falsification" on Cook's part if there ever was any. As Rawlins says (‡3 §C5), "Using a double-standard for positive and negative evidence is not science but advocacy." When a scientist engages in such double standards, as Koryakin does throughout his paper, one wonders about his ability to recognize scientific truth at all.

K "Frederick Albert Cook: the Discoverer as Defendant in the Court of Historical Inquiry," by Sheldon Cook-Dorough

K1 Anything I could say about this paper has already been said. [E.g., *DIO 9.3*, www.dioi.org/vols/w93.pdf, ‡6 §C4.] Because Sheldon has only a few drums, he beats them endlessly. As with all of Sheldon's writing, we must wade through the non-sequitur-laden verbiage, the misinformation, the partially muddled "facts," the outright mistakes (I will spare the reader this time) and the carefully selected positive evidence, including once again, the sacred First Eskimo Testimony. If ever anyone needed an editor it is Sheldon; but Gibbons was no editor. He left in all of the endless repetitions, even when they occur in consecutive sentences. In the end, however, Cook-Dorough says that the narrative Cook gave of his polar journey is only "almost certainly true." His conclusion: "In the court of historical inquiry, on the basis of this evidence, he should be recognized for his magnificent achievement." Perhaps, on the basis of this evidence. But on the basis of all evidence, as presented in *Cook & Peary*, Cook's claims to McKinley and the Pole both are condemned at the bar of justice, although he is recognized there for the achievements he actually accomplished (as Herbert pleads for below: §L2) and studied for what he can tell us about ourselves and the predicament of human life (as Malaurie hopes for, below: §M).

K2 It must be said that Sheldon's paper delivered orally was very close to the one printed in the proceedings, and its delivery showed the great lawyerly memory he had at the time, because though he read some parts of it, for the most part he delivered it nearly word for word without looking at his notes. No question period for this talk is included in the video record.

L "Following the Tracks of Both Cook and Peary: Did they Reach the Pole?" By Sir Wally Herbert (died 2007)

L1 After his appearance at a conference concerning Peary's claims at the Naval Academy in 1991 [U.S. Naval Institute seminar, "All Angles: Peary and the North Pole" U.S. Naval Academy, Annapolis, MD, April 19, 1991] Wally Herbert vowed he would never participate in any discussion of the Polar Controversy again. He had been both personally and financially hurt by the storm of criticism of him led by the National Geographic Society in the wake of his negative findings against Peary in his book *The Noose of Laurels* (NY, Atheneum, 1989), which he admitted at one point in his talk was a dreadful experience: "I had gone through this awful sort of book about Peary, that I didn't want to write!" he exclaimed. The abuse was continued by pro-Peary speakers at that conference. Although he appeared at the present conference, he was true to his word: he did not in any way discuss any aspect of the Polar Controversy despite what his printed paper says. Instead, after some personal comments, he showed slides which outlined his own career, only touching on Peary's at two points: in that by coincidence he had arrived at the Noth Pole in 1969 on the very day Peary had claimed to have been there 60 years previous, and his inability to understand why Peary never took along a boat on his polar attempts in case he got cut off by open water. Going into all the detail of the Polar Controversy was unnecessary, he said, emphatically: "You simply take what you know they did." But even today it is impossible to know that exactly. Where did Peary go instead of the pole? Probably no one will ever know that. Where did Cook go? The answer to that is clearer, but by no means certain.

L2 As for Cook, his name came up only when Herbert narrated slides taken during his shakedown trip in 1968 in which Herbert attempted to follow Cook's route from Greenland to Cape Thomas Hubbard. In so doing, he declared Cook had undoubtedly gotten that far, but took no stand on the extent of his polar trek. At the end Herbert made some desultory remarks concerning how people should not focus on the details of the rival claims, but see the two men in the larger context of their rôle in a long series of rivals who pushed each other to accomplish what they were incapable of accomplishing without external pressure. He concluded that it "doesn't matter a damn" whether they reached the pole or not, but it was "what they were trying to do that was important." I would answer: They may have been trying to reach the North Pole, but what they actually did, the both of them, was to try to deceive the world for personal gain. Does that make them "great men" as Herbert adamantly declared in his summary remarks? (See §N below.) Hardly. This brought to my mind Cook's statement "It's not what you have actually done, but what people wish to say good or bad about you, that makes history." The fact is, it does matter, because what both Cook and Peary tried to do was deceive the world and science by faking their respective claims. It is not what they were trying to do, but what they actually did that matters, and not what others choose to say good or bad of them, but Truth that makes History. Therefore, the paper as printed bears no resemblance at all to what Herbert presented at the conference. His printed paper, however, shows that he rejects Cook's claim based on the evidence he uncovered in research for his book on Peary and upon the folk memory of the Polar Inuit.

L3 In his printed paper, the eminent British explorer outlines his reservations about his rôle in the latter day manifestations of the Polar Controversy, which he characterizes as a "waste of time." He brings forward some solid evidence, but also some useless hearsay. He is mistaken in saying that the photograph he importantly discovered at the Library of

Congress showing Cook faked his "Glacial Island" is the "original plate". [It's a lantern slide made from a negative of the original photo. See www.dioi.org/vols/w93.pdf, ‡4 Figs.2-4.] He is also incorrect in his statement that "the original plate [of Bradley Land] is missing from the Cook Collection at the Library of Congress, as are also the plates of the two other crucial pictures: those of his 'North Pole' camp, and his 'summit picture' of Mount McKinley." Based on my experience researching the Polar Controversy, it is a dangerous thing to say any piece of evidence "is missing." I have held, not the original plate [there is none; on his polar journey, Cook used a folding "Postcard" camera using roll film], but an original print of the picture of Bradley Land in my hands [see note 81, p.1104 *C&P*]. And, of course, as related in detail in *DIO* [www.dioi.org/vols/w73.pdf, ‡7 §B], I recovered an original print of the "summit" picture. [See http://www.nytimes.com/1998/11/26/nyregion/author-says-photo-confirms-mt-mckinley-hoax-in-1908.html, *New York Times* 1998/11/26 p.1 (John Tierney), kindly citing & printing the centerfold photo of *DIO 7.2* ‡7 Fig.18.] Both photos show that they are as fake as the one Herbert turned up. [The print of Cook's "polar camp" photo is also at the Library of Congress, but this does not include any clues not visible in the published version. See 1st publication at www.dioi.org/vols/w93.pdf, ‡4 Fig.1.]

L4 Herbert states in his abstract that "both [Cook and Peary] must be judged on their reports and claims and toward this end we can and will make judgments." And what is Herbert's judgment of Cook's polar claim? "He fell to the same temperature [*sic* for temptation] he had on McKinley," that is, he perpetrated a hoax.

L5 Herbert goes on to say: "Writers on this subject (particularly those whose obsession is the controversy over who reached the North Pole first), tend to focus so intensely on the detail that they are unable to see how this period of history fits into the grander scheme of things. And yet from this, shall we say 'higher' perspective, it is 80 [*sic* for so] very obvious that Peary and Cook were essentially as much a part of each other as were, say, Shackleton and Scott." This is similar to what Herbert would say at the close of his slide presentation, and here Herbert has hit on the theme of my book — it is not just Cook, or just Peary, it is *Cook & Peary* — and also why partisans cannot accept my book's resolution to the Polar Controversy. Those only obsessed with who reached the Pole first have missed the larger implications of the Polar Controversy, which is actually the reason I wrote *Cook & Peary*. Yes, it is stuffed with detail, but all of it is aimed at that "higher perspective" of the controversy not only in the context of its own time, but ours, and even the future's. Herbert agrees with me that both explorers failed, but as Dr.Cook's line with which I closed my book says, "the world has important use for dreamers, even if they fail."

L6 We finally have in Herbert's paper something absent from *Cook & Peary*. He chooses to rely heavily on Inuit folk memory as evidence. I chose not to. It's absence from my book was a matter of choice, not one of ignorance, as I had read Herbert's *Noose of Laurels*. It appears from what Kenn Harper says of Inuit folk memory in his paper, it was a wise decision on my part.

L7 Even a gold medalist of the RGS is not exempt from the editorial curse of Russell Gibbons. Herbert's paper suffers from hung footnotes throughout, the loss of all of his end-notes and some of the most amusing typos in the whole typo-ridden Report #18. And oddly, in the author note, his own book is miscited as being published in 1991, rather than 1989.

L8 Herbert characterized the argument over the rival claims "a waste of time except in proving . . . that those who do not want to hear will never ever listen." I do not know if Herbert "wasted" any time on reading *Cook & Peary*. If he did, he would have seen that he was wrong in his belief that "the last word in this controversy will not be given in our lifetime." But I suspect not. Like other old explorers before him, including Cook and Peary, and Amundsen, he had become bitter when he realized that he had wasted so much time on unimportant and even chimeral pursuits, and probably did not want to hear it.

M "Reflections about Cook and Peary: The Inuit at the heart of the Problem," by Jean Malaurie

M1 It would be hard to sum up this rambling paper; but this is unnecessary, as already noted [www.dioi.org/vols/w93.pdf, ‡4 §F]. Of all the presenters, M.Malaurie and I seem to have the most similar thoughts on Dr.Cook and the essence of the Polar Controversy. This is what Malaurie is most interested in, not with obsessing on who did or did not reach the Pole first. On that question, he immediately bows to Herbert's conclusion: neither Cook nor Peary did.

M2 Malaurie's paper is not fact-oriented, but it has its share of factual mistakes, nonetheless. He repeats Myerson's pernicious anemia fallacy, though he uses language that qualifies if he actually believes it to be true; he has "August" Marvin for Ross Marvin; we read of him searching for the "secret place" where Cook hid his polar records in Northern Greenland, when it was Peary's henchmen who buried them in an exposed cache on the shore near Etah; he dates Cook's letter to Franke from the Polar Sea as March 20 instead of March 17; he says Peary "fired" Captain Bartlett, and on and on. But the thrust of his paper is psychoanalytical, not factual, seeking that "controversy within the controversy" he enunciated so well at the speech he gave after the symposium's dinner. [See www.dioi.org/vols/w93.pdf, ‡4 §F2.] In fact, the printed paper in the proceedings is closer to, but certainly not identical to, Malaurie's speech and is not the paper he read at the conference. [Unfortunately, according to my inquires to FACS, Malaurie's fascinating after dinner speech was not filmed, and to my knowledge has never been published].

M3 As regards the printed paper, in some things we are in basic agreement: Cook was a complex, powerful person in total command of himself and others, Cook envied Peary and was "obsessed by a theatrical will to astound." In all of this, Malaurie shows good instincts about who Cook really was, which is what actually lies at the heart of the Polar Controversy. That is something almost everyone else at this conference was deceived about. He also asks for many answers to unsolved questions throughout his paper, and as usual, each and every one already had an answer in the pages of *Cook & Peary*.

M4 Malaurie says, "What creates truth in the history of exploration is the coherence of the expedition's journals, sustained by the perfect honesty of the explorer, especially if he is alone." The converse could also be said. And the discoherence of Cook's expedition journals, as detailed in *Cook & Peary*, is the badge of fraud. Malaurie demands "perfect honesty of the explorer." He contends that "the slightest falsification absolutely destroys the credibility of the explorer in question." Thus Cook's gross falsification of his key photographs leaves him with "a devastatingly and abhorrent impression." Even so, Malaurie admits that "I have never hidden my sympathy for Dr.Cook's exceptional personality." In spite of that and everything else, nor do I.

M5 In sum, Malaurie's paper is not one of advocacy for Cook, but for understanding of him, and just as his Ohio speech seemed a call for the publication of the book I had already written, this was the purpose I, too, sought in writing it.

M6 As already mentioned, however, this is not the paper read before the conference. In his actual presentation, which can only be summarized because the speaker's thick Gallic accent made some of his thoughts unintelligible, Malaurie went through a brief history of the Inuit, from the time before contact with the outside world, to their changing perception of whites, starting with their first contact with James Ross in 1818. He mentioned these perceptions forming in regard to their experiences with the rough whalers, the gentle Kane, the deceptive Hayes, the observation of the conflicts between C.F.Hall and his doctor, Emil Bessels, and A.Greely with his, Octave Pavy, characterizing each with specific incidents. He then moved on to the Inuits' changing attitudes toward Cook and Peary, and how they moved away from favoring Cook to a unanimous approval of Peary by the 1960s. He summed this up by quoting the statement of Iggianguaq, the son of Ootah, who had been with Peary, made before a scientific conference in Paris, which he quotes in full on p.130

of the proceedings. Malaurie also spoke of the Danes' change of heart in regard to Cook.

M7 Malaurie said he once asked the Inuit what they thought of Peary's monument at Cape York, which he said they also approved. But Malaurie was of the opinion that this answer, and these changing perceptions of the two men were the result of the outside opinions they had heard over the years in regard to each, and that they had adjusted their own to favor whom they perceived was favored by outsiders — to please their hearers, as was alluded to in Kenn Harper's paper. Malaurie elaborates on their attitude in his printed paper on pp.130-131 confirming his oral remarks.

M8 Dr.Malaurie also touched upon a few points of his previous evening's remarks, when he had said Cook was difficult to understand, and "a man unknown," and that he thought Cook had made a fatal mistake by trying for the pole while Peary was still in the race. Malaurie called this a mistake of ethics, and said he could understand why Peary was furious that a one-time friend and colleague had inserted himself into his field of operations, because if one of his colleagues had done the same in his field of work, he would be just as furious. To reinforce this, Malaurie read the passage from Corneille's *Le Cid*, which appears on p.133 in the proceedings. This section of his talk is the most similar to his printed paper.

M9 Malaurie said he believed Peary was so infirm by 1908 that he had no real chance of reaching the pole, and that Cook should have waited until the field was clear before making his try. However, this is exactly what Cook actually did. Peary had loudly announced before leaving in 1905 that he was making his last try for the pole, and so Cook began to make his plans after word came that Peary had failed in 1906. Cook then secured the backing he needed, and by the time Peary announced he would go north again, he was committed to his venture with John R. Bradley. Cook well knew that such an opportunity might never come again, so he went ahead, even knowing he might have to deal with Peary in Greenland.

M10 Malaurie expressed the belief that we did not really know the whole story of what happened between the two men that caused Cook to not wait. However, Malaurie called Cook an "honest man" and emphasized what he perceived as his humanitarianism in his dealings with the Inuit and his later behavior in prison. He said it was up to "the writer" to dig out the details that would clear up the questions that needed to be answered, and though he did not identify me as that writer, at this point he paid me the complement of saying that he had sat with me at dinner, and that I "knew everything about Dr.Cook!" He also said that the Inuit would like to know a little more about these great men, and that they might change their attitudes if they did.

M11 Finally, Malaurie once again called for the publication of the papers of Cook and others relevant to the mysteries he felt needed to be solved, and gave as his motto in Latin, "Even if everybody is moving this way . . . me . . . NO!" Thus Malaurie struck the exact opposite note of Wally Herbert, who rebuked those who seemed "obsessed with every detail" and called for an end to the digging out of minutiae of the Polar Controversy. Perhaps it was Herbert's own failure to do so on his own attempt with "this awful sort of book about Peary, that I didn't want to write!" that made him think that way.

M12 Again the paper is beset by typos, misplaced footnotes and other editorial errors.

N Summary Concluding Remarks and Panel

N1 After the final speaker, there was a summary given by each of the speakers who were still present, and there was to be a panel for give and take through questions posed by the speakers and from the audience. None of this is reflected in the printed proceedings, and only the highlights of this session are given here.

N2 Perhaps in response to Malaurie, Herbert moderated his stance from his spoken remarks, saying instead that "of course, we should try to look for evidence, but that it should be done with respect" for the disagreeing side, perhaps reflecting the disrespect he thought he had been shown by the National Geographic Society and at Annapolis. "We

should obviously try to get it right" he added. He then said that Cook and Peary were greater men than anyone in the room, presumably because of "what they actually did." I thought this was somewhat odd, because the point of the conference was the result of a near-century old dispute over what one of them actually did, and because anyone who read Peary's personal correspondence, as Herbert must have, or at least should have, would know what a flawed character Peary had.

N3 In his closing remarks, Joe Fletcher seemed to join with Malaurie as well. He called for a study of the "tactics of the Polar Controversy," that had kept the dispute alive nearly 100 years, saying that in itself would make a "fascinating study." This had already been done in the manuscript of my book.

N4 Several of the other speakers remarked on the friendly atmosphere that pervaded the conference on a topic that had always generated such bitter disputes. Dennis Rawlins made some effusive and, as it turned out, overly optimistic remarks on the attitude of FACS, Russ Gibbons and the BPRC saying that, during the conference, they "exemplified the academic ideal of free speech" better than those in academia, giving it high marks for balance in the presentations. He did add, however, that "I might be fantasizing" and that he probably would have negative things written about him in the next five years in *Polar Priorities*. In that respect Russ Gibbons exceeded his expectations, and BPRC later made every effort to negate Rawlins's findings concerning the authenticity of Admiral Richard Byrd's alleged flight to the North Pole, which Rawlins brought convincing documentary evidence against [http://www.nytimes.com/1996/05/09/did-byrd-reach-pole-his-diary-hints-no.html, *New York Times* 1996/5/9 p.1; *Polar Record 36*:25-50 (2000); *DIO 10* (2000)]. Rawlins flatly disagreed with Herbert that Cook and Peary were both great men, worthy of respect. Rawlins expressed his respect for Peary, calling him "a real blood and guts explorer" [who possessed scientific skills] but said he had no respect for Cook at all. The summaries of Cook-Dorough and Ted Heckathorn did not add anything materially different from their papers. If the other presenters gave summaries, they are not part of the video record.

N5 Each panelist was allowed a question for the others. Rawlins initiated a discussion of Cook's gross errors in his published "observations" in *MAP* [§G1 above & ‡3 Fig.1 below]. Heckathorn tried to defend and deflect these, citing evidence in the Cook papers at the Library of Congress that he did not have with him, and so couldn't document. Heckathorn asked Herbert to compare his timetable to Cook's in 1908 (and later MacMillan's) over Sverdrup pass. Herbert seemed suspicious of the question, saying he thought he knew "what you are trying to get me to say." Heckathorn said he was just looking for data. Herbert then attributed his slower progress over this section of Cook's route to less snow when he crossed, when he had actually taken less time than Cook, because Cook's published timetable was far ahead of his actual progress, as indicated in his field notebook. This was unknown, however, before the transcription of Cook's field diary became available late in 2013. (See "Finding the Smoking Gun," ‡4 in this *DIO*, or the full story in the author's *The Lost Polar Notebook of Dr.Frederick A. Cook*, 2013.) Then Herbert took the opportunity to act upon his suspicion of what the question was aimed at, saying that it wasn't possible to travel as rapidly on sea ice as along firm land ice, and that comparing Peary's speed to such things as the Iditarod was a "waste of time . . . for anyone who has any brains at all." All other panelists passed on their allotted question, but Sheldon. He asked Rawlins about the analysis of Clarke Brown,[3] a civil engineer who examined Cook's published observations without criticism. They got into a back and forth, but what they were saying could not be made out on the tape because Malaurie and Heckathorn were discussing something with open microphones, which drowned out the exchange. There was only one inconsequential question from the floor before the buses arrived to make airport connections for some of the attendees, and the meeting broke up.

[3] Brown was not an astronomer, so Cook's refraction errors (‡3 §C7) made no impression. See www.dioi.org/vols/w93.pdf, *DIO 9.3* ‡6 fn 18.

N6 After the conference the annual meeting of FACS was held. I was invited with the idea that I might deliver an extemporaneous paper on Cook's rôle in the "theft" of the Yahgan dictionary, but there was not enough time for me to do so. (It was subsequently published: see above at §E5.)
N7 Perhaps the reader can now understand how I felt at the end of the conference. The presenters seemed to be calling for just the thing I had already done, and I felt confident that what I had to publish would "get it right," detail "the tactics of the Polar Controversy" and clear up many of the "mysteries" of this "man unknown," Dr.Frederick Albert Cook, and thus resolve the Polar Controversy.

O The Presenters' Conclusions

O1 Now to sum up and tally the conclusions of the presenters at this [§A1] "long-awaited" and "watershed exploration of the subject" of Frederick Albert Cook and hear their verdicts on his claim of having attained the North Pole on April 21, 1908.
• Myerson: no opinion expressed on the Pole claim, but Cook was an innovative doctor. • Gilberg: no opinion expressed on the claim, but Cook was a better anthropologist than Peary. • Harper: no opinion expressed on the claim, but you can never have the truth of what the Eskimos said about it. • Shoemaker: if you assume Cook reached the pole, his field notes lend support to the claim, though there are major inconsistencies in his data compared with modern research. (During the oral presentation Shoemaker clearly said he believed Cook reached the North Pole, however.) • Rawlins: Cook is a justly persecuted hoaxer; he did not reach the pole. • Fletcher: no opinion expressed in his printed paper, since no paper was delivered or written. In his remarks during the conference, however, he felt it possible that new evidence "from Mother Nature" might throw further light on the matter. • Heckathorn: no opinion expressed on the claim (though there is a strongly implied advocacy); but an answer should be possible when the primary evidence is thoroughly examined. • Koryakin: Cook reached the pole because some of his narrative is supportive in terms of what is now known about the area of the Arctic through which he claimed to have traveled, although parallel conflictual evidence is discounted. But this paper was not delivered at the symposium and therefore I had no experience of it. It therefore could not have been "ignored" in *Cook and Peary*. • Cook-Dorough: based on the evidence in his paper, Cook almost certainly did reach the pole. • Herbert: In his printed paper, Cook faked his journey to the pole. But in his oral presentation he did not express any opinion about Cook's polar claim at all. • Malaurie: Cook faked his journey to the Pole; if he had actually gone there he would not have been able to return, but he was an exceptional personality.
O2 The verdicts of the nine presenters who actual delivered a paper in October 1993:
For: Cook-Dorough, Shoemaker, Heckathorn (by implication)
Against: Rawlins, Herbert, Malaurie
Neutral: Myerson, Gilberg, Harper
O3 This is really not much of a ringing endorsement when you consider that Cook-Dorough and Myerson were officers of the advocacy "Society" that bears Cook's name, and Heckathorn was closely associated with the FACS (they published everything he wrote and bankrolled his jaunt to the Ruth Glacier to try to retrace Cook's route up Mt.McKinley, though he was not a dues-paying member). Remove them from the list and Frederick A. Cook loses, hands down, by a three-to-one margin.
O4 And so this is the record of the symposium whose proceedings were so important, so crucial, and that so compromised my conclusions, that I studiously "ignored" them in writing *Cook & Peary*, a symposium where 75% of its non-FACS affiliated presenters who ventured an opinion concluded that Cook was a fraud. That was all a fine argument before the proceedings could be read. Now the solution is simple. Read *Cook & Peary*. Then read Report #18. Place the two records side by side. Compare them. I shall be satisfied with your decision.

‡3 Cook as Non-Navigator, Inept Liar, Thief of Glory
by
Dennis Rawlins 1995/7/17[0]

A Unreconstructing Cook

A1 Frederick Cook is generally viewed as either an unjustly persecuted hero or a gentlemanly hoaxer. I regard him as a justly persecuted hoaxer. He is also sometimes portrayed as a loner-outsider.[1] Actually, he was — up until his 1909 fall — an amiable, charming insider, e.g., 2nd President of the eminent Explorers Club of New York [and 1st explorer to winter in both polar regions: Rawlins *loc cit*]. Even some who disbelieve Cook regard him as a harmless jester. But, in truth, hoaxers [distort science and not only (obviously) steal hard-earned glory from genuine achievers (like ‡5 §A1) but embarrass and harm innocent peripheral individuals[2] as well. Rawlins 1973 p.93: "In 1909, explorer Greely predicted, 'If Cook lies, a terrible retribution awaits him and his children.' "]
A2 The 1993 Cook Symposium is attempting to rewrite history, massively — a quest I much sympathize with (having myself occasionally engaged in it, in other[3] arenas). But some history doesn't need essential rewriting, and Cook's current classification as a grand-scale exaggerator is a good example. [a] In 1906, he claimed (see his 1908 book, *To the Top of the Continent*) to have climbed the tallest mountain in N.America, though (§C2) he had never previously summited a {serious mountain, though his fearlessness & durability occasionally impressed even his then-severest critic R.Dunn}. [b] In 1908, he reported (see his 1911 book, *My Attainment of the Pole*) reaching the North Pole, after an alleged dog-sledge journey that included roughly 1200 nmi[4] of sea-ice[5] travel, though his previous[6] sea-ice experience (on the 1897-1899 *Belgique* expedition, without dogs) was ordmag 1 percent of such a distance. No self-sustained dog-sledge sea-ice journey before or since has matched [the alleged trip]. Or ever will.
A3 There's no need for me to review Cook's early career, which has been well covered by other contributors to this symposium. Let's get right on to the fun parts of Cook's career: his alleged 1906 ascent of Mt. McKinley & his alleged 1908 attainment of the North Pole.

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[0] Post-1995 edits appear in normal brackets, except for those corrected through the expertise of Bob Bryce, which are repaired in {curly brackets}.
[1] E.g., Who Eames 1973 p.186: Cook was "not one of the boys." Contra this, see Rawlins 1973 pp.79-80, Hunt 1981 pp.228-230.
[2] In 1978, I interviewed [in Copenhagen] astronomer Bengt Strömgren (1908-1987), illustrious son of Elis Strömgren, the wellknown astronomer whose overswift 1909/9/5 interview-certification of Cook had led the Danes into national embarrassment (Rawlins 1973 p.85). B.Strömgren told me that his father never permitted family discussion of the affair, because he felt ashamed for the rest of his life that he had been responsible for disgracing the king. [Leading to international currency of the joke-reply to any ridiculous story: "Tell it to the King of Denmark."] There is an oft-cited 2nd-hand report (*NYTimes* 1909/9/7:5:3) that E.Strömgren had "put an exhaustive series of mathematical, technical, natural, and scientific questions to Dr.Cook". (Incomplete text at Andrew Freeman's invaluable *The Case for Doctor Cook* 1961 p.150; copied by Eames 1973 p.119, though p.319 cites it to *NYT* not Freeman. [Similar "research" at ‡1 §§D3&§§L3-L4.]) In response to my suspicion as to whether his father was comfortable with English, B.Strömgren emphatically confirmed that he was not.
[3] My own historical revisionisms have involved such figures as, e.g., Aristarchos of Samos [‡7 §A3&§C2 and ‡9 §§B1&B2], Aristyllos, Eratosthenes, Hipparchos, Ptolemy, Tycho, Lemonnier, Leverrier-Adams, Cook-Peary, Byrd, Amundsen-Ellsworth-Nobile, Schmidt-Papanin, Plaisted, Herbert.
[4] We use "nmi" throughout for nautical mile, which is $1'$ (1/60th of a degree) on the Earth's surface. One nmi is slightly more than 15% larger than the familiar statute mile.
[5] Most polar sea-ice is a buckled, fissured horror for travelers. The accounts of Peary, Plaisted, & Herbert are unanimous on that point.
[6] Rawlins 1973 p.92.

A4 The controversy over Cook's 1906 expedition has generally centered on whether he did or didn't get to McKinley's top. Actually, the more serious [1906] question is: did Cook ever get to Mt. McKinley's bottom?

A5 Likewise for the controversy over whether Cook got to the North Pole. The real question ought rather to be [§C1]: did he ever in his life get within 500 nmi of the N.Pole?

A6 The first explorer of a territory must: [a] find his way along, and [b] map his discoveries. For long-distance sea-ice travel in Cook's day, both tasks required use of navigational math (spherical trig) & instruments (sextant or theodolite). However, in the entire Cook Papers (US Library of Congress), there is not a scrap of navigational spherical trig in Cook's hand.[7] Note: the records of Cook's arch-rival, Robert Peary, are brimming with competent sph trig-log calculations. (See the Peary Papers, US National Archives. Note also the coincidence that, for Peary's highly suspect Arctic Ocean trips, 1906 & 1909, no sph trig calculations exist in the Peary Papers. See[8] *DIO 1* ‡4 and *DIO 2* ‡5 & ‡8 §B. Also *Washington Post* 1993/6/1 p.3, *Science 260*:1587, 1993/6/11 [& *NYTimes* 2009/9/8].)

B Mt.McKinley: Getting to the Bottom of It

B1 Cook's 1906 trip was his 2nd venture into the McKinley region. His 1st McKinley expedition occurred in 1903. His companion then, Rob't Dunn, later wrote brutally (Dunn 1907 p.93) that Cook "hasn't the least idea of Alaskan travel" and (1903/3/17 entry) ". . . just packs and unpacks his instruments.[9] I wonder if he can use a theodolite after all." Since a key element in the standard defense of Cook is the suggestion (§§B8&D4) that criticisms of him were inspired by Peary Arctic Club influence, Dunn's testimony is of particular significance: [a] It occurred before the Cook-Peary fracas. [b] It is confirmed by the entire lack of [celestial sextant or] theodolite observations in the newly-opened Cook Papers.

B2 The maps appearing in Cook's two published accounts of his McKinley trip (Cook 1907 p.826 & Cook 1908 pp.152-153) are nearly the same, though (contra the inaccurate implication of Rawlins 1973 p.80) Cook attempted to draw his route upon the 1907 version (not the later 1908 one, curiously [that omits his 1907-alleged route entirely!]), which shows him coming in (toward McKinley's peak) from the northeast, through a long nonglacial valley — a valley which is in fact the Harper Glacier. The actual path up Mt.McKinley's NE slope, Harper Glacier (which his chart shows him not taking) is instead mapped by Cook as angling past McKinley, missing it by several miles to the south. The fact that the Harper-Muldrow Glacier splits along this route (right where the Cook 1907 map puts him) — dividing around Karstens Ridge — is nowhere indicated on the Cook map.[10] (Note that [1st genuine McKinley-conqueror Hudson] Stuck's 1913 map of his route up McKinley, published in his 1914 book, is correct on all these points.)

B3 During[11] the Cook-Peary controversy, another witness to Cook's 1906 difficulties with navigation was the expedition's co-leader, Columbia University's Prof Herschel Parker (*NYTimes* 1909/12/10:4:2): "In all the time I was with Dr.Cook, I never knew him to take an observation to determine our [geographical position]. . . . he was evidently . . . little interested in [such . . . . Cook] is not a scientific man at all and knows nothing about the requirements that scientific men look for in records." (See below at §§C10-C18.)

B4 Expedition-members Parker and Belmore Browne gave detailed testimony on this point to the Explorers Club, as recorded in the [long-suppressed] minutes of the 1909/10/15 session of the Club's special committee for investigation of the McKinley controversy. Browne (p.14): "I never, on the whole McKinley trip, saw Dr.Cook make an aneroid reading, either of his own instrument or Dr.Parker's." Parker (pp.18&20): "To the best of my knowledge [Cook] would be unable to make an accurate [hypsometer] reading, as it requires practice and great delicacy of observation . . . . he never watched me [taking such readings], and I believe that he did not take any interest in observations. I may also add that he took no interest whatever in mountain equipment. . . . [ere the 1906 try], Dr.Cook asked a few questions concerning hypsometers, which leads me to believe that he was not familiar with their use prior to the expedition of 1906." [And see Rawlins 1973 p.86.]

B5 Having failed to climb McKinley during the 1906 midsummer, Cook then learned[12] (upon his return to Tyonek, Alaska) that his prime backer, Henry Disston, had unexpectedly pulled out, leaving Cook drowning in red ink.

B6 Though the prime season for climbing had passed, Cook suddenly departed his group, heading towards McKinley on 1906/9/9 accompanied only by a single guide, Edward Barrill. Before setting out, Cook told his people (Expl.Club minutes 1909/10/15 pp.6-8) that he merely intended to reconnoitre. (As properly noted in the Explorers Club 1909/12/24 report on the McKinley matter, item 6: "Dr.Cook's action in attempting the ascent . . . immediately upon the departure of the rest of the party, after entering into an agreement with them that no further attempt should be made for the [1906] season, was unfair to his associates.") But he slyly stuffed a (large) silk US flag[13] into his rucksack — a flag which next appears in his "Top" photograph (Cook 1908 opp. p.227), evidently[14] shot on 1906/9/12.

B7 Cook & Barrill reappeared[15] about ten days later, and Cook claimed success — to the incredulity of {some of} the rest of the party, especially after Barrill {was only fitfully

[7] My certainty §C9 regarding Cook's inability to navigate is such that I have not wasted time searching the Cook Papers for his nonexistent sph trig navigational calculations. (Others' searches [including Bryce's incomparable sifts] have, naturally, found none.)

[8] *DIO* (and its occasional supplement, *The Journal for Hysterical Astronomy*) is available from: *DIO*, Box 19935, Baltimore, MD 21211-0935 (phone 410-889-1414; answering machine always on).

[9] Cook's pretense of using instruments did not cease following 1903. Barrill describes Cook "using instruments for the purpose of taking . . . elevations, and the like." See Washburn 1989 p.119.

[10] Belmore Browne reports on one of his post-expedition encounters with Cook (Expl.Club minutes 1909/10/15 p.17): "anyone having made an ascent of a peak is thoroughly familiar with the topographical features of that peak . . . indelibly imprinted . . . . [however, even] with [his own] photographs . . . before him, Dr.Cook was unable to draw an accurate map of his route over the glaciers to the top of the Northeastern ridge in response to a question from me." I thank Janet Baldwin, archivist of the Explorers Club of New York, for transmitting (1993/10/4) copies of the Club's 1909/10/15 & 10/17 minutes, H.Wack's 1909/10/15 Statement, & the Club's 1909/12/24 "Conclusions" against Cook. (Note: 1909/12/24 is the date on which Cook was dropped from Expl.Club membership: Freeman 1961 p.205. See also below at fn 30.) These four records were made during the 1909/10/12-12/24 Explorers Club investigation of the McKinley matter, carried out by a special committee of the Club. (It is plain from the minutes that some of the committee's members were initially friendly towards Cook, who had recently {1906 December} been elected President of the Club, on the crest of his McKinley fame: Freeman 1961 p.92.) It should be noted that the semipopular Explorers Club and National Geographic were the only US societies that had the decency to officially condemn either of Cook's false claims (though see Rawlins 1973 p.291) — while the purely academic US societies said nothing whatever on the record. The University of Copenhagen eventually rejected Cook, but it is: [a] not a geographical society (a point explicitly noted at the Peary Hearings), and [b] foreign.

[11] But see Freeman 1961 p.91-93, Rawlins 1973 pp.81&291. And the Explorers Club 1909/12/24 report's item 12 states: "the so-called Cook controversy of the present year would not have arisen had Prof.Parker and Mr.Browne presented to the Board of Governors of the Club in 1907 the same evidence which they have recently presented to this committee. These gentleman preferred, however, to await the appearance of Dr.Cook's book." (By which time, Cook had departed civilization, to launch his trip north to Cape Thos. Hubbard.) Freeman 1961 p.183 quotes item 12 (from *NYT* 1909/12/25:3:1), except for the last sentence.

[12] See Cook 1908 pp.180-181, Expl.Club minutes 1909/10/15 pp.8&15, Cook 1911 p.523, Freeman 1961 p.89, Rawlins 1973 p.80.

[13] Rawlins *loc cit*. H.Parker reported (Expl.Club Minutes 1909/10/15 p.6) that Cook was challenged (from the audience) at one of his lectures: why take along a flag if he (according to his own story, e.g., Cook 1907 p.824, Cook 1908 p.181) merely intended to reconnoitre? Cook replied that the flag was packed by accident.

[14] See Washburn 1989 pp.118-119.

[15] B.Browne notes the absence (in Cook's accounts) of dates on the climb & return: Expl.Club minutes 1909/10/15 p.15. According to Barrill, he & Cook left their boat 1906/9/8, and were alone from 1906/9/9 to 9/19. See Washburn 1989 pp.118-120.

steady in his support of the claim}, under the immediate private questioning[16] of Belmore Browne. (It was added in later testimony that Cook — while not overlooking to take along his flag— seems not to have taken climbing rope, axes, or hypsometer.)[17] Barrill later confessed in detail[18] that he & Cook never got close even to the base of Mt.McKinley.

B8 By a very generous interpretation, Cook had amazingly bad luck in choosing his exploring companions. All 3 of those persons, Barrill 1906 and Eskimos Etukashuk & Ahwelah 1908, who accompanied him during the (equally generous) suspect portions of his two contended trips, later claimed that he had invented those portions. Cook & his Occam-defying believers have traditionally attributed this simple circumstance to a complex web of bribery & conspiracy[19] by the Peary Arctic Club. (Danish Admiral de Richeleau to Cook 1909/9/10: "Green-eyed envy and jealousy is doing its envenomed work, but we in Denmark believe in you absolutely.")[20] But the Peary-power-clique issue (interesting though it is, relative to [a] Peary's putting his own N.Pole hoax over on the public, and [b] stifling dissent, including Cook's frequently accurate criticisms[21] of Peary's claims) becomes irrelevant to Cook's claims if the witnesses against Cook are independently verified. And they are.

B9 Barrill testified that, instead of proceeding straight north, up Ruth Glacier towards McKinley, he & Cook turned eastward (more than 10 nmi short of McKinley, which is over 20,000 ft high) — and then climbed a nearby minor peak [only 5,338 ft high], a little over 15 nmi {16.9} from McKinley's top. (A detailed B.Washburn photo[22] permits one to follow Cook's movements.) There the flag was unfurled, and Barrill was photographed holding it. Then, returning to Ruth Glacier, they went north a little again, into the Great Gorge — soon stopping (after Cook carefully examined McKinley's slopes), 1906/9/15, "on account of falling through crevasses".[23] These Barrill statements were publicly made in 1909, well before anyone had actually checked the matter by returning to the geographical region itself. But, next summer (1910), Browne & others went back and — following Barrill's directions & map (again: openly published in 1909)[24] — {eventually} located & photographed the Fake Peak. Over 40 years later, veteran mountaineer Bradford Washburn (longtime head of the Boston Science Museum) took Cook's 1908 book, *To the Top of the Continent*, into this region and identified[25] the location of every single vista photograph in the book.[26] (Washburn rightly regards Cook as a first-rate alpine photographer.) All were taken (contra Cook's lofty captions) at low altitudes (below 6000 ft) and more than 10 miles from McKinley's top. This finding is consistent with Barrill's account, not Cook's. Washburn's approach was the ideal way to test Cook's 1906 claim. His achievement should have ended the McKinley controversy.

B10 And, there is an elementary question which requires no detailed investigation: what was Cook doing near and on[27] this side peak in the first place? — climbing up to and photographing an insignificant point, with no relevance to his alleged goal.

B11 In passing, note the curious fact that Cook's accounts include no photo of the unique vista visible from the top of McKinley, which would be impossible to fake. By contrast, the first actual climber, Hudson Stuck (1913), includes just such a photo opp. p.102 of his 1914 book, *The Ascent of Denali*, looking towards Mt.Foraker ("Denali's Wife").

B12 Some pro-Cook accounts, e.g., H.Eames' *Winner Lose All* (Little-Brown 1973, on the *NYTimes*' exclusive "New & Recommended" list for 3 weeks), accept that Cook's "Top" photo was taken on Fake Peak — but insist that this "slight slip" (p.67) doesn't prove Cook didn't climb Mt.McKinley. If one believes in Cook's innocence, one must eventually adopt logical positions of this type.

B13 The fraudulence of Cook's McKinley "Top" photo (Cook 1908 opp. p.227) is obvious, due to a fortunate bit of Cook carelessness: the peak of another (distant) mountain is visible in the lower righthand corner (contra Cook's reported intent).[28] And one is clearly looking upward at it — which naturally isn't possible for a photograph taken from the highest mountain in N.America. . . . Moreover, this peak may be identified from another photo, opp. p.239: it's the same 8450 ft peak which is labelled "Mt.Grosvenor" in the photo opp. p.192 of Cook 1908. The top of Mt.Grosvenor[29] in the photo opp. p.239 matches the lower-righthand-corner peak in the infamous "Top" photo. Note: [a] This side of Mt.Grosvenor does not face Mt.McKinley. [b] It does face Fake Peak. A number of scholars, myself included, have discovered this independently. But the credit for first noting it should go to 1906 expedition-members Herschel Parker & Belmore Browne. It's all in their private 1909/10/17 testimony (pp.17-19) before the Explorers Club's committee investigating the McKinley affair. Browne added (Expl.Club minutes 1909/10/17 p.19) a revealing (and [as of 1993] hitherto-unpublished) item: he had personally seen the full detailed original of

[16] Rawlins 1973 p.81. [D.Roberts *Great Exploration Hoaxes* 1982 p.108 vs *C&P* p.280.]

[17] See §B4 & 1909/10/15 Explorers Club minutes pp.9-10, 12, 17-20. See also items 4&9 of the Club's 1909/12/24 negative "Conclusions" on Cook's 1906 claim. Cook (e.g., 1907 pp.832&836) speaks of using ice-axes, though Parker certifies that Cook & Barrill had none during their 1906/9 foray. {Bryce notes: Cook's 1907 article bears photos of Barrill with an ice-axe sometime on the trip.}

[18] Affidavit 1909/10/4, published *NYGlobe* 1909/10/14 (diary 10/15). (Summary at Freeman 1961 p.179.) See affidavit text's reprinting in Washburn 1989. I am grateful for Janet Baldwin's transmission (1993/10/4) of a photocopy of this article, as well as of the testimony of E.Barrill's daughter, Marjorie (Barrill 1988).

[19] See §D4 (also §C9), and, e.g., Eames 1973 pp.67, 176f, & 229f (well evaluated at Hunt 1981 p.227). The $350,000 Peary Arctic Club war-chest (*ibid* pp.177&283), allegedly devoted to "see [Peary] through" the Cook Controversy, is a fantasy based upon the oral recollection of octagenarian Cook-believer Clark Brown. (Cited *ibid* p.321 n.3.) This vision of vast sums subscribed to ruin Cook is merely a misunderstanding of the $350,000 the Club put up *before* 1909 to see Peary through *to the Pole*. (See *NYTimes* 1909/9/15:2:1. And note the common sense remarks at Hunt 1981 pp.227-228.) As Ted Heckathorn has found, the prime party that was hellbent on destroying Cook was not the Club but Peary himself (through Peary's well-paid personal lobbyist, L.Alexander: Rawlins 1973 p.248) — who was understandably anxious to divert attention from his own exploration-claims' shortcomings, taking every possible opportunity to spotlight Cook's instead.

[20] As quoted by Eames 1973 p.132. Incompletely quoted by Rawlins 1973 p.90.

[21] See *DIO 1.1* ‡4 §B4.

[22] See Barrill 1988 p.81, where the Fake Peak is clearly marked with an arrow. Arrow also [at www.dioi.org/vols/w73.pdf, Fig.1] & at Washburn 1989 p.112. And see map of route at *ibid* p.117.

[23] Washburn 1989 p.116 map in Barrill's hand. (See also p.120.)

[24] Barrill's hand-drawn sketch map locating Fake Peak (right where it was later found & photographed by Browne, Washburn, & Carter: fn 29) was published in the 1909/10/15 *New York Globe*. This page of the *Globe* is photographically reproduced at Washburn 1989 p.116.

[25] The interested scholar is urged to consult Washburn's full, highly detailed unpublished ms (which is buttressed by numerous charts & photos), copies of which are preserved at the American Alpine Club (NYC) and at the University of Alaska.

[26] No photo found in the newly opened Cook Papers has altered the situation. [Note added 1995: my wife Barbara & I happened to be meeting Brad and Barbara Washburn for the first time on 1995/7/16 (at their Belmont home), when an Alaska phonecall at 11:15 AM from Brian Okonek brought the news that the only previously unidentified Cook photo had just been located. Its location was, like all the others, over 10 miles from the McKinley summit.]

[27] The photo opposite p.239 of Cook 1908 was taken from Fake Peak, which proves positively that Cook was there. [Original 1994 error here corrected by DR 1995/7/17. Independently detected by Bryce: ‡2 §G1.]

[28] From Barrill's affidavit at Washburn 1989 p.119 (which also reports at p.118 that Cook ordered Barrill to forge his diary entries for 1906/9/12-18), "I made the remark [to Cook] that the eight peaks [including Mt.Grosvenor] on the other side of this point where I had been photographed ["Top" photo] would probably show in the picture, and he said that he had taken the picture at such an angle that those peaks would not show." (Note that, according to Cook's two 1908 Eskimos, he was looking out to avoid telltale background topography when faking his Arctic photos, too: Rawlins 1973 p.90.) The version of the "Top" photo published in Cook 1907 was cropped. (For similar case, see §C6.) But the 1908 version inadvertently got published with the summit of Mt.Grosvenor showing at the lower-right corner of the picture.

[29] Hunt 1981 pp.142b-c reproduces various photos of Fake Peak: Cook 1906, Browne 1910, Washburn 1956, Adams Carter 1957.

the “Top” photo and recalled seeing Mt.Grosvenor more clearly there than in the published version. Therefore, on 1909/10/17 (minutes pp.19-20, 23), Cook was formally requested (by the Explorers Club special committee) to produce[30] the original photo or negative. Cook promised (Expl.Club 1909/10/17 minutes pp.2, 12, 14, &15) to “come back in a few days and take the matter up” and additionally to do everything in his power to assist the Club’s investigation within a month. Instead, after the month had elapsed, Cook abruptly disappeared for a year. [This very ploy has been replicated (1997-to-date: Rawlins 2018 fn 47) by the archon now since 2013 atop the history-of-astronomy community — which lacks the Explorers Club’s integrity.] His choice to flee — instead of producing requested evidence — has been excused by the pressure he was under from detractors. (See, e.g., Freeman 1961 pp.197-201.) But it is circular to excuse Cook’s nonproduction of evidence by complaining that scientists & press were so churlish as to push him for evidence. . . .

B14 Cook’s sudden {1909/11/24} exit was during not just his alpine controversy. By then, he’d added to his notoriety by claiming yet another remarkable First: the North Pole.

C Frederick the Navigator

C1 Cook went north to Greenland in 1907 on the yacht of gambler John Bradley. In early 1908, Cook & a few companions dog-sledged west across Ellesmere Land. The sole other non-Eskimo of the party, R.Franke, returned before the Polar Sea was reached. So Cook was without literate companionship well before he reached [his farthest] point at about 81° 1/2 N latitude [see his companion-Eskimos’ map of their trip at Rawlins 1973 p.94 or Bryce 1997 p.424 (which differ in label-lettering size), hereinafter referred to as the “Eskimo map”], roughly a dozen nmi north[west] of {Cape Thos. Hubbard} (81°22′N, 94° W), the north tip of Axel Heiberg Land ([Cape T.Hubbard being] the last northward position[31] which all scholars agree Cook reached). This was the takeoff point for an alleged 500 nmi sea-ice trip, with two young Eskimo companions, north to the Pole — where he reportedly arrived on 1908/4/21. However, according[32] to those of Peary’s men who in 1909 interviewed these two Eskimos [creating above-cited Eskimo map], the three-man 1908 party instead went from this point (the beginning of rough[33] sea-ice north of C.Hubbard) more nearly southward, past the Ringnes Islands. [Where the Eskimo-map put him east of these islands while Cook (1911 p.285 map) claims to have gone between them, though (while calling the Hassel Sound between them “wide”: Cook 1911 p.329) not noticing in his map that (as V.Stefansson points out) Sverdrup had greatly underestimated the width of the channel.] So Cook’s account claims he went 500 nmi north of land, while his Eskimos [told Henson he (wisely: §C9) never left sight of land (‡2 §E1)] — and actually traveled only about 12 nmi north[west of it (Rawlins *loc cit*) **until meeting rough sea ice**. In 1914 MacMillan met it at 14 mi (*Bull.AMNH 16*:390), *yet another confirmation of the Eskimo Map’s accuracy*, along with 1916 rediscovery of the map’s accurately placed Meighen Island. Traveling with D.MacMillan in 1914 NW from C.Hubbard, Etukishuk told MacMillan (W.Herbert 1989 p.316, Bryce 2013 p.339) that, at 25 mi out, they’d already passed Cook’s farthest point, and he later told Stallworthy it was a modest distance “within sight of land”. The 2 Eskimos said (Herbert 1989 pp.278&316) after Cook pruned the group to them&himself, they were soon blocked by rough ice (fn 33) & open water, & so slept 1^d-2^d, returned to Cape Hubbard & headed south. (See Bryce 2013 pp.343-345 & 1997 {pp.423-424} & 969-975 for why he rejects these small distances.) Since Bryce 2013 p.340 notes “the rough ice thrown up against the shore” it’s reasonable to conclude Cook stopped at the end of the smooth land-ice, whose boundary is drawn on the Eskimo-map. From Earth’s curvature & the unalpine heights of Axel Heiberg’s Cape Hubbard & Garfield Coast’s Cape Colgate (heights 1600′ & 2000′, resp), Cook’s party couldn’t see these peaks when over 50 nmi from them; to see them from 100 nmi out (2/3 as far as navigators DonMacM&Green got!), they’d have to be c.4 times higher than reality. Sea-horizon-distance in naut.mi from height h in meters is $2.1\sqrt{h}$ (S.Newcomb 1906 p.199).] [Foregoing bracket added 2017, rev 2021.]

C2 Questions in passing (§A2): what was Cook’s previous experience in long-range sea-ice dog-sledging? The same as his pre-McKinley experience in climbing {tall} mountains. None. He had never climbed [such] a mountain — before suddenly claiming conquest of the tallest mountain in N.America (in one of the roughest mountain terrains anywhere). And, before 1908, Cook had never dog-sledged on sea-ice (a particularly hellish kind of traveling surface: high “pressure-ridge” ice-walls and wide open-water “leads”) — before announcing in 1909 that he had in 1908 sped over nearly 1200 (beeline) nmi of sea-ice to the Pole and back. (He allegedly went over 500 nmi from C.Hubbard to the Pole and returned by a route that took him more than 650 nmi further, before he struck land at the Ringnes Islands. [One would expect Cook to leave a cairn-record at the spot of his salvation, after 1200 alleged miles over floating ice. But he uniquely ***left no record anywhere throughout his journey*** (Rawlins 1973 p.83, Bryce 1997 p.857): keeping his timetable options open.])

C3 Mystery: why did Cook start by going west (across) rather than north on Ellesmere — thus necessitating so much more sea-ice distance than R.Peary’s intended route? By contrast, Peary’s 1909 near-miss of the North Pole was launched from the north tip of Ellesmere, 413 nmi from his object. (Note: sea-ice-veteran Peary genuinely tried to reach the Pole in 1909, going out roughly 350 nmi from land, to within ordmag 100 nmi of the N.Pole — & back: in all, the longest self-sustained sea-ice journey [as of 1995].) Possible[34] solution: was Cook initially[35] hoping to be the first to reach Peary’s “Crocker Land” [see also Bryce 2013 pp.16-17], an attainment which would not only bring glory (since it might turn out to be the most northern land-mass on Earth) but could also permit Cook to head for the Pole *by land*, not sea-ice — a reasonably nonsuicidal idea, since [a] Cook was not a navigator (§§A6, B3, & C7-C18), and [b] he could replenish food-supplies from the game presumed to inhabit Crocker Land (better than trying to live off the relatively barren Polar Sea). Against this theory: Cook couldn’t survey; so, had there been a Crocker Land, he could have hunted & traveled north on it but couldn’t map it. (Note: Peary’s nonexistent Crocker Land — first reported by him in his 1907 publications — was long regarded as an innocent error. This writer has found that his diary entry of the alleged discovery date,

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[30] Hunt 1981 p.111. (Date at p.271 is a misprint, perhaps for 1909/12/29?)

[31] Cook calls C.Hubbard by Sverdrup’s name, Svartevoeg. See Cook 1911 pp.200-201n, which suggests (since he found no Peary cache) that Peary did not reach C.Hubbard in 1906. Actually, Peary, not Sverdrup reached the north tip of Axel Heiberg Land. (The reason Cook found no record is that Peary’s published reports of his 1906 western journey do not guide the searcher to such. For why, see §C3 and Rawlins 1973 p.75.)

[32] On 1972/11/6, I phoned RCMP explorer Harry Stallworthy (not a member of the Peary clique), and learned that he had long ago heard the same account directly from Cook’s two 1908 Eskimos.

[33] [Rev. 2021/5/31.] I suspect photos opp. Cook 1911 pp.172&332 were taken here. No sea-ice photos in Cook 1911 (opp. pp.204&205, 236&237, 268) show sledges ***going up&over pressure-ridges***, or crossing open-water leads. Barrill said all 1906 camps were on level ice (Washburn 1989 p.122), consistent with 1908 Cook not getting past the level 10 nmi-wide ice-foot, as shown on his 2 Eskimos’ 1909 map (§C1): [a] the 10 nmi distance ***AND*** [b] ceasing northing at the 1^{st} rough-ice-encounter. Compare to Peary’s hardfought 1909 drive north: reality backed by Peary 1910 photos opp. pp.216, 224, 240, 306, 308, 309. {DR earlier said here Cook’s fan-harness differed from Peary’s, a truly dumb error (given dogs-equidistance’s whip-advantage) which Bryce thankfully corrected 2021/5/30.} And Cook’s relevant photos show him sledging only over flat&smooth not rough ice. Contrary picture at Bryce 2013 p.339 (*C&P* p.882): not a Cook photo {but an excellent 1912 drawing by W.R.Leigh}. Both sides of the issue of Cook’s farthest north are provided at the end of this issue: ‡10

[34] Cook said he hoped to travel poleward along a nonexistent “magnetic meridian”. (T.Hall 1917 pp.243, 389, 451-454; diagrams pp.244-245 & §C10.) This still does not explain why when [fn 38] short of food throughout the return trip, he didn’t (upon leaving the Pole) veer to the east of his outward track, aiming to come down upon north Ellesmere Land’s game (and relatively smooth ice-foot). [*Cook’s entire trip (real&faked) after C.Hubbard is* **weirdly** *far-west obviously to duck other explorers*.]

[35] However, he had been warned, by the Eskimos who had accompanied Peary at the time of Peary’s fraudulent “Crocker Land” sightings, that there was no such land: *DIO 1.1* ‡4 §B4. [To verify existence: why not just climb Cape Hubbard’s peak (for its view) rather than supposedly fighting ice for weeks?]

1906/6/24, states: "No land visible". Thus, Crocker Land constitutes the clumsiest of Peary's several exploration hoaxes: www.dioi.org/cot.htm#cair & *DIO 1.1* ‡4 §B2, 1991.)

C4 Cook's eventual story has him passing by Crocker Land (at speeds which, though occasionally rather high, are more reasonable[36] than some of Peary's ludicrous 1906 & 1909 claims in this regard)[37] and then discovering his own nonexistent land, "Bradley Land", farther north. Another mystery: since sledging along a land's ice-foot is much speedier and less wearing than sea-ice travel, and since land may have game,[38] why did Cook — by his own account & map (Cook 1911 pp.244, 246, 285) — travel for scores of miles parallel to this alleged land while never going on board it? Some Cookites regard his report of the direction of ice-drift north of Axel Heiberg Land as evidence for his attainment of the Pole. But such information is not astonishingly specific — and Cook could have seen this drift from Cape Hubbard's ice-foot. Let us compare the alleged vindication of Cook by drift to his definite nonvindication by Bradley Land.

C5 Cook's 1911 book describes Bradley Land (pp.243-247) and even displays a photograph of it (opp. p.236) [‡1 §I1]. And his "Field Notes" (Cook 1911 p.571) report that it has a table of 1000 ft, with height up to 1800 ft. Had such land later been found, Cook would be confirmed to this point in his story. But no such land exists. Question: if it is said that Cook is vindicated because he reported a rough direction for drift (and, e.g., he correctly said that N.Pole ice can look purplish),[39] then why isn't he disconfirmed when he reports in detail — *and photographs* — a wholly mythical land? As a measure of verifiability, this is as solid as can be. Most researchers, when encountering such a black&white crucial-experiment, correctly gauge the value of the Cook claim — and then move[40] on to other, more fruitful fields of endeavor. Using a double-standard for positive & negative evidence is not science but advocacy. (A believer will accept an ice-drift report as happy pro-Cook evidence; but, then, when faced with the Bradley Land disaster, will resort to supposing that Cook must have seen an ice island — though, no ice island is anything like 1800 ft high.) Again: a neutral investigator will not [a] treat vague alleged positive evidences as vindicating Cook, while [b] treating all negative evidences as mere Problems or Paradoxes that prove nothing but the need for increased effort at dedicated Re-explaining.

C6 The next Discovery on Cook's imaginary journey was his alleged "land ice" at 87°-88°N. He stated in the *NYHerald* (1909/9/2 p.1, Cook 1909 10/3 p.4) and in his book (Cook 1911 p.265) that there was no elevation to the land ice, yet his photo of it (Cook 1911 opp. p.236) shows a dramatic rise in altitude as one approaches it. Moreover, Cook describes it as like a glacial surface and says at *NYH* 9/2 p.1 that he found "no positive sign of land or sea." (See also Cook 1909 10/3 p.4 & Cook 1911 p.266.) Yet, Wally Herbert (1989 p.319) made the remarkable discovery that {a lantern slide} of Cook's "land ice" photo survives — and examination of the whole photo (made public for the first time at p.288b of Herbert 1989) shows that, when publishing the picture, Cook had simply (like fn 28) cropped off an inconvenient feature: a substantial hunk of land (near the photo's right edge), in order to make the scene *look like the pure ice he reported it as*.

[36] See just irony at Freeman 1961 p.138.

[37] See, e.g., Rawlins 1973 pp.144-145, and Herbert 1989 pp.184&261f.

[38] Cook 1911 p.244 (emph added): "delay was jeopardous, and, moreover, *our food supply* did not permit our taking time to inspect the new land." Cook continues later in a footnote on the same page: "Whether animal life existed there, I do not know, for the impetus of my quest left us no time to investigate. I passed the last game at Heiberg Land."

[39] Cook 1911 p.598, Hunt 1981 pp.164&225.

[40] After my own dreadful 1988 error on Peary's Betelgeux Document (*DIO 1.1* ‡1 §C3, 1991), I am not pretending to omniscience. But, when mounting evidence goes against one's theories, one should openly & frankly alter one's opinion to accord with new data. (See *ibid* fn 14.) This is not only the decent thing to do — unwillingness to face one's errors dooms one to a lifetime of ducking and-or alibiing evidential challenges. (Anyone supposing that such behavior does not occur at the heights of academe is advised to examine the sad behavior pattern discussed at *DIO 1.2* §D4. [Or *DIO 16* ‡1 §§A1-A2; *DIO 22* ‡3 §B.])

C7 Finally, Cook's story arrives at the Pole. His first alleged sextant observation appears at p.292 of Cook 1911. Its computation of refraction contains an oddity discovered by the present writer (designer of the first accurate compact zenith-to-horizon refraction-correction format).[41] For the alleged 1908/4/21 noon solar altitude, 12°, the correct[42] refraction (at the reported temperature & atmospheric pressure) would have been 5′ — but Cook 1911 (pp.292&302) instead plagiarized[43] a refraction of 9′ from the observation reproduced in Peary's 1910 book *The North Pole* at p.362 (allegedly 1909/4/6 noon), for solar altitude 7°. (Cook obviously didn't even know that refraction is a function of altitude, and thus that a value which is valid for one altitude isn't apt to a different altitude.)[44] Moreover, Cook (1911 p.302) claims that he applied the same 9′ refraction-correction to 7 pairs of such sextant observations (fn 53) spread over 36 hours, from noon of 1908/4/21 to midnight of 4/22-23. Thus, all 7 of the "Pole" observation-pairs, correctly computed (using the appropriate 5′ instead of 9′ for refraction-correction), actually placed him 4 nmi off the N.Pole, towards the Sun. As noted at p.86 of Rawlins 1973 (*Peary at the North Pole, Fact or Fiction?*), a passage quoted from an earlier Oslo University paper (Rawlins 1972 p.135), these 14 data "demand that [Cook] must have hovered for [over 24 hours straight] four miles[45] sunward of the Pole, while the Earth spun just beneath his feet. The indication that Cook was riding a flying saucer is not to be taken lightly — e.g., his only [published] doublelimb solar altitudes (April 8 and 14) make the Sun's apparent diameter 1/4 degree (not 1/2 degree, as it appears from the Earth), thus placing him about two astronomical units from the Sun, presumably on the planet Vesta!" The 1908/4/8&14 alleged observational data (both double-limb double-altitudes by sextant) appear in Fig.1 here, photocopied from pp.257&274 of Cook 1911. And 1912. And 1913. In the original 1911 edition, the upper-limb record is 1/2 degree higher than the lower-limb. However, since these are altitudes supposedly taken with an artificial horizon,[46] the differences ought to be [just over] a full degree. (Rawlins 1973 p.87: "No one who had ever used a sextant and artificial horizon *once* — *any*where — could have made the [blunder] responsible for this.")[47]

C8 In his book's later editions, Cook "corrected" this slip (fn 53 here & Fig.1) ***but without ever telling readers of his error or its patchup***. [Same dishonest ploy by today's toppe history-of-astronomy politician-editor: Rawlins 2018C fn 11.] Indeed, in the 1912 edition, at p.274, *BOTH* (necessarily contradictory) versions of the 1908/4/14 data (before AND

[41] Now found in most of the world's navigational manuals, this simple format, for computing refraction r as a function of altitude h, is: $r = a\cot[h + b/(h + c)]$. It was first published by Rawlins *Publ Astr Soc Pacific 94*:359 (1982/4) p.363 eq.8a. New professional-level Rawlins formulae for refraction (including corrections for temperature & pressure) recently published at *DIO 2.1* (1992) ‡4 fn 17 [subsequently refined by the late Keith Pickering].

[42] By the 1992 Rawlins refraction formulae cited at fn 41, refraction $r = 5'.4$.

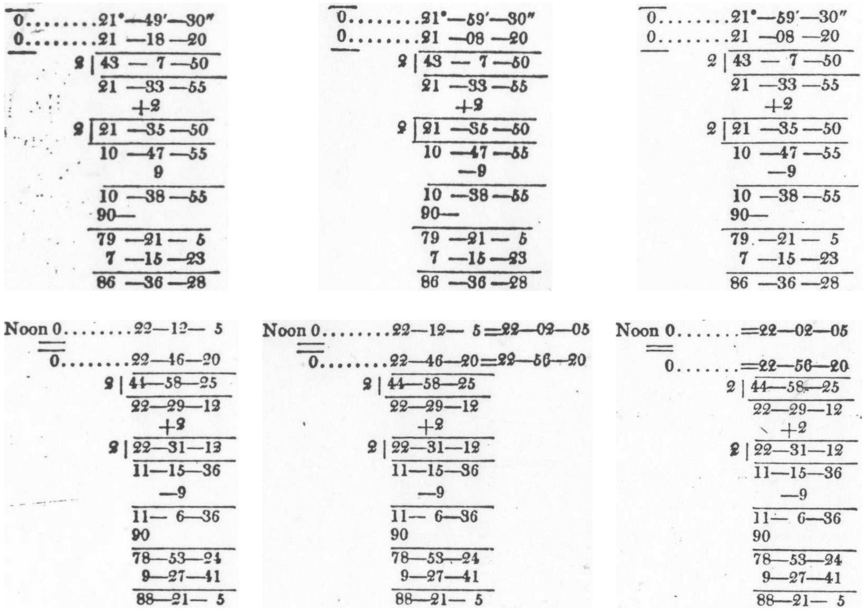
[43] Note that Cook 1911 p.292 [photo opp.p.364] & p.245 also copies Peary's [ultra-]minor error (1910 p.362) of applying refraction-correction after (not before) semidiameter-correction.

[44] As pseudo-explained at Cook 1911 p.246n, he applied 9′ of refraction not only to his 7 "Pole" sextant shots (Cook 1911 pp.292&302) but to all the others (Cook 1911 pp.245, 257, 274): 10 observations in all. The 9′ refraction-figure used by Cook was wrong in every case: 10 for 10.

[45] Cook may have been warned of this error. Cook 1911 (pp.289, 296, 302, 573) says he moved camp 4 nmi "magnetic south" between the 2nd & 3rd pair of observations. However, since the last 5 observation-pairs were all allegedly taken at the same spot, the hovering-saucer hypothesis is solid for the 24 hr period over which these 5 observations were allegedly taken.

[46] Cook's sextant was later recovered (& publicly displayed in Copenhagen), without artificial horizon.

[47] The discovery of this Cook slip was made by Rep. Henry Helgesen. (See his astute analysis in the *Congressional Record 54 Appendix* p.56, 1916. [Bryce found (‡2 §G1) that the actual author was Ernest C.Rost {Cook's lobbyist posing as Helgesen's secretary} (who in 1916 sued Cook for nonpayment of lobbying fees: Rawlins 1973 pp.247-248), one of a vast array of new revelations in Bryce's definitive 1997 book. The refraction error (§C7) was Rawlins' find, as was the 1912 double-printing (§C8) of 1908/4/14 data.] {The artistic 1913 drawing of a sledge dog's face at Cook 1911 p.567 was by Rost's son, a gifted etcher.}



0........21°—49′—30″
0........21 —18—20
2 | 43 — 7—50
21 —33—55
+2
2 | 21 —35—50
10 —47—55
9
10 —38—55
90—
79 —21— 5
7 —15—23
86 —36—28

0........21°—59′—30″
0........21 —08—20
2 | 43 — 7—50
21 —33—55
+2
2 | 21 —35—50
10 —47—55
—9
10 —38—55
90—
79 —21— 5
7 —15—23
86 —36—28

0........21°—59′—30″
0........21 —08—20
2 | 43 — 7—50
21 —33—55
+2
2 | 21 —35—50
10 —47—55
—9
10 —38—55
90—
79 —21— 5
7 —15—23
86 —36—28

Noon 0........22—12— 5
0........22—46—20
2 | 44—58—25
22—29—12
+2
2 | 22—31—12
11—15—36
—9
11— 6—36
90
78—53—24
9—27—41
88—21— 5

Noon 0........22—12— 5=22—02—05
0........22—46—20=22—56—20
2 | 44—58—25
22—29—12
+2
2 | 22—31—12
11—15—36
—9
11— 6—36
90
78—53—24
9—27—41
88—21— 5

Noon 0.......=22—02—05
0.......=22—56—20
2 | 44—58—25
22—29—12
+2
2 | 22—31—12
11—15—36
—9
11— 6—36
90
78—53—24
9—27—41
88—21— 5

Figure 1: Successive 1911&1912&1913 editions of 2 badly faked 1908 double-limb solar double-altitudes. Top row 1908/4/8. Bottom row 1908/4/14. Vestal Versions (§C7) on left. Final Fudged Versions (§C8) on right. From Cook's *My Attainment of the Pole* pp.257&274.

after doctoring) are included and (the ultimate oddity) *equated*: "$22°12'05'' = 22°02'05''$" and "$22°46'20'' = 22°56'20''$". (The appearance of this amazing 1912 simultaneous double-version was probably caused by a printer's misunderstanding of orders scribbled by Cook on the galleys, indicating replacement of the former data by the latter.) By the time the 1913 edition appeared, he had completely suppressed the original version, having enhanced both upper limb values by $10'$ while shaving $10'$ off both lower limb values — which keeps both pairs' critical averages the same as before (necessary for holding-fixed the already-published latitudes "deduced" from the data), but simultaneously requires the believer to accept that Cook was the victim of two misprints which *perfectly* cancelled. Twice. (I.e., 4 misprints in all.) See Fig.1's comparative photocopies, of all 3 editions for both dates. (Final upper-vs-lower differences, $51'10''$ & $54'15''$, are poor approximations to correct $64'$; but patching up more exactly would require accepting 8 misprinted digits.)

C9 To a scientist, it has always been obvious (Rawlins 1973, e.g., p.89) that Cook was not a navigator. (And he said he wasn't: §C17.) Returning to the question (§C1) of whether Cook's 1908 trip was c.12 nmi (Eskimos) or 500 nmi (Cook) north of land, we find that we need not debate whether (fn 19) the Eskimos were coerced or bribed or misunderstood when they testified that Cook went only about 12 nmi north[west] — because we have (as in the case of Barrill's testimony) independent confirmation of the witnesses: if Cook couldn't navigate, it would have been suicide for him to go *out of sight of land*. (The Eskimos testified[48] that the party never left sight of land at any time on the trip. [Consistent with mere land-ice photographs: fn 33. Tho MacMillan said he saw land from 85 nmi out in 1914.] Further Cook navigational oddities follow.

[48][Fn 33, §C1, & ‡2 §§E1&§E2. See also] Cook 1911 p.206n, Freeman 1961 pp.105, 157-158, Rawlins 1973 p.89, Hunt 1981 p.142o (reproduction of 1909/9/8 NY *Evening Telegram* p.1).]

C10 At several places in his 1911 book, Cook assumes that, at the N.Pole, the compass pointed to the N.Magnetic Pole. Since the Earth is not a simple magnet, this assumption was wrong by about 30°. (See Cook 1911 pp.288-292, 573; also Rawlins 1973 pp.91&234.) According to August Loose, Cook privately claimed[49] that the key to his alleged 1908 navigation was steering compass-south along the 95°W meridian from land to the Pole — a method squarely based upon the same naïve simple-magnet assumption (*NYTimes* 1909/12/9 p.3 col.4). Such a mistake could not be made by a genuine attainer of the Pole.

C11 Cook frequently gives longitudes precisely to the arcminute even while closely approaching the Pole, though all navigators know the folly of this.[50] E.g., at latitude $89°31'$N, Cook says (1911 p.279, contra the dead-reckoning claim of p.573) that his longitude calculations (performed *before* the noon latitude, a feat that will further astonish navigators) gave $95°03'$W. However, just 29 nmi from the Pole, an arcminute of longitude is (in great-circle angle) less than an arcsecond: a distance of just 16 meters. Dead-reckoning (Cook 1911 p.573) to such precision after many miles of compass-course marching is superhuman. Nothing like it in the history of exploration (except Peary's pole-in-one 1909 alleged aiming: Rawlins 1973 p.145). Only someone completely unversed in the relevant math would make such errors. (For an equally astonishing similar slip by a prominent [former] prof in the University of Chicago's Astronomy Dep't, see *DIO 2.3* ‡8 fn 31.)

C12 In his first account (*NYHerald* 1909/9/2:1:4), Cook reports his arrival at the Pole, "On April 21 the first correct altitude of the sun gave 89 deg. 59 min. 46 sec. The pole therefore was in sight. We advanced the fourteen seconds". His next account (Cook 1909 10/5:4:1) is, "The observation gave latitude 89 deg. 59 min. 45 sec. . . . We advanced the fifteen seconds". By 1911, Cook had been informed by amused scientists that such precision is meaningless, so the Pole-arrival account at Cook 1911 pp.288-289 was substantially rewritten: "Several sextant observations gave a latitude a few seconds below 90°, which, because of unknown refraction and uncertain accuracy of time,[51] was placed at 90°."

C13 Cook 1911 p.580 equates 1 timemin with 1 nautical mile. Since the latter equals 1 arcmin (fn 4), this is just a confusion of timemin with arcmin, a distinction which ranks as chapter-one navigational material. No one familiar with navigation makes such mistakes.

C14 Where are the data for Cook's alleged 1908 steering and longitudes? (See fn 53.) A common excuse is the claim that Peary's people stole[52] the data. (Contra this, see Rawlins 1973 p.87.) However, we recall that Cook claimed he took a "round of [theodolite] angles" atop Mt.McKinley in 1906 — and these data are (as I predicted long ago to Cook's daughter, e.g., 1974/1/19; see also Rawlins 1973 p.80, years before the Cook Papers' unsealing) no more to be found than Cook's alleged N.Pole navigational[53] data. Nowhere in the Cook Papers or in the scientific materials of Cook's several expeditions (1892, 1894, 1897, 1903, 1906, 1908) are there records showing that Cook had ability with a sextant — or had the ability to compute geographical position from sextant data.

[49]See above at fn 34.

[50]Cook 1911 p.502 says this was just due to automatic computational routine. However, no other polar explorer exhibits such naïvete about longitude.

[51]Nonetheless, when allegedly only a quarter of a nmi from the Pole, he estimates his longitude as 97°W (Cook 1911 p.292; "ORIGINAL" data sheet photo opp. p.364) — i.e., an implicit longitude precision of ±4 meters (±1°/2)!

[52]See below at fn 63. Conversely, the *NYHerald* (1909/9/23 p.5 col.3) reports a theft of documents on Peary's ship, shortly after his return from the North.

[53] The sextant data Cook (eventually) published have no relation to steering, being merely latitude-sight arithmetic. (There exist no Cook observations for longitude, real or fake: Rawlins 1973 p.87, [Bryce 1997 p.463].) H.Abramson sent (1988/12/13) photocopies of these meridian-arithmetic Cook "original observations" (Cook Papers, Library of Congress) for 1908/3/30, 4/8, 4/14, & 4/21-23, the same figures appearing at Cook 1911 pp.245, 257, 274, 292, & 302. The data sheets are conveniently smudged at the suspect (§§C7-C8) spots. Nonetheless, the 1908/4/8 first sight is unambiguously discernable as $21°49'30''$, not $21°59'30''$ (as later "corrected" in the 1912 & 1913 editions of Cook 1911).

C15 This matter goes to the heart of the Cook claim, and it ought to be faced by his defenders. I quoted Dunn & Parker earlier (§§B3&B4) on Cook's navigational disabilities. Let us now turn to the testimony of the man who offered to act as Cook's secret navigational double: ship's captain August Loose, a figure unknown[54] to Cook's friends (including even his lawyer, Henry Wellington Wack, who saw Cook frequently at this time). Loose reported in a 1909/12/7 affidavit — gleefully page-one-displayed by the *NYTimes* (1909/12/9) — that he was hired by Cook to manufacture celestial data proving navigation of a 500 nmi trip to the N.Pole, data computed indoors (in {Bronxville}) for the specific places&dates Cook had already published. From Loose's story, "How to Discover the North Pole without Leaving New York" (*NYTimes* 1909/12/9 p.3 col.3, partially quoted in Rawlins 1973 p.86):

> It took me only about three minutes on my first acquaintance with Dr.Cook to get the idea into my head that he had never found the north pole. I found that he was entirely ignorant on many points of the method of taking observations. It amazed me that a man who needed so much enlightenment would have the nerve to come out and say he had discovered the north pole. . . . He could not answer simple questions on matters that he should have been intimately familiar with . . . . Of course, I have no way of knowing that the doctor did actually copy my "observations" and send them in [to the Univ Copenhagen scientists preparing to judge Cook, but] . . . . if he used the stuff he had before I started in to help him, he would never convince those Danish scientists.

Cook did not[55] send Loose's fakes to his Copenhagen judges. And he didn't convince them.

C16 As previously, when we must choose which party to believe (Cook vs. one of the succession of witnesses against him), we may look for independent confirmation. In this case, consultation of the several giveaway slips (§§C10-C13) of his trip is powerfully consistent with Loose's often-hilarious account of Cook as a non-navigator.[56] (To a navigationally-trained reader, Loose's account is that of one conversant with the math & practice of navigation. Cook's account is not.) When Loose's credibility was attacked in 1909, he countered by publicly challenging Cook to demonstrate that he could use a sextant. (See Rawlins 1973 p.86.) Cook did not pick up the challenge.

C17 Cook couldn't deny his meetings with Loose, since the *NYTimes* of 1909/12/9 had page-one-published a facsimile of his 1909/11/4 handwritten note to Loose, describing his needs: "Svarteveg [*sic*], start March 17-18. Strong wind — Haze. March 30 — obs. Lat & Long. daily observations to April 23." Though the incident is highly suspicious[57] — no matter what the precise details — Cook tried to pass it off as just a misunderstanding (Cook 1911 pp.537-538):

> he [Loose] made the audacious suggestion that I let him go over my material. I flatly refused.
>
> He pointed out, what I myself had been thinking about, that all observations were subject to extreme inaccuracy. He suggested his working mine out backward to verify them. As I regarded him as an experienced navigator, I thought this of interest. I was not a navigator [!!], and, moreover, had had no chance of checking my figures. So, desiring an independent view, and thinking that another man's method might satisfy any doubts, I told him to go ahead, using the figures published in my story in the New York *Herald*.

C18 Comments: [i] If one isn't a navigator (which Cook admits [§C17]), then one can't find the N.Pole, especially when traveling over moving ice-floes. [ii] Working "backward" (from geographical position to data, not vice-versa as for real navigation) equals faking data. (Mathematically, faking celestial altitudes is easier than using real data to find position. See Rawlins 1973 p.154, which also adds the little-known item that: the easiest places on Earth to fake such data for are the N.Pole & S.Pole.) [iii] What about observations after April 23? Cook, not returning on his outward sledge-track, would have had to navigate back to land, just as he allegedly navigated to the Pole. [iv] When Cook claims he asked Loose to work "backward" (fake data) from his newspaper account, he reveals more than he knows. The hitherto-unnoted blunder here: the detailed *Herald* accounts[58] provide *no times for the longitude observations*, so there is no basis for computing anything (forwards or backwards) — and, needless to add, one must have longitudes [and Cook asked Loose to do longitudes: §C17] in order to steer. (See Rawlins 1973 p.87; also, Ted Heckathorn's & Wm.Rawlins' recovery of the longlost longitude observations of Roald Amundsen's legendary 1911 trip to the South Pole: D.Rawlins 1992, *Wash Post* 1993/6/1, and *Science* 1993/6/11.)
[***Bottom line: Cook never in his life celestially mapped a single geographical feature.***]

D Journey Ends. Controversy Doesn't. [Until . . . .]

D1 After disappearing in early 1908, Cook reappeared in Greenland over a year later. To explain his nonproduction of celestial observations, he usually said he'd preserved at least some (but see fn 60) of his original records in a box which, as Pearyite Prof.Hobbs temperately puts it,[59] Cook had left "in the keeping of a wandering sportsman [H.Whitney] in Greenland" — whom Cook expected (Rawlins 1973 p.166) back in civilization no earlier than mid-October (about the end of Cook's highly lucrative whirlwind lecture series). When Whitney instead returned at virtually the moment of Cook's first US lecture (Carnegie Hall) without any knowledge of such records [*idem*; Freeman 1961 p.171; questioned by *C&P* p.910], Cook (Rawlins *loc cit*) "thereupon said that he wasn't sure he'd told Whitney the papers were in the box; besides, he reassured believers, he'd kept copies with him all along." (For the evolution[60] of Cook's story regarding what data he allegedly left with Whitney, see Rawlins 1973 p.87 and corresponding citations in notes at p.298.)

D2 As Peary rightly noted,[61] such records add but a "featherweight" to one's burden. Is it not slightly nervy to come out of the Arctic, claiming one of the greatest exploring triumphs in history (and asking thousands of dollars for the story: Freeman 1961 p.137) — while simultaneously treating the crucial supporting data-records as of little import? (See Cook 1911 pp.244-245n & Rawlins 1973 p.83.)

[54] As first uncovered in 1935 by grammarian C. Henshaw Ward, Peary also got clandestine navigational advice from an expert, Hudson B. Hastings (Bowdoin College), who secretly stayed at Peary's home during the crucial weeks before the belated 1909/11/1 presentation of Peary's N.Pole "data" to his friendly National Geographic judges. Full details at Rawlins 1973 pp.285-290.

[55] Cook 1911 p.502, Freeman 1961 p.206, & Eames 1973 p.235 all understandably have some fun with this point.

[56] Three more navigational peculiarities: [a] Cook mimics Peary's habit of shooting the Sun only at quarter-day intervals — which renders faking the data a matter of mere arithmetic. (Rawlins 1970 p.35; or Rawlins 1973 p.154.) [b] Why deal with solar semidiameter if using observation-pairs? (Actually, Cook 1911 p.289 says he took "Several" — not two — observations on arrival at the Pole.) After all, if one merely pairs upper & lower limbs, as Cook states he usually did, then the semidiameter will virtually cancel out of the math; though, see fn 43. [c] If, as claimed at Cook 1911 p.302, all the "Pole" observations were doubled, then how did the single-limb observation of Cook 1911 p.292 agree to $2''$ with the mean cited at Cook 1911 p.302?

[57] Hunt 1981 p.116 cuts past details to the main point (which also applies to Peary's needlessly long-secret dealings with Hastings, fn 54): "Cook knew where to go for reputable verification of his data if that was really all he wished to have done. Among his acquaintances at the Explorers Club and Arctic Club, there were several men whom he might have contacted, including Captain Lewis Nixon. He could have asked the help of respectable academic or government scientists skilled in navigation; instead, he allowed the approach of two dishonest men."

[58] *NYH* 1909/9/2, Cook 1909.

[59] W.Hobbs *Peary* 1936 p.372, quoted Rawlins 1973 p.85.

[60] In one early version of Cook's gelatinous account of the disposition of his records, he told Danish astronomer E.Strömgren that he had left all of his original "data" & diaries with Whitney. (See P.Gibbs *NYT* 1909/9/7:5:1-2; Freeman 1961 p.150.)

[61] Freeman 1961 p.171. Confirmed (evidently inadvertently) at Cook 1911 p.499.

D3 Cook claims[62] Peary caused the burial of some of his data, but this alibi avoids the key issue: why did Cook ever let such data out of his possession? (Peary noted that he himself never did; explorer G.deLong froze to death in north Siberia with his records in his hands.) Had Cook done so, there would be no Cook Controversy. Thus, the responsibility for his inability to prove his claim is his own. We must not forget that, in science, the burden of proof is on the claimant, not the skeptic. That is why, though it's certain that the Controversy will never die [though, see ‡4 §K1], there is another, equally-solid Never: the Cook claim will not & cannot be accepted in scientific circles.

D4 Applying normal philosophy of science to the options here (innocence or guilt), we ask the Occam's-Razor question: which theory is simpler? The classic astronomical-history comparison (for the planets' motion) is geocentrism vs. heliocentrism: the former requires a complex, neatly-rigged set of epicycles. (See Rawlins 1987 p.238. Or see *DIO 1.1* ‡7 "Figleaf Salad".) By contrast, the latter is spare & simple. In the Cook case, we have on the one side the believers' theory: [a] Cook innocently left his precious original records in the Arctic. [b] Peary hid, destroyed, or stole them.[63] [c] All Cook's companions (1906 & 1908) were intimidated, misquoted, or bribed[64] by Peary money to testify that Cook never went anywhere near the top of Mt.McKinley or the N.Pole. [d] Peary forces bribed Loose to lie. Etc, etc. [A jungle of] epicycles.

D5 But there is a much simpler theory which easily explains an otherwise ultracomplex[65] saga. This elementary theory is that: Cook was a liar — even though Peary said he was.

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[62] Cook 1911 pp.244-245n, pp.499-500.

[63] See §D1, Freeman 1961 pp.168, 170-173.

[64] See §B8, Cook 1911 p.528, Eames 1973 p.67. A difficulty with this theory is that Barrill (& Parker) had, years before Cook's fight with Peary, claimed that Cook never climbed McKinley. See, e.g., Expl.Club minutes 1909/10/15 p.16 (also pp.1-2), Freeman 1961 pp.177-178, Rawlins 1973 p.89, Hunt 1981 p.30.

[65] See above at §§B8&D4.

‡4 Finding the Smoking Gun

The Long-Overlooked Central Document Recovered

by

Robert M. Bryce

A Three intriguing books

A1 Almost no story stands in isolation. This story begins with another, that of the eccentric Arctic explorer, Charles Francis Hall.

A2 In 1971, I was in the U.S. Army and home for Christmas at my parents' house in Mt.Dora, Florida. My father had long been a reader of books about exploration and mountain climbing. Perhaps that's why my sister gave him a book that she had found on the remainder rack in a Chicago bookstore. It was the first biography of Hall, *Weird and Tragic Shores*, by Chauncy Loomis (NY, Knopf, 1971). It was the title that got me to pick it up, and the back of the dust jacket that showed Hall's disinterred body, its ghostly face staring from under a folded-back American flag with which he had been covered before his coffin was placed into the Greenland permafrost in 1871, nearly a century before, that got me to look inside. Though my father had vicariously accompanied most of the famous expeditions up Annapurna or Everest and suffered through many harrowing and failed attempts in the Arctic or Antarctic to reach their poles, I had never read any book on these topics. After reading Loomis's book, that all changed. The mysterious demise of Hall at the farthest place ever reached by a ship up to then on the desolate, barren coast of northwest Greenland left me strangely disturbed and set me to reading other stories of obsession and hardship on the icy fringes of the earth.

A3 I soon found that Hall's bizarre end, which may have come at the hands of his own expedition doctor, was just one of the gruesome and fascinating stories woven around the mad quest to reach the North Pole, a place that is less a place than a mathematical conception. Soon I was frozen in with Elisha Kent Kane at the top of Smith Sound; I watched as most of Greely's men slowly starved to death, one by one, at Cape Sabine, or I was off on one of the innumerable expeditions seeking the solution to the riddle of the fate of Sir John Franklin, who disappeared into the Arctic in the 1840s.

A4 Before long I discovered the smoldering controversy over who had reached the North Pole first. Like so many others, I had simply accepted as a matter of fact that Admiral Peary had, on April 6, 1909. I had never even heard the name of Frederick Albert Cook. When I read about his claim of having attained the North Pole on April 21, 1908, I was curious as to why his story had been initially accepted and then discredited three months later. I quickly found that there were some who still believed that he had told the truth and some who believed Peary was the liar.

A5 The only way, I reasoned, that I could resolve the conflicting opinions was to go to the original sources. I wanted to let the explorers speak without an intermediary. I wanted to put myself in the position of a person in 1909, unconfused by later apologists and advocates, and be convinced by the evidence one way or the other.

A6 I was soon immersed in a copy of the first edition of *My Attainment of the Pole*, which was loaned to me via interlibrary loan by the Peabody Institute in Baltimore. It was little more than a pile of loose pages, really, with the boards joined to it only when the rubber bands that encircled the tattered mass were in place. Because of the condition of the book, the Peabody specified that I could not take it home, but that it had to be read in the library. Each day for two weeks after work, instead of going home, I detoured to the storefront that then served as the public library in Damascus, Maryland, to read it and soon found myself taking notes from it. The voice of Frederick Albert Cook spoke to me from those crumbling pages, and 35 years after that voice had been stilled, it asked me to believe that it spoke the truth.

A7 When I first read that amazing book, I was unprepared to understand the discussion it contained of incidents once familiar to every reader of the newspapers, but the narrative of the polar journey itself I found irresistible. It was certainly an exciting adventure tale — but was there any truth in it? Still, page by page, it wove its spell: "I felt the glory which the prophet feels in his vision," the voice whispered, "with which the poet thrills in his dream" — and I, too, felt it.

A8 It was not the last time I would read that book. I obtained a personal copy at an old bookstore I used to visit when I lived near the ancient mill town of Ellicott City. Mr.Deeds, who owned the shop, tracked down a copy of the common third edition on the used book market. I paid him $10 for it, and $7 for a copy of Cook's last book, *Return from the Pole*, published posthumously in 1951, containing Cook's philosophical account, written in the 1930s, of the winter he spent with his two Inuit companions on North Devon Island in 1908-09 before he returned the following spring to his winter base in Greenland. I reread *My Attainment* several times over the next few years. I became steeped in Cook's version of events, which essentially argued that he had been cheated of his true accomplishments by a monied conspiracy of the rich and influential backers of Robert E. Peary. In some ways I found *Return* even more compelling, and the excellent bibliography appended to it was a great aid in future explorations into the literature of "The Polar Controversy" between Cook and Peary over who was first to the Pole, helping to answer some of my initial questions about why Cook's story had been ultimately rejected.

B Accumulating Knowledge

B1 By then I had entered graduate school in the College of Library and Information Science at the University of Maryland, with a goal of becoming an academic librarian. Maryland had a very good collection of original narratives of the famous polar expeditions in its stacks, including *Through the First Antarctic Night*, which, once read, only added to Cook's mystique. For the required computer project for one of the courses, I spent long hours punching hundreds of cards that when fed into a computer the size of a wardrobe, would print out a bibliography of all of the early expeditions' original narratives. My 18 months as a graduate student also provided me with the techniques of information retrieval necessary to ferret out scores of articles related to Cook and Peary among the deep periodical holdings in the stacks of McKeldin Library not listed in *Return from the Pole*'s excellent bibliography, and eventually to find and evaluate manuscript materials of primary importance. It was also during this time that I read Andrew Freeman's 1961 *The Case for Dr.Cook*, the first attempt at a comprehensive Cook biography. The book was effective in establishing "the case" for belief in Cook's rejected polar claim because it was based on interviews with Cook himself, and because it avoided an obvious and all-out bias for Cook. Instead, it left the door open for the reader to make his own inferences. Its sins, I learned only later, were mostly those of omission.

B2 I had an excellent memory for details, so by the late '70's I could recite, chapter and verse, many items, some of them compelling, that had led some to believe that Dr.Cook might well have been cheated of his deserved place in history, just as he said he had been in *My Attainment of the Pole*. In proof of this, one evening I famously kept an old friend who visited our house in Damascus, and a perfect stranger who came with him to see the sights of Washington, D.C., entertained late into the night with an extensive recitation of the "facts" and contradictions of the controversy. As time went on, other people learned it was a dangerous subject to bring up with me, and it even became a part of my family's lore.

B3 During the 1980s, on a trip through Pennsylvania, the family went out of its way to visit the statue of Peary at his birthplace in Cresson, and I left signs reading "Dr.Cook Lives!" on the shed that held the groundskeeping equipment for the park that surrounds it. On another occasion I taped a small sign, drawing attention to the claims of his rival, upon the runner of Peary's actual 1909 sled in Explorers Hall at the headquarters of the National Geographic Society on 17th Street in Washington, D.C.; I wondered many times thereafter how many people might have actually read the sign before it was removed.

B4 My first job as a librarian was not in academia, as I hoped, but for an environmental research company in Towson, Md. One of my duties was to visit Johns Hopkins University's Eisenhower Library and gather articles for the PhDs of the company in their specialties in marine science. In those pre-internet days, that meant searching various databases to which Johns Hopkins gave free access, and then locating the articles referenced in the print literature stored in its stacks. These in turn had to be shuttled to the copy room, where they were photocopied by staff there. While waiting for my orders to be filled, I occupied my time reading many contemporary articles about the Polar Controversy in the original periodicals, many of which I made copies of for future reference. I also discovered there the library's copy of Captain Thomas Hall's landmark book, *Has the North Pole been Discovered?* (Boston, Badger, 1917), which I read with great interest and made numerous photocopies of its pages myself. I thus began to amass a file of original sources, though I never seriously considered writing anything on the subject myself. What else, after Captain Hall, could be said, I reasoned? And even he despaired of ever "unfolding" the truth. Not that there had been a dearth of others who had tried. At the rate of about one a decade, a major book had taken up the dispute, with varying effectiveness.

B5 Eventually I read all of those secondary books that had appeared on the subject since Captain Hall's, or which touched on it in some degree, but I was still unconvinced of just what the truth was. Although I wanted Dr.Cook to win, I realized that there were very good reasons to doubt his story. After all, the burden of proof in such cases ultimately rests on the explorer himself, and, apparently, he had never provided the evidence such a burden requires. Over most of the 1980's, however, The Polar Controversy remained little more than an intellectual hobby, still there, but now in the background of workaday life and a growing family.

C The Polar Controversy reawakened

C1 All that changed in 1988, when I groggily awoke one morning to a story on National Public Radio that soon made me sit right up in bed. Conclusive evidence had been found, it said, that the famous claim of Robert E. Peary to have reached the North Pole in 1909 was a fake. Although the story was based on fresh evidence, the chain of events that led to it began in 1962, when the Peary family granted John Edward Weems access to Peary's enormous collection of personal papers.

C2 In 1960 Weems had published an account of the Polar Controversy favorable to Peary entitled *Race for the Pole* (NY, Henry Holt, 1960). That, and the renewed interest in Dr.Cook engendered by Andrew Freeman's biography of him the next year, led to Weems's access for a new biography of Peary, based on his extensive personal papers, long stored at his home on Eagle Island, near South Harpswell, ME. The result, *Peary, the Explorer and the Man* (Boston, Houghton Mifflin, 1967), though sanctioned by them, was not entirely unobjectionable to Peary's two children, because while it tended to confirm Peary's polar claims, it revealed more than they wanted about his unpleasant personality. After Weems finished his research, the Pearys donated the papers to the National Archives, but they were closed to all others under the deed of gift until the last of Peary's children had died.

C3 That changed in 1984 after the broadcast of a made for television film, *Cook and Peary: the Race for the Pole*, aired on CBS on December 13, 1983. With the influence of the

Frederick A. Cook Society, a small group of family members and boosters of the discredited explorer, the script proved to be biased in Cook's favor, and just as he had argued, made him seem to have been the true discoverer, cheated of his glory by a conspiracy funded by Peary's powerful backers, including the National Geographic Society, which called the film a "blatant distortion of the historical record, vilifying an honest hero and exonerating a man whose life was characterized by grand frauds."

C4 The National Geographic Society then prevailed upon the Peary family to drop the restrictions on Peary's papers to allow an investigation of his claim, presumably to vindicate "an honest hero." The society commissioned for this task Wally Herbert, a British polar explorer who, among other things, had, in 1969, been the first to reach the North Pole by dogsled since Peary claimed the same feat for himself in 1909. Although favorably disposed to Peary, Herbert, after examining Peary's papers, reluctantly came to the conclusion that Peary's navigation toward the Pole had been faulty, and he had missed it by some 30 miles.

C5 These findings were put into print in the September 1988 issue of *National Geographic* only when the society learned that Herbert was writing a new biography of Peary that would incorporate them into it. At this point Dennis Rawlins reentered the fray.

C6 Rawlins, a physics professor and astronomer, had published his own book on the controversy, *Peary at the North Pole; Fact or Fiction?* (Washington, Luce, 1973). Fiction had been Rawlins's unequivocal conclusion.[1] And he had judged the National Geographic Society as at least carelessly complicit. Now, he said, he had examined a long-secret Peary document, which he believed was proof that Peary had not only not been at the North Pole, but knew he had not. The document was in an envelope bearing an inscription in Josephine Peary's hand saying it contained Peary's original April, 1909, North Pole observations. It had been deemed so important that it had been kept in a safe deposit box along with Peary's original North Pole diary, separate from the bulk of his papers.

C7 Rawlins took this label at its word and, working out the figures, concluded that instead of being at the Pole, Peary had been about 100 miles short of it. Rawlins's discovery got wide press coverage and resulted in the NPR story that had awoken me that morning. Meanwhile, NGS had been quietly trying to find someone who would refute Herbert's findings, choosing Admiral Thomas Davies. Now Davies was asked to look at the observations Rawlins had discovered, and he announced that the paper on which Rawlins based his "proof" was actually not Peary's North Pole observations, but what Davies believed[2] was a comparatively insignificant set of data known as a time-sight, from 1906. On reexamination, taken in isolation from the 1909 April inscription on the envelope that contained them, Rawlins swiftly admitted (*Washington Post*; www.dioi.org/vols/w11.pdf, *DIO 1.1* ‡4 §A2), that Davies was correct in claiming that the document was not from 1909 April, but called for National Geographic to admit its own error in 1909 in hastily approving Peary's polar claim without adequate examination of his evidence for it.[3]

C8 Instead, the society turned to Davies's Navigation Foundation to investigate Peary's papers yet again to settle the matter in an unbiased, independent report grounded in all the navigational information they contained. Though Rawlins's analysis eventually proved incorrect, that early morning's report also had reawakened my own interest in the Polar Controversy. When I read an article in the *Washington Post* in which Davies and *National Geographic* Editor Joe Judge sounded distinctly biased in regard to the "independent" investigation it had commissioned, I wrote to Judge questioning the society's impartiality. In return I got the standard National Geographic line: Peary was the true discoverer of the North Pole, and Cook nothing but a transparent conman. Judge dismissed many of my points as "intemperate, illogical, and ill informed" and was more than a little outraged at my skepticism of Davies's impartiality. "Admiral Davies is a man of veracity and character, and I find your allegations that he is biassed intemperate. If the reasoned study of the data were placed in hands like yours, it would be an Inquisition, so convinced are you that Peary was a fraud and liar." [Letter Joe Judge to the author May 30, 1989.] To this I sent a more lengthy reply pointing out why some of his statements about mine were incorrect and gave the sources of my information, and to Judge's credit I received a much less emotional reply, but no change of stance. "Whatever the Navigation Foundation concludes, we will stand by it, as we were happy to publish Herbert's conclusions. But I truly sense that you will not." [Letter Joe Judge to the author July 30, 1989.]

C9 I also made contact with the Frederick A. Cook Society and asked it if there was any collection of Cook's papers comparable to Peary's at the National Archives. FACS's people referred me to Janet Vetter, Cook's granddaughter, who lived in Florida and owned the explorer's papers and other information gathered by her then-deceased mother, Helene Vetter, Dr.Cook's only natural daughter. I wrote a letter to Janet Vetter and inquired if she ever allowed scholars to view her grandfather's hoard. In a noncommittal reply she said "Yes — I do make Dr.Cook's diaries available to 'bona fide researchers.' As they are kept in a safe deposit box, they can only be seen during banking hours." [Letter: Ms.Vetter to the author, August 2, 1989.] But before I could arrange anything more concrete, Ms.Vetter died suddenly on August 10, at the age of 51. Under the terms of her will, her grandfather's papers were to go to the Library of Congress, just 40 miles from my home. When I read of the gift, I wasted no time in contacting the Library of Congress and was told that Vetter's papers would be arriving sometime in early 1990. A further inquiry revealed, however, that there were no plans to catalog the papers in the near future, but that some of the most important ones, including Cook's original field diaries had already arrived and could be seen by appointment. I was soon in the office of Dr.William Hutson, Chief of the Manuscript Division.

D An intuition

D1 In my initial interview with him, he said that the diaries had been looked at by only one person before me, Dr.Bradford Washburn, the Director Emeritus of the Boston Museum of Science, who was an expert on and thrice conqueror of Mt.McKinley and had long been the most prominent foe of the controversial claim of Dr.Cook to have been the first to climb it in 1906. Many Cook doubters held this claim up as a virtual rehearsal for his fraudulent polar claim announced three years later. Dr.Hutson said Washburn had looked at all the diaries — being most interested in the one Cook kept on his 1906 McKinley attempt — but hadn't spent much time with them. Finding Cook's writing to be nearly indecipherable, he had quickly given up. I was most interested in the diaries Cook kept on his 1908 polar attempt, and these were served up to me when the interview ended. In the first twenty minutes of looking at them I made a significant discovery. My multiple readings of *My Attainment of the Pole* allowed me to recognize that important details in the diaries did not match Cook's published narrative. After that brief encounter, I felt intuitively that Cook had not actually done as he said. I remember returning to my office where the wall bore a portrait of Cook dressed in his polar furs, and feeling I was looking into the clear blue eyes of one of the most self-assured fakers in history. That intuition at first put aside any

[1] Primarily from navigational & magnetic analyses, inside witnesses Henshaw Ward & Hudson Hastings, plus cairn records (& etc.), showing Peary's 1906 Crocker Land was a knowing fraud (‡2 §I5), and Henson's long-neglected testimony on activities at the final camp. (The book was regarded as convincing to most reviewers, including *Annals of the Association of American Geographers 65*:79-82, 1975 March.)

[2] Rawlins later showed (and since-deceased Davies' son Doug privately agreed: 1991/4/19) that the data were really 1894/12/10 transit-observations of stars Betelgeux & Vega, seen from Peary's Anniversary Lodge, Greenland, 77°40′N, 68°35′W, just after 1894/12/10 midnight. But Rawlins stressed that Davies' numerous errors on the document (www.dioi.org/sict.pdf, 1989/8/23; ‡5, 1990/3/30) in no way excused his own.

[3] For the ultimate fate of the NGS-Navigation-Foundation's attempted witwash of public perception of Peary, see ‡5 below. (For the NF's research-reliability, see §G below.) Recognizing NGS-NF's failure: *Scientific American* March & June, 1990, *Nature* 1990/4/26, *Science* 1991/6/11.

thoughts of future research, much less writing on the subject. I felt I had satisfied my own curiosity, which had been at the base of my interest in the matter; Cook's story very probably wasn't true. After all, Captain Hall had said in the 1920 supplement to his analytic book as to material conflicts in an explorer's narrative, "Did all these various writings agree with themselves . . . it would not prove their statements to be true, because they might, nevertheless, be fabrications; but as they contradict each other in every particular, it proves falsehood absolutely. If one is true, the other speaks falsehood. If the other is true, the one speaks falsehood. There is no authority for believing either; and if the author cannot be believed in what he sets out to prove, the author is not entitled to be believed in anything he may say at any time. Truth is a uniform thing." But that didn't put an end to it. Soon a larger question came to dominate my thoughts on the matter that was even more compelling than the simple question of who had first stood at the North Pole. It was no less than the fundamental nature of "Truth" itself; could it ever be ascertained absolutely? And why was the argument over Cook's and Peary's claims so important to so many people? Why had it been so important to me? I wondered, if Cook's story was nothing but a lie, *why* he had lied, and how he could justify those lies to all of those who had placed their trust in him or wanted him to win, as I had.

D2 I called Dr.Hutson and told him I thought that the diaries were extremely important, but not of my intuitions, and that I wanted to have a look at the entire gift. I therefore urged him to have the papers cataloged as soon as possible. Whether this conversation was the motivation or not, the Cook papers were earmarked for early organization, and I was able to start my examination of them during the summer of 1990, served to me as they were processed, still in the transfer files. Not far into this process, I began a parallel examination of the vast Peary gift, housed just seven blocks away from the Library of Congress at the National Archives. For a person as steeped in the published Cook-Peary literature as I was, I quickly realized that despite all the previous articles and books already written on the Polar Controversy, there was much significant that had never been known about the dispute between the two explorers. I was certain that I could make an original contribution to the subject through a systematic and careful examination of these original materials. I decided then and there to write a book evaluating their content and how they related to the historical controversy and the larger question of it as an example of historical truth.

E Writing the resolution

I purchased a Compaq 286 computer to use as a word processor and set to work. The result was published seven years later as *Cook & Peary, the Polar Controversy, Resolved* (Mechanicsburg PA, Stackpole, 1997). It was the fruit of three years of intensive research into not only the papers housed in Washington, to which I commuted three times a week for nearly six months, but just about every accessible collection of primary documentation on the subject, including a detailed reading of much of the massive printed literature, primary and secondary, personal interviews with living connections to the story, hundreds of letters of inquiry, thousands of miles of travel and eventually seven years of writing and revision. All this was documented by more than 2,400 source notes. By 1993 the manuscript, which filled an entire box of continuous-feed computer paper, was in reasonably good shape, and I set off on another three-year quest to find a publisher for it. Many publishers were enthusiastic after they read my cover letter; they were less interested when they weighed my manuscript. Eventually, I sent only the first three chapters to Stackpole Books after getting a positive response to my proposal. There I found an editor on the same page as I; she asked for three more chapters and by the time she had read them, she was hooked. A contract was signed. Even as I prepared my manuscript for actual publication, new things came to light, new leads developed and new revisions were made as a result, some even after the galley proofs were printed.

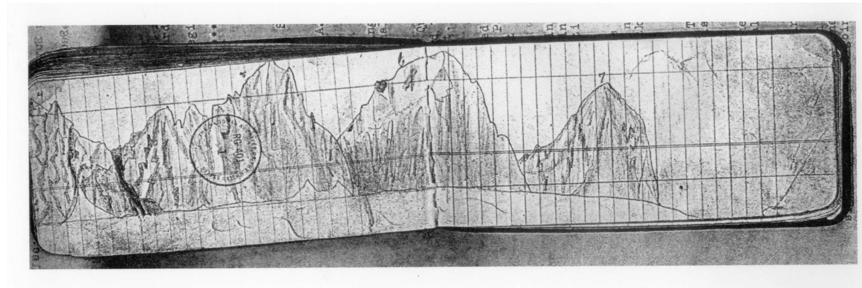

Figure 1: Barrill Diary drawing of the last peaks along Ruth Glacier leading rightward to the Gateway to Ruth Amphitheater, which he & Cook were 1st to see. The rightmost peak of these Cook named "Mt.Barrille." Mt.McKinley's twin summits are beyond at upper right.

F The Big Three

Before I began to write, or knew the content of the sources I would read, based on my sound basic knowledge of the conflicting details of the Polar Controversy, I had made a fantasy list of the most important documents I would like to find. Among many important but lesser items, the top three on that list were the diary kept by Edward Barrill, Cook's only witness to his Mt.McKinley attempt in 1906; an original negative of the photograph that showed Barrill standing on what Cook said was the summit of Mt.McKinley; and the original field notebook which Cook kept on his journey toward the North Pole in 1908. I had already examined all of the notebooks among Cook's papers at the Library of Congress, but none of them appeared to be the original field notebook kept on his journey from his 1907-1908 winter quarters toward his goal of the North Pole. I really never expected to see many of the lesser items on my list, let alone the "Big Three," *as not a hint of any of them had ever been seen by all the others who had ever delved into the documentation of the Polar Controversy* [italics by DR]. There had even been a published plea made by Bradford Washburn for information regarding the whereabouts of the original Barrill diary, but that had never been answered. However, I eventually found every item on that list, and Barrill's diary was the first of the big three that I found.

G The Diary of Edward Barrill

It was among the Peary papers at the National Archives, still attached to Barrill's original affidavit declaring Cook's claim to have stood at the top of the tallest mountain in North America a fraud. Although the Navigation Foundation claimed that at least one of its researchers had looked at every paper in Peary's 235 cubic foot hoard, it had gone undetected up until my recovery of it. Its textual content was not really all that important; that had already been published in October 1909 in the *New York Globe*; even some of its pages had appeared there in facsimile, but the original contained unpublished drawings and other features [www.dioi.org/vols/w73.pdf, *DIO 7.2* ‡7 Figs.11, 21, & 23] that provided highly significant evidence that Barrill's declaration was in all of its particulars correct.

Figure 2: Cook's Probable 1907-1909 Route. Base map drawn by Alexandra Kobalenko ©J.Kobalenko, used by permission. Magnetic Pole location modern, not 1908 when near 71°N, 97°W. Despite Bryce 2013 pp.339f (his map: p.348, www.dioi.org/rmbmap.jpg), Publisher DR judges Cook stopped 10 nmi past CTH (vs 92 nmi: *C&P* p.973) & so edits map thusly. Debated at ‡10.

H Dr.Cook's summit photo

An original print of the photograph, although not the negative, turned up in the collections of the Frederick A. Cook Society in Hurleyville, NY in 1991. Although Janet Vetter, by will, had given all of her grandfather's papers to the Library of Congress, for some reason the papers had been divided, and a significant portion of them went to the society instead. On a visit to the Sullivan County Historical Society Museum, where the society was headquartered, I came across a series of original negatives of Cook's 1906 photographs, some unpublished, and a number of prints made from them, including one of his famous "summit" photograph. The uncropped and perfectly exposed print contained conclusive evidence that it was not taken on the summit of McKinley, but rather where Cook's opponents conjectured — at the so-called "Fake Peak," 19 miles from the actual summit and at a height of a little more than 5,300 feet, rather than the more than 20,000 feet of the true summit. Later, the photo I saw in 1991, and all the original negatives, disappeared from the collection and have never been seen again, as well as a tell-tale photo of Ed Barrill standing next to Cook's distinctive silk tent pitched opposite the mountain that now bears his name, exactly the spot that Barrill's affidavit said was the last camp he and Cook made before turning for home, and a place where Cook said they never camped at all. None of these unique negatives or prints were included when the Frederick A. Cook Society donated their portion of the Vetter papers to Ohio State University in 1993, and to this day their whereabouts is still unknown.

I Rediscovering a lost notebook

I1 The North Pole notebook I also discovered in 1991, but at first it escaped recognition for what it was. I made an inquiry to the University of Copenhagen concerning the original materials Dr.Cook had submitted to be considered in support of his claim to have reached the North Pole on April 21, 1908. These had been described in the press as 26 pages of foolscrap containing a narrative basically similar to that which had been previously published serially in the *New York Herald*, and a copy of portions of a notebook containing the field notes Cook had written while on his actual journey. These materials were rejected by the Konsistorium that sat to review them as containing no scientific evidence that Cook had done as he claimed. However, as far as I could determine, no one had since reviewed these same materials to verify their description or that conclusion. In reply, I got a letter from the Rigsarkivet, the Danish Royal Archives, saying that these materials did not seem to be among the holdings of that institution, where the records of the University for that period had been archived. It did say, incidentally, however, that there was a photographic copy of a notebook written by Dr.Cook in the collections of the Royal Astronomical Observatory. I was disappointed by this information, logically concluding that the notebook copy must be of one containing Cook's so-called "field notes," which I had seen the original of in the Cook papers at the Library of Congress. That notebook had a note, however, that it was "copied" during the winter Cook spent at Cape Sparbo. In fact, there were six notebooks in all at the Library of Congress recording events during Cook's expedition, but only one contained any entries made on Cook's alleged journey from his winter base at Annoatok to the North Pole, and those appeared to be for the first few days after he left Cape Thomas Hubbard.

I2 As I reviewed and edited my completed manuscript, however, I noticed references in Cook's other notebooks indicating there was one, and possibly two notebooks not among Cook's papers. In *My Attainment of the Pole* Cook wrote, "My three notebooks were full, and there remained only a small pad of prescription blanks and two miniature memorandum books." There were references in a notebook labeled "N.III" to specific pages in "N.I," and also to "N.II." It then occurred to me that the copy in Copenhagen might be of one of those. Two years after I learned of its existence, I wrote again to Copenhagen asking for a

photocopy of the first few pages and also the last few, to compare them with Cook's other notebooks at the Library of Congress. The copy of the notebook in Copenhagen proved to be "N.I," having the exact match for the references to it in "N.III." Most important, the book was titled on its first page: "From Annoatok Northward and Return." Here then was a photographic copy of the actual diary of Dr.Cook's polar journey containing its complete record, including the originals of the field notes Cook eventually published.

I3 In rejecting Cook's "polar proofs," the University of Copenhagen noted that there were no original data included in them, only copies. Perhaps in an attempt to remedy this, late in 1909 Cook forwarded one of his original polar notebooks to Europe via his wife. The book was delivered to the Konsistorium by Walter Lonsdale, Cook's private secretary, in early 1910. The Danes were not impressed by it. They said its contents not only did not alter their previous rejection of his polar claim, but also it, in fact, raised further doubts about the authenticity of the narrative it was sent in support of. No doubt, the keen-eyed Danes noticed many of the same irregularities and inconsistencies that I discovered in my own examination of it during the writing of *Cook & Peary* (§I4). The Cook affair had been an acute embarrassment to Denmark, extending even to the Royal Family (‡3 fn 2), who along with the rest of the Danish populace, had taken Cook at his word when he landed in Copenhagen on September 4, 1909 claiming to have reached the North Pole. He had been given high, even unprecedented honors, and now it appeared he had nothing of substance to prove his claim beyond his own word. "Tell it to the Danes" became a catch phrase in response to any dubious statement. Although in turning over his original notebook Cook had specified that no part of it could be copied or published, the Danes wanted no more egg on their faces. They made a complete photographic copy of the book, page by page, and stored it away quietly in the Royal Astronomical Society Library. This was the copy brought to my attention in 1991, where it had lain, apparently unnoticed, for nearly a century. The copy having been rediscovered, it was moved to the Rigsarkivet for safekeeping.

I4 I purchased a complete copy of the notebook so that I could examine it in the way I had already examined the six other notebooks kept by Cook in 1907-1909, which were deposited with the Vetter gift. But in the early 1990's the only type of copies obtainable were standard photocopies, and many of these were insufficiently clear to decipher every word the notebook contained. This was partly due to the quality of the photocopies themselves and partly due to Cook's difficult handwriting and its oft-tiny size. Enough was legible, however, to get an accurate idea of the notebook's content, page by page, and that content, along with some peculiarities of the way the entries were entered into the book, proved strong evidence that Cook's polar journey was more one of imagination than fact. A detailed analysis of why this was so was presented in *Cook & Peary*.

J Resisting resolution

J1 But few absorbed the import of this evidence, let alone that of the whole sweep of my 1,000+ page book, even those with prior knowledge of the subject or those who had the patience to study it thoroughly. It was dismissed out of hand, of course, by Cook's partisans. They had convinced themselves that I was writing a book that would vindicate Cook and were shocked that it produced convincing evidence that he had lied about the results of his McKinley attempt that fell far short of the summit and his equally failed attempt to reach the North Pole. But even some others scoffed at the subtitle, "The Polar Controversy, Resolved." They may not have been Cook partisans, but they had some stake in wanting to see the controversy continue, like proprietors of "adventure" companies that promoted ultra-expensive "Last Degree" treks to the Pole, persons with a previous self-interest in justifying their version of the controversy that they had put into print, or others who simply liked to argue over it interminably. They said there would never be an end to the controversy, simply because it would never be possible to produce actual documentary evidence that proved Cook's story a lie, the proverbial "Smoking Gun" that would end it, absolutely.

J2 In the early 1980s, when Cook's papers were still in the possession of the Vetters, William Hunt published *To Stand at the Pole* (NY, Stein & Day, 1981), a book examining the history of the controversy which contained many reasonable perspectives on Cook and his advocates' motivations. Although he concluded that Cook was a damn liar, Hunt believed that Cook's personal papers, even if they could be freely examined, would furnish nothing more conclusive than the weight of the circumstantial evidence already brought for or against him. He reasoned that if they contained any proof of Cook's claims they would have been furnished to his advocates long before, and if they contained any proof of his fraud, they would have been destroyed by Cook or his descendants long since.

J3 Even as late as a decade ago, Richard Sale, in his book, *The Arctic: the complete story* (London, Frances Lincoln, 2008), which contained an accurate summation of the highlights of the evidence for and against both Cook and Peary's claims, had this to say: "At this remove in time the truth of the two claims can no longer be ascertained. No new evidence is likely either from the North or from the diaries and logs of the two men and their expeditions. No one will ever know for certain which, if either, was telling the truth." But Hunt and Sale and the others were wrong. The truth was there all along, written by Cook himself on the pages of his long hidden expedition notebooks.

K *Cook and Peary*'s Aftermath

K1 After my book was reviewed widely and was featured in the pages of influential newspapers including the *New York Times* and *Washington Post*, reference after reference published subsequently cited *Cook & Peary* [and Rawlins 1973 (‡3 §A6) & W.Herbert 1989] in their evaluation of Cook's claim. By the time of the centennial of the outbreak of the dispute between the two explorers in 2009, it had become a virtual consensus that certainly neither reached the Pole when he said he did or ever. Even the National Geographic Society had nothing officially to say in support of Peary on the 100th Anniversary of his supposed attainment to commemorate it, something they had never failed to do on any significant occasion in the past involving Peary's alleged discovery. (There was a blogpost by David Maxwell Brun on the NGS blog, *Voices*, on April 6, 2009, but it took a neutral stance on the truth of Peary's claim.) Even a feeble rewrite of Andrew Freeman's *The Case for Doctor Cook*, that appeared in 2005 under the title *True North* (see review above: ‡1), probably with the financial aid of The Frederick A. Cook Society, did nothing to stem the trend of dismissing Cook's polar claim out of hand as it had been early on, reversing the trend of a more positive consideration of Cook's polar attempt in the light of the collapse of Peary's claim. In the wake of my publication of Cook's fake "summit" photo in *DIO* in 1997, www.dioi.org/vols/w73.pdf, ‡7 Fig.18, following my recovery of it from a badly faded copy that had indeed made it to Ohio State, and its subsequent publication in the *New York Times*, www.nytimes.com/1998/11/26/nyregion/author-says-photo-confirms-mt-mckinley-hoax-in-1908.html, 1998/11/26 p.1, Cook's McKinley claim that also still had its advocates even beyond The Frederick A. Cook Society, and which was called in mountaineering circles, "The Lie that Won't Die," finally breathed its last. Signs even went up at a viewpoint of the great mountain's southern flank in Denali State Park, Alaska, detailing the evidence explicit in Cook's original photo, which was prominently displayed showing (by dotted-lines) exactly the two revealing side-portions he'd snipped off, next to his published, doctored version of it (Fig.4).

K2 The controversy was, it seemed for all practical purposes, indeed resolved, the record corrected. But it had always been my intention to return some day to the Copenhagen copy of Cook's notebook and do a complete transcription of it. I felt that although it had already been decisive in destroying the credibility of Cook's claim, it might yield yet further information if subjected to a more detailed examination than I was able to do from the poor photocopies I had already examined. My long experience with the original documents of the controversy and my familiarity with Cook's difficult handwriting placed me in a unique position to see the connections of its content with other sources and allowed me to explain

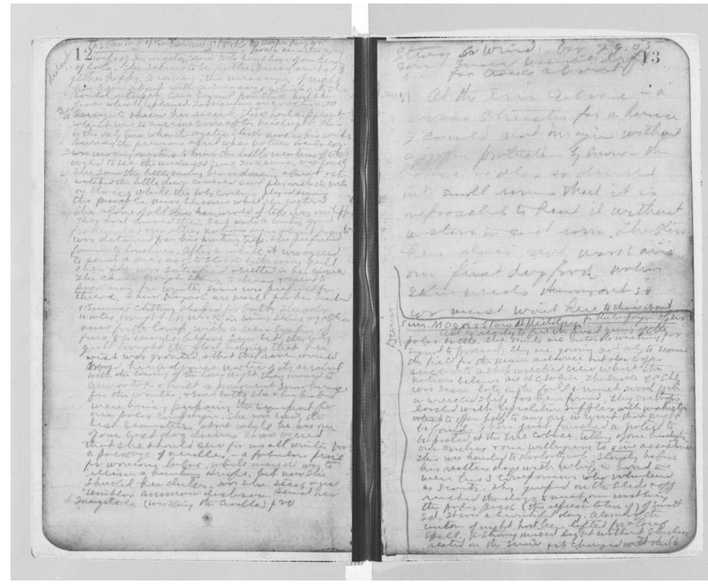

Figure 3: Typical Pages of Cook's Semilegible Scrawl in Polar Notebook.

what those connections meant. Consequently, although there was absolutely nothing to be gained from a personal financial standpoint, I felt that it was almost a scholarly obligation to do a transcription of the notebook and publish such a detailed analysis.

L Decoding the Lost Notebook

L1 I applied for a grant from the National Endowment for the Humanities to work on the project. Although my proposal was given serious consideration, one of the limited NEH grants was not forthcoming. It was deemed not to be a topic of wide enough interest to merit one. Without such a grant, I felt it prudent to wait until the copyrights expired on Frederick Cook's unpublished writings before taking up the task. Under the revisions of the Copyright Act of 1976, that would come 70 years after the death of the author, or 2010, which coincided with my retirement, leaving me with an open-ended amount of time to work on the study at my leisure, which from the first, I planned to self-publish.

L2 In preparation, I again wrote to the Rigsarkivet. Since my last inquiry, the notebook had been moved again, this time to the "Black Diamond," the newly built Royal Library in Copenhagen. After some negotiations on technical difficulties, I was eventually able to purchase a digital copy of the Danish photographic copy of Cook's notebook.

L3 Using various digital techniques to enhance the copy, I was able to make a virtually complete transcription of every word in the notebook with the exception of a very few that were either illegible or could not be deciphered because of Cook's idiosyncratic penmanship. The transcript took about a year to finish and check against the original.

L4 The analysis took longer. In preparation for this phase, I purchased digital copies of significant portions of four of Cook's polar notebooks, held at the Library of Congress, using my extensive notes on them to identify the portions I would need. These, along with Cook's several published accounts of his polar attempt were all used for comparison of the lost notebook's content. I say "lost" because the Danes' copy is the only known source for the book's content. The original has never been seen since it was returned to Cook's private secretary via a power of attorney in 1911, although there is some evidence to suggest that Cook may have still had it as late as the 1930s. This exhaustive comparison took the project into the early part of 2013.

L5 Just as I considered likely, the notebook shone new light into many of the still dark corners of the Polar Controversy. I realized in the transcription, for instance, how closely many of the pages in the after-the-fact narrative sections written on the left-hand pages, which had been too small to read in detail on the photocopies, matched the finished content of *My Attainment of the Pole*. This settled conclusively the fundamental question regarding the authorship of that book: it was almost entirely written by Frederick Cook himself; thus, the portion contributed by T. Everett Harré, Cook's editor, was as minimal as Cook said it was in his introduction to the third edition. But it was the actual daily diary entries on the right-hand pages that produced the evidence that irrefutably destroyed Cook's claim and decisively branded it a premeditated fraud.

M A New Calendar

M1 As had already been deduced in *Cook & Peary*, a pattern of deception in his reported dates, many of which were rubbed out or written over in the notebook, when compared to his other manuscript and published writings, showed that Cook's reported leaving-date of February 19 was not accurate. This was confirmed by several entries referring to phases of the moon which would not coincide with the dates Cook substituted for them. Still, I found that by analyzing what remained of the content of his actual entries (many were altered, some erased or written over, and some destroyed outright), it was possible to follow his actual progress after leaving his winter headquarters in Greenland and estimate his chronology with good, if not precise accuracy. Because Cook apparently failed to rub out two of his entries' original internal dates, they could be used as checks against his actual progress. Therefore a careful reading of the diary entry content allowed a fairly close estimate of the actual dates, and a telling remark concerning the weather at one point confirmed that the date estimated in this manner was within two days of the actual date as of March 29. Using this method, a calendar was worked out to estimate approximately where Cook was during his journey from his winter quarters in Greenland to his starting point for the Pole at the north tip of Axel Heiberg Island. This follows, including miles since the last camp.

M2 In *Cook & Peary* it had already been deduced from existing evidence that Cook had set back his date of departure from his winter quarters by seven days. This the diary confirmed, but his timetable actually lagged behind his published reports even more than that, due to delays along his route. The entries in the diary revealed that that his journey just to Cape Thomas Hubbard took nearly twice as long as he later reported. It therefore was undoubtedly clear to Cook that he had no chance to reach the Pole long before he arrived in Nansen Sound, some 120 miles from his jumping off point. This placed him at Cape Thomas Hubbard [81°22′N, 94°.1 W] about April 11, give or take three days, and indicated that he did not start across the circumpolar ice until about April 13, 1908. [Versus March 18 claimed: *MAP* pp.200&569. Full *MAP* vs Notebook comparison: Cook 2013 p.396.] He reported that he arrived at the North Pole on April 21 of that year, which all of his field reports and observations supposedly corroborated. Obviously, he could not have made the 518 nautical mile [ideal bee-line] trip from Cape Hubbard to the North Pole in a mere eight to eleven days, or even if the proposed calendar was off by as much as a week, which it surely was not because of the diary entries' internal evidence. This alone was proof of Cook's failure to have obtained his goal. But the notebook also contained evidence of premeditated deceit, and that long before he reached Nansen Sound he was already erecting the means by which to perpetrate a fraudulent polar claim.

Camp#	Date	1908 Camp Name Given by Cook (if any)	Miles
1	Feb 26	On the ice of Smith Sound about 20 mi NE of Annoatok	20
2	Feb 27-28	At Peary's caboose at Payer Harbour on Pim Island	30
3	Feb 29	6 miles above Cape Rutherford on Buchanan Bay	18
4	Mar 1	Cape Koldewey near the Weyprecht Islands	20
5	Mar 2	On a small island off Cape Koldewey	6
6	Mar 3	In Flagler Bay off the coast of Knud Peninsula	18
7	Mar 4-6	A mile from the head of Flagler Bay	25
8	Mar 7-8	"Divide Camp" 20 mi into Sverdrup Pass	21
9	Mar 9-10	"Musk Ox Camp" 25 mi into Sverdrup Pass	5
10	Mar 11-12	"Storm Camp" 45 mi into Sverdrup Pass	20
11	Mar 13-17	"Glacier Camp" on east side of glacier blocking the valley	7
12	Mar 18	On the west side of the same glacier above Bay Fjord	19
13	Mar 19-22	In Irene Bay, 3 mi into Bay Fjord	23
14	Mar 23	"Bear Camp" 30 mi into Bay Fjord on the south shore	27
15	Mar 24	Near the junction of Bay Fjord and Eureka Sound	25
16	Mar 25-26	Near the north cape of Vesle Fjord	15
17	Mar 27	Northeast of Depot Point	35
18	Mar 28	Near the northeast cape of Slidre Fjord	38
19	Mar 29	Near the junction of Eureka Sound and Greely Fjord	21
20	Mar 30	In upper reaches of Cannon Fjord NE of Cape Lockwood	40
21	Mar 31	Near Caledonia Bay on the eastern shore of Cannon Fjord	26
22	Apr 1	Near the location of Camp 20	26
23	Apr 2-3	"Berg Point" at the mouth of Greely Fjord	35
24	Apr 4	On the west coast of Schei Peninsula	35
25	Apr 5	At the cache site at the SW corner of Schei Peninsula	20
26	Apr 6	Near the cape of Stangs Fjord	40
27	Apr 7	Off Grant Land [north Ellesmere] near Stangs Fjord	26
28	Apr 8	Near the north cape of Otto Fjord	25
29	Apr 9	Near White Point	20
30	Apr 10	At the base of the Svartevoeg Cliffs	27
31	Apr 11-12	"Cache Point" near Cape Thomas Hubbard	22
	Apr 13	Cook & 4 Inuit depart Cape Hubbard, onto the Arctic Ocean	

N Suspicious side trips

N1 This came not only in the form of changes made to dates within the daily entries that were inconsistent with the record of events therein (e.g., some entries clearly recorded events that could not possibly have happened on a single day, but are given the same date), but also the inconsistency of internal references to dates at various points within them to manipulate the time of his arrival at Cape Thomas Hubbard that would be in keeping with his eventual story of arriving at the Pole on April 21. Most revealing was evidence of actions no explorer needing to reach his jumping off place for a journey to the North Pole as soon as possible would have taken. Chief among these was a planned detour far out of his way to lay caches in Cannon Fjord [Cañon Fjord on Fig.2]. This was necessary to allow Cook and his "polar party" to return by a different route than he had taken outward-bound and thus avoid contact [‡3 fn 34] with any members of his supporting party who would travel back along the same route by which they came. This was a completely new revelation provided by the notebook that proved Cook had already given up any thought of a serious attempt to actually reach the North Pole in 1908, long before he reached Cape Thomas Hubbard.

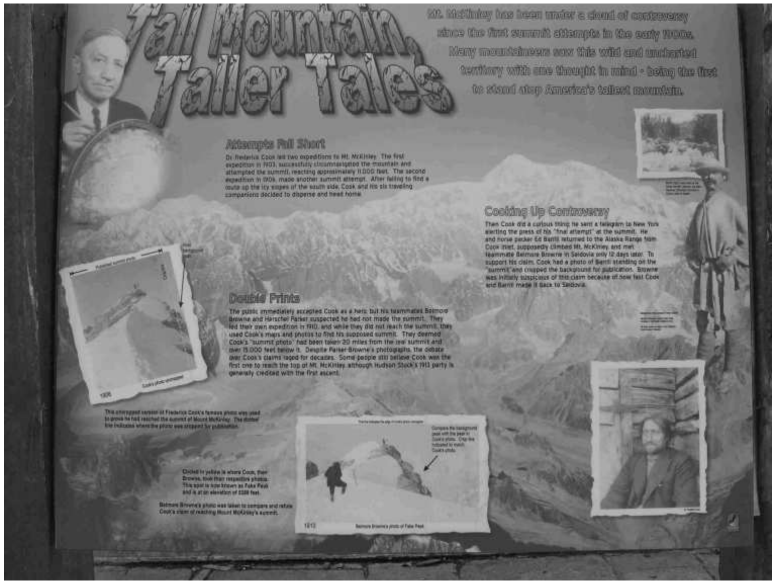

Figure 4: Cook Sign at Denali State Park, detailing his tall tales.

N2 Cook had closely read Otto Sverdrup's account of his crossing of Ellesmere Island in 1899 in his book, *New Land* (London, Heinemann, 1904). He noted Sverdrup's difficulties with poor snow conditions and an intervening glacier that blocked his route. He hoped to avoid these by gaining Ellesmere Island's icecap from Flagler Bay (south of Bache Peninsula) and descending into Cannon Fjord as the route to Nansen Sound rather than Sverdrup's more southern route via Bay Fjord [just west of Sverdrup Pass]. To this end Cook had sent a party of Inuit out before he left his winter quarters with instructions to scout out a suitable route onto the icecap from the head of Flagler Bay. The Inuit were unable to find such a route, however, and Cook was forced to follow Sverdrup's old one. Because of this he was delayed by a general lack of snow cover and was forced to spend five days clearing a way across the same glacier that had blocked Sverdrup in 1899. The day he finally overcame this obstacle was probably March 18, the day he said he set out on his journey across the Arctic Ocean. Even so, up to then, Cook had been delayed only by circumstances beyond his control. That would soon change.

N3 As Cook explains in the narrative account written in the lost notebook, but not in *My Attainment of the Pole*, he laid a cache at the head of Greely Fjord [eastward extension of Nansen Sound] with the intention of returning to Greenland by crossing the Ellesmere icecap via Cannon Fjord and descending on the head of Flagler Bay, the reverse of the route he had hoped to take outgoing. This route over Arthur Land [east of Cannon Fjord] would avoid a return via the already snowless Sverdrup Pass over which he had come, but even later in the season. Such a return route would also ensure that he did not run into his support party, whom he had instructed to return along the outward route via Flat Sound. Separating himself from any other witnesses was important to a hoax because he needed to keep out of sight long enough to simulate a lengthy trip to the Pole and back.

N4 Oddly, Cook's genuine discovery that Sverdrup was mistaken when he announced the existence of Schei Island also weighs against his intention of making an actual polar attempt. Cook's discovery that Schei was actually a peninsula (west side Eureka Sound's mouth: Fig.2) did not come from the passage through what Sverdrup called "Flat Sound" [southeast off Schei Pen.] below the supposed island, as Cook later claimed to have done in *My Attainment of the Pole*. Actually, evidence in the lost notebook shows that Cook nearly circumnavigated the supposed island after crossing over to it from Ellesmere Island at the peninsula's northern tip and then proceeding down its western coast to the southwest corner, where he found the "island" was attached to Axel Heiberg Island proper by a low, narrow neck of land. His reason for following this route instead of going directly up Nansen Sound to Cape Thomas Hubbard, was again to lay a cache. This time it was for the use of his supporting party, which he wished to return through Flat Sound rather than via Cannon Fjord as he intended to do. In *My Attainment of the Pole* (e.g., p.203), Cook leaves out all these suspicious side trips. (See Cook's key unmentioned side-trip in 1906: *DIO 9.3* ‡6 §H2.) There he says he crossed Eureka Sound from near Slidre Fjord, and via Flat Sound reached Nansen Sound, leaving out both the detour into Cannon Fjord and his actual route to the point of discovery that Schei was not an island, because both were not only incompatible with his published route and time schedule, but also incompatible with the motivations of an explorer bent on making an actual attempt to reach the Pole.

N5 Cook did actually depart Cape Thomas Hubbard, on about April 13, heading NW [vs *MAP* pp.285, 291, & 570], probably to acquire 1st hand experience of sea-ice travel, which he would have needed to write a convincing account of an actual journey toward the Pole; or, maybe in hopes [‡3 §C3] of avoiding navigational math by traveling over solid land, which Peary in 1907 reported he'd seen in 1906: Crocker Land, lying c.100 miles to the NW, but which, in actuality did not exist at all. He might also have hoped to convince his Inuit witnesses he'd reached the Pole, of which they had no concept so far as its geographical location. But if not the North Pole, where did Cook actually go after leaving the cape?

N6 Another notebook now at the Library of Congress seems to hold the answer. It appears to contain the actual record of his experiences on the Arctic Ocean. According to this account, he turned back about April 19 having gone approximately 114 miles to the northwest of Cape Thomas Hubbard and making perhaps 60 miles net northing. If so, he was thwarted by impossible ice conditions when still about 460 miles from the goal he later said he was the first to attain. It seems probable from circumstantial evidence that, after turning back, his subsequent route was very close to that drawn for Peary on a copy of Sverdrup's map in 1909 by Cook's two 1908 Inuit companions.

N7 In summing up Cook's claims and narrative of his attainment, Captain Hall had written in 1917: "I have not seen a copy of the papers which Dr.Cook left with the Copenhagen University. There may be something in them that would indicate, or possibly that might prove, that Cook has practiced deception. But if this were true, I think that the University would have considered it their duty to have shown, for the benefit of science and of history, wherein the deception exists. But having only the published report that the university found nothing deceptive in the papers — nothing that they could condemn, I conclude that nothing exists in those papers that indicates deception." But Captain Thomas Hall had also written "the Truth is a uniform thing." He never saw Cook's notebooks, but if he had he surely would have agreed that because they contradict his published narrative and one notebook account contradicts another, "it proves falsehood absolutely." With the 2013 publication of the author's *The Lost Polar Notebook of Dr.Frederick A. Cook*, the Polar Controversy of which Cook was the major player, for his part, is absolutely over: the "Smoking Gun" is found. The simple truth is that Frederick A. Cook did not attain the North Pole in 1908, nor was it possible for him to have done so. Whatever his original intentions, he simply ran out of time. As to those larger questions of if there is really such a thing as Truth itself, the quest for Absolute Truth is a goal even harder to attain than the Geographical North Pole was by dogsledge in 1909. And even that was impossible.

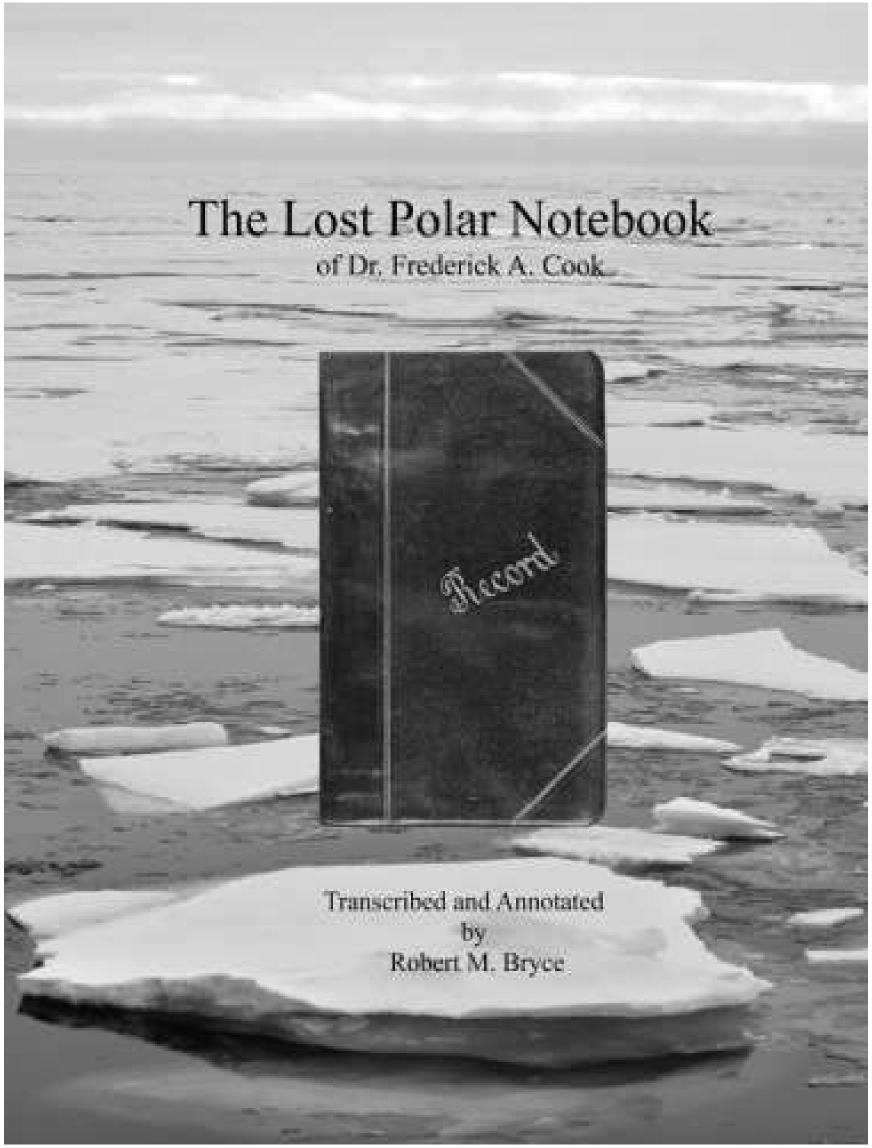

Figure 5: *The Lost Polar Notebook of Dr.Frederick A. Cook*, R.M.Bryce, 2013.

‡5 Incontinental Drift: the "Davies Movement"

Snow Job and the Seven Dwarfs

National Geographic's Unimpeachable Longitude Authority Proves Errant Brazil Kissed Africa, Under 500 Years Ago! Plus Further Navigation Founderation HyperDiscoveries Moon a Planet! Tachyonic Tectonics! Relativity a Hoax!

by

Dennis Rawlins 1990/3/30

[Circulated 1990 in naval circles. Non-trivial later edits appear in brackets.]

A How-to-Commit-Navigational-Suicide 101: That Darned L-Word

A1 The uncontrollable urge, of "Navigation Foundation" [NavFou] President Tom Davies (Rear-Admiral USN), to exonerate dubious explorers via equally dubious scholarship, is here examined in a little-known 1988 incarnation 1^y prior to his far-better-publicized [1989/12/11-press-conference-launched National Geogr Soc-funded $250,000 whitewash of NGS' fave polar explorer, R.Peary. A failed, amateurishly refereed attempt to snow scientists & public with Unimpeachable Expertise, resulting in a NavFou report which NGS still refuses (http://tierneylab.blogs.nytimes.com/2009/09/07/who-was-first-at-the-north-pole/) to disavow. [*NatGeoHistory* 2020/1 p.87 dumps 1989 NF verdict, saying NGS undid Peary: *NGM* 1988/9.] Verdict unanimously robo-endorsed by the NavFou's tractable windowdressing 7-man Board of Directors. When Davies died in 1991 Jan, none of his Board's seven would replace him at the 1991/4/19 US Naval Inst debate. See *DIO 1.1* ‡4 End-Note C, 1991, for details of the NF Peary report's ultimate collapse.] In a lecture at the Fels Planetarium (Franklin Inst, Philadelphia) on Wednesday, 1984/10/17, Adm.Davies revealed to the world the fruit of his *years*[1] of research into the Amerigo Vespucci controversy, illustrating his points with a score of slides, as well as the planetarium's reproduction[2] of the sky for the evening of the 1499/8/23 Moon-Mars observation he was using to test Vespucci. [We here focus on examining "DVD" (www.dioi.org/dvd.pdf, final to-NGS 1988/9/18 edit of Davies' Vespucci apology), referring also to the 1984/10/17 version, "DVC" (www.dioi.org/dvc.pdf, whose added markings aren't DR's).] The math of both papers is the same, with but 2 minor corrections noted below: fn 59.) In this and subsequent platform effusions and distributed material, Davies attempted to defend Vespucci against longstanding "derogatory statements" & "denigrations". (DVC 6 & DVD 13: §D7 here. And DVC 7 remarks that D.Leite "spends many pages running down Vespucci" [quoting Leite: "a fatuous person not capable of innovative thinking, amateur astronomer, navigator only average, cosmographer who repeated concepts of others, false discoverer who appropriated the glory of others" (similar to ‡3 §A1)]. Davies' all-too-evident [rather religious] distaste for skeptics here is prototypical of his current hagiographic Peary reports.)[3] Davies' astronomical-navigational computations contended that Vespucci's purported tropical observation of Mars' lunar distance on

[1] This according to a scholar at the US Naval Observatory (1989/12/2), who also stated (1989/11/30) that he was the USNO astronomer who had recommended Davies to NGS in 1988 for the Peary investigation, largely on the basis of impressions formed from occasional long conversations at him; in fact (since Davies doesn't publish in serious science journals), the only written paper of Davies that had actually crossed his desk was the one here under review. (The USNO astronomer never got around to checking its calculations.) He assumed Davies had the sort of time on his hands to dig into a problem thoroughly, [and thought] that the Vespucci paper demonstrated this virtue. **NB**: Davies analysed Vespucci much longer than he did Peary. He circulated his Vespucci paper long after its initial presentation, though his 7-man board presumably saw it. [After Davies' 1991 Jan death, none of the Unmagnificent Seven would sub for him at the 1991/4/19 US Naval Inst debate on Peary's Pole claim.]

[2] This use of the Fels Planetarium is mentioned at DVC 1&8. The current Ass't Director says (1990/2/27) that lunar parallax was not part of the planetarium's 1984 capabilities.

[3] NG 2: "Peary . . . mercilessly pilloried by a vociferous minority". (Reviewers have universally noted that Herbert's 1989 book *Noose of Laurels* is exceedingly gentle regarding Peary's sins. DR's

1499/8/23 proves the observer was at longitude c.37°.75 W. Thus, Davies says that his math vindicates Vespucci's controversial claim to have reached Brazil in 1499, not to mention Vespucci's priority in devising the historically crucial astronomical longitude-determination method known as "lunar-distances". "I believe . . . our use of modern computer methods have [*sic*] shown Vespucci to be a credible navigator & innovator . . . . this application of Archeo[*sic*]-astronomical methods is a 'first'."[4]

A2 However, when Davies' work is corrected for various extraordinary astronomical-math howlers (one of them spectacular both for its size and for what it reveals of his NGS-advertised Expertise), his calculations prove instead that Vespucci was at least 27° or 1600 [nautical] miles east of Davies' deduced Brazil position — at 11°W, the longitude of Liberia[5] (Africa). Since I made this little item public (1989/12/11), Davies has been asked by reporters[6] about the Vespucci paper, his longtime former (pre-Peary) pet "historical detective story" (DVC 10) project; but he refuses [like ‡3 §B13] to answer press questions.[7] One isn't accustomed to [seeing US Admirals departing] under fire.

1973 book *Peary at the North Pole: Fact or Fiction?* was appraised similarly, e.g., by geographer Wm.Warntz in the 1975/3 *Annals Assoc Amer Geogr 65.1*:79, "Rawlins' dismissal of the final Peary claim does not thereby mean that he does not understand Peary's overall importance and his many earlier contributions. He notes and appreciates them. He writes with compassion and awe of the physical suffering endured. He recognizes Peary's many virtues no less than his extraordinary frailties.") NGD 60: "on a personal note, we [the NF] cannot but hope that this marks the end of a long process of vilification of a courageous American explorer." As the *Wash Post* headlined it (1989/12/12), in a Nixonian echo: the NF deems Peary "Not a Fake". And, on the Vespucci observation (which even Vespucci's defender Stein calls "fictional": fn 47), Davies concludes that it probably "could not have been faked" (DVC 7; conditional satisfied at DVC 11). Davies goes so far as to justify the naming of the New World for Vespucci by stating (DVC 10): "even the erroneous Longitude of 1499 was enough to raise doubts that the new lands were off China." (See also DVC 11. Davies knows better at DVD 14, though he still believes Vespucci eventually recognized that this was a new world.) But in fact the very 1500/7/18 letter under discussion states (right in the sentence following that quoted by Davies at DVC 8, describing fresh water — which he supposes refers to the Amazon mouth): "it was my intention to see whether I could sail round a point of land, which Ptolomey [in his crude delineation of China] calls the Cape of Cattegara [*Geogr Dir* 7.3.3: the Chinese anchorage, Kattigara: Καττιγαρα ορμος Σινων] (which is near the Great Bay [*Geogr Dir* 7.2.7: Μεγαλου κολπου; see L.Renou *La Géographie de Ptolémée l'Inde (VII, 1-4)* Paris 1925 pp.62-64]). In my opinion it was not far from it, according to the degrees of latitude and longitude, which will be stated hereafter [§C4 & §D5]." (C.Lester 1855 p.153.) Ptolemy places Kattigara at latitude 8°1/2 north of the Equator & longitude 177° east of his prime meridian, the "Fortunate Isles" [which *DIO* in 2008 discovered to be the Cape Verde Islands: www.dioi.org/vols/wm0.pdf, *DIO 22* p.8]. ("Great Bay" is the Gulf of Thailand; Kattigara, now Ho Chi Minh City=Saigon [*DIO 5* fnn 64&68]. Latitude sign rightly north in Ptolemy's *Handy Tables*: Honigmann 1929 [fn 30] p.206. Wrongly south in his *Geogr Dir*, distorting maps for 1000^y, eliminating the Pacific.) Ptolemy's prime source (Marinos) made [eastern China's] longitude nearer 15^h east (*Geogr Dir* 1.11.1) or 9^h west of the [Fortunate Isles], still in poor agreement (discrepancy of nearly 4^h) with respect to Vespucci's stated longitude, $5^h 1/2$ west of Cadiz (Γαδειρα) [which Ptolemy maps] about $1^h/3$ east of the [Fortunate Isles] (*Geogr Dir* 2.4.16 & 8.4.[5]).

[4] Fn 47, DVC11; 1499/8/23 moonrise earlier computed by Pohl (1944) & (correctly) by Stein (1950).

[5] The Vespucci "observation" is here treated as having been made near the terrestrial Equator, as he reported — and as Davies' analysis assumes.

[6] *Washington Post* (1989/12/11 & thereafter) and *Washington Times* (1990/2/12).

[7] Davies will say only that the paper is in his private files and wasn't (as initially reported) delivered to the SocHistDisc on 1985/11/16. (Davies protests he was abroad at that time.) But he has repeatedly refused to answer the *Wash Post*'s queries as to where he *did* deliver the Vespucci paper. Davies' artfully selective responsiveness could impart the false impression that his Vespucci analysis was never presented in public. But the paper was given publicly (and the printed text distributed), repeatedly. (On 1990/2/27, the Ass't Director of the Fels Planetarium confirmed that Davies had indeed delivered his Vespucci paper there. DVC 11 itself refers to the 1984/10/17 presentation as a "lecture".) And this point is far less important than the following facts: [a] The paper was submitted by Davies to National Geographic for publication. [b] According to delightedly enthusiastic NGS Senior Assoc Editor Joe Judge (1989/12/11 morning), publication of Davies' Vespucci researches was being quite seriously considered as late as 1989/12/11 — right up until DR revealed the paper's epochal implications [at a press conference later] that day, at NGS. (See *Wash Post* 1989/12/12 & *Wash Times* 1990/2/22.) [c] Judge's preening 1989/12/11 expansiveness also included the information that Davies' Vespucci paper was the direct cause of Davies' happy selection (by Judge, who refused to share that supreme credit with the US Naval Observatory) as NGS' Peary investigator (§A3). (Judge & Davies both reside in fashionable Potomac, MD.) My two favorite Joe Judge statements are neatly juxtaposed by Kelly (*Wash Times* 1990/2/22): "this [Vespucci] article . . . so impressed . . . Judge that he saw to it Mr.Davies was brought in to oversee the [NGS] Peary project. . . . Mr.Judge dismisses the Vespucci [analysis' dubiousness]. 'What the hell has that to do with Peary?' he asks." (And see §A3.) The precious rareness of National Geographic's knack for entertaining the public remains unquestioned.

A3 One of the reasons DR finds all of this rather ironic is that Davies is being widely quoted (§C7) and aggressively promoted as an authority on *the determination of longitude* (which is most of the very title of his Vespucci analysis! — §B1; & see fn 7) with respect to the Peary affair, where he is being generously paid by National Geographic while saying that his awesome navigational experience senses no difficulty[8] with Peary's weirdly barren 1909 story of navigating his way to the North Pole over 413 nautical miles of irregularly drifting ice-cakes (I use nautical miles[9] here throughout unless stating otherwise) — this without any astronomical observations giving longitude or compass variation data (required for steering). The key to both problems (Peary and Vespucci) is the same awful L-word: *LONGITUDE*. We now find (grisly details below: §B) that National Geographic's latest admiral-idol, allegedly highly expert in the math & the history of longitude-determination, is innocent of how to compute the most famous of all astronomical methods for finding longitude at sea (the first exact such method, increasingly popular from the mid-18th century on — even retained in the US Navy's Bowditch *American Practical Navigator* into the early 20th century),[10] namely: the "lunar-distances" technique. Davies bills himself as an expert in the Bowditch *Navigator* — while massively inept at the method that made that work's reputation. (See historical intro, Bowditch *Navigator* 1958 pp.4, 45, 54.) From reading this Davies paper, one doubts its distinguished author ever computed a lunar-distances problem (the very subject of his paper!) — or indeed any lunar observation[11] for longitude or position, anywhere in the world, throughout his Navy career. (On 1989/12/11, NGS' Peary-project chief Joe Judge — #2 man at NGS — proudly stated in the presence of DR & the *Wash Post*'s B.Rensberger that it was the masterful precision of Davies' Vespucci study that got Judge to choose [consultant] Davies for NGS' Peary investigation! Judge [boasted of] this to *Wash Times*' Tom Kelly on 1990/2/14: fn 7.) Of course, another explanation is possible: parallax-correction is virtually negligible for navigators when the observed body's distance is that of the planets; thus, we may suppose that Davies has discovered the previously unsuspected fact

[8] See *Christian Sci Mon* 1989/3/27 (emph added): "Davies is *not bothered* by Peary's lack of longitude readings. 'There are several ways to stay on the meridian. Some are better than others,' he says, noting that he's *not yet sure* which method Peary used." I ask that genuine navigation experts absorb carefully the attitude displayed.

[9] A nautical mile is very nearly one arcminute (1/60 of a degree) of great-circle distance on the Earth. An international nautical mile (1852 meters) is c.15% larger than a statute mile.

[10] After Bowditch's 1802 creation of a rapid method of calculation, the lunar-distances method's formulas and tables were carried in the standard Bowditch *Navigator* (published since 1868 by the US Navy) until 1914. See Bowditch 1903 pp.128, 288-332 which provides the method of the US Navy's W.Chauvenet, with extensive auxiliary tables; history of method briefly noted at Bowditch 1958 pp.44-45, 53-54. (Due to improvement of portable timepieces throughout the 19th century, the lunar-distances method became, in this century, useful only in case of general shipboard chronometer failure.)

[11] The only lunar observations where parallax's effect on celestial longitude may be skipped are: [a] Observations taken such that the ecliptic is parallel to the horizon. (I believe that it has not previously been pointed out that Hipparchos used this technique for his observations of −127/4/5 and −126/7/7. See *Almajest* 5.3&5. The only drawback to such a technique is that parallax's first time-differential is usually near an extremum at the time of null parallax.) [b] Lunar eclipses, where the Universal Time [UT] of mid-eclipse is [virtually] independent of location, though the Moon's topocentric coordinates are nonetheless affected by parallax.

that the Moon's distance is of planetary magnitude. Since such an arrangement would place the lunar orbit almost completely under the Sun's gravitational domination, the Moon must be a planet — a planet whose orbit nonetheless manages to appear geocentric, doubtless due to extraordinary (& also previously unknown) perturbation terms contained somewhere else in the Davies-New-Astronomy: a remarkably fruitful & revolutionary universal-physics, which is about to provide us lots of other equally enlightening gems, below.

B Davies' Modern-Science Discovers the Simultaneous Worldwide Lunar Appulse!

B1 At Philadelphia's Fels Planetarium, on 1984/10/17, Adm.Tom Davies announced a remarkable revelation regarding Amerigo Vespucci (the Florentine merchant-banker after whom America is named). Vespucci, too, posed (§A1) as an expert on exploration and navigational astronomy, convincing Spanish royalty sufficiently that he was appointed[12] Chief Pilot of Spain from 1508 to his death in 1512. He was undoubtedly a gifted storyteller, at least some of whose alleged explorations are now almost universally regarded as nonevents. But Tom Davies' highly-expert, state-of-the-art, astronomical-computer-ephemeris-based analysis convincingly vindicated Vespucci as to both his truthfulness (below: §B2) and his supposed pioneer understanding of the important method of lunar distances (§D7). The final version (DVD) of Davies' paper is "Amerigo Vespucci & the Determination of Longitude".

B2 Davies quotes (DVD 7-8), analyses, & certifies (§§C&§D here) Vespucci's alleged sight (& longitude-computation): a supposed 1499/8/23 observation (near the terrestrial and celestial equators) of a conjunction (or "appulse") of Mars with the Moon. Throughout, Davies' impressive refrain-accompaniment (repeated no less than 4 times: at DVD 1, 6, 10, 13; all 4 passages quoted here) is that he will use "*the tools of modern science*"[13] (DVD 1, emph added) and *US Naval Observatory celestial computer ephemeris-programs* to test Vespucci's observed conjunction. "It is this phenomenon [the conjunction] that Vespucci used to ascertain his longitude in the New World. Using *information available only four centuries later*, we have the opportunity to *test his veracity*" (DVD 6, emph added).

B3 Davies' *entire paper's crucial math basis* (a Nobel-Prize-winning discovery, if true! — DVD 6 [www.dioi.org/dvd.pdf, p.6], emph added): "Determination of longitude by lunar distances is based upon the fact that a 'celestial event,' a conjunction of the Moon and a planet or star, represents an event readily observable *SIMULTANEOUSLY at widely separated points on the earth*. The 'Local Time' of the event at each of the two points will differ by an amount equal to their difference in longitude measured in hours rather than degrees. Since the earth rotates 15 degrees per hour, these hours of time difference are directly convertible to longitudinal distance in degrees." (See also DVC 3.)

B4 A classically perfect case of Dangerously-Little-Knowledge. [Which sadly undoes Davies' **earnest try at *adding* to knowledge** — unlike *DIO 22* ‡3's zoo of ***subtractors***.] I recently gave the Davies analysis to one of the world's best known and most respected astronomers, Charles Kowal (Chiron's discoverer; now at Space Telescope Science Institute). His amusedly incredulous appraisal of this keystone paragraph: it's based on an error "any freshman astronomy student wouldn't make" (*Wash Post* 1989/12/12; see also *Wash Times* 1990/2/22). Kowal independently also finds that Vespucci's 1499/8/23 "observation", correctly computed, puts him near Africa (§G4), not Brazil. As is self-evident to [any positional astronomer][14] a conjunction of the Moon with another celestial object (planet or star — or the Sun, as in a solar eclipse) will in general NOT occur "simultaneously[15] at widely separated points on the earth." If such a fantasy were in fact able to [materialize], then solar eclipses would be seen the same way and at the same time for all observers (who could see the eclipse at all), the world over! [The same elementary confusion of use of solar & lunar eclipses in an establishment attack on DR occurred as recently as late 2016 in *Isis*, History of science Society's flagship journal: www.dioi.org/vols/wm0.pdf, *DIO 22* ‡1 §D.] Most of us, from professional astronomers to highschoolers, have heretofore believed otherwise. But it seems we all have to catch up to Davies' revolutionary New Astronomy: solar eclipses are henceforth to be seen simultaneously & at the same magnitude all over the entire sunward side of the Earth. Thus, the Moon's solar-eclipse shadow does not move over the Earth's surface — and it has no locale. (Understand: this is the [NGS-quarter-million-dollar-remunerated] Expert — demonstrably innocent of the behavior of the best known shadow in astronomy, the solar-eclipse umbra — who on 1989/12/11 assured the public that his Navigation Foundation has competently analysed [on NGS grounds] the shadows and spatial relations in Peary's photos and thus concluded that he got to the Pole.)

B5 Davies' elementary §B3-mangling of the lunar distances method is the novel navigation principle that he applies to the computer-generated places adopted in both his detailed analyses (DVC & DVD) of Vespucci's observations. But, as any astronomer reading Davies' epochal New Astronomy (§B3-§B6) has by now mirthfully noticed, both the Davies procedure (§B3) and his analysis (given below: §D4) omit *the* most elementary correction characteristic of the lunar distances method, namely: *LUNAR PARALLAX*. And, Davies' bad luck: the observation he is examining and computing was allegedly made at the horizon & in the tropics [those 2 circumstances combining] to virtually maximize the effect of parallax on the Moon's apparent east-west ecliptical motion, that motion being the entire basis of time & longitude measurement via the lunar distances method. A navigator pretending to expertise in historical longitude-determination methods, while innocent of when to apply lunar parallax, is akin to a purported Shakespeare authority who never heard of *Hamlet*. [See www.dioi.org/sha.htm, for *DIO*'s 2014 take on the Shakespeare-authorship flap.]

B6 Davies prefaces his monumental discovery of *widespread simultaneous lunar conjunctions* by exhibiting (à la Vespucci: fn 30) his classical scholarship, showing that the Davies New-Astronomy is implicitly assented to by Ptolemy (DVD 6): "It seems likely that [Vespucci] knew of the significance of the statement in Ptolemy's 'Almagest' (Vol. VII, Chapter III) of the Local Time difference between the observation of a conjunction of the Moon and Spica at Rome and the same celestial event observed at Alexandria" . . . . (Any-

[12] Markham 1894 (pp.xiii-xv) argues convincingly that the appointment was unmerited (possibly assisted by overgullible acceptance of his exploration claims). Vespucci's apparent navigational eminence appears to have been more the result of political connections than of genuine expertise. Lucky that kind of *HMS Pinafore* stuff is a thing of the past.

[13] This boast (see also fn 57 & irony there; further invocation of modern computer weapons at DVC 8, 11) echoes that of Davies, in 1989, regarding his National Geographic-sponsored [NavFou] "investigation" of Peary's 1909 North Pole claim. *Baltimore Mag* flacks for Davies thusly (*BM* 1989/7 p.86): "The major difference between his [Davies'] investigation and all the previous ones is that his will be more comprehensive and will draw on 'modern methodology and modern information.' . . . Another tool is a [celestial] computer program Davies got from the Naval Observatory".

[14] The NF's months of inability (NG App.A) to solve its own Betelgeux Document (BetDoc) hypothetical time-sight problem reflect more than mere innocence of standard spherical trig and fundamental astronomy. For, in truth, one need not know the rigorous equations in Wm.Chauvenet's *Manual Sph & Pract Astron* (1906 1:257f) to perceive that if one has 2 stellar altitude observations at a known time interval apart, the observer's latitude is precisely determinable, virtually without regard to longitude, time-zone, [date c.1900], etc. Just an experienced feel for spatial relations (and awareness of the slowness of precession & stellar proper motion) suffices to establish this. Thus, even without analytic calculation, merely a bit of (informed) trial-and-error could easily have shown the NF that only at latitude 78°N (in the Arctic) would Betelgeux and (NF's proposed 2nd BetDoc star) Ras Alhague have appeared successively at the respective altitudes [taken as double-altitudes & corrected for r&p] shown on the BetDoc, the former event 44^{m} before the latter (as on BetDoc).

[15] All such events (lunar conjunctions with Sun, planets, or stars) can, depending upon where one is on the Earth, be seen (with varying aspects) at times differing by over 3^{h} [UT]. Celestial events that actually do occur virtually simultaneously [in UT] (as seen from all over the Earth, where the weather is sufficiently clear and the action occurs above the horizon): lunar eclipses (below: §B11, §C9). Much more frequent and useful for chronometer-checking: eclipses of Jupiter's Galilean satellites, tabulated in almanacs even into this century (but visible only in a telescope, so not relevant to Vespucci).

one who had ever opened the *Almajest* would know that it is divided into 13 "Books", not "Volumes".) It happens that DR has published at least 4 scholarly analyses discussing this very [*Almajest*] chapter (*Publ Astr Soc Pacific 94*:359, 1982, end of App.A; *Isis 73*:259, 1982, n.17; *Vistas in Astronomy 28*:255, 1985, §3; *Amer J Physics 55*:235, 1987, n.14). So, when I read Davies' citation of it, I knew instantly that Peary was not the only US Admiral who faked when he pleased. Yes, *Alm* 7.3 contains observations of lunar conjunctions with Spica, two (not one) from Alexandria and one from Rome. The catch is that the Alexandria observations of Moon-Spica conjunctions were in 294 BC & 283 BC (by Timocharis), while the Rome observation of a Moon-Spica conjunction was in 98 AD (by Menelaos) — ***almost 4 centuries later***. This large time-interval is the heart and basis of Ptolemy's entire discussion here (his demonstration, by lunar-conjunction data from different centuries, of the reality of precession,[16] a very gradual phenomenon). It cannot possibly be missed by anyone reading the source Davies cites.

B7 I have (*Amer J Phys 55*:235, 1987, §II.4) criticized Ptolemy for being the only astronomer in history who claimed he had observed the same celestial event[17] on 2 widely separated occasions (37^{d} apart). But now we have a new champion in the scholarly pretense department: an expert who has discovered that "the same celestial event" was seen from 2 different terrestrial places at times nearly 400 years apart! (Back in §B3-§B4, Davies contracted events, separated by hours [fn 15], to simultaneity — now he's compacting whole centuries! Is it unfeeling to pull the magic carpet out from underneath such delightfully accelerating science fiction. . . ?)

B8 A particularly suspicious type of reader just might entertain for a fleeting moment the notion that Davies didn't actually read the Ptolemy passage he expands so confidently upon — which would entail his cribbing the source from another, *uncited* work (a cardinal sin of scholarship: ‡1 §§D3&L). Naturally rejecting the idea of a Davies hoax, we turn to other, permissible explanations. According to special relativity, two events 400^{y} apart in one frame can only be simultaneous in some other frame (moving relative to the first) if the events' rest-distance apart exceeds 400 light years. So, either: [a] Light takes about 4 centuries or more to travel the 1055 mi from Rome to Alexandria, which establishes light's speed as less than 3 mi/yr (a snail is faster — and so is Davies' newly-discovered Incontinental Drift, as we'll see below: §G7); or [b] Davies (who has training in physics, so he cannot be taken lightly) has: shattered 4-dimensional light cones, debunked Einstein & Minkowski, and revolutionized our theories of physical causality.[18]

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[16] Davies (who obviously did his navigation at sea out of standard cookbook-style Navy tables, with uneven comprehension) is as innocent of precession as of parallax. In his first Vespucci paper, we find (DVC 2-3): "we must divert again for a discussion of navigation and navigational methods of the 15th century. . . . The navigators of the northern hemisphere have always had an easy way of determining their Latitude roughly. The star called Polaris lies less than a degree from the point in the sky around which the celestial sphere (or sky) appears to rotate. Measuring its altitude above the horizon, which gives Latitude, was done by various means from the earliest times." Polaris or α UMi is indeed now within a degree of the true north celestial pole, but, its coordinates change due to precession. In fact, Polaris was $3°.4$ from the true pole in 1499 AD. Ptolemy notes (*GeogrDir* 1.7.4) that Hipparchos (c.130 BC) found it $12°2/5$ distant from the pole. (The bright star then nearest the pole was at declination $82°+$: Kochab or β UMi, more than half again closer to the Pole than α UMi.) Incidentally, Davies cites both these astronomers (at DVC 3) as if he has read them (though "their wording was somewhat obscure", he knowingly judges); however, he gives the wrong century for both (also DVC 3), and we are now learning independently here (§B6) that Davies is not quite the Ptolemaic scholar he poses as.

[17] The 136 AD greatest evening elongation of Venus, which Ptolemy dates to +136/12/25 & +136/11/18 (*Alm* 10.1-2).

[18] For accessible discussions of the pre-Davies physics of these matters, see R.Feynman *Feynman Lectures on Physics* 1963 pp.15-7, 17-4, or A.Davis *Classical Mechanics* 1986 pp.376, 386. For a more sophisticated development (4-vector invariance): see, e.g., R.Leighton *Principles of Modern Physics* 1959 pp.30f.

B9 How often does a single paper augur discoveries in so many fields at once? (And more's to come below.) It must be added that, had Davies consulted the chapter he cites (*Alm* 7.3) in either of the only two scholarly[19] editions of the *Almajest* (K.Manitius 1912-3 or G.Toomer 1984), he would have found this chapter festooned with footnotes (catch irony of fn 19 here) discussing ancient astronomers' calculations of lunar parallax. Indeed, by chance, Toomer (as he tells the reader in his n.77 to *Alm* 7.3) chooses the 283 BC Moon-Spica conjunction as *the* example he uses to illustrate (in great detail: Toomer 1984 App.A pp.652-653 Examples 9-10) just how the ancients computed the numerous lunar parallaxes that appear in the *Alm*'s text.[20]

B10 The obvious reason one must include parallax in any computation of the Moon's observable non-zenith celestial position is that the Moon is so close to us: its mean distance from the Earth's center is only 60.3 Earth radii (ER). Thus, when seen near the horizon, the Moon's apparent position is lower (on the average) by 1/60.3 radians or 57.3/60.3 degrees (57 arcmin or $57'$) as compared to an ephemeris' computed position, which is invariably *geocentric*. (And when was the last time you observed the Moon from the Earth's center?) For a tropical observation near the horizon (such as the Vespucci 1499/8/23 "observation" analysed by Davies), ecliptical parallax is nearly an entire degree. That is, the observer-centered or "topocentric" lunar celestial longitude[21] will differ from the *geocentric* position by roughly a degree. So, for tropical moonrise, the observed (topocentric) celestial longitude will be roughly $1°$ higher than the celestial longitude given by an accurate ephemeris (geocentric); for tropical moonset, lower by same. This will seriously affect conclusions regarding time or the observer's longitude (deduced via lunar distances): since the Moon's mean celestial motion takes it about a degree every 2 hours,[22] Adm.Davies' "glaring error" (to quote the 1989/6 *National Geographic* on DR) in his deduced geographical longitude (caused by his omitting lunar parallax in a tropical moonrise observation like Vespucci's) must be very roughly 2 hours or $30°$! — which is the difference between Brazil and Africa. It is also comparable to the size of the error ($45°$ of longitude) Davies asserts Vespucci made (due to poor tables), which Davies claims (DVD 10) to have "vastly" improved! (In truth, if Davies' speculative §C8 reconstruction of Vespucci's math is corrected for parallax etc., the resulting longitude is by chance much nearer Brazil than is Davies' own modern-calculated longitude, similarly corrected. . . .)

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[19] Davies & National Geographic perversely condemn DR's *Peary at the North Pole: Fact or Fiction?* (Washington 1973) for not being a scholarly work (e.g., not having a normal footnote system). This despite the fact that all the book's professional reviews agreed that its evidence was convincing.

[20] Similar Davies innocence interprets oversurely the Vespucci §C2 statement, that he was "correcting with calculations from the Alphonsine Tables", as merely referring to a 90^{m} longitude shift from Regiomontanus' meridian to Cadiz (a simple addition, hardly worth mention as plural and noteworthy "calculations", especially since Regiomontanus' own tables show about this time-difference between Spain and Nürnberg). Davies indicates (DVD 9) that, in order to check on this easy matter of the 90^{m} difference, he has consulted "several variants" of the Alphonsine Tables. But did he not then notice that these include substantial tables for computing the vital longitudinal & latitudinal lunar parallax (cited Delambre *Histoire de l'Astronomie du Moyen Age* 1819 p.255-256)? The Vespucci §C2 passage [presumably taken from elsewhere, given his calculation's innocence] makes much more sense if the reference is to parallactic corrections, which everyone (well, almost) knows are required when using lunar conjunction observations. A knowledgeable navigator would have realized that possibility.

[21] Celestial "longitude" is not the same as geographical longitude. Here, the former is virtually the position measured along the great-circle ecliptic's 360 degrees, the approximate path of the Moon's motion — which is the "clock" (see fn 22) being used by the lunar distances method.

[22] In geocentric angular motion, the Moon is by far our fastest long-term celestial neighbor, with a mean sidereal motion (movement in celestial longitude, against the stellar background) of 360 degrees per sidereal month (27 1/3 days) or $13°.2$/day or $33'$/hour. Since the Moon's mean diameter is barely $31'$, a rule of thumb is that the Moon's average geocentric motion is: its own diameter every hour. However, this average is seriously affected by the eccentricity and perturbations of the lunar orbit as well as by parallax; thus, for reliable investigations, one must compute precise absolute positions and use differential methods only with great care.

B11 I might add that, 2 millennia ago, the best ancient Greek astronomers, by competent use of lunar observations (lunar eclipses, where parallax doesn't affect the time of the event) mapped longitudes to an accuracy of roughly a half degree.[23] This is about 60 times better accuracy than that of Adm.Davies' impressively attired "modern methodology" calculation.

B12 I will also remark that over 5 years ago (*Queen's Quarterly* 1984/12), I playfully criticized most modern astrologers for computing horoscopes without including lunar parallax — especially since its omission will foul up the loveliest of all celestial conjunctions (conjunctions being astrologers' meat), specifically: solar eclipses. (I also added that some few 20th century astrologers do include it: G.Noonan & G.Allen alias D.Bradley. So even these mystics, like astrologer C.Ptolemy, are way ahead of a certain Admiral.)[24] But I did not expect ever to encounter such a catastrophe in the work of a purported expert in navigation — much less in the output of one who has made so much (§G9) of his special experience & skills in questions involving the L-word. (And Davies can hardly be excused as a novice: when he first announced his vindication of Vespucci, Davies was 70 years old.)

B13 Was Davies later apprised of his "Colossal error" (to quote a 1989 Davies attack upon a seemingly vulnerable quarry)?[25] Well, when DR asked to see this DVD paper, a possessor of it stated (1989/11/13) that the reason he can't send DR a xerox is because Davies, when he heard DR was interested, had specifically asked that DR be not given a copy. Davies' excuse? — he hadn't finished the paper yet. (No hint that the paper was grandly miscomputed.) Question: was Davies worried about finishing the paper? — or about the paper finishing him?

C Vespucci's Alleged Observation & Calculation

C1 Vespucci's report, taken from his contemporary Strozzi's copy of a supposed[26] 1500/7/18 Vespucci letter[27] to L. di Medici, is quoted by Davies (DVD 7-8):

C2 "As to longitude, . . . I was put to great pains to ascertain the east-west distance that I had covered [since leaving Cadiz 1499/5/16]. . . . I found nothing better . . . than to . . . take observations at night of the conjunction of one planet with another, and especially of the conjunction of the Moon with the other planets, because the Moon is swifter in her course than any other planet. I compared my observations with the Almanac of Giovanni da Montereggio [Regiomontanus], which was composed for the meridian of Ferrara [actually Nürnberg][28] correcting with calculations from the tables of King Alfonso [the 'Alphonsine' tables].

C3 ". . . one night, the 23rd of August [1499], there was a conjunction of the Moon with Mars, which according to the Almanac was to occur at midnight [within a half hour]. I found that when the Moon rose an hour and a half after sunset, [the conjunction had already occurred]. That is to say that the Moon was about 1 degree and some minutes farther east [in celestial longitude: along the ecliptic] than Mars, and at midnight her position was 5 and one half degrees to the east, a little more or less.

C4 "By such means was made the proportion: if 24 hours equals 360 degrees, what do 5 and one half hours equal? I found that I had come 82 and one half degrees. So much I computed to be the longitude from the meridian of the city of Cadiz."

C5 It is true that 15 times 5 1/2 equals 82 1/2, but little else is clear about this passage. Davies notes (DVD 8): "At a latitude near the Equator a longitude of eighty two and a half degrees west of Cadiz [whose actual longitude is $6^\circ 18'$W of Greenwich — DR] would have put Vespucci in the Pacific Ocean; this fact has been cited as one reason to believe that he knew nothing of navigation and faked the entire story."

C6 Davies is "not bothered" (to quote his equally blasé attitude regarding Peary's peculiar first-time 1909 lack of observations for longitude; fn 8) by several gross errors in the Vespucci report: [a] Was Vespucci so inept at dead-reckoning that he did not know he had actually traveled (since departing Cadiz) barely 30° or 1800 miles of longitude west instead of 82°1/2 or nearly 5000 miles?! (Note that the 1494 Treaty of Tordesillas division of new lands created a Line of Demarcation between Portugese and Spanish claims: Spain got anything west of 370 leagues west of Cape Verde [the same "Fortunate Isles" zero-point which Marinos-Ptolemy had used: see fn 3 above]; Portugal, anything east. This Line was at approximately 50°W longitude by the modern Greenwich convention. Vespucci's alleged trip was Spanish, thus his claim that Brazil was at c.90°W happily pushed his "discovery" so far across the Tordesillas Line of Demarcation that a Spanish claim to it seemed unarguable.) The exaggeration of the supposed distance to Brazil was by a factor of nearly three! I see that Davies does not mention that Vespucci also alleges wildly exaggerated attainments in *latitude* — claiming[29] that, on his 3rd reputed voyage, he had

[23] D.Rawlins *Vistas in Astronomy* 1985: paper delivered at 1984 Greenwich centenary celebration of modern prime meridian's establishment.

[24] See fn 57 for another suspected Navigation Foundation descent to the level of astrological expertise in astronomical calculation.

[25] National Public Radio 1989/2/1. Davies was referring to DR's 1988 belief that the chronometer serial numbers on Peary's Betelgeux Document were azimuths. Davies failed to note that DR's computed position for Peary did not depend upon this secondary matter (the computation being accurate, though the basis was false), nor that the same interpretation had been made by leading scientists of the American Geographical Society and the Carnegie Institute. Scientifically speaking, the "Colossalness" of this DR error is trifling beside Davies' incomparably amateurish miscalculation of Vespucci's lunar position due to omitting parallax — an error which, to my knowledge at least, has been made by no reputable astronomer since the Big Bang.

[26] Original now lost. The official Hakluyt Society collection of Vespucci's output omits the letter on which Davies' entire paper is founded, stating that it is one of "three spurious letters now so universally held to be forgeries, that they need not occupy our time" (Markham 1894 p.iii). (DVC 6 calls the letter's genuineness "unquestioned".) Reminds one of the prime issue raised by Mencken's unstopperable Bathtub Hoax (as well as Peary's Pole prank): is some misinformation unkillable? H.Wagner's 1917 opus (which I have not seen in the original), on the Vespucci lunar distances question, suggested that a reason for disbelieving in the authenticity of the 1500/7/18 Vespucci letter (supposedly written from Seville) was that a different Vespucci account said he was not back in Spain from the trip until 1500 Sept. See A.Wedemeyer's review at *Annalen der Hydrographie und Maritimen Meteorologie 46*:196; 1918. (DVC 4 says there is archival evidence that Hojeda returned in 1500 June. And J.Hitt tells me [1990/3/1] that there is said to be similar proof that someone with a name similar to Vespucci's sailed with Hojeda.) For another problem noted in this review, see fn 31.

[27] Slightly misdated as 1500/7/15 at DVD 11 but correctly dated elsewhere by Davies.

[28] DVD's Fig.6 caption notes this correctly. However, Davies' 1988/9/18 memo on the Regiomontanus tables says: "Note: Regio's time is at [Prussian] Koenigsberg, Longitude E 14-30." [Actual Königsberg: $54^\circ 42'$N, $20^\circ 29'$E.] This odd confusion arose because Johannes Müller (Regiomontanus) would sign his works (e.g., bottom of last page of *Der Deutsche Kalendar des Johannes Regiomontan* — same year and computational base as his *Ephemerides*): "M. Johan von köngsperg." But this has nothing to do with the meridian or the publication-place (which was in fact Nürnberg [fn 50]) of the Regiomontanus tables — rather it signifies that he was born (1436 AD) at the other Königsberg (in Franconia: $50^\circ 05'$N, $10^\circ 34'$E). In ancient times, one's native town was commonly attached to one's name. (By the way, how Davies arrived at a longitude of 14°1/2 E is a mystery. Perhaps he couldn't find this tiny town on a map but assumed that Frankfurt was in or near Franconia and so used it, but then got the wrong Frankfurt, an-der-Oder at $14^\circ 32'$E instead of am-Main at $8^\circ 41'$E.) Since Davies has sneered at DR's minor (& not original) 1988 chronometer-number error as akin to confusing someone's license-plate number with scientific data, I may as well point out a funnier, [original], and absolutely unique aspect to Davies' 1988 misplacing of Regio's meridian. It is well known (first sentence of his entry in *DSB 11*:348; 1975) that "Regiomontanus" is just a latinized form of Königsberg (King's Mountain). Has any other self-advertised longitude expert ever confused someone's *name* for his longitude?

[29] Text at Markham 1894 p.45. The attached Vespucci statement that he was thus only 17°1/2 from the Antarctic Circle (23°1/2 in radius around the terrestrial S.Pole) would put him nearby at latitude 49°S. The implicit Vespucci value of the obliquity in these statements appears to be 22°1/2. Another Vespucci text quoted here (fn following) makes the obliquity 23°. The correct value in 1499 was $23^\circ 30'$.

reached 50°S. But latitude (unlike longitude) is so easy to determine astronomically that there can be no Davies-rehab by "modern" recomputation of celestial data. In a similarly Münchhausenesque vein, Vespucci claims on his supposed 1st voyage to have gone along an American coast (starting at 23°N)[30] 870 leagues[31] to the NW — over 3000 miles! (I use Vespucci's 3.6 mi leagues: see fn 31. Markham 1894 p.xxvi comments: "Such a course and distance would have taken him right across the continent of North America into British Columbia.") [b] Off the coast of Brazil on 1499/8/23, the Moon rose about 2 1/3 hours after sunset (not $1^h1/2$).[32] [c] It is impossible for the Moon to have moved nearly 4°1/2 in the reported $4^h1/2$ time interval between the 2 reported observations (19:30 to 24:00). It is incredible to me that anyone who had regularly performed lunar observations, as Vespucci claims he had (§C2), could make such mistakes. It is widely suspected that Vespucci was an unreliable reporter and that the first of his supposed 4 journeys to the new world was invented. Apologists' current preferred defense is to reject as unauthentic the formerly glorious Vespucci accounts (written to Soderini) on which this journey was based, while retaining the Medici accounts. Markham accepted the Soderini accounts as real, while rejecting some of the Medici letters: fn 48. The main point to keep in mind is: when it comes to Vespucci, nothing is firmly established as authentic.[33] I recommend the observation of Harvard's S.Morison *European Discovery of America: the Southern Voyages* Oxford 1974 p.309: "Let it be remembered that Vespucci lived until 1512 and thus had plenty of time to disassociate himself from the Soderini and the Medici printed letters, had he chosen to do so." On p.308, Morison comments: "you cannot convince anyone who has the Vespuccian faith." UFOlogists, Pearyites&Cookites, [Strats&Oxfordians] are no different. Morison generously credits Vespucci with 3 journeys (albeit in a trivial rôle), but also regards him as a repeatedly exposed "Liar": p.297. And pp.294-295: "We regard all the pretentious apparatus of celestial navigation in Vespucci's writings as so much dust thrown in the eyes of important Spaniards and leading Florentines. . . . It would weary the reader to pick out every inaccurate statement made by our genial faker. His distances are palpably wrong . . . . His claim to have used lunar distances to find longitude is fantastic."

C7 Davies passes over the 2nd (midnight) alleged 1499/8/23 observation (supposing it was due to a scribal error) since he thinks he can make the 1st observation fit Vespucci's purported [South] America location.

[30] Text at Markham 1894 p.17: "the Tropic of Cancer . . . where the Pole is 23° above the horizon, on the verge of the second climate [klima]." (The last reference is to the ancient expression for the latitude where the longest day of the year was 13.5 hrs; some ancients called this the second klima: see E.Honigmann *Die Sieben Klimata und die* Πολεισ Επισημοι Heidelberg 1929 e.g., pp.52, 184, 189. Markham 1894 p.vii notes that Vespucci "was fond of airing his classical knowledge, though it was a mere smattering" — some correct and incorrect Vespucci references to the classics are provided.) The text continues (*idem*): "We departed from this [23°N] port. . . . and we navigated along the coast, always in sight of land, until we had run along it a distance of 870 leagues, always toward the North-West . . . ." (Note that a voyage along a visible coast leaves no room for mistaken distance due to current or wind.)

[31] Leagues are commonly taken to equal 3 miles. But the supposed Vespucci 1500/7/18 letter equates 1 degree with 16 2/3 leagues — which makes a Vespucci league equal to exactly 3.6 naut mi (something over 4 statute mi). Wagner in 1917 brought forth yet another ground for rejecting the 1500/7/18 letter: in 1503 Vespucci called 1 degree equal to 21-22 leagues: see Wedemeyer's review, cited in fn 26.

[32] For various independent reasons, one suspects that the author of this letter computed indoors rather than observed outdoors. It is possible that he calculated, from Regiomontanus' tables, the lunar elongation (angular distance from the Sun) for the wrong (1499/8/22-23) Nürnberg midnight. (The time of Moon-Mars conjunction predicted in Regiomontanus' aspect-tables is the following midnight: 1499/8/23-24; DVD Fig.2.) Division by 15 for an approximate sunset-to-moonrise time-difference would yield about 1.6 hrs, virtually the amount reported in the alleged Vespucci letter (1 1/2 hrs). When dealing with midnight events, such 1-day computing errors are commonplace.

[33] See fn 39. Jack Hitt of *Harper's Magazine*, in an upcoming story (for *Esquire*), suggests that the Vespucci letters were severely re-written by successive later hands, for publication-sensation sales purposes.

C8 Davies offers his own recontruction (DVD 8-9) of Vespucci's math. (We have another example of NF reconstruction: the NF reconstructs a physically impossible solution [NG App.A], for Peary's BetDoc, via: 4 invisible observed data [www.dioi.org/sict.pdf, §D2 item 1], a lengthy invisible sph trig calculation, plus [fn 54] an invisible star, all fantasies of utter & unanimous [NavFou] miscomprehension: see also §E4.) Davies takes Vespucci's reported ecliptical difference between the Moon's center and Mars (§C3: "1 degree and some minutes") and generously rounds it up to 1°1/4 (DVD 8), thus $2^h1/2$ of lunar motion (says Davies: DVD 9). The moonrise observation was reported by Vespucci as occurring $1^h1/2$ after sunset, which was at 18:03[34] local mean time (LMT) — Davies' sole correct[35] figure here. Davies rounds 18:03 to 18:00, which puts Vespucci's reported time of observation at 19:30 LMT, $1^h1/2$ later. Thus, Vespucci's hypothetical Moon-Mars conjunction time was 17:00 LMT (19:30 minus $2^h1/2$: DVD 9). Correcting for a (false) tabular $1^h1/2$ longitudinal difference between Montereggio's meridian and that of Cadiz puts the former's (predicted) midnight conjunction at 22:30 Cadiz local time, $5^h1/2$ greater than the observed LMT. Thus, taking the ephemeris' prediction as correct,[36] Vespucci computed (by Davies'

[34] On 1499/8/23, at longitude 37°3/4 W (near the Earth's Equator), the refracted upper limb of the Sun disappeared below the sealevel sea-horizon at 18:03 LMT, exactly the value Davies gives at DVC 8. Curiously, he later needlessly rounded this to 18:00 (DVD 10; §D4).

[35] It is perhaps unkind to sully this lone credit by mentioning that it is sheer luck, but that is the case. Davies shows no awareness of the fact that "mean time" was a mere abstraction in Vespucci's era — which was long before the ubiquity of reliable day-round chronometers (much less portable ones). Thus, since well before Ptolemy, astronomers' & navigators' time of day was apparent time, not mean. And, since a celestial body's motion is a function of mean time (actually dynamical time), a correction ("the Equation of Time") had to be applied to apparent time before entering astronomical ephemerides (based on mean time). But, since the EofT never exceeds about 20^m, it was a serious problem only for the Moon's rapidly changing position. (Ptolemy never bothers with the EofT for any other celestial body.) Davies' good luck in this instance is that, at the time of the 1499 conjunction, Apparent Time exceeded Mean Time by less than 1^m — thus the EofT happens to be so tiny that we can ignore it here, and Davies' indiscriminate melding of Mean-Time & Apparent-Time data in DVD Fig.3 (virtually identical to DVC slide 15) does no damage. Note, however, that this makes *four* corrections here which were neglected by Davies — even aside from the several others (§E) which were bungled. (Are we supposed to accept unquestioningly that Davies' 1989/12/11 Peary analysis handles all necessary corrections accurately?)

[36] Here we find yet another difficulty with Davies' apology-exercise: by the time Vespucci was writing his 1500/7/18 letter recounting his math, he was back in Europe where an astronomer (or sailor — or anyone) could have told him that the predicted 1499/8/23 Moon-Mars conjunction in fact took place some hours before the Regiomontanus aspect-table's predicted time (which was Nürnberg midnight). Generalizing this point: someone knowledgeable in astronomical matters (as Vespucci pretends to be) would have tested the lunar distances method at home (thus at known longitudes), to see how well it worked. Once this point is realized, we see that no one living about 1500 could have used the method of lunar distances in the way Vespucci claims, for the simple reason that the lunar ephemerides were demonstably inadequate. When we laud someone such as Vespucci for "inventing" the lunar distance method, we are inevitably committing an injustice, because: [a] the idea of checking time by lunar conjunctions is self-evident, not an "invention" — while [b] anyone claiming to use the method effectively in 1499 cannot have been an outdoor astronomer or he would have known from repeated observational experience that the required ephemerides were so inaccurate as to render effective use of the method impossible. Why heap praise upon a phony of that ilk? Rather, let us reserve our admiration for the genuine, competent pioneers who first made possible effective on-the-spot use of the method, scientists such as Mayer & Bowditch (fn 39); and, before them, Tycho Brahe, (d.1601) who discovered & evaluated the large perturbative terms in the lunar orbit, terms which caused intolerable errors in all prior lunar ephemerides (e.g., Ptolemy, Regiomontanus) — errors sometimes exceeding a *degree*, which entailed errors of roughly 2 hrs or 30° in longitude-fixes calculated by lunar distances. I should add (since no discussion has heretofore done so) that the above suggests a means by which lunar distances could have been used before Tycho: if, after a trip, one wished to find one's at-sea longitude for mapmaking, it would be possible to compare one's at-sea observations of lunar conjunctions (not with the unreliable predictions of the ephemerides of the period but) with acquaintances' at-home

reconstruction) that the observation was made $5^{h}1/2$ or 82°1/2 west of Cadiz (DVD 9).

C9 Since this longitude (actually in the Pacific Ocean — near the Galapagos Islands!) was impossible for a Brazil journey, critical historians have not been kind to Vespucci. Moreover, even Vespucci's advocate J.Stein (who places Vespucci in Aruba on 1499/8/23) brings forth (1950 p.351) a provocative coincidence: 5^{y} earlier, Columbus, from observing (at Saona Isle) the pre-dawn lunar eclipse of 1494/9/15 (other details below: fn 50), had celestially deduced precisely the same longitude as Vespucci later pseudo-celestially found! — $5^{h}1/2$ (or 82°1/2). (And in 1500 AD, this was the only available Columbus astronomically-based longitude. Columbus' 1504 longitude-estimate obviously hadn't occurred yet.) Specifically, Saona (18°07′N, 68°42′W) was placed (by Columbus' 1494 eclipse-based calculation) just 82°1/2 west of Cape St. Vincent (37°01′N, 8°59′W; near Cadiz which is at 36°31′N, 6°18′W). Note the coincidences that Saona is just north of Aruba (where Stein induces that Vespucci's nonastronomical writings put him), only about 6^{m} east of it, while Cadiz (6°.3 W) is only 11^{m} east of Cape St. Vincent (9°.0 W), both places being in the west Iberian peninsula. Thus, the Vespucci $5^{h}1/2$ longitude "deduction" is a virtual replication of Columbus' result. (Note that, though his result was poor, Columbus' chosen astronomical method for finding his longitude at sea was fairly reliable: fn 15, fn 50. Vespucci's was not: fn 36.) Even the sympathetic Stein (1950 p.351) concludes that longitude 82°1/2 must therefore be a "presupposed value". Which would require that Vespucci (or someone) did not compute to it but from it. The implications are too obvious to belabor here.

C10 Noting the striking equality of the degrees and hours (both 5 1/2) in Vespucci's report, it is credible to suppose[37] that Vespucci merely equated hours of lunar motion with degrees and thus made just as simple a calculation as appears on the surface — if he computed at all. I suspect that the whole 1499 report may be merely a mizzled[38] appropriation of another's calculation. (I remark at fn 20 that the letter's reference to corrections from the Alphonsine Tables suggests that the hypothetical original computer perhaps took account of lunar parallax. If so, he must have been a sufficiently knowledgeable astronomer that he could not have committed the blunders & innocences so evident in the Vespucci rendition.)

C11 It is known that Vespucci transposed events from one journey to another: see [§C11 or] Roy Geogr Soc President C.Markham *Letters of Amerigo Vespucci* (Hakluyt Society, London 1894 p.xxvii). Markham adds:[39] "The investigation of Vespucci's statements

outdoor *observations* of the same conjunction. Correcting (both observations) for such matters as lunar parallax, deduction of a longitude accurate to ordmag a degree might be possible — but only after returning from the journey, not during it.

[37] I see that geographer Hermann Wagner comes to the same speculation by a different route: *Annalen der Hydrographie und Maritimen Meteorologie 46*:105; 1918 p.280.

[38] Another Vespucci bungle that is inconsistent with his being expert at navigational math is found in the paragraph just previous to that quoted by Davies, when Vespucci states (C.Lester *Life & Voyages of Am Vesp* New Haven 1855 p.158): "we extended our navigation so far south, that our difference of latitude from the city of Cadiz was sixty degrees and a half, because, at that city, the pole is elevated thirty-five degrees and a half [the latitude], and we had passed six degrees beyond the equinoctial line [equator]. Lester shows that Vespucci (or whoever wrote this strange letter) had confused colatitude (54°1/2) with latitude (35°1/2) and had added 6° to the former to find 60°1/2. I also note that Cadiz' actual latitude is 36°31′N, so Vespucci's value (35°1/2) is oddly mistaken for an alleged observer: a little over 1° — indeed, it is about equal to the entire basis of Davies' paper (1°+).

[39] See Las Casas at, e.g., Markham 1894 p.81. And see *ibid* p.xl, which also contains the comments: "There is no mention either of Vespucci or of Giocondi, who is alleged to have brought him the invitation from the King to come to Portugal, either in the voluminous Portugese archives, or in the contemporary chronicle of Damian de Goes. This remarkable silence points to the conclusion that if Vespucci was really in any Portugese expedition he can only have filled some very subordinate post . . . ." (J.Parry *Discovery of South America* 1979 pp.99-102 suggests this might not be meaningful but notes at the same time that: [a] no Vespucci account is definitely genuine, and [b] the fourness of his alleged voyages suggests "a deliberate analogy with Columbus.") Markham also notes (p.xx): "The feature in

contained in the first and second [of his 4 alleged] voyages destroys all confidence in his unsupported word . . . ." Markham's conclusion is (p.xxv): "The first voyage appears, both from internal and external evidence, to be imaginary. The second voyage is the first [1499] of [Alonzo de] Hojedo inaccurately told, while two or three incidents of the Hojeda voyage are transferred to the imaginary first voyage." We note that the 1499/8/23 "observation" under review was reported from Vespucci's account of his alleged second voyage.

C12 If Vespucci wrongly supposed that the Moon moved 1°/hr, then the 2 "observations" are in perfect geocentric accord — and, additionally, the striking juxtaposition of 5°1/2 and $5^{h}1/2$ is also explained; this simply & immediately yields the result: 82°1/2 longitude.

D Admiral Rehab's "Surprising Correlation"

D1 But Davies' and my respective speculations on Vespucci's math are not our prime concern here. Let us look at Adm.Davies' own incomparable math, where there is fortunately no doubt of the author's intent or identity.

D2 Davies states that he has vindicated Vespucci's honesty and his presence in S.America. Davies does so as follows, starting with his customary invocation of Modern-Methodology, which is intended to lend science's authority to his remarkable adventures (DVD 10-14; DR emphases added here & there):

D3 "Precision in dealing with the positions of celestial bodies at any time, the tabulation of which is the essence of an Almanac, is a relatively recent development. The first[40] modern theory of the Moon, compiled by Brown in 1919, is still the definitive theory.[41] The Jet Propulsion Laboratory has now completed a new numerical integration of all available data on the bodies for any desired time with remarkable precision. Using [these] data and the excellent computer model developed by Dr.Leroy Doggett of the US Naval Observatory, we can plot the positions of the Moon and Mars for the 23rd of August, 1499, with great confidence.[42] Figure 3 shows a plot of the Celestial Longitude for these bodies, with the time of conjuction at Greenwich, 20h 06m, indicated by the intersection[43] of the two curves. From this evidence we can determine for ourselves what Vespucci's real longitude must have been, regardless of his calculations. [DVD 10]

D4 "Using modern data on the relative positions of the Sun and the Moon, we can calculate a more accurate time of moonrise. On the 23rd of August, 1499, at latitudes near

Vespucci's letters that has struck nearly all the students who have examined them, is their extraordinary vagueness. Not a single name of a commander is mentioned, and in the account of the two Spanish voyages [1497, 1499] there are not a half-a-dozen names of places." (At pp.vi-ix, Markham supposes Vespucci may have been on the 1499 Hojeda voyage in a minor capacity.) Obviously, these lacunae no more disturb Davies than those in the records of R.Peary (fellow Rear Admiral USN), whose veracity Davies also seeks to prove with Modern-Methodology.

[40] C.Cotter (Fellow Inst Navig) *History of Nautical Astronomy* London 1968 pp.28-29: "Lunar tables were improved to a degree sufficient for the needs of ocean navigation, largely through the efforts of Tobias Mayer of Göttingen. Mayer's tables were used by Nevil Maskelyne, who was appointed Astronomer Royal in 1765, for the *Nautical Almanac* . . . published for the first time in 1765 for 1767 . . . ." Also, P.Hansen's justly famous 1857 lunar tables (which, typical of Hansen's approach, apply perturbations to mean celestial longitude, not true) were accurate to a degree far exceeding the needs of navigators.

[41] E.Brown & H.Hedrick *Tables of the Motion of the Moon* Yale U 1919. However, starting with the 1984 *Astronomical Almanac*, all solar, lunar, and planetary positions for the US Naval Observatory's ephemerides are computed by JPL numerical integration (supervised by Myles Standish of CalTech & *DIO*), not by theories based (as was that of Brown-Hedrick) on general perturbations.

[42] Whenever Davies says anything about Vespuccian astronomy "with great confidence", he's reliably mistaken. See also fn 57.

[43] Anyone possessing the knowledge to compute the event for the moment of its occurence would hardly do this work by finding curve-intersections: inaccurate, and the introduction of lunar parallax into such a procedure is cumbersome. Such a computation is best done directly for the event's time.

the equator sunset was at 18h 00m [see fn 34] Local Mean Time. A comparison of the Right Ascensions of the Moon and Sun indicates that the Moon rose 2h 05m later, at 20h 05m. The lunar distance at moonrise reported by Vespucci (taken as 1.25 degree) results in the conjunction being about 2h 30m earlier, at 17h 35m local time. Thus the difference in time of the conjunction between Vespucci's location and at the modern standard meridian (20h 06m [§D3]) calculates as 2h 31m, or 37.75 degrees of longitude. Although vastly better than Vespucci's 82.5 degrees, there are enough approximations in these calculations that we must consider this only a probable value. . . . [DVD 10-11]

D5 "There are a number of significant landmarks in Vespucci's recitation which match well with the details of the voyage shown in Fig.4 [NE coast of Brazil]. The coast at their landfall was shallow and . . . covered with trees . . . a 'world of shallows,' a good description of the coast today. . . . the sea water was fresh within 25 leagues of the coast: Figure 5 is an Isohaline chart of the area showing this phenomenon. . . . Continuing south and eastward . . . encountered an ocean current . . . so strong that they could not make headway against it, and so finally reversed course and headed back to the west and north. At the farthest east point Vespucci estimated latitude as 4 to 6 degrees south of the Equator. The *Sailing Directions for South America* (1952) describes the Tidal Currents inshore (out to 10 miles) as running west (at ebb) up to 4 to 5 knots. Farther out they would have encountered the west-running South Equatorial current. [DVD 11-12]

D6 ". . . there is such a *surprising correlation of the data* that it is hard to believe that these *details of geography*[44] *and astronomy* could have been fabricated by someone with no knowledge of even the existence of that coastline. [DVD 12]

D7 "What conclusions can be drawn from this use of our *modern data and methods of analysis*? The literature includes several earlier but incomplete analyses of this incident: while such distinguished critics as the astronomers[45] Hermann Wagner (1917) and Duarte Leite (1958) have denigrated Vespucci without any[46] mathematical analysis of his methodology, there have been others who have said that his grossly inaccurate longitude was the result of errors in the Almanac used. I believe none have calculated his *actual location* as I have done above.[47] From this analysis I draw the following conclusions: 1) Vespucci understood and attempted to apply the method of lunar distances to the determination of longitude well before the 1514 [discovery] date ascribed to Johannes Werner, 2) the location of Vespucci on the 23rd of August, 1499, has been *reasonably established* as on the northern coast of South American, somewhere in the modern state of Ceara (in Brazil). *The* [Davies] *calculations are in accord with the* [Vespucci] *recitation of the geographical details of the voyage.* [DVD 13]

D8 "These conclusions do not necessarily exonerate Vespucci from the charges of [skeptical contemporary] de las Casas, but demonstrate *a strong probability that the 1499 voyage was carried out as recited* in the 1500 letter. Consequently, they also *build credibility for Vespucci's other writings* and support an evaluation of Vespucci as an *insightful practitioner of the art of navigation*: perhaps one of the earliest *nautical astronomers* to grapple with the realities of navigating the 'Ocean Sea.' " [DVD 13-14]

D9 It is always of interest when an analyst, though hugely miscomputing, *nonetheless* finds perfect agreement with his prejudices. We next examine the Davies errors that lead him to the felicitous harmony (§D7 conclusion) which he has proudly announced as his discovery. Given the slight uncertainty of Vespucci's purported latitude,[48] we will compute (below) for the terrestrial equator (as does Davies' calculation: §D4) unless explicitly stating otherwise. (Testing shows that varying the observer's geographical latitude $\pm 5°$ varies the solution's longitude by less than $1°$, trivial in the context of this problem; thus, the equatorial assumption is a valid and useful approximation for our search.)

E Rearward-Admiral's Navigation Founderations

E1 Omission of parallax is the most disastrous of the many reefs Davies' math founders upon: the distance of the Moon at the time of the reported Vespucci observation being 61.7 ER, the altitudinal parallax at the horizon is 57.3/61.7 radians or $56'$. Since the event is near a celestial equinox at moonrise, the ecliptical component (for an observer at the terrestrial equator) is this times the cosine of the obliquity, namely: $51'$. Since the Moon's geocentric sidereal motion was then $12°.6$/day, the time-error introduced by ignoring lunar parallax will be $51' \cdot (1°/60') \cdot (24^{h}/1^{d})/(12°.6/1^{d}) = 1^{h}37^{m} = 97^{m}$. Which is $24°$ of longitude to the west; so, correcting this error alone shifts Davies' result eastward from $38°$W longitude to $14°$W longitude — and thereby definitively ashcans the paper and Davies' whole long-nurtured Vespucci-vindication-thesis, since $14°$W is far from S.America but well east of the westernmost point of Africa.

E2 Davies' conjunction time is gotten not from direct computation but by finding the intersection of two drawn lines[49] in his artwork: DVD Fig.3. This is touchingly quaint, but (as noted above: fn 43) it simply reveals Davies' inability to compute planetary places on his own. Moreover, his DVD Fig.3 has Mars going in the wrong direction! At this time, Mars (an impressive ruddy spectacle at magnitude -2) was nearing closest approach to Earth, not far past Martian perihelion. (Which is a prime reason why Regiomontanus' Mars tables looked so bad here: Mars' unusual proximity to Earth magnified the geocentric effect of all errors in the unstated theory underlying the tables.)[50] Thus, as even an astronomical

[44] Stein 1950 pp.349-350 notes that most scholars analysing Vespucci's descriptive and geographical (not astronomical) accounts make Vespucci's 1499/8/23 position to be off the coast of (not Brazil but) Venezuela, longitude roughly 70°W: fn 47. See also F.Pohl *Amerigo Vespucci: Pilot Major* 1944 pp.64f, 218f.

[45] Same identifications at DVC 7. But neither Wagner nor Leite were astronomers. Wagner was a German geographer & statistician; Leite, a Portugese mathematician, whose interest in astronomy may have been stimulated by his interest in ancient work. (Much of our knowledge of ancient math comes to us through the astronomy of that time.)

[46] I have not seen the full works of either person, but Wagner's 1918 brief attack on Vespucci does in fact contain some amusing "mathematical analysis of his methodology". Understandably, he does not think the matter worth more than a cursory differential glance.

[47] Davies' pretense here (see also above fn 3) that he is familiar with "the literature" is just one more of his scholarship-poses. Had he actually searched the literature on this conjunction, he would swiftly have found a wellknown book on Vespucci (Pohl's) which computes his 1499/8/23 position (though not very accurately), alleging that it agrees with his writings in placing him off the coast of Venezuela. And a citation in another popular book (G.Arciniegas *Amerigo & the New World* 1955 pp.193-194) would have informed Davies that, in 1950, Vatican astronomer J.Stein checked Vespucci's alleged location by celestial computations — finding it consistent with the isle of Aruba (12° 1/2 N, 70°W: just north of Venezuela), using the very datum Davies discards (in favor of the impossible moonrise "observation"), namely: the second (midnight) "observation". (*Memorie della Società Astronomica Italiana 21*:345; 1950. Stein of course realizes one cannot fix position very exactly this way, so he attempts to locate Vespucci from his writings and then just checks how well the midnight observation agrees with this location.) Davies might also have been enlightened by Stein's calculation of the Moon-Mars moonrise situation (the "observation" which Davies analyses at such length). Even Stein, an admirer of Vespucci and a defender of his integrity, declares the moonrise observation "fictional". This for the very simple reason that, naturally accounting for lunar parallax, he finds (as do Kowal & I: §G4), that, at 1499/8/23 moonrise on the north coast of S.America, the Moon-Mars separation was well over 2°, not barely 1° as Vespucci reported (Stein 1950 pp.350-352). Yet again we find: had Davies made even a modest effort to examine the very literature he consistently pretends to have consulted, he would have learned of Stein's warning, as well as the need for including the lunar parallax correction (repeatedly noted by both astronomer Stein and even Pohl 1944 pp.68, 219 n.12) — an item on which one would not have supposed an expert required education. . . .

[48] One alleged Vespucci letter (to P.Soderini) has it between 15°N & 5°S (Markham pp.28-29); the purported 1500/7/18 letter (to L.Medici) makes the southern limit 6°S (Lester 1855 pp.156, 158).

[49] Based on a few discrete, well separated positions provided by the Naval Observatory.

[50] The mean error at this time is about 3°. Ptolemy's 150 AD tables were far better: mean error $-1°$, with superposed error wave [of ordmag 1°] . . . . (At DVD 12 & 9, Davies supposes that such errors are

novice would instantly discern, Mars was obviously proceeding in retrograde: rear-ward. But Rear-Admiral Davies' Fig.3 shows[51] Mars' motion as direct (forward: positive slope) not rear-ward (negative slope). By contrast, the Retro-Admiral's Fig.6 has Mars moving rearward at the very same time: negative slope. (Navigation Flounderation?)[52] Or, given the manipulation of velocities possible under Lorentz transformation, do we have here yet another hint of the Davies paper's curiously-undeclared relation to relativistic math?) By miseyeballing in the usual direction (the direction that will get Vespucci westward to S.America, where he "belongs"), Davies finds a too-high conjunction-time from his graph. Were the graph's lines (***NB***: Davies' shadow-lines are his basis for supporting Peary) drawn & read correctly, he would find about 20:02. The 4^{m} difference between this and the time Davies deduces (20:06: DVD 10) from Fig.3 provides yet another 1° of westward error.

due to scarcity of historical Mars data and to the fact that the Regiomontanus tables were published in 1474, thus the 1499 places represent "a 25-year extrapolation, with consequent accumulating errors." Lacking access to Regiomontanus' entire tables, I have not checked the matter directly, but I doubt that the error in the difference between the lunar & Martian mean celestial longitudes grew appreciably in just 25^{y}, since the mean synodic motions of the Moon and Mars were both so well known. I have already cited above the probable cause of Regiomontanus' large Mars errors for 1499/8/23: §E2. For a discussion of his precession, see Wagner 1918 pp.157f. Precession errors would of course not affect times of conjunction.) Note that Ptolemy's mean synodic Mars tables (probably based on ones issued at the outset of Kleopatra's reign) are still — in 1989 AD! — off by only 0°.4. Incidentally, Ptolemy unfailingly included lunar parallax in his work: see *Alm* 2.10-13 for math analyses, plus pages of tables for its computation. So it appears that even astrologer Ptolemy (whose "observations" were fictional and whose tables were simply appropriated from prior observing scientists: Rawlins *Amer J Physics* 1987/3) was, as a conceptual astronomer, superior to Davies. There is some suggestion that Vespucci or his source computed with parallax (above: fn 20). There is no doubt that Regiomontanus accounted for parallax, as the most cursory Davies comparison of Oppolzer's wellknown *Canon* with Regiomontanus' solar eclipse times would have shown: these times are all corrected with parallax for Nürnberg, which is at 49°27′N, 11°04′E. Morison *Admiral of the Ocean Sea* (unabridged 2-vol edition) 1942 1:251, 262-3 n.35 correctly remarks that Regiomontanus' tables were really issued for workers of horoscopes, not navigators. Regiomontanus invented a still-popular astrological house-division system. (I see that Davies' caption to DVD Fig.2 confuses houses with zodiacal signs.) We note that Regiomontanus tabulates not just conjunctions but all five of the aspects astrologers live by: conjunction, sextile, square, trine, & opposition. (See right page of Regiomontanus sample reproduced at Fig.2 of DVD.) None of the others would be of serious navigational use to a sailor of 1500 AD, except for those occasional Sun-Moon oppositions that were near enough to a lunar node to result in visible lunar eclipses — which Regiomontanus does tabulate. Columbus says he used Regiomontanus' predicted lunar eclipses for longitude-determination. The Regiomontanus ephemerides' error for the 1494/9/14-15 eclipse (§C9) seen by Columbus at Saona (18°07′N, 68°42′W) was $+20^{m}$ (7:45 Nürnberg Apparent Time = 6:52 UT predicted vs. 6:32 UT actual). Columbus also used the 1504/2/29 lunar eclipse in Jamaica. (Both for longitude and for impressing the natives. Vespucci's astronomy likewise impressed the natives. Back home [fn 12].) On this occasion, Regiomontanus' error was $+27^{m}$ (1:36 Nürnberg Apparent Time = 1:02 UT predicted vs. 0:35 UT actual). These mistakes caused westward errors in Columbus' deduced longitudes: 5° (1494) and 7° (1504) — though both his eclipse-based longitudes' total westward errors are far larger. I note that the rms lunar-eclipse-time error of Hipparchos' wellknown 146 BC lunisolar tables was 16^{m}, an error later reduced by him to just 10^{m} [www.dioi.org/vols/w11.pdf, *DIO 1.1* ‡6 eq.32]. So the accuracy of Regiomontanus' eclipse predictions appears to be not quite as good as Hipparchos'.

[51] The original diagram, DVC slide 15, correctly has Mars in retrograde motion. Note also lunar position disagreements (e.g., at 14 hrs) between DVC slide 15 and DVD Fig.3.

[52] More confusion: in a 1988/9/18 memo, Davies consults the Regiomontanus ephemerides to check 1492-3 lunar conjunctions with planets, finding six: 1492/8/7 Jupiter, 1492/9/19 Venus, 1492/9/24 Venus, 1492/9/30 Jupiter, 1493/2/15 Jupiter, 1493/2/26 Saturn. Only one of these is correct: 1492/9/24 Venus. Davies' memo consistently confused Jupiter for Saturn and vice-versa — an error which suggests Davies didn't know that Saturn is more distant than Jupiter, since Regiomontanus' aspect-tables exhibit the phenomena in columns, one for each planet, ranked in order of decreasing distance (sample aspect-table at right side of DVD Fig.2). The alleged 1492/9/19 conjunction with Venus (only 5 days before Davies lists another Venus conjunction — some trick!) is really a lunar sextile-aspect. . . .

E3 Davies' Fig.3 provides 1499/8/23 ecliptical longitudes of Moon and Mars graphed vs. "Greenwich Mean Time" or Universal Time. But Earth's gradual spin-deceleration renders UT not apt for celestial mechanics, thus ephemerides are computed with terrestrial dynamical time (TDT) or (a few years ago) Ephemeris Time. (The difference between TDT and ET is trivial in this context, so I will speak of the traditional ET below.) On close examination, I found that my own ephemeris program gave positions in neat accord with Davies' Fig.3 if I used ET where he speaks of UT. But UT was less than ET by several minutes in 1499 AD. So I checked with the writer (the astronomer Davies cites: §D3) of the Naval Observatory program Davies uses and was told (1989/11/30) by him that indeed his program is for ET or TDT, not GMT (or UT). (I make the ET−UT difference to be $+4^{m}$. Most specialists in this area would call it about the same or even a little higher.) So we have yet another Davies error of innocence. Again: 1° westerly mistake.

E4 Using the Sun-Moon Right Ascension-difference (cited §D4), Davies also miscomputes the time of moonrise, presumably[53] by consulting the lunar Right Ascension for a time previous to the event. We recall that Davies speculates-reconstructs Vespucci's supposed computed conjunction time as 17:00 local time (DVD 9: §C8); had Davies used this time to compute the Sun-Moon Right Ascension difference from the Greenwich time positions given him by the Naval Observatory, he would have found this Right Ascension difference to be $14^{h}05^{m}$ — just as required for his stated result [§D4] that the equatorial moonrise-sunset difference equals $2^{h}05^{m}$. But this hypothesis requires that Davies forgot to convert Greenwich time to local time when performing the computation. (Traces of a possible occurrence of the same NavFou-Snafu[54] glimmer through in the "patently absurd"[55] 1906/2/27 Cape Hecla pseudosolution [fn 54] of the Peary Betelgeux Document,

[53] Another possible explanation of Davies' 4° error here: supposing the Gregorian-Julian calendar gap = 13 days (true now, but the gap was 9 days in 1499) and then computing the solar right ascension at the wrong Gregorian equivalent date (for Julian 1499/8/23), 1499/9/5 (actual equivalent 9/1).

[54] A reporter recalls a question being raised at the 1989/2/1 NGS pressconference (Annapolis) on the Betelgeux Document (BetDoc), with respect to the NF's assurance that the BetDoc was a time-sight pair (almost certainly taken at Cape Hecla on 1906/2/27). This unanimous NF time-sight interpretation, of whose truth there could be "no doubt", according to the 1989/6 *National Geographic*, is now definitively known to be false. (Sample of funniest NF time-sight ravings provided parenthetically at §§C8&E4. For full details, see DR's *DIO* preprint, "Sic Transit" [www.dioi.org/sict.pdf, 1989/8/23] which all astronomer-reviewers agree has proved that the BetDoc is instead Peary's 1894/12/10 record of his 3-wire transit observations of Betelgeux & Vega [from his Anniversary Lodge, Greenland].) The 1989/2/1 press question was: could the NF solution's proposed 2nd BetDoc star, Ras Alhague, be seen so near noon as the NF's solution required? The NF reply is reported to have been that the Sun was 4° below the horizon. However, the Sun was not 4° below the horizon but just 1°31′ below. Curious coincidence: if, in using the celestial program which the Naval Observatory had lent the NF (since the NF cannot itself write such programs [while *DIO*'s people can&have]), the NF had inputted (for the proposed time of day on 1906/2/27) 1:02 PM Greenwich Civil Time (13:02 Universal Time) rather than 1:02 PM local time [like 1926 NGS error: *DIO 10* Fig.1 Comments], the Sun's altitude would have come out −4° 1/4.

[55] Borrowing diplomat Davies' gloriously malapropos language (regarding DR) at NG 116 n.2: "The [Rawlins F11] notion that the [Peary 1909] soundings are still on the [modern hydrographic] charts is patently absurd in view of the vast of [*sic*] amount of bottom data available at the time Rawlins was writing (1973)[.]" In fact, all of the bottom-touching 1909 Peary soundings are on the chart $CP^{n}7$ GEBCO C1 (1966/12) published by the International Hydrographic Bureau; also on provisional 1967 Canadian Hydrographic Service chart #896. I possess not only copies of both charts but a thick file of 1967-1973 correspondence with three national hydrographic offices (US, Canada, UK) discussing in detail the advisability of retaining such loosely-placed soundings on modern charts. (There was never any question that the Peary soundings were on the charts — that point was obvious and undisputed by all parties.) Davies believes (NG 121) that, for what it's worth (so far from the Pole), modern bathymetry has at least vindicated the longitude of Peary's 1909/3/20 sounding: 310 fathoms or 567 meters at c.85°.4 N, supposedly near 70°W longitude. If the Peary soundings' retention on modern maps infected the NF's work, it's possible that a key west-extended spur in the NF's 500 m isobath in this

"independently" produced & agreed to by various [hypnotized teamplayers] of Davies' univoice [NavFou], announced in Annapolis 1989/2/1 as its Virtually-Certain identification of the Document, which the NF and the 1989/6 *National Geographic* unanimously decreed was unquestionably a time-sight data sheet — a complete misidentification of even the *type* of observation: see §C8.) At the position Davies posits for Vespucci (4°S, 37°.75 W, sealevel),[56] the local mean time (LMT) when the lower limb of the Moon was seen clearing the horizon was 20:21 LMT, not 20:05. (DVD 10; previously, DVC 8 had it 19:33, by accepting Vespucci's false report that moonrise occurred $1^h1/2$ after sunset.)[57] This 16^m error is another 4° of Davies geographical longitude misreckoning toward the west.

E5 Yet another slip: by rounding the lunar motion to 0°.5/hr, Davies makes the Moon move 1°.25 in $2^h.5$ (§C8); but the Moon's actual geocentric motion (see slope in his own Fig.3) at this time was distinctly faster than 1/2 degree per hour: 31′.5/hr or 12°.6/day. Thus, the correct amount of time it would take for the Moon to move 1.25 geocentric degrees would be 2^h23^m; so: an error here of 7^m or about 2 degrees of longitude (moving Vespucci towards the west — as usual). To assume that Davies didn't know this is to assume he can't do grade-school arithmetic: $75'/(31'.5/\text{hr}) = 2^h23^m$ or 143^m. Davies' rounding here is doubly peculiar because we have textual proof that he originally did *not* round the Moon's motion: his first version of this paper used the precise (and wrong) value 0°.48/hour (DVC 8). Once he subsequently realized that this was an incorrect lunar motion (DVD Fig.3), why did Davies then so round the right value (0°.525/hr) that he could still keep Vespucci well west of where Davies' own figures should put him?

E6 Davies' rounding of Vespucci's "one degree and some minutes" to equal 1°15′ (§C8) is questionable. I believe that most of us would take Vespucci to mean something nearer 1°05′. A 10′ difference is worth about 5° of deduced geographical longitude: and, yet again, Davies' arbitrary rounding decision takes Vespucci to the west, towards S.America. However, in order to allow for easier comparison, and so as not to make Davies' problems any worse, I will generally (in the calculations that follow: except at §G3) adopt his value of 1°15′.

E7 Finally, I see that Davies includes no consideration of differential refraction.[58] Since this is the only Davies error which helps his argument (moves Vespucci's location westward) when corrected, I will do so. The difference in mean refraction between the lunar center at moonrise (when the lower limb touches the horizon) and any object apparently 1°1/4 higher than that is: nearly 10′. This corresponds to about 5° of longitudinal difference, and this Davies omission moves Vespucci to the east, not (as do all the other errors cited above) toward the west. Note, however, that this exceptional error (like his omissions of parallax & ET−UT, both pushing Vespucci westward by chance) is one of ignorance, not of intent.

region (a spur stretching nearly to the 70°W meridian) on the NF digital-terrain-model bathymetric map (reproduced at NG 122-123 & NGD 49) is ultimately based on the 1909/3/20 sounding. A succesful comparison of a sounding to itself would be circular. Herbert has sent DR a detailed profile of the bathymetry along 70°W, taken in 1976/10 by the submarine *HMS Challenger*, which is quite inconsistent with the existence of the NF's convenient spur. The US Naval Research Lab 1985 chart of this region (based on over 5 nations' data; reproduced at NG 119) is beautifully consistent with the *Challenger* profile, but does not (near 85°.4 N, 70°W) agree with the NF model (NG 122-123). The NF claims (NG 120) that its modifications of the 1985 map had no effect on its evaluation of the Peary claim; however, the NF could defend the 567 meter 1909 sounding only by altering the 1985 Navy map: stretching its 500 meter isobath far west of its original western bound, as a comparison (NG 119 vs. NG 122-123 or NGD 49) readily shows.

[56] I ignore dip throughout these analyses, since we don't know the observer's height; its effect would be small and would apply nearly equally to all parties' analyses. Incidentally, if one changes the geographical latitude from 4°S to the Equator, the LMT when the Moon cleared the horizon is still 20:21. Note that, throughout, I use this implicit definition of moonrise (lower limb touching horizon), because the 1499 observation's purpose was supposed to be to gauge visually how far past conjunction (of the lunar center and Mars) the Moon had traveled, and this could hardly be done until the entire lunar disk rose.

[57] Curiously, Davies contradicts this figure at DVC 1: "The precision of the methods of modern astronomy are such that we can extrapolate backwards to 1499, and say with great confidence [see fn 42] that the moon rose that night, one hour and twenty two minutes after sunset." Since Davies has just quoted Vespucci as calling it $1^h1/2$ hrs, he implicitly regards (DVC 1) this near-agreement as one of his verifications of Vespucci's accounts — a "verification" that had evaporated by the time Davies wrote DVD (which recognizes that the correct figure is over 2^h: §E4). How Davies so grossly miscomputed this figure is a mystery. The most likely explanation is a simple slip of 1 hr: since the correct figure is 2^h21^m, a 1^h error would explain DVC 1's 1^h22^m figure almost exactly. However, this only produces another mystery: when Davies finally discovered this error, why did he then round 2^h22^m (virtually correct) down to the conveniently false result 2^h05^m (§E4)? — an alteration that was critical to vindicating Vespucci's S.America claim. Without this forced 4° error, Davies' placement of Vespucci would have been at a longitude less than 34°W: that is, ordmag 100 mi east of S.America (§F2), out in the Atlantic.

F Westward Lo: the Judge's Fudges

F1 I am in awe of Davies' amazing *precision*: a quarter of a degree or 15 mi! Though he makes a formal remark at §D4 that his 37°3/4 W longitude result is only "probable", he nowhere correspondingly rounds his computed longitude even to whole degrees: 38°. One of the obvious follies of this paper is its unwillingness to tell[59] the nonspecialist reader frankly (what is obvious to an astronomer): since the observational basis is obviously uncertain by ordmag 10′ (e.g., §E6), the deduced result, even if it were correctly computed, must be uncertain by roughly ±5° of longitude or 300 miles. (Davies, hired by National Geographic to act as its allegedly neutral judge, overseeing its investigation of its very own Peary hoax, has gotten Peary to within ordmag 10 mi of the Pole! As a knowledgeable science-newsman has already commented: sounds like a classic case of high-precision-low-accuracy.[60] Indeed, we notice that the DVD analysis of Vespucci carries this ideal to extremes: Davies' nominal precision is a quarter-degree, while his accuracy is roughly 30°: about a hundred times worse. In fact, since DVD 10 puts Vespucci at "37.75" W longitude, the formal factor is 3000!)

F2 Was systematic fudgery at work in Davies' Vespucci analysis? Consider: whereas Davies did not think to correct for parallax, ET−UT, or differential refraction, he did find 4 numbers (§E2 & §E4-§E6) which produced his final Vespucci longitude (37°3/4). And it is remarkable that [a] had he computed these 4 numbers correctly, Vespucci would have been placed by Davies' math at a point out in the Atlantic Ocean, hundreds of miles east of Brazil (this independently of the massive parallax gaffe, note); but, by a felicitous coincidence (one chance in 2^4 or 16, *a priori*) Davies' 4 errors *in every single case* shift Vespucci to the *west*: a total of 7° of longitude (below: §F3); thus, correcting these 4 errors shifts Davies computed position (38°W) to about 31°W. But the easternmost point in South America (near João Pessoa, Brazil) is at longitude 35°W!

F3 Momentarily forgetting parallax, ET−UT, and differential refraction (since Davies did): even dropping his arbitrary rounding-up of 1°+ to 1°.25 (§C8, §E6), Davies' accumulated errors (the math in all cases easily done correctly by a scrupulous analyst) nonetheless come to: $1° + 4° + 2° = 7°$ — a total of about 400 miles. (Including the effect of Davies' odd §C8 decision to round the Moon-Mars 1°+ gap upward would roughly

[58] Thus he is tacitly assuming that the observation was well clear of the horizon, though in fact that would entail a more eastward position than he wishes — and would eliminate the only correction I could find that would help Davies' push west (§E7). See below: §G2-§G3.

[59] Nor does Davies admit that his first (1984/10/17, Fels Planetarium) version of the solution had Vespucci at longitude 47°1/4 W, that is, 9°1/2 (over 500 mi) to the west of his later solution. DVC 8 has moonrise at 19:33 LMT, 32^m earlier than 20:05 (DVD 10; §E2) and has the moon (moving at 0°.48/hr) taking 2.6 hrs to go 1°1/4, which is 6^m more than 2.5 hrs (DVD 10; §E5). The net difference is $32^m + 6^m = 38^m$, which exactly equals the 9°1/2 longitude difference just noted here. Otherwise, the calculations (and the hilarious underlying astronomy) of the 2 Davies papers on Vespucci are identical.

[60] Compare to the attitude of Stein in fn 47; see also §G2.

double this.) And thus the actual baselessness of his attempted "vindication" of Vespucci would (and should) have been obvious even to author Davies. (And nobody hired him to vindicate Vespucci. Now, imagine the outcome of siccing a Davies onto a case where his wealthy employers desperately *seek* a legend's exoneration — and you've just visualized the Davies-NGS report on Peary's Pole claim.)

F4 The systematic westward errors of Davies' analysis of Vespucci's observations remind one that Davies is now using Peary's 1909/4 photos to "prove" he was then right at the North Pole, as Peary claimed. (Result announced at National Geographic: 1989/12/11.) Lots of little arbitrary factors enter into that analysis, too. If Davies' errors always got Vespucci further west until he's where he's "supposed" to be, then: do manipulations in the "Navigation Foundation" analysis of the 1909 photos get Peary further north until he too is where he ought to be?

F5 Let us next perform a rough differential summing-up of the effects of correcting Davies' extensive series of creative miscomputations of Vespucci's moonrise "observation". We have 7 Davies errors: 3 cases of a nonspecialist's ignorance, and 4 cases of easily-knowable nudging of Vespucci westward by clumsy miscalculation or arbitrary roundings. The 7 error-corrections here: [a] parallax (24° eastward: §E1); [b] retro-retrograde Mars-flounderation and graph-misreading (1° eastward: §E2); [c] ET−UT correction (1° eastward: §E3); [d] miscomputed moonrise-time (4° eastward: §E4); [e] overcrude rounding of lunar speed for time elapsed since conjunction (2° eastward: §E5); [f] up-rounding of 1°+ to 1°1/4 (roughly 5° eastward: §E6); [g] differential refraction (5° westward if Moon on horizon, much less otherwise: §E7, §G2-§G3). Adding up all but [f] (for which I mercifully opted at §E6 to compute largely with Davies' dubious up-rounding), we find a required total eastward longitude shift of 24° + 1° + 1° + 4° + 2° − 5°, which comes to about 27°. This moves our Retro-Admiral's impressively computed longitude solution from 37°3/4 W to about 11°W — quite close to the correct result, directly (nondifferentially) computed (§G1). (The foregoing breakdown of errors shows that differential methods — of the sort Davies tries — can work,[61] but only if cautiously & competently handled.)

G The "Davies Movement" & Admiral Rehab's New Kissmology

G1 Taking Davies' version of the celestial situation Vespucci describes (the Moon-Mars ecliptical longitude difference = 1°15′), the 1499/8/23 location on the Earth's Equator computes as: 10°45′W (LMT 20:17).[62] So this is the actual solution to Davies' problem *as he himself posed it* (not the first time he's required such assistance: see fn 14 & fn 57) — roughly 27° or about *sixteen hundred miles distant* from the Vespucci (Brazil) location Davies has deduced.

G2 And note that, realistically, it is improbable (as Davies implicitly agrees: §E7) that such an observation would be made with the Moon just clearing (lower limb touching) the horizon; if the Moon is instead assumed to be a few degrees up, then the solution is moved eastward both from [a] the rotation[63] (of the observer along the terrestrial Equator) required to raise the event's altitude and from [b] the attendant sharp decrease of differential refraction. Repeating the same problem (1499/8/23, Equator, 1°15′ visible Moon-Mars ecliptical longitude difference), but asking that the lower limb of the Moon be seen not on the horizon but at an apparent altitude of 1°, the deduced geographical longitude of Vespucci is 8°W (20:22 LMT); for 2° up, 6°W (20:26); for 3° up, 4°W (20:30); for 4° up, 3°W (20:34). (Naturally, one computes the longitudes & times more exactly than displayed here, but the precision is meaningless in the context of a naked-eye report: §F1. So I round to the nearest degree of longitude and minute of time.)

G3 For comparison, we repeat these same solutions but using a Moon-Mars ecliptical longitude difference of just 1° (much nearer the sense of the Vespucci letter in question) instead of Davies' overinflated 1°1/4 (discussed §E6). For the Moon's lower limb on the horizon, the computed geographical location is 3°W (20:16 LMT); for that limb to be 1° up, 1°W (20:21); for 2° up, 1°E (20:25); for 3° up, 3°E (20:29); for 4° up, 4°E (20:34). These are the more realistic of the various solutions given here.

G4 Any likely member of the foregoing families of solutions would put Vespucci well into Africa's Gulf of Guinea (also astronomer Kowal's solution), roughly 2000 mi east of Brazil. Even the most generous (to Davies) of our calculated geographical positions (0°N, 11°W: §G1), puts Vespucci well into African longitudes (just south of Liberia) — way east of the westernmost point of Africa (Dakar, Senegal: 17°1/2 W). But since the easternmost point of S.America (Brazil) is at 35°N (as noted: §F2), there is no chance that the purported Vespucci observation (which Davies' 14 pp DVD paper has carefully "proved" was Brazilian) could have been made as far west as S.America now resides. In fact, if we put Vespucci at Davies' location (37°45′W tropical), the observed Moon-Mars ecliptical longitude difference[64] at moonrise would be 2°10′ — roughly a degree larger than Vespucci's reportedly observed value ("1 degree and several minutes"). Thus, a skeptical type might say that the report is altered, faked, or so inaccurate as to be worthless. But we will instead follow mentor Davies — to see where trust in Vespucci will transport us.

G5 Thus we know that, if the Vespucci observation happened and if he was off a coast (both of which propositions Davies accepts) then that coast was simply Africa — unless something very exciting has happened since 1499!

G6 Note that Davies is extremely convincing and [Melvillianly] persistent in his detailed comparison (§D5-§D6) of Vespucci's account to the eastern part of the north coast of Brazil. A moment's reflection reveals the glorious resolution of our seeming contradiction: Davies' resurrected-Vespucci is definitely off the hump of Brazil, but Vespucci's astronomical observation places him in the Gulf of Guinea, which is the familiar big indentation or bend in the coast of west Africa — the *very* part where Brazil's hump used to be, before continental drift removed it.

G7 And so Rear-Adm.Davies has led us to the door of a much more revolutionary realization than anyone expected to come out of his heretofore unjustly neglected rehab of Vespucci. According to National Geographic's *Atlas of the World* (1981 pp.22-23), S.America's hump & Africa's bend were originally together (125 million years ago) as a seam in the single continent of Gondwana — but, before Davies, it was generally thought that the 2 present continents had separated over 55 million years ago. How inspirational that National Geographic's own Admiral Rehab has now (in his piqued pursuit of the Great White Wash) ineluctably proved that Brazil was kissing up to Africa less than 500 years ago! The astonishing kissmological ramifications of this sensational revelation include the discovery that continental drift must be proceeding 100,000 times faster than anyone realized before. . . . With a tectonic speed of at least 1600 miles in 500 years, the newly-revealed "Davies Movement" must be over 3 mi/yr. However, we learned previously (§B8) that 3 mi/yr somewhat exceeds the new Davies value for the speed of light; since Einstein says nothing can travel faster than light: well, that's it for Einstein — whose Relativity-humbuggery has now twice been sent to the bottom by Admiral Rehab's crafty torpedoes.

G8 The "Davies Movement" goes faster than light; thus, the bowels of the Earth are alot more active than previously supposed. Hark! — a bold new world of tachyonic geophysics

[61] One might expect agreement to ordmag 1° using the methods given. The precise agreement here (to the exact degree) with direct calculation is slightly lucky. E.g., the correct equatorial 1499/8/23 moonrise was not 20:21 LMT (Brazil) but nearer 20:17 LMT (Africa), which would affect the differential method by 1° of longitude. This small shift was obscured by rounding (of all data to whole degrees), during the differential method's addition process: §F5.

[62] Moon: geocentric 13°41′ (celestial longitude), −4°42′ (celestial latitude); topocentric unrefracted 14°32′, −5°03′; topocentric refracted 14°03′, −4°51′. Mars: geocentric 13°08′, −4°54′; topocentric refracted: 12°48′, −4°46′.

[63] This approach is assisted by the fact that differential parallax is null at the horizon.

[64] This is the same regardless of whether one uses null latitude (Equator) or (as Davies posits) about 4°S.

beckons. How pathetically blind of lesser scientists not to have noticed any of this — until the Navigation Founderation's insightful President faced them with ironclad *proof*: proof that "will hold water with any scientist anywhere" (quoting Davies' pre-publication reference to his 1989/12/11 Peary report for NGS: *BaltoMag* 1989/7 p.86).

G9 Many of the greatest geniuses of science are modestly unaware of their brilliance. Happily, the discoverer of the "Davies Movement" is not in the dark on this subject, either. As our Retro-Admiral Rehab has often reminded us (§B12), his clear superiority on navigational matters is based on "experience" (gruff, deepvoiced military-authoritative 1989/2/1 putdown) and "familiarity with *normal* practices of navigation" (NG App.A, e.g., p.2; emph added). As one may see even from Davies' first published paper (*US Naval Inst Proc* 1937/2: an unwarranted attack upon mathematician E.Willis), from his youthful, pre-Nobelist period: he has long been aware that mere perfessers are not nearly as smart as he is.[65] (Martin Gardner's delightful history of pseudoscience, *Fads & Fallacies*, is filled with equally gifted entertainers of this stripe.) Davies' attitude — which has directly led to the unique recognition bestowed upon him in this paper — is exemplified by an exasperated anonymous' legendary sneer:

"People who think they know everything are needlessly annoying to those of us who do."

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F D.Rawlins *Peary at the North Pole: Fact or Fiction*? Washington 1973.

Afterword [2017]:

None of the foregoing should detract from our appreciation of Tom Davies' considerable contribution to the US' historic Antarctic expedition seven decades ago.

We see from ‡4 §K1 here that, during the last decade, National Geographic has evidently gotten saner on the Peary case, as hope-predicted at the end of *DIO 9.3* (1999) ‡6 fn 70. The foregoing 1990 paper is published here less with NGS in mind than with the thought of enlightening those who still kittylitter the internet with chauvinistic bile on the Peary-N.Pole ex-controversy, oblivious to serious scientists' rejection of Peary's claim, e.g., www.dioi.org/EMS-facts.pdf, CalTech's Standish (fn 41). On the 100th anniversary of Peary's claim, the *NYTimes* Science page reported (see internet citation at §A1 above), that NGS officially still held with the NavFou report! (though no longer publicly defending its 1909 N.Pole embarrassment: ‡4 §K1) — so the *NYT* writer found it irresistible to spoof an oft-repeated 1909/12/30 (*Independent* magazine) comment on Cookites' impenetrable loyalty — "There will be a 'Cook party' to the end of time" — by observing that:

There will be a Peary party too.

[65] When Davies 1st (1989/2/1) attacked DR in repetitiously abusive terms ("nonsense" & "ridiculous": NG App.A pp.2, 6, 7, 12, 14), DR: [a] responded with gratitude for the few items where Davies was correct, [b] agreed strong words were in order for DR's BetDoc error, [c] sent NF pro&con evidential material on Peary, & [d] suggested mutual cooperation (in the NGS' continuing Peary investigation) in order to create a memorable monument of scientific probity & belief-adjustment: 1989/2/1. Also: 2/23 letter to Davies congratulating him & NGS Chief G.Grosvenor 2 on the correctness of their contention (against DR) that the BetDoc was mislabelled by Mrs.Peary. But NGS declined the involvement of DR or even its own 1988/9 *NGM* author Herbert; & DR simply got further abuse from both Grosvenor (*BM* 1989/7 pp.49, 84) & Davies: NG republication (as App.A) of original 1989/2/1 attacks, adding fresh & quite baseless ones (e.g., "patently absurd"; irony: fn 55). Such aggressive behavior invites brutal counter-punches. DR instead here jovially nominates Adm.Rehab for a gaggle of Nobel Prizes.

‡6 Galileo vs 1618's Great Comet: "Fly like an Eagle"

by

Nicholas Kollerstrom[0]

Yee men of Brittayne wherefore gaze yee so / Upon an angry Starr?
Verse by King James on the great comet of 1618.[1]

Abstract

A polemic between Galileo Galilei in Florence and the Jesuit Collegio Romano in Rome developed over one of the most spectacular comets in history, the third of three comets in 1618 — 400y ago this year. Owing partly to his poor health, Galileo literally did not see any of these three comets — being possibly the only European astronomer in this curious position. This did not impede him from publishing on the matter, keen to display his new position as court philosopher to the Medicis. His first statement on the subject had been in 1619, "Discourse on the Comets" (penned by another, but on his behalf) where he advanced the traditional Aristotelian view that comets were vaporous exhalations from the Earth, but he added that they were, nonetheless, able to rise up somehow out from the atmosphere, so their subsequent motion in space was away from the centre of the Earth. The Jesuit Horatio Grassi countered this view, in his *Libra Astronomica* of that same year: explaining why a huge comet seen by all of Europe could hardly be a mere "vapour". Galileo then wrote his *Il Saggiatore* of 1623 as a riposte. It had no observational data, and compounded his initial errors by misplacing the comet into the constellation Scorpio rather than Libra.

It is surely the case that nothing Galileo wrote about the comet of 1618 was correct. There has been a failure of science historians over the centuries to acknowledge this fact. The Collegio Romano astronomers applied their telescope to the comet, arrived at a decent estimate of its parallax, reviewed several European accounts of it, and placed it beyond the Moon. We begin with a British comment upon this great comet by way of introduction.

A John Bainbridge, in London

A1 In 1618 John Bainbridge, lecturer at Gresham College on astronomy and medicine,[2] became one of the first astronomers to observe a comet telescopically.[3] The 3rd and last comet of that year was brilliant and spectacular, with estimates of its tail-length across the sky ranging up to 90°.[4] Bainbridge published his description of it, after having observed it continuously from Old Style (Julian) 18th November to 16th December, finding its average celestial movement to be 2°2/3 per day. Initially it moved faster, then progressively slowed before it finally vanished. He estimated its parallax against the fixed stars at less than 6 arcminutes, so he figured the comet's remoteness as at least 600 Earth-semidiameters, or about ten times the Earth-Moon distance.[5] Britain was on the Julian calendar, putting Bainbridge's first-detection of the comet on the 28th of November, New Style (Gregorian), which fits with other reports, e.g, Johannes Kepler first espied it on November 29th.[6]

[0] [Publisher's note.] Nick Kollerstrom will be ever remembered as saviour of the key document cracking the British Neptune conspiracy: see www.dioi.org/vols/w91.pdf, *DIO 9.1* ‡1 p.4 and §H8. His equally sensational exposé, *The Dark Side of Isaac Newton*, will be at bookstores in 2018 November.

[1] It was printed in the *Journal of the British Astronomical Association*, 1987, Vol.97, p.74.

[2] "Bainbridge, John" in the *Biographical Encyclopedia of Astronomy*, Ed T.Hockey, 2014, Vol. 1.

[3] T.Heidarzadeh, *A History of Physical Theories of Comets*, 2008, p.84.

[4] D. Seargeant, *The Greatest Comets in History*, 2009, pp.110-1.

[5] After reaching its perihelion on November 8th, comet C/1618 W1 then came closest to Earth on December 3rd, passing by at 0.36 AU — somewhat further than Bainbridge estimated: http://cometography.com/orbits_17th.html.

[6] Kepler, De Cometis Libelli Tres, 1619.

A2 John Bainbridge estimated its tail on December 3$^{\text{rd}}$ as being 45° in length,[7] noting how it always streamed away from the Sun, and surmised that the Sun's light was pushing it. [Fig.1 displays several lines reverse-extending the comet's tail towards the Sun's vicinity.] He described the comet as moving "continually retrograde" and Northwards. His diagram showed how it crossed over the ecliptic in between the scales of the Balance, moving in a straight line,[8] "appearing in the heavens to be the arch of a perfect great circle." Moving almost perpendicularly to the celestial equator (the dotted line slanting through Virgo's gown and Ophiuchus' hand in Fig.1), it crossed the ecliptic (marked line horizontally across bottom of Fig.1), about a degree east of the mid-point of Scorpio (15°Sco or 225° longitude). The astronomical difference between the *sign* of Scorpio (longitude 210°-to-240° on the ecliptic, thus marked with Scorpio's stingered-*M* symbol at Fig.1's very bottom) and the *constellation* of Libra (the Scales) which lay to the west (rightward in Fig.1) of the *constellation* Scorpio (a difference which seems to have confused Galileo), is clearly shown in Fig.1, where Libra's picture extends from longitude 216° (6°Sco) to 233° (23°Sco).

B Father Grassi in Rome

B1 The Jesuit Father Orazio Grassi, the Collegio Romano mathematics lecturer, published his observations upon the three comets of 1618, in his 1619 *Libra Astronomica et Philosophica*, or the "Astronomical and Philosophical Balance," under the pen-name "Sarsi".[9] He estimated it had appeared in 1618 on November 28$^{\text{th}}$ in the constellation Libra, a few degrees above the ecliptic. Its point of origin [his own Nov.29 original 1$^{\text{st}}$ sighting?] he estimated as having been 11°1/2 Scorpio, moving some 3° per day across the heavens; he wrote:

> On the twenty-sixth day [scribal error for Nov.28$^{\text{th}}$ Greg., 18$^{\text{th}}$ Julian], it [had passed] the ecliptic [& was] nearly 14°1/2 inside Scorpio, and on the twenty-ninth this new foetus was established in Scorpio at a longitude of about 11°1/2, between the two scales of the Balance [which, semi-contra Fig.1, he took to be α&β Libra], with a northerly latitude of almost 7°.[10]

B2 After thus describing the comet's motion, Grassi explained that he would not describe its origin because that question would be "astrological," i.e., he adopted the position of a mathematicus, whereby he would only describe the motions of the heavens, refraining from comment on the more philosophical issue of the comet's nature, or of what its "cause" might have been. He did however point out that the comet had appeared near to where the Sun and Mercury had last been conjunct,[11] also noted by Bainbridge.

B3 Grassi ascertained the comet's parallax by comparing three different sets of European observations with his own. Comparing his in Rome with some in Antwerp on December 6$^{\text{th}}$, he found a parallax of 16′. At Innsbruck the comet was seen to pass by Arcturus on 13$^{\text{th}}$ December at 10°53′ whereas he had found 10°55′, a difference of two arcminutes. He admitted that his telescope apparatus did not give reliable arcminute accuracy — for that,

[7] J. Bainbridge, *An Astronomical Description of the late comet*, 1619, p.11; reprinted 1975.

[8] The discussion by Stillman Drake on comets may be of interest here, in *Galileo at work, His Scientific Biography*, 1978, pp.268-270, as to how, as a comet swings around the Sun, the absolute magnitude becomes brighter and the tail extends.

[9] Translations of the four texts here alluded to have been published in 1960 by S.Drake & C.O'Malley in *The Controversy of the Comets of 1618*: "An Astronomical Disputation on the Three Comets of the year 1618", Anon, 1619 (by Sarsi), pp.3-19; "On the Three Comets . . ." by Mario Guiducci (by Galileo), pp.23-65; "The Astronomical and Philosophical Balance" (by Sarsi) pp.69-132; and Galileo's "The Assayer" pp.163-336.

[10] *Ibid*, p.10. According to Fig.1, the comet crossed the ecliptic on the 27$^{\text{th}}$, at 16°Sco. Sarsi's later 11°1/2 on the 29$^{\text{th}}$ does however concur.

[11] *Controversy*, p.9. [Mercury's solar conjunction was 4$^{\text{th}}$ November (Greg.) at longitude 12°Sco.]

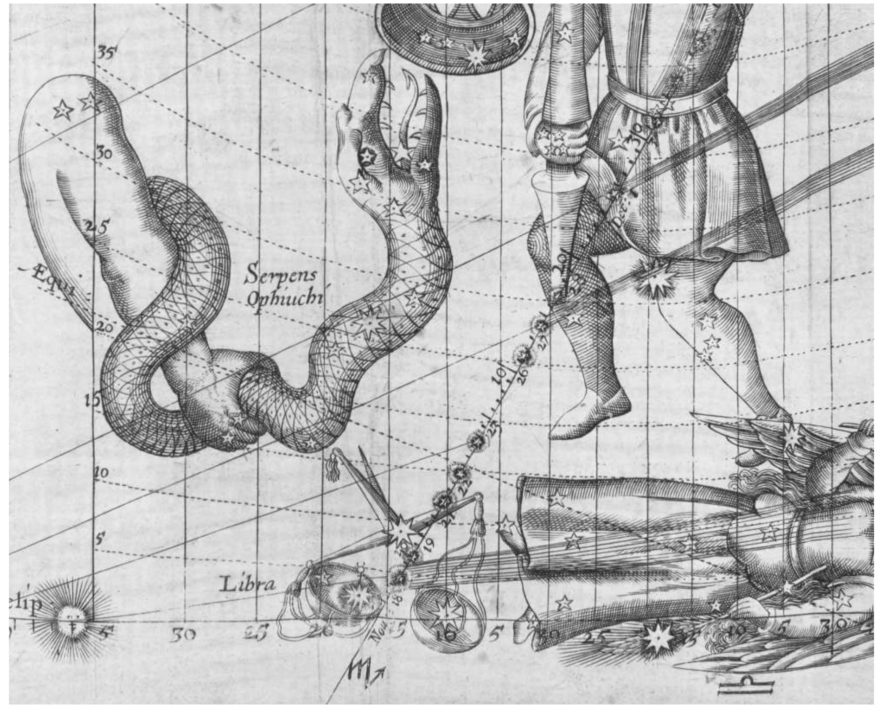

Figure 1: Path of 1618 great comet, drawn by astronomer John Bainbridge, with symbols of the signs Libra (right) & Scorpio (center) at the map's bottom, the horizontal ecliptic passing just above it, marked in celestial longitude degrees from 25°Virgo (175°) at far right to 10°Sagittarius (250°) at far left, near the Sun (6°Sgr). Curved dotted lines mark celestial latitude at 5° intervals. The slanted, near-straight cometary path bears Julian dates, "Nov 18" at its base, where appears the comet's huge tail near its max. The Northern Crown is at the map's top, Gemma (commonly Alphecca) its brightest jewel-star. At bottom right (southwest) is most of wingéd Virgo, its 1$^{\text{st}}$ magnitude star Spica (α Vir) just south of the ecliptic at longitude 19°Lib (199°); 33° above it (also at 19°Lib) lies brilliant zero-magnitude Arcturus (α Boo) in Boötes the Plowman, dominating the map's right side. At map-left is fanged Serpens Ophiuchi, in Ophiuchus' hand. Below that is Libra the Balance or Scales, containing a (partly) stellar near-equilateral triangle, whose northern apex-star is Zubeneschamali (β Lib) and whose southwestern (right) point is Zubenelgenubi (α Lib) in the Balance's western (left) weighing-pan. The triangle's southeastern (left) point is in the Balance's eastern pan: the swift planet Mercury (its tiny symbol just atop it), shown at its (very) temporary position of Nov 18, the date on which Bainbridge 1$^{\text{st}}$ saw the comet. Zubenelgenubi & Zubeneschamali are Arabic for southern & northern claws, respectively, vestiges of a remote pre-zodiac era when the scorpion grasped the stars now forming Libra.

he says, one would need something like the equipment of Tycho Brahe. On December 13th he saw it occult one of the stars in Bootes, as likewise in Cologne that same occultation was seen on the same day.[12] Thereby Grassi was confident in locating the comet beyond the Moon, because his trigonometry gave him that distance, owing to its small parallax. It was the similarity of perception of the comet's path from the different European locations which ruled out Galileo's claim (made on his behalf in the "Discourse of the Comets" 1619) that it had been a mere atmospheric exhalation — the Aristotelian view, plus its huge size also ruled out any such view. Seeing its straight-line motion in the sky, Grassi gave to the comet a circular orbit around the Sun, or maybe an elliptical one.

B4 Heidarzadeh has described Grassi's account as "a good example of technical writing about comets,"[13] noting how he compared his results concerning parallax and distance with other European observations, estimated the limits of telescopic accuracy, and tried to compute the dimensions and volume of the comet. However, Grassi achieved more than that: within the context of Tycho Brahe's scheme of things, Sarsi described the comet as probably going around the Sun in an elliptical path, and moreover as having its tail swing round to be always streaming out from the Sun. This is little-appreciated! Historians have normally averred that Grassi's theologically-determined view of an immobile Earth stunted his view; however, within the Tychonic system, the planets circled the Sun and so too could the comet. For Grassi, "If the comet were driven round the Sun . . . . What if the circle in which it is carried be eccentric to the sun . . . . What if the motion be not circular but elliptical. . . ?"[14] Grassi did not as such here aver that the view of a moving Earth was mistaken, he merely stated "these things are in no way permitted to us Catholics."

C Galileo Galilei in Florence

C1 In 1623 Galileo the court philosopher to the Medicis — no longer the mathematics lecturer at Padua — composed *Il Saggiatore*, "The Assayer".[15] His publication displayed a frontispiece of the three bees of the Barberini family, because Galileo's friend the Tuscan poet Maffeo Barberini had that very year been appointed as the new pope. His frontispiece also displayed, above Galileo's countenance, the use of a telescope and sextant, with observations being duly recorded. Not having seen the comets himself, how could he be in a position to comment on the matter, and know better than the Jesuits? The title he chose, *Il Saggiatore*, alluded to the assayer's precision balance, on which gold was weighed. His text moreover compared himself to the bold, high-flying eagle,[16] his critics being mere sparrows who shitted all over the place: "true philosophers are like eagles," he boldly affirmed:

> I believe, Sarsi, that they [good philosophers] fly alone, like eagles, and not in flocks like starlings. It is true that because eagles are rare birds they are little seen and less heard, while birds that fly like starlings fill the sky with shrieks and cries, and wherever they settle befoul the earth beneath them.[17]

Galileo was never burdened by undue modesty. These words were as Westfall observed a "provocative display of egotism".[18]

C2 Thus the titles of the publications by the two protagonists both allude to Libra the Balance, where no English-language science historian in four centuries has noticed what was, as we shall see, Galileo's catastrophic error. Galileo had received information on the

[12] Heidarzadeh, p 57; *Controversy*, pp.13-14.
[13] Heidarzadeh, p.60.
[14] *Controversy*, p.75.
[15] "The Assayer" at: http://content.wdl.org/4184/service/4184.pdf, online.
[16] James Reston, *Galileo a Life*, 1994, p.185.
[17] *Controversy*, p.189.
[18] R. Westfall, *Essays on the Trial of Galileo*, 1989, p.54.

comets from Virginio Cesarini. He had not actually seen any of the three comets of 1618, nor turned his famous telescope towards any one of them, having been too ill to get up and view them. Also he had eye trouble: "As a result of a certain affliction I began to see a luminous halo more than two feet in diameter around the flame of a candle, capable of concealing from me all objects which lay behind it. As my malady diminished, so did the size and density of this halo."[19] None of this prevented him from composing his polemical riposte. As *Il Saggitore* was published in 1623 one might have expected him to allude to other published comments upon the huge comet, e.g., that by Kepler of 1619 — but, he didn't. Perhaps as a philosopher, his new status in the Medici household, he did not feel the same need for observational data as a mathematicus.

D Grassi contra Galileo

D1 Galileo rejected Sarsi's rather logical argument that the comet's head was a solid object, placed above the Moon from considerations of parallax instead viewing it as a mere exhalation of "vapours" of the Earth, which had somehow drifted upwards, or possibly that it was a mere optical illusion which could not have any parallax (p.190): "the comet might be a mere simulacrum to which the argument based on parallax does not apply." It would not in that case have a distance from the Earth, any more than would a rainbow.

D2 In an earlier text Galileo had argued that exhalations from within the Earth had risen up, having a motion directly away from the Earth's centre, and that these somehow looked like a comet. In that case, replied Grassi, one would expect the strong northerly winds then blowing to have dispersed such a "smoky vapour." And why, wondered Sarsi, would such a vaporous exhalation want to move ever upwards, even beyond Earth's atmosphere? Anyone who had actually seen the comet, wrote Sarsi[20] — clearly enjoying the image of a bedridden Galileo too ill to get out and look at it — would not have taken such a view. Heidarzadeh politely summarised Galileo's theory as one "in which a century of observational and computational achievements is neglected," and according to which, "a cluster of exhalations moves uniformly along a straight line."[21]

E The Scorpion and the Balance

E1 Galileo denied the great comet of November-December 1618 had appeared in the Balance: Sarsi's motive for naming his monograph after the constellation Libra, Galileo explained,

> is that this comet mysteriously hinted to him by originating and appearing in the sign of Libra that he should balance and weigh it on accurate scales. . . .

Grassi assuredly had not placed it in "the sign of Libra," but in that of Scorpio. (The *constellation* of Libra was in the *sign* of Scorpio. See Fig.1. [Constellation Scorpio's main stars begin at δ Sco, longitude $27°$Sco ($237°$), latitude $-2°$, a position which is on Fig.1's map but no Scorpio stars are shown since Bainbridge couldn't see any due to solar glare.])

> . . . I note that Sarsi confidently begins altering things to suit his purpose at the very first opportunity, a style maintained thereafter throughout his essay. He chanced to think up this pun on the correspondence between his balance and the celestial Balance, and, since it seemed to him that his metaphor would be considerably enhanced if the comet had first appeared in Libra, he freely asserted that it arose there. He felt no concern about contradicting the truth.

[19] *Controversy*, p.319; Dava Sobel, *Galileo's Daughter*, 1999, p.86.
[20] *Controversy*, p.87.
[21] Heidarzadeh, pp.63&64.

"*If* the comet had first appeared in Libra" — had it not done so? Is Grassi accused of "contradicting the truth" in asserting such? Then, alluding to an earlier anonymous publication *Disputation* from the Collegio Romano (in fact also by Grassi) re the comet,[22] which had stated ". . . it was born in Scorpio, that is in the principal house of Mars" Galileo sarcastically concludes that Sarsi

> might have entitled his work, "The Astronomical and Philosophical Scorpion" — that constellation which our sovereign poet Dante called that ". . . figure of the chilly animal / Which pricks and stings the people with its tail."[23]

Galileo's polemical reply had the great comet appear in the constellation of the Scorpion, not the Balance: he was far from being the first, or the last, to muddle up signs and constellations. Both titles of these works alluded to the Balance: Sarsi's "philosophical Balance" and Galileo's more delicate precision-balance, that of the gold-assayer. Then, shifting his metaphor, Galileo claimed to have been stung in some Scorpion-like manner, but it's hard to see why. He and no-one else had muddled up the sign and constellation of the Scorpion — which had been moving apart from each other for two millennia.

E2 Galileo has accused Sarsi of "contradicting the truth" because of his title's allusion to the "celestial Balance" — i.e., the constellation of the Scales. He has quoted Sarsi re the comet's origin in the *sign* of Scorpio, "the principal house of Mars": that has to be an astrological allusion (Scorpio was traditionally "ruled" by Mars), i.e. the tropical-zodiac sign, which cannot be a *constellation*. But then in the same sentence, Galileo goes on to quote Dante re the Scorpion constellation — as would have been, he avers, more appropriate for this comet! In no way did that comet pass across the stars of the heavenly Scorpion.

E3 Stillman Drake, in his opus *Galileo at Work, His Scientific Biography*, merely says of the comet that "its home was determined as near the middle of Scorpio," leading us to wonder whether his view had been derived more from reading Galileo than the views of European astronomers. The comet was travelling at a steep angle to the ecliptic, so it is misleading to his readers to allude to the tropical-zodiac sign in this manner rather than the constellation. James Reston in his biography *Galileo a Life* described (p.177) how the great comet of 1618 "moved towards the northern scale of the Libra, growing in intensity, and finally arrived in the constellation of Scorpion." It didn't do that at all (see Fig.1). Reston has evidently been unduly swayed by Galileo's text.

E4 *Il Saggiatore* has Galileo famously expressing the view: "Philosophy is written in this very great book which always lies open before our eyes (I mean the universe), but one cannot understand it unless one first learns to understand the language . . ." where the characters of that language were "triangles, circles and other geometrical figures." Not having seen any comet since that of 1577 in his youth, he is here claiming to have access to some interior universe: a Pythagorean view which delighted his readers.

E5 Galileo interpreted a truly massive comet as being a mere optical illusion — and, in the wrong sign! His polemic, heavily sardonic, could not have been more wrong. Had he discussed his views with other mathematici, who had actually seen it, he might have avoided bungling the issue so badly. Galileo experts down through the centuries have never taken their hero to task for this.[24] Laced with heavy sarcasm, his book's insulting tone towards the Collegio Romano paved the way for the more complete breakdown in their relationship in his later trial.

[22] *Disputation* (anon), Collegio Romano, 1618: Reston, *Galileo a Life*, p.178.

[23] *Controversy*, pp.171-2. Vol.VI of Galileo *Opere*, 1933, p.221.

[24] David Wootton, in his 2010 Yale University Press biography, *Galileo, Watcher of the Skies*, has a 14-page chapter about Galileo's *Il Saggiatore* entitled "Comets" from which one would hardly gather that the opus was about this 1618 great comet. His discussion does not make contact with any astronomical issues around that comet. Should scholars not acknowledge the wrongness of "heroic" biographies — which themselves do not acknowledge when their main character was totally wrong.

‡7 *DIO*'s Bottom-Ten List of Establishment Myths

by

Dennis Rawlins

A Enlightenment by Hoaxery

A1 Not just for jest: bored with hyped and corrupt media-promoted "Top-Ten" lists, we have compiled a (patently *DIO*-selective) "Bottom-Ten List" (§B below) of historical and/or continuing establishment follies, three of which *DIO* has been successful in very prominently eliminating. Seven items are religion-related; the other three were long accepted and pushed&protected by institutions who ought to have been less secretive, haughty, and too-readily inclined to suppress dissent — or even alter, hide, or destroy evidences supporting it.[1] Those three may add to the historical and evaluative education of the sort of questioning people who presumably constitute our readership.

A2 E.g., that Neptune was discovered by math-prediction may be new to many; as that geocentrist Ptolemy said Mercury&Venus merely appeared heliocentric, but weren't. Likewise, it is not even controversial that, by the 3rd century BC, ancient scientists' estimated length of the month, $29^d191°00'50'' \doteq 29^d.530594$, was correct well within 1^s (‡9 §B1).

A3 The case of Ptolemy-vs-Aristarchos is that of an astrology-saturated religion's data-faking priest vs a genuine pioneer scientist, with the history-of-science establishment prominently, persistently (‡9 §L4) promoting Ptolemy & (www.dioi.org/vols/we0.pdf, §A) dissing Aristarchos, while astronomers so eschew fake data that the great modern star-mapping satellites are chronologically named successively *Hipparcos* & *Tycho*, skipping Ptolemy.

A4 The List's "Scorecard" also links (§C1 below) to a *DIO* play, www.dioi.org/jq00.pdf, "Getting Away With NonMurder", on the Marlowe-Shakespeare controversy. Some of the play's dialogs are enlighteningly and stimulatingly skeptical, e.g., §§D3, E1, F2, & G3. Shakespeare's slight Latin in connexion with Latin as the standard language-of-instruction at Brit grammar schools, re-verifies his long-suspected inadequate education. This novel connexion was 1st published at www.dioi.org/vols/wi0.pdf, §I3, and appeared in the latest Marlovian play, composed by Stanley Cloud (*Time* magazine), performed at Washington's Cosmos Club 2017/4/15, myself in followup-contributory attendance. Further Marlovian advances include Westminster Abbey's question mark ("?1593") for Marlowe's death-date and the latest edition of *Henry VI*'s on-cover-admission of partial authorship by Marlowe.

A5 Organized freethought might take cognizance (as it has stubbornly never done) of the fact that the man who probably wrote the works of "Shakespeare" (see plain, undisputed raw facts at www.dioi.org/vols/wi0.pdf, *DIO 18* §A1) was a courageous atheist who (like G.Bruno only 7^y later) faced execution for his heterodoxy. The discovery that Shakespeare's 1st work appeared just 13^d after Marlowe's exit is quite recent & is now admitted even by the top antiMarlovian (*ibid* fn 30; *Shakespeare Beyond Doubt* 2013 Cambridge University p.29). The only question is if the coincidence is meaningful. But since newspapers' drama departments and universities' English departments loathe all doubts of WS' authorship, few if any mention even the bare fact of the 13-day-segue, to let readers decide on their own.

A6 From olden times to ours, established gov'ts, churches, schools, and/or media have taught the literal truth of the following myths, too-often via threat of career-snuff or worse:

[1] See *DIO 22* (2018), especially ‡3 §§C-G. A particularly persistent instance in another arena: *DIO 1.1* ‡7 §A2 (1991) printed the text of rationalist-icon Carl Sagan's 1985/12/5 pretense (on ABC's *Nightline*) re Comet Halley's outdoor celestial location. In 2008 on Wikipedia, Sagan-groupies attacked (e.g., www.dioi.org/dec.htm#qztp) the relevance and very reality of our quote of Sagan's embarrassment (www.dioi.org/dec.htm#sgkp), calling *DIO* too Unreliable to trust. On 2016/6/15, *DIO*'s posting of a video of Sagan's mis-echo, www.dioi.org/0sb.mp4, was linked to his Wikipedia bio. It matches our 1991 text verbatim. So: how did Saganists react to proof of *DIO*'s accuracy & Sagan's inaccuracy? The link was destroyed at once. And these worshippers think they're Rationalists?!

B Establishment Myths & Hoaxes

1. On the 6th day, god created man, entirely without natural selection's tedious assistance.
2. Under tight security,[2] the 10 commandments were carved into stone by god, not Moses.
3. When earthly god Jesus couldn't dodge the Jerusalem bunko squad, he was executed, though said to have risen. (But hid instead of strolling the city. Resurrectile Disfunction?)
4. The Sun orbits Earth. The swings of Mercury&Venus back&forth on either side of the Sun may look suspiciously heliocentric, but actually *they're merely circling a point in line with the Sun*. (This transparent figleaf was scientific orthodoxy for 1000^y: *DIO 1.1* ‡7 §B.)
5. *Hamlet* & *Macbeth* were created by a loanshark who failed grammarschool and whose 1st work debuted ***in London***, under **two weeks** after vanishment of torture-threatened spy Christopher Marlowe, long previously the #1 playwright ***in London***. (Parallel at ‡8 §B1.)
6. UK's J.C.Adams discovered Neptune, though Frenchman Urbain Leverrier's final published prediction was spot-on to 1° while Adams', lodged privately 2d later, was 12° off.
7. The pope is infallible within certain boundaries, so voted in Rome, 1870, by the College of Cardinals.[3] Themselves presumably possessing infallible wisdom as to persons&bounds?
8. Robert Peary, who'd already (‡2 §I5) in 1907 fantasized having discovered nonexistent land in 1906, was 1st to the North Pole in 1909, steering to it without a scrap of steering-data.[4]
9. Richard Byrd was 1st to the Pole by air, 1926 May, sending his report to NatGeogrSoc in June, not to real (*DIO 10* p.6) science societies 'til Nov., after stripping it of all raw data.[5]
10. Ancient scientists were so unempirical and incompetent that they just copied prior work. Astrologer Ptolemy, who taught #4 & whose "observed" Sun-places were 50 times nearer to 280^y-old indoor tables than to outdoor reality, was "The Greatest Astronomer of Antiquity", says AAAS' *Science* (1976/8/6) & the longtime head of Harvard's history-of-science dep't (*idem*), who got the post by teaching just such unattested fantasies as established verities.

C Current Scorecard

C1 Much of the US still accepts myths #1, #2, #3, & #7. Myth #4 is now unfashionable; but myth #5 lives on, since litwits can't face weakLatined Shakespeare's uneducability at grammar school (www.dioi.org/jq00.pdf, fnn 34&36) and cannot admit that they have, for over 400^y, missed dramatist and rebel[6] Marlowe's slyest and riskiest play (see *ibid*'s finale: its §G3) — his defiant, proud, and unfathomably-scripted Atheist-Easter resurrection.
C2 Myths #6, #8, & #9, generally believed in scientific and popular media 50^y ago, have since gone the way of all flush, undone by *DIO* at, respectively, *Scientific American* 2004 Dec, p.98, *N.Y.Times* science page 2009/9/8, *N.Y.Times* 1996/5/9 page one. (The American Geographical Society never bemedalled either of the Peary & Byrd North Pole hoaxes. *DIO* vol.10, www.dioi.org/vols/wa0.pdf, ending Byrd's polar claim, was co-published in 2000 by *DIO* and Cambridge University.) Myth #10 survives from: [1] The history-of-science establishment's grantmanship-need to protect Ptolemy's historically-invaluable *Almajest*. (Though his alleged observations in it have been known to astronomers as fraudulent since Tycho Brahe's 1598 alert: www.dioi.org/vols/w30.pdf, fnn 29&141.) [2] Ignoring genuinely-great scientist Aristarchos of Samos, [a] who was 1st to publicly announce heliocentrism (400^y before geocentrist Serapic priest Ptolemy insisted on trying to refute the truth in *Almajest* 1.7); [b] who determined the month's length to a fraction of a second (‡9 §B2); [c] whose universe's volume exceeded geocentrists' size a trillion times (‡9 §K2).

[2] See www.dioi.org/vols/wa0.pdf, *DIO 10* fn 168.
[3] The US contingent, led by Baltimore's Cardinal Gibbons, mostly voted con. When Gibbons later was asked if the pope is infallible (*DIO 9.3* ‡6 fn 75), he replied: well, he called me "Jibbons".
[4] Rawlins *Peary . . . Fiction?*, Wash 1973 Chap.10, "Quiver in the Arrow". *Jeopardy* & dubiously honest (*DIO 10* §R) *American Experience* (2018/1/9 "Into the Amazon") remain unqualified believers.
[5] See www.dioi.org/vols/wa0.pdf, §F; for the pre- & post-stripped versions, see *ibid* Fig.7.
[6] Marlowe was arrested for atheism and for fighting wage-gutting mass-immigration: *DIO 18* §P5.

‡8 AIR-ERRing ERatosthenes

by
Dennis Rawlins

A deft edit of this paper, fully illustrated, 1st appeared in *Griffith Observer* 2018 August.

Two very different ancient Greek estimates of the Earth's circumference were widely adopted in antiquity. Eratosthenes and Hipparchos held for roughly 250000 stades, a stade being a tenth of a nautical mile. This value dominated for about two centuries. Later, Poseidonios and Ptolemy championed 180000 stades, which became standard among geographers for 1000^y. The ever-wheelspinning debate over what went wrong with these estimates turns out to have a very simple and neatly-precise long-available resolution.

Previous investigators have narrowly pursued strictly a metrological key to the problem: manipulating the stade's definition — without a satisfactory resolution. For 259^y now. Shock: for those 259^y ***metrologists have been barking&chasing up the wrong rabbit-hole***. The true tri-fit solution presented below is generically distinct: **just air's bending of light**.

A Two Hugely Disparate Ancient Earth-Measures

A1 Most of us have encountered the oldest of legendary astronomical measurements — which we are about to see (§B4 below) isn't astronomical at all — the 1st precise estimate of the Earth's circumference C, by the 3rd century BC Alexandrian Greek, Eratosthenes of Kyrene. He contended it was about 250000 stades — actually nearer 256000 stades (§B3 below) — supposedly from solar observations at Alexandria and Aswan. Though dead for over 2000^y (suiciding c.195 BC) Eratosthenes today has inspired enough fans to envyize a rock-star, his apologist-army regularly launching article after article after article — Eratosthenes-Reconsidered-Reevaluated-Revigorated-Reconstructed-Redux-Reverberated — invariably trying to alibi why his circumference C was about 6/5 too high, just as invariably arguing *by pure attestationless speculation* (ever-disguised as solid ancient reality) that Eratosthenes' C just *seemed* inaccurate, only because he had adopted a stade-length much smaller than the anciently standard 185 meters, so his C was actually correct within a few percent. Unfortunately for ever-loyal Eratosthenians, most scholarly opinion and unambiguous evidence (§B3) puts the Greek stade at 185 meters, which makes Eratosthenes' 256000 stades about 19% too high.
A2 But that overestimate is rigidiously excused by an eternal Eratosthenian cult, invincibly certain that its hero musta used a runty stade c.157 meters long, though (D.Shcheglov *Isis 107.4* p.698; 2016) there's zero ancient evidence of any kind for that value. Its advocates also ignore the inconvenient fact that there were not one but two widely adopted ancient Earth-circumferences. The other was 180000 stades, 17% too low, which ultimately displaced Eratosthenes' earlier value *and was considered standard far longer*: over 1000^y. It even convinced Columbus the Earth was so small one could reach Ptolemy's Kattigara (Saigon: www.dioi.org/vols/w50.pdf, fnn 64&68, Table 25) quicker going west than east. The 180000-stade estimate is thought to be originally from Poseidonios, 1st century BC, and was adopted by Ptolemy for his millennium-dominant 2nd century AD *Geography*.
A3 Though for over 2 1/2 centuries (D.Engels *Am.J.Philology 106*:298-311 p.299), hundreds of articles have undeterrably tried to explain the ancient Earth-size mystery by fiddling with the stade, three problems prevent the issue from ever being thusly resolved:
[1] Eratosthenes' 256000 stades is over ***forty percent higher*** than Poseidonios' 180000.
[2] Assuming any stade-length that makes Eratosthenes closer to the truth simultaneously puts Poseidonios farther therefrom. And vice-versa.
[3] All such solutions fit only one of the 3 quantities in play (& that not necessarily to 1%): defying the near-universally accepted 185 meter stade, while fitting at best only one of the two standard ancient Earth-size estimates.

A4 These difficulties suggest that we look outside of metrology for a solution that fits all three. Fortunately, such has been available in the professional — and even popular — scientific literature for nearly 40^y^, and it is not metrological but physical.

B Triple-Fit Physical Solution

B1 It is commonly thought that Eratosthenes' C was obtained by desert travel between Alexandria&Aswan, combined with Sun sights at each city. But that would have produced a correct value. So, some believe that his C's origin lies elsewhere, in cleverer and less laborious stay-at-home methods. Could the "Pharos", Alexandria's legendary Lighthouse (2^nd^-most enduring of the Seven Wonders of the World), have been used for the purpose? That obvious possiblity is suggested by the double-coincidence that it was built **at the very time AND place** of Eratosthenes' Earth-size estimate. (Similar potentially productive space&time confluences: ‡7 §B #5, and Rawlins *Peary . . . Fiction?* 1973 pp.262-263.)
B2 As shown in 2008's *DIO 14* (www.dioi.org/vols/we0.pdf, p.2 fn 1 & ‡1 p.12), the Pharos' height $h =$ c.300 feet. If its designer Sostratos wanted a world-record lighthouse-tallness of ***exactly*** 300 feet, that would equal half a stade. The equation for determining Earth-radius r, from the Pharos-flame's visibility distance v, is $r = v^2/2h$ (*ibid* eq.2), so $h =$ 1/2 stade renders the equation's denominator = 1, reducing the equation to simply $r = v^2$ (*ibid* eq.21): the square of the visibility distance v **in stades *equals the radius of the Earth* in stades**. The Eratosthenes r implied by Eusebios is 40800 stades (*ibid* eqs.11&18) which happens to be 202 stades squared (*ibid* eq.24), conventionally rounded (www.dioi.org/jm03.pdf, Table 1) to the nearest 100 stades. The coast southwestish from Alexandria being nearly linear, with the Pharos sitting a km offshore into the sea, one could wheel-odometer-measure that the flame was visible over water out to $v = 202$ stades.
B3 As 1^st^ realized in 2008, multiplying 40800 stades by 2π yields $C = 256000$ stades, exactly agreeing with the circumference extracted back in 1982 from Strabo's Eratosthenes Nile Map. (Hugh Thurston *Early Astronomy* Springer 1994 p.120.) The royal stade was years ago shown beyond doubt to be 185 meters by D.Engels (*op cit* p.309). Thus, 256000 stades is 19% or almost 6/5 too high; and by glaring inverse-contrast, the Poseidonios-Ptolemy value, 180000 stades, is exactly 5/6 low.
B4 Now to the shockingly elementary key to the long-intractable Earth-size mystery: if an ancient scientist had indeed accurately measured how far over the sea one could spy the Pharos' flame (§B2), and done the easy computation of the Earth's radius from this, the result would have been ***wrong on the high side by factor 6/5, due to the bending of horizontal light by air***, "atmospheric refraction". (Because the curvature of a horizontal light ray is 1/6 of the Earth's curvature.) The flame-idea was not unknown in antiquity: Pliny (*Nat.Hist.* 2.65.164) noted that if a lantern were hung on the mast of a receding ship, it would disappear when sufficiently distant, due to the Earth's curvature. Realize in-passing that this measurement would be totally non-astronomical.
B5 Another obvious stay-at-home method: if 2 ancient scientists coordinated to compare times of a clear-atmosphere sunset seen from the Pharos' base&top, the difference would be unmissably large, exceeding a minute of time. Computing Earth's circumference from such data would result in ***a figure too low by 5/6, again from airbending of horizontal light***. Such a value rose to domination c.200^y^ after Eratosthenes'. The delay's likely cause: computing it required spherical trig (D.Rawlins "Doubling Your Sunsets" *Am.J.Physics 47*:126-128, 1979, Tables I & II), not available until the 2^st^ century BC (*DIO 22* ‡3 Table 1).
B6 So airbending of light can explain each of the anciently adopted Earth-sizes cited above (§§B4-B5) to ***within one percent in both cases*** (www.dioi.org/vols/we0.pdf, ‡1 eq.28). And this is accomplished without the slightest *ad hoc* manipulation of the standard length of the stade. Thus, the solution simultaneously satisfies all three desiderata: *both* Earth-sizes ***and*** the royal 185 meter stade. Again: all three to 1%.

C Enlightenment by Satellightenment

C1 Readers who have difficulty accepting the foregoing analysis would do well to consult our ultra-simplified illustration-by-extremes (provided at www.dioi.org/vols/w23.pdf, *DIO 2.3* ‡8 §§A5&A7), which considers what would happen if the Earth's atmosphere bent sealevel horizontal light 6 times more strongly than the reality. On that hypothetical Earth, the light-ray's curvature would equal the Earth's, so a level light-ray would (if not for atmospheric extinction) circle the globe forever! — a new class of non-artificial satellite.
C2 The resultant misleading of refraction-innocent scientists, aiming to calculate the size of a satellightened Earth, would be spectacular.[1] Sostratos&Eratosthenes would see the Pharos' flame above the horizon — no matter how far they receded — so that §B2's v, in www.dioi.org/vols/we0.pdf, ‡1 eq.2, would be infinite, thus (by that equation) rendering Earth-radius r infinite: *a flat Earth*. On the other hand, since the Sun would never set, the (presumably-Poseidonian) double-sunset method's time t would be infinite. So, since t^2 resides in the denominator of Rawlins 1979's eq.13 (for computing r), calculated r must be infinity inverted, or **zero**, indicating the Earth to be an infinitely small point-mass.
C3 These two extreme illustrations (§§C1&C2) should satisfy anyone who at first finds it hard to accept that the two proposed stay-at-home Earth-measure methods must produce seriously disparate Earth-sizes, due to atmospheric refraction.

D Historians-of-Science Versus Science — and Scientists

D1 Pieces of the airbend solution have appeared for decades in *American Journal of Physics* (1979), *Scientific American* (1979), *Archive for History of Exact Sciences* (1982), H.Thurston, *Early Astronomy* (1994), *DIO: The International Journal of Scientific History* (2008), and in the 1990s as opening-page applied-physics example (with credit to Rawlins), in the ubiquitous physics textbook, Halliday, Resnick, & Walker. Despite such broad availability, professional historians-of-science continue pursuing exclusively their fruitless, endless, chimeral stade-scrunching metrological search — e.g., *Isis*' 2016 December enormous ***lead*** article. (Which also attacks *DIO* for the "delusion" that Greek scientists were accurate [an issue taken up in ‡9 below], but has its own delusions [§E2; www.dioi.org/vols/wm0.pdf, ‡3, §D; or www.dioi.org/islg.doc, §D], e.g., on telling addition from subtraction [like *JHA*'s Editor: ‡9 §I5] and between solar and lunar eclipses.)
D2 Meanwhile, historians-of-science unanimously — **without a single exception** — ignore [a] the airbend solution and [b] the independently-verified (§B3) 256000 stades circumference of Sostratos-Eratosthenes, not mentioning either even when citing and discussing articles explicitly recommending both. And no historian has ever indicated awareness that the 6/5 air-bend factor[2] has been standard in navigation manuals — e.g., the Bowditch — for over 100^y^. Astronomers can make up their own minds as to whether historians or scientists ought to write astronomical history, when it involves mathematical science.

[Observation added 2018/4/19. If there is historical reality in ‡9 §F's hypothesis of Timocharis' precise Earth-measurement, resulting in 600 stades/1° (c.300 BC), then: why did 700 stades/1° wipe it out just decades later? Potential answer: the very explosion of science — which proposedly measured Timocharis' C to within 1% — soon after also produced the Pharos and nourished math ingenuity that invented the lighthouse method of Earth-radius determination. See analogous fast-cascading enlightenment (c.1600 AD) at *DIO 3* fn 13.]

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[1] "Curvature" is defined as the inverse of the radius of the light-ray's path. Venus and the Solar System's outer giant planets all have atmospheres that would refract horizontal light to a curvature far greater than their bodies'; so (since a ray from an observer's nadir can curve into the observer!), sunrise & sunset could never occur there, even if atmospheric extinction did not inhibit such events anyway.

[2] Noticing the 6/5 connexion, between longstanding navigation-practice & both ancient Earth-size errors, is yet another entirely original and multi-perfectly-fitting *DIO* discovery (like ‡9 §J2), thus aSymmetrically-uncitable by the aHoly Trinity aTop the *JHA*: www.dioi.org/vols/wm0.pdf, *DIO 22* ‡3 p.46 & fn 1; & such academically criminal shunning is not restricted to DR: see, e.g., *ibid* fnn 5&17.

‡9 Accurate Ancient Astronomical Achievements

History of Astronomy Cult: In-Denial and In-Decline

Journal for the History of Astronomy Editor Beatified

by

Dennis Rawlins

A History-of-Science's Persistent Mis-Denigration of Greek Science

A1 Histories of ancient Greek astronomy routinely describe it as inaccurate, geocentrist, and so nonempirical that Greek scientists allegedly destroyed data conflicting with prevailing theories. Prominent denigrations: Neugebauer 1975 pp.108, 284, 642-643; Gingerich 1976 p.477; ScAm 1979; Van Helden 1985 pp.6-7 and 167 n.8; Evans 1987; Evans 1992 p.68; Evans 1998 pp.273-274 & n.32; Duke 2008W p.287; Shcheglov 2016 pp.687&693.

A2 Such unhinged speculation is contrary to a broad range of easily verifiable aspects of ancient science. Greek astronomy was sufficiently competent that it determined a surprising number of celestial and geographical quantities, to virtually the limit of pre-telescopic possibility. E.g., the Moon's distance was found by eclipse analysis (R.Newton 1977 p.174 Fig.VIII.2) to 2% accuracy, 59 Earth-radii (*Almajest* 5.13) vs actually 60; the mean motion of Mars (*Almajest* 9.3) was found (by stationary points: Neugebauer 1975 p.390) to c.$1'$ per century; the Sun's angular diameter was correctly observed (Rawlins 2012T eqs.8-9) by Archimedes as between $27'$ & $33'$. (Full list of Greek accuracies: Rawlins 2018A §B.)

A3 But the alert reader may already have discerned an enlightening contradiction in the foregoing brief introduction, namely: *how could Greek astronomers advance to numerous highly accurate measurements by just mimicking the results of their predecessors?*

B Ancients' 3 Adopted Monthlengths Good to 1 Time-Sec or Better

B1 The Greek achievement that is most surprising, to those who are not familiar with ancient science (and even to some who think they are), is the 3rd century BC estimate of the length of the synodic (civil) month, good to one part in several million, by Aristarchos of Samos, also famous as the 1st to teach that the Earth goes around the Sun.

B2 How was such accuracy possible in an era without telescopes or reliable clocks? Simple: as told at *Almajest* 4.2 (2nd century AD), ancients had noted a 4267 month eclipse-return cycle (nearly 345^{y} long) which happened to coincide so near-exactly with 4573 anomalistic months, that the time of the interval between eclipses was virtually invariant no matter where on the ecliptic the eclipse-pair occurred, or when. (The anomalistic month is the mean time for the Moon's return to its apogee.) Centuries of eclipse data preserved at Babylon (*Almajest* 4.11), compared to like data from classical antiquity, showed that the 4267 month eclipse-pair interval never varied by more than a fraction of an hour from a mean of $126007^{d}01^{h}$. Dividing this by 4267 yielded $29^{d}12^{h}44^{m}03^{s}.3$. Sure enough, we find a monthlength of $29^{d}12^{h}44^{m}03^{s}1/3$ explicitly attested on cuneiform text BM55555 (c.100 BC). Also at *Almajest* 4.2 (c.160 AD), which says: [1] that this was the monthlength of Hipparchos (c.130 BC), and [2] that the 223 month saros expression, which has been (Heath 1913 pp.314f; Rawlins 2002A §A) mathematically traced to Aristarchos (280 BC), was $18^{y}10°2/3$, one 223rd of which is $29^{d}12^{h}44^{m}03^{s}.2$, agreeing to 1 part in 24 million with the "Babylonian" value, the difference just a rounding-imprecision. The Aristarchan monthlength, later adopted by Babylon (c.200 BC) and even later by Hipparchos, was correct to a fraction of a second — the actual synodic month then being equal to $29^{d}12^{h}44^{m}03^{s}1/2$.

B3 Since 4267 synodic months equals 4573 anomalistic months, removing common factor 17 (*Almajest* 4.2) produced the famous relation 251 synodic months = 269 anomalistic months. So ancients simply multiplied 251/269 times §B2's synodic month, to determine that the anomalistic month = $27^{d}13^{h}18^{m}35^{s}$, just 1^{s} less than the truth then.

B4 The draconitic (eclipse) month is the mean time for the Moon's return to a node, where eclipses can occur. The method of determining its length is provided at *Almajest* 6.9, where Hipparchos preliminarily chooses an appropriate eclipse-pair, Babylon-observed −719/3/8 and Hipparchos-observed −140/1/27, separated by almost exactly 7160 synodic months and 7770 draconitic months, so the eclipse month would be 716/777 times the synodic month, or $27^{d}05^{h}05^{m}37^{s}.0$, too high by 1^{s}. But, realizing that a longer interval would improve accuracy, Hipparchos switched (Rawlins 2002H eqs.1-3) his choice of prior eclipse back to the now-lost Babylon eclipse of −1244/11/13, establishing that 13645 synodic months equalled 14807 1/2 draconitic months, which after division by 5/2 produces the ratio 5458 synodic months = 5923 draconitic months, attested at *Almajest* 4.2. So Hipparchos could just multiply §B2's synodic month by 5458/5923, to find that the draconitic month = $27^{d}05^{h}05^{m}35^{s}.9$, off by but a fraction of 1^{s}, the actual value then being $27^{d}05^{h}05^{m}36^{s}$.

B5 One may reasonably inquire of those who keep on teaching that Greek science was non-empirical: are they seriously contending that all three Greek-adopted ancient monthlengths were correct to 1^{s} or better

BY ACCIDENT?!

C Two Related Cases of 1% Precision — But Systematic Inaccuracy Eratosthenes' Earth-Size Error Unfaced by Metrological Loyalists

C1 The weird exception to mob-insistence on Greek inaccuracy is modern fealty to the famous myth that Eratosthenes measured the Summer Solstice Sun's ZD (zenith distance, or angular distance from overhead) at Local Apparent Noon (LAN) in Alexandria as 1/50th of a circle while at Aswan's LAN, 5000 stades further south, the Sun was overhead ($ZD = 0°$), so he computed that the Earth's C is 50 times 5000 or 250000 stades (Kleomedes 1.10). That is 16% higher than the actual C, 216000 stades, since the royal stade was 185 meters. But an unending succession of modern scholars have tried to justify it anyway, shrinking the stade **solely to fit Eratosthenes**. The resulting conveniently-small, perfectly mythical "Eratosthenian" stade is on permanent fanatical display in Wikipedia. For the most thorough demonstration that it has always been fiction, see Engels 1985 p.309's table.

C2 The difficulty for stade-scrunchers is that there were not one but two successively-adopted standard ancient values for Earth's circumference: Eratosthenes' too-high 250000 (or so) stades in the 3rd century BC; and Poseidonios' later (1st century BC) too-low 180000 stades (Strabo 2.2.2.). The latter was used in the *Geographical Directory* (*GD*) or "Geography" of astrologer Claudius Ptolemy (c.160 AD and no relation to the Ptolemies who ruled Egypt centuries earlier), which was dominant for 1000^{y} until the Renaissance. A lethal impediment to flexing the stade is that if one chooses a stade smaller than 185m in order to improve 250000 stades' fit to reality, this simultaneously degrades 180000 stades' fit. And vice-versa.

C3 But, as ‡8 [& Rawlins 2018V] demonstrate, there is a potential solution that, even while requiring none of the traditional *ad hoc* shrinking of the 185 meter stade, *simultaneously solves both disparate standard ancient Earth-size values*. **Despite their 40% disagreement**. Without exception, historians-of-science ignore this solution (assuming they even understand the physics), though for nearly 40^{y} pieces of it have appeared in various prominent science sources. (Rawlins 1979. *Scientific American* 1979 May. Later, Thurston 1994E p.120. D.Halliday, R.Resnick, & J.Walker 1997 *Fundamentals of Physics* 5th ed. p.7 & sunset frontispiece. Also www.dioi.org/vols/we0.pdf, Rawlins 2008Q eq.28.) This easy solution is merely: ***airbending of horizontal light*** (atmospheric refraction). But: just how well does this approach satisfy the 2 attested ancient standard Earth-sizes?

C4 We have 2 independent evidences (Rawlins 1982N eq.4; Rawlins 2008Q eqs.10-11) that Eratothenes' raw empirical circumference estimate was not 250000 stades or 252000, but 256000, which **is 19% or nearly 6/5 too high**, while Poseidonios' 180000 stades **is exactly 5/6 too small** — a striking inverse-symmetry ***based on the ratio of 6 to 5***.

C5 These numbers fall right in line with a very simple, spare physical explanation. Sealevel horizontal light is airbent with curvature virtually 1/6 of the Earth's curvature. Thus, for over 100^{y}, navigation manuals have recognized that geometric calculation of the horizon's distance needs adjustment by factor $\sqrt{6/5}$ (S.Newcomb 1906 p.203).

C6 Provocative coincidence: the wondrous Pharos lighthouse at Alexandria *was built at the very time&place of the Eratosthenes C's appearance* (see www.dioi.org/jq00.pdf, §G3 for Shakespeare→Marlowe parallel confluence), and the distance of its flame's visibility could measure Earth-circumference C. (Ancients were aware of such outdoor testing, knowing that a light hung in a receding ship's mast eventually disappears due to the Earth's curvature: Pliny 2.65.164.) But the lighthouse-flame method would yield a size 6/5 too high, since its computation (Rawlins 2008Q eq.2) of the Earth's radius results from squaring the flame's visibility-distance ***in stades***, which also squares §C5's $\sqrt{6/5}$ expansion-factor for v — thereby multiplying the geometrically-calculated radius (*idem*) by 6/5.

C7 But there's an alternate equally precise (and comparably inaccurate) stay-at-home method: a sunset seen from the Pharos' top occurred *more than a time-minute* after one seen from the sealevel base, an easily repeatable empirical measure which would yield an Earth-size too low by 5/6, since computation of Earth's circumference from the twixt-sunsets time-interval (which is proportional to the horizon's $\sqrt{6/5}$-enhanced distance seen from the higher perch) requires division by its square (Rawlins 1979 eq.13). (See fun demo-by-extremes, showing why flame & double-sunset results hugely disagree, at ‡8 §C.) The 180000 stade circumference probably appeared historically later than 256000 stades primarily because the smaller value's calculation needed spherical trig (Rawlins 1979 Tables I&II), for which there is no evidence of existence (§I4 below) as early as Eratosthenes.

C8 The triple-match of theory to *both* attested ancient Earth-sizes ***and*** to the 185 meter stade — to 1% in all 3 cases — provides [a] more evidence of ancient scientists' high scrupulousness & precision, and [b] revelation that modern Eratosthenian-justifier stade-manipulators have spent a century on a fruitless track, as the solution now turns out to be physical not metrological, and is thrice consistent with reality: again, to 1% empirical accuracy. An extra implicit irony: the most famous of ancient astronomical legends, Eratosthenes' experiment, wasn't astronomical.

D Ancient Observatories' Latitudes Found to Ordmag One Arcmin

D1 A further irony of the foregoing is easily-missed: none of the narrowly-focused apologia for Eratosthenes' famous supposed Alexandria-Aswan solar experiment have ever noticed that Alexandria & Aswan actually ***are*** exactly 5000 stades apart in angular latitude, as are Aswan & Meroë. (Rawlins 2009S §C. Strabo 2.1.20.) Both figures are given together at Strabo 2.5.7. By Eratosthenes' 700 stades/degree scale (*idem*), both latitude gaps are $7°1/7$, fitting to c.$1'$ these cities' actual respective north latitudes, $31°12'$, $24°05'$, $16°57'$.

D2 Rounding-accident? No, Greek astronomical latitude-fixes were really that reliable. Misled by the disgraceful ordmag 1° errors in astrologer Ptolemy's *Geographical Directory*, scholars were unaware of this ere 1982. But statistical investigations during that year, Rawlins 1982G n.17 (& Rawlins 1982C Table V's solutions for y), confirmed later by Maeyama 1984 & Brandt *et al* 2014B, have consistently found that all 4 of the astronomers, c.300 BC to c.160 AD, whose star data survive at *Almajest* 7.3&5-8.1, fixed their latitudes to ordmag $1'$ (1 nautical mile), pretelescopic vision's rough limit. (Declination-based results summarized at Rawlins 1994L Table 3; improved at Rawlins 2018D Table 1.) As early as c.300 BC, Timocharis, 1st of the 4, knew Alexandria's latitude exactly: $31°12'$ (§F2).

E Ancient Longitude Accuracy by Lunar-Eclipse Comparison History-of-Science's Backfironic Denial of the Achievement

E1 Running a least-squares on a 16-city sample, Rawlins' 1984 Greenwich Meridian Centenary speech showed (Rawlins 1985G eq.16) anciently tabulated Mediterranean longitudes, from the Roman-Carthaginian region east, were consistent with Greek scientists' ordmag-1°-accurate longitude determinations by comparing lunar eclipses' local times, assuming these had been ignorantly (Rawlins 2018A §F) multiplied by 4/3 or 7/5 by Ptolemy (or Marinos) before coming down to us in his *GD*. This conclusion fits several evidences: [a] The lunar-eclipse method is explicitly recommended by Hipparchos (Strabo 1.1.12). [b] The mean error is roughly as expected for eclipse-based data (Rawlins 1985G §9). [c] Absolute errors of longitude gaps are independent of gaps' sizes (Rawlins 2018A §O), as they should be if eclipse-time-based.

E2 In late 2016, history-of-science's toppe journal, *Isis*, launched an insulting, fake-refereed (Rawlins 2018A Postscript) attack on these solid findings as merely a "delusion", an attack (Shcheglov 2016) revealing that both the author & *Isis* Editor H.F.Cohen lack the most elementary math-science skills. Cohen leapfrogged beyond the sin of non-refereeing by proceeding on to naked coverup, refusing even to acknowledge receipt of our 2017/2/27 letter to him, www.dioi.org/isx2r.pdf, alerting him to *Isis*' 2015&2016 misadventures, or our 2017/3/20-4/1 letter (Rawlins 2018A) to *Isis*' Board, www.dioi.org/islg.doc, correcting Shcheglov 2016 n.8's false science in his claim that eclipse time-gap reports by Kleomedes & Pliny are "badly overestimated". His lethally central, truly astonishing errors (in the sole scientific evidence he brings against Rawlins 1985G): [1] uncomprehendingly treating a solar eclipse as lunar; [2] placing Spain (& two Chinese cities) into the wrong hemisphere.

E3 Incredibly, anyone reading the *Isis* author's own citations (Shcheglov 2016 n.7), to esteemed historian-icon Neugebauer 1975 pp.668&844-848, watches Neugebauer overturn all of Shcheglov's contentions regarding "badly overestimated" eclipse-gaps, just as at Rawlins 2018A §D. So the data esteamed *Isis* **carelessly, ignorantly** threw at long-resented but maddeningly-invulnerable Rawlins 1985G redounded — telling all who's Delusional.

F The 185-Meter Stade: Its Long-Unrealized Potential Implications One More Hint of 1% Precision — *Plus Like Accuracy in This Case*

F1 But how arose the royal Alexandrian 185 meter stade so prominent in §C above? There is much ancient testimony (Strabo 2.5.7; Neugebauer 1975 pp.590 [n.2], 733, 1364 Fig.43) that 300 BC Greek geographers divided terrestrial meridians into 60 parts, not 360. Given that standard Hellenistic fractionalization was sexagesimal, the search for smaller geographical units would continue successive divisions by 60. If the early Ptolemies' surveyors had wished to define a royal stade, they would have done so much as later scientists did — when they defined the meter or the nautical mile — and kept dividing Earth's circumference by 60 until finding a suitable unit. To check the theory, thrice divide 40000000 meters by 60. Result: 185 meters. Admittedly speculative, **this is nonetheless the sole available scientific theory explaining that central Greek unit**. Its implications are even more provocative: how could Greek scientists have found the Earth's circumference to a fraction of 1%? Did Ptolemy I order his new empire surveyed? Martianus Capella 598 refers to "King Ptolemy's surveyors"; and Kleomedes 1.10 hints at such.

F2 The one known Alexandrian scientist (of the four cited at §D2), who, via ringed instruments, measured (Rawlins 1994L §F6) his city's exact latitude correctly on-the-nose as $31°12'$, flourished at the time of Ptolemy I — Timocharis. His −302-271 observations of stars, Moon, & Venus are remembered at *Almajest* 7.1-3 & 10.4. He clearly had the talents and experience to supervise a hypothetical survey, including precise odometer-measure of a meridian arc of 1578 km, traveling (via camel?) from Alexandria due south (along longitude 29°.9 E), *without interruption by the Nile*, to the pre-measured (Strabo 2.1.20;

Rawlins 2008S §§C2-C3) latitude of Meroë. Both cities' latitudes were indeed accurately known (§D1 here), differing by 14°1/4. From such data, simple arithmetic would yield the Earth's circumference: 360°/(14°1/4)·1578 km ≐ 40000000 meters. (And since C was defined as 216000 stades, a check-multiplication by 185m of course finds 40000000m.)

G The Earth's Tilt — Hipparchos' 135 BC Measure

G1 No explicitly attested anciently adopted obliquity was close to the actual value in Hipparchos' time, 23°42′.7. But, over 80[y] ago, a world expert in ancient geographical mss, Diller 1934, discovered that a dozen (Strabo-preserved) data of Hipparchos indicated that his final observed and adopted obliquity was 23°2/3 (Rawlins 2018C Table 1).
G2 If obliquity was accurately measured by the standard solstitial method (*Almajest* 1.12), then, accounting (as at §H3) for atmospheric refraction, the result when ancient-conventionally rounded to the nearest 5′, would be 23°40′, agreeing with Diller's discovery.
G3 Several scientists' (not historians') statistical studies of the Ancient Star Catalog, Hipparchos' *Commentary*, and Pliny's latitude data, all confirmed the same 23°2/3 Hipparchan obliquity: Rawlins 1982C, Nadal & Brunet 1984, Rawlins 1985G, Rawlins 2009S fn 50. And *idem* Tables 1&2 (improved by melding them into Rawlins 2018C Table 1, or www.dioi.org/biv.htm#lkhs) demonstrated that Diller's theory *perfectly satisfies all 14 Strabo-attested data*, ***even ones Diller didn't know of***. Typically, historians-of-science shun him, preferring instead the failed joke-theories of their own people: Neugebauer 1975 (pp.304-6, 334-335, 734 n.14) whose paraplegic formula fit only c.1/2 Strabo's data; or least-squaresless Jones 2002E, whose theory fit *so poorly that he won't even tabulate it*. (And none among the JHAD-rabbitariate will even ask him to.) Unique: Diller, Neugebauer, & Rawlins all tabulate, to check how well their hypotheses fit the Hipparchan data. Despite the Aubrey Diller 1934 theory's obviously superior — indeed flawless — triumph: **in the *eighty-three years* since publication no historian-of-science has ever admitted it**.

H Equinox or Solstice? Historians' Bad Science & Oblivious History

H1 Too many of the most prominent historians (including a MacArthur recipient and the Editor of the *Journal for the History of Astronomy*) aver that ancients observed equinoxes more accurately than solstices, an elementary (R.Newton 1977 pp.81-82) confusion of precision with accuracy: actually believing that the Sun's lack of north-south motion at solstice prevents accurately measuring its time. (Swerdlow 1979 p.527, 2010 p.173, Evans 1998 p.206. Contra R.Newton 1991 fn 20 and Rawlins 2018U eqs.5&21, Table 3, §§D-J.)
H2 Besides Swerdlow's and Evans' patent unfamiliarity with the relevant **SCIENCE** — the simple, well-known equal-altitudes technique (e.g., Bowditch *American Practical Navigator* 1981 ed. vol.2 p.799) — we note also their **HISTORICAL** innocence of the fact that all known ancient scientists found yearlengths via solstices, not equinoxes: Meton, Euktemon, Kallippos, Dionysios, Aristarchos, Hipparchos; and see Astronomical Cuneiform Text #210. Equal-altitudes (§H3 below) is the obvious ancient solstice-fix method. Recently, the details were fully laid out for the 1st time at Rawlins 2018U (eqs.5&10-21), plus invention of an equation for finding the method's modest systematic error (ordmag 1^{h}) due to the Earth's orbital eccentricity (*ibid* eq.10). And those who've long doubted ancients' solstice-accuracy have lately been surprise-confronted with the freshly translated papyrus *P.Fouad* 267A, testifying to a Hipparchos −157/6/26 18^{h} solstice, **accurate to under an hour**. The papyrus also showed he was at this date tabulating Kallippic solar motion. ***NB***: Hipparchos' [a] search for a −157 solstice & [b] use of Kallippic motion, were both predicted years prior by Rawlins 1991W §§K8-K9&M4, from groundbreaking *DIO* analysis of Hipparchos' 2 eclipse trios. Neither [a] nor [b] were previously suspected by anyone. (See www.dioi.org/vin.htm, for several dozen other equally undeniable *DIO* vindications.) *However, no JHA cultist will* ***or can*** *admit either of these 2 ineluctable predictive successes*. [Such behavior is the hallmark — the very definition — of shun-fear-driven robotic cultism.]

H3 Equinoxes were observed on the large public equatorial ring in Alexandria. The sole surviving one, from −145/3/24 11^{h} (*Almajest* 3.1) was off by -4^{h}, but observer-errors were only c.1′, considering all refraction (of Sun's light, plus polestars' light when the ring was originally set). Hipparchos' 14 Rhodos equinoxes show 2^{h} or 2′ scatter. Their systematic error was 7′ (Britton 1967 p.24, R.Newton 1977 p.78, Rawlins 2018U §B4), all but ordmag 1′ of which was from non-observational factors (*idem*), none degrading solstices found by equal-altitudes: measuring the LAN Sun's northness a few weeks before solstice, recording when it's repeated later, and simply taking the mid-time as solstice; these data's ere&aft systematic errors are of virtually equal size but opposite sign, and so cancel each other.
H4 The accuracy (§H2) of papyrus *P.Fouad* 267A's −157 solstice isn't isolated. Reconstructions of Aristarchos', Kallippos', & Hipparchos' solstices show errors of just 0^{h}-3^{h} (Rawlins 2018U Table 3), despite accuracy having been vitiated by the ancient habit of rounding cardinal-point times to the nearest quarter-day. (See *ibid* for full analysis.)

I Eclipse-Fixed Stars: *Journal for the History of Astronomy* Miracles

I1 The most prominent argument for alleged ancient empirical incompetence appeared 30[y] ago (Evans 1987, Evans 1998 p.259), citing 2 awful Hipparchan placements (*Almajest* 3.1) of Spica's ecliptical longitude by measuring its angular distance from the mid-eclipse Moon (virtually 180° from the Sun) during the eclipses of −145/4/21 & −134/3/21. Errors were huge, −33′ & +33′, respectively, thus double-backing the denigrators.
I2 But, just to make sure, the author — today esteamed Editor of the "premier" & pseudo-refereed (Rawlins 2018C fn 4) *Journal for the History of Astronomy* — repeated the experiment himself, outdoors in Seattle, using the 1981/7/16 eclipse to place star λ Sgr with an antique cross-staff, finding that his longitudinal result was "too small by about 40′ " (Evans 1987 p.275 n.50), convincingly in line with the size of Hipparchos' two gross errors.
I3 But, wait a minute: all 3 errors are larger than the Moon! — whose mean diameter is merely 31′, so *in none of the 3 cases did the reported lunar disk overlap or even touch the real one*. Is this credible? — given that the healthy human eye's discernment-limit is better than 1′, & given that all Hipparchos' other lunar observations exhibit errors of merely 0°.1. (Thurston 1998A ⊙11; Rawlins 2009E fn 22.) Can one believe an error exceeding the lunar diameter even once, much less thrice! So we have *three* miracles, felicitously just enough for Evans' official canonization to sainthood, according to longstanding Church practice.
I4 A key proof, of 2nd century BC spherical trigonometry, is Hipparchos' use of lunar parallax tables — still surviving at *Almajest* 2.13. (A collection of prime evidences for spherical trigonometry's 2nd century BC currency is found at www.dioi.org/cot.htm#mmsz.) The Moon is the only natural celestial object that is (long-term) so close to the Earth that the naked eye can easily discern diurnal parallax, the difference between a topocentric position of the Moon seen outdoors from a site on the Earth's surface, vs a calculated geocentric position of the Moon as seen from Earth's center, the viewpoint of ancient and modern ephemerides. Parallax tables conveniently supply said difference so that an outdoor observation can be compared to a calculated geocentric position with parallax added.
I5 But it is easy to instead subtract the tabular parallax by mistake. So, why not test that possibility upon the three observations adduced by our planet's brand-new §I3-saint? The needed data are easily acquired: Hipparchos would pre-calculate mid-eclipse-time from his lunisolar tables; and, by 1981, eclipse mid-time prediction was in the newspapers. Undoing all 3 wrong-signed parallaxes, the errors that were supposed to be −33′, +33′, & −40′ instead (Rawlins 2009E eqs.6-8 & fn 22) drop to, resp, −2′, +1′, & +2′.
I6 Well, does anyone seriously contend that human vision was 10 times worse 2000[y] ago than today? Though none of history-of-astronomy's networkers will ever confront their sly Leader, all who've checked the foregoing long-circulated shocker-facts realize that these have howitzered *JHA*'s pathetic pretensions to integrity & scientific knowledgeability, besides promoting false math in support of §A1's ultimate historical crime of cult-spurning, spitting-on, distorting — indeed ***inverting*** — THE central, overarching truth of ancient

astronomical history (*DIO 1.1* ‡1 fn 24): **Greek scientists' high competence & accuracy**. (More seemingly incredible JHAD inversion-excesses are detailed at Rawlins 2018B §N.) *JHA* hopes *never* to admit *DIO*'s unanswerable success in neat-fit-explaining (§I5) all three Evans 1987-selected hitherto-mysterious observations. Saint Evans intends to keep **HIDING** (*sneeeaky* details at www.dioi.org/jg01.pdf, §A1 & fn 7), fleeing exposure of what he hope-imagines (since colleagues stay silent) is his private guilty secret. Which isn't.

I7 To comprehend the foregoing, recall Hume (*Enquiry Concerning Human Understanding* 10.2; 1740): "knavery and folly of men are such common phenomena, that I should rather believe [miracles] to arise from their concurrence than admit . . . a violation of the laws of nature." For St.Evans' 3 miracles, were the laws of human vision suspended? Or: are even saints — or more exalted yet, Editors — vulnerable to Humean-fallibility?

J Distance to the Sun: the Origin of Order-of-Magnitude

J1 At *Almajest* 5.15, nonastronomer Ptolemy put the Sun 1210^r (Earth-radii) away, affecting 10^r precision where uncertainty is way higher, as genuine ancient scientists knew. For real Greek scientists, solar distance (the AU) was so uncertain, it became the historical origin of order-of-magnitude as ancients resorted to rounding the AU to the nearest power of 10: Eratosthenes 100^r (Rawlins 2008Q eq.11; Carman & St.Evans 2015, inflating a 6-line *DIO* footnote [Rawlins 2008Q fn 6; & eq.9] into a 16pp *Isis* Pb paper); Hipparchos 1000^r (Rawlins 1991W eq.23); Aristarchos, Archimedes, & Poseidonios 10000^r (*Sandreckoner* Archimedes, Heath 1913 p.348, Neugebauer 1975 p.656 eq.16, Rawlins 2008R eqs.13-15).

J2 Aristarchos, a student of vision (Thomas 1939&41 vol.2 pp.2-3), presumably knew that the discernment limit of the human eye is ordmag $1/10000^{th}$ of a radian; thus (Rawlins 1991W fn 272), diurnal parallax's invisibility, given a parallactic baseline of ordmag 1^r, would've been convincing evidence — esp. at Mars stationary points — that the Sun's distance was at least 10000 Earth-radii, ordmagly-right (since the Sun is 23000^r away).

K Stellar Distances: Heliocentrist Trillionfold Universe-Expansion

K1 One reason for geocentrists' resistance to Aristarchos' 3^{rd} century BC announcement, that the Earth went around the Sun, was that it implied the stars would show annual parallax. ***The before-the-nose fact that all the planets exhibited annual parallax had made no impression upon ancient geocentrists***. Or, indeed, upon modern historians-of-science — see Rawlins 1991P §F for an astonishing collection of naïve claims by prominent history-of-science archons that geocentricity-vs-heliocentricity remained an undetermined issue until the 19^{th} century! The Church might relievedly agree: www.dioi.org/vols/w93.pdf, ‡6 fn 75. But J.Bradley's discovery of stellar aberration ended any reasonable scientific debate regarding geomobility, a century before infallible Holy Church quietly turncoated c.1835.

K2 Aristarchos replied to §K1's doubters that stars' parallax was there, but was invisibly tiny from stars' huge remoteness, which, again applying vision's limit [with parallactic baseline ordmag 1 Astron Unit] must be at least 10000 AU distant, ***an epochal discovery***. (Made by learning-from evidence, rather than cult-loyally rejecting it.) Multiplying this by §J2's parallel finding that the AU was at least 10000 Earth-radii: the stars must be at least 10000-squared or 100 million Earth-radii away. Given that annual-parallaxless Ptolemy's stellar distance was ordmag 10000 Earth-radii (Van Helden 1985 p.27 [vs *Almajest* 2.6]), the heliocentric universe's width was ordmag 10000 times greater than geocentrists', & volume greater by ordmag 1000000000000 — a trillion. ***NB***: Connecting Aristarchos-Archimedes remotenesses — 10000^r solar & 10000 AU stellar — to $1/10000^{th}$-radian eye-precision, is another original *DIO* discovery. (Ever uncited.) Why not previously perceived? Well — ever met a historian-of-science who knew the limit of human vision is c.1/10000 radians? For advancing history of science, knowing ***science*** matters. (As above at §H2 & ‡8 §D2.)

L Doing Justice to Ancient Scientists

L1 Historians-of-science treat Aristarchos, the scientist who pioneered heliocentrism and (§B) measured the month to 1 part in millions, as an incompetent fabricator (JHAD denigrations sampled at §A1 above and www.dioi.org/vols/we0.pdf, ‡2 §§A1, A3, & A6), which could be projection at its funniest. Prime basis for such is *On the Sizes and Distances of the Sun and Moon* — hitherto universally accepted as Aristarchos' — which makes the Sun&Moon 2° wide, thus implying wildly unreal events, like lunar eclipses lasting half a day (Neugebauer 1975 p.642), and even (Rawlins 2008R §§C1[e]&C3) *which no historian ever noticed*: **the Moon visibly retrograding every day for Mediterranean observers**.

L2 But this allegedly-Aristarchan opus is contradicted by the greatest ancient mathematician-scientist: *Sandreckoner* reported (Archimedes p.223) that Aristarchos' lunisolar diameter was half a degree, which is correct and is 4 times smaller than *Sizes* claimed. What can explain the 4-factor descent into historians' abuse of Aristarchos? Well, try this theory: a hypothetical student of Aristarchos' work uncomprehendingly extrapolated from it, creating *Sizes* — where the lunisolar diameter is $1/15^{th}$ of a ***part*** of the zodiac (Heath 1913 pp.352-353), thinking "part" meant a zodiacal sign, 30° wide. But the real Aristarchos he copied this datum from was referring instead to an ancient angular unit known as the "part" or meros (μερος), $1/48^{th}$ of a circle, or $7^\circ 1/2$ (Neugebauer 1975 pp.652&671) — and $1/15^{th}$ of that is a half-degree, in accord with both reality and Archimedes' testimony.

L3 The myth of unempirical Greek science has endured for decades due to a confusion of ancient pseudo-science with the real thing, a confusion which began (ScAm 1979) as a means of ameliorating the error-enormity of Ptolemy's fabrications, by claiming that it was normal to preserve & report only data agreeing with prevailing theory. This is not only [a] logically impossible, for the self-evident reason given above (§A3), but [b] contrary to well-known ancient science, as Hipparchos reported data disagreeing with his theories & even with each other (*Almajest* 3.1, 4.11, 6.9), & [c] disproven by all sections of this paper.

L4 The gulf between fake & real ancient science is typified by the following: the 4 allegedly outdoor "observations" of the Sun (*Almajest* 3.1&7) by history-of-science & AAS icon (and astrology-bible author) Ptolemy, "The Greatest Astronomer of Antiquity" (Neugebauer 1975 p.931, echoed verbatim by Gingerich 1976 p.477 & Gingerich 2002 p.70) are ***50 times closer*** to positions calculable *indoors* from Hipparchos' obsolete 280^y old solar tables (*Almajest* 3.2&6) than they were to the Sun's actual positions in the real outdoor sky, so their errors average c.1° (the tables' mean error in 160 AD: Thurston 1998A ⊙1), ***twice the Sun's angular diameter***. Those historians who think this is science are less likely to look askance at the still-unretracted 1987 *Journal for the History of Astronomy* proposal (§I above) by *JHA*'s present Editor, that Hipparchos, a genuine outdoor observer, would commit equally outré ocular errors, ***contra his customary accuracy*** (§I3 above). Context: Hipparchos' Rhodos equinox observations are only 3 times closer to his tables (versus Ptolemy's 50), and even that only because, after all, the tables are computationally based (Neugebauer 1975 p.58) upon his slightly imperfect outdoor observations (as of 146 BC).

L5 The astronomical and geographical errors of occultists have nothing to do with the brilliant work evaluated above, and should never be melded with it. One hopes that the foregoing survey's refinement-revelation of ancient science, shorn of superstition, will encourage future historians' realistic and grateful appreciation of ancient astronomy — man's 1^{st} attainment of precise universal prediction, the heart of mathematical science.

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‡10 Cook's Farthest North

Introduction

Cook&Peary: the Polar Controversy, Resolved Mechanicsburg 1997 by Robert M. Bryce (RMB) was ambitiously titled but in the event did just what that title promised, ending the Polar Controversy by establishing in overwhelming detail that both Cook and Peary not only failed to reach the North Pole in 1908 and 1909 but faked their claimed successes. (The actual 1st North Pole attainers were Norway's Roald Amundsen, the US' Lincoln Ellsworth, & Italy's Umberto Nobile in the dirigible *Norge* on 1926 May 12.)

DIO 21 ‡ 4 = www.dioi.org/jL04.pdf published in 2017 *DIO* Editor Bryce's autobiographical history of his personal entrancement by the Cook saga, also an account of his laborious (dedicatedly self-funded) retrieval from Danish archives of a copy of original 1908 Cook diary material, his decypherments (of often murky script), plus a map & tabular précis (pp.54-55&p.61) of Bob's largely very well reasoned reconstruction of the actual 1908 Arctic trip of Frederick Cook, before he emerged 1909/9/1 from the Arctic to hoax the world with a tale of reaching the North Pole 1908/4/21. Bob's account of the trip is based on Cook's private, long unplumbed original 1908 notebooks, largely written while wintering 1908-1909 in a hut at Cape Hardy, on the north (Jones Sound) side of Devon Island at c.76°N. This research was the most significant advance since 1909 into determining Cook's actual 1908 movements: from his leaving Annoatok, Greenland, to sledge west across northern Canada's Ellesmere Island, thence NW to Cape Thomas Hubbard (CTH), virtually the northernmost point of Axel Heiberg Island — the last geographical place he reached in the Spring of 1908 about which there is no disagreement.

DIO 21 pp.54-55 Fig.2 printed a map exactly taken from p.348 of Bob's pioneering 2013 book *The Lost Notebook of Frederick A. Cook*. Many details of the Greenland→CTH part of this map are new and well-induced. But it goes on to extend Cook's trip ***92 nautical miles*** (nmi) beyond CTH, in a mostly-west direction, specifically about 20°-30° north of due west or roughly WNW, partly based upon the same Cook diary matter cited above. On this basis, *Lost* pp.339-340 is convinced ***Cook went c.100 nmi out onto the Arctic Ocean***, where he encountered chaotic ice associated with the previously unknown Beaufort Gyre, within c.480 nmi of the North Pole or closer, ***placing him at 82°N (& c.102°W), farthest north his alleged "North Pole" trip ever actually achieved & location of a supposed original discovery***, so it cannot justly be deemed a mere sideshow, & loyalty to a speculation despite coherent contrary data confirms that it isn't, as does its prominent mapping as a genuine extremal Cook achievement on *DIO 21* ‡4 = www.dioi.org/jL04.pdf Fig.2 (Bryce's original version: www.dioi.org/rmbmap.jpg), centerfold map of a scientifically refereed journal.

DIO's Publisher D.Rawlins has long contended that Cook never in his life set foot on the Arctic Ocean's ice past the rough-ice edge at CTH, nor (e.g., *Peary . . . Fiction* 1973 p.92) was he ever within 500 nautical miles of the North Pole he faked a 1908 conquest of.

Cook's proposed 92 nmi trip drastically conflicts with that reported from Cook's two 1908-trip Eskimo companions, Etukishuk&Ahwelah, who allegedly said that Cook went merely about ***10 nautical miles*** northwest of CTH — not even significantly past the smooth land-ice around CTH — before returning to CTH, and went west of South from there before heading generally South (§17) until reaching Jones Sound (c.76°N) for the winter. That testimony appeared pictorially via what will below be called the "Eskimo Map" (EskMap: www.dioi.org/eskmap.jpg), Cook's alleged 1908 path, as drawn in 1909 under the supervision of competitor Peary's men: Henson, Borup, MacMillan, Bartlett. EskMap reproductions are found at D.Rawlins *Peary at the North Pole: Fact or Fiction?* Wash. DC 1973 p.94; R.M.Bryce (RMB) *Cook&Peary* p.424; R.M.Bryce *The Lost Notebook of Dr. Frederick A. Cook* 2013 p.343.

Since the Editor and Publisher have a collegial but irresolvable disagreement regarding whether to believe Cook's diaries or the EskMap on this point, it seemed best that *DIO* publish both sides, as each party agreed after unbudging exchanges of emails.

These viewpoints follow below:

Editor R.Bryce's View 2021/6/23

1 I'm sorry you are still so obsessed with this. As I mentioned, the trip I *hypothesized* is neither "provable" or unprovable because it is an unwitnessed assertion. To make the categorical statement that it is a "fantasy" is only supportable if you have a preponderance of actual evidence that shows it wasn't a *possibility*. As I have stated over and over, I believe there is a preponderance of evidence that my hypothesized trip is not only reasonable but likely.

2 You cite Cook's position readings in *MAP*[1] [*My Attainment of the Pole* 1911-1913] as provable evidence [2021/6/22 DR email §A4]. You have to keep in mind that Cook always claimed he went to the North Pole in *MAP* and forever after, or, in his words, reached a point reasonably close to it. Therefore, naturally, he wouldn't tell you where he really went. The precedent for this is that he didn't tell where he really went on the way to CTH [Cape Thomas Hubbard], or where he really went on the way back from whatever point he did reach. But those can be deduced from his original diary. In fact, you give Cook more credit than he is due by saying he could travel due north at all. Dead reckoning is, of course, just a guess, based on an estimate of distance traveled. Without knowing the exact direction you are traveling, it doesn't give any basis for saying where you actually are, but just how far you have gone. Cook by his own admission traveled by [dead reckoning] and compass (he says so in *MAP*), he didn't ever know exactly where he was because he couldn't fix his location by celestial observations. So the mere citing of latitudinal positions without any supporting data is proof of nothing (as he learned when he submitted his unsupported claimed navigational positions to Copenhagen). The only statement of latitude he makes in Notebook III is that he thinks he is by dead reckoning 50 miles from his starting point when his two extra Eskimos leave him, and therefore at "82.10 lt". Cook was so navigationally ignorant, he may not have taken into account that if he traveled somewhat west of his starting point (which he had to do if he was aiming for the 95$^{\text{th}}$[W] Meridian) he was not making a minute of latitude north for every mile traveled. So that is not evidence that he went due north.

3 It's "BAD science" to carry prejudice into an argument rather than looking at all the "evidence" that can be gleaned from all extant sources before deciding what opinions may be labelled "fantasy." According to Webster, a fantasy involves "a process of creating especially unrealistic or improbably mental images in response to psychological need." As such, the western trip doen't fit the definition, because it is neither especialy unrealistic or improbable, but has ample circumstantial evidence including eyewitness testimony, albeit often contradictory. So it's not a fantasy; it's a well-considered opinion. Any opinion on where Cook actually went (and opinion is all it can be) should never contain pejorative language; it should only contain evidence-based statements. And you really haven't considered all the evidence, you know.

4 Let us take just for instance Peary's own "evidence" that appeared in October 1909 intended to prove Cook didn't reach the Pole. Here's what it said about his trip out from CTH: "After sleeping at the camp where the last two Eskimos turned back, Dr. Cook and the two boys went in a northerly or northwesterly dircction with two sledges and twenty dogs, one more march when they encountered rough ice and a lead of open water. They did not enter this rough ice, or cross the lead, but turned westward to Heiberg Land at a point west of where they had left the cache and where the four men turned back." Keep in mind that in the testimony written down on the spot by Borup in August 1909, Inughito said that he and Koolootingwah went two sleeps north with Cook, Etukishuk, and Ahwelah, and turned back at the end of the third day without sharing his camp, just as Cook said in Notebook III. So, if, as it says in Peary's statement, they made one more march from "the camp where the last two Eskimos turned back," that is verification of at least 4 marches, which directly

[1] DR: The only 6/22 citation (§A6; also www.dioi.org/brzvg.tex §G4 item [1]) using *MAP* data was for CTH's latitude. No alternate CTH latitude has been found in the diaries.

contradicts your previous statements of belief in MacMillan's claim of 10 miles and one sleep from land (even Peary gave him credit for 2 sleeps!). In Cook's narrative he claimed to be 92 miles from shore at the end of the fourth march. That's why Captain Hall settled upon 92 miles as the minimum for Cook's verifiable distance of travel on the ice from eyewitnesses. But notice that from where he met the lead he, according to Peary, "turned westward" and though it adds "for Heiberg Land," Heiberg Island is not west of even their starting position. Furthermore, in the published Borup testimony there is the flat statement: "Itookishoo says . . . from Cape Hubbard they went Northwest." None of this is speculation; all of it is *alleged* eyewitness testimony that the westward trip I outlined [is real], and therefore [not] an especially unrealistic or improbable "fantasy."

5 No use going through the points you make [2021/6/22 email: www.dioi.org/brzum.pdf], here. As I said, if you object to my stating what I believe is supportable by evidence (I could bring to bear much more than the above), you should put in an editorial note saying you don't believe a word of it. You are entitled to you opinion, as am I, and leave it at that. As I mentioned, if you want to present your best evidence-based opinion against the trip, in detail, I would be happy to present my opinion of why I believe otherwise if I don't agree. That would allow the readers of *DIO* to decide which opinion they think the more likely, instead of retracting that portion of the map, because without a convincing counter-argument as to why there is so much evidence for the trip and confirmation of all its physical descriptions by later travelers, I stand by it. Because I am the author, to retract the map outright is to say I am admitting I am wrong. But I'm not admitting that. I don't mind being proven wrong at all based on evidence (I already modified many of my statements in *Cook&Peary* on this subject in 2013 and stated clearly that the latter is what I now believed to be most likely upon further study). But without explaining away all the eyewitness testimony and scores of "coincidences" that support it, I stand by my opinion.

6 And again, no one is going to "attack" *DIO* over the map. Stating an evidence-based opinion is not the same as stating a fact. I published the same map in *The Polar Record* (another refereed journal of presumably far wider circulation more than a year before *DIO* Vol.21), and to date I am unaware of a single word being raised against it.

7 As to Cook being "the better observer" [B5] compared with MacMillan, that was in the context of their differing explanations of the abrupt chaotic disturbance of the ice about 95 miles to the northwest of CTH after similarly described "good going" up to that point. MacMillan attributed it to the ice's "passage over shoal ground" associated with a vast unknown landmass to the west,[2] whereas Cook attributed it to "an unknown westerly current." MacMillan was wrong: there's no landmass or shoal ground; Cook was right; it's exactly there that the then-unknown Beaufort Gyre, caused by a huge trapped body of inflowing freshwater, radically disrupts the ice. If Cook's observation was a dead guess, what is the probability of the "guess" being correct, even though you claim Cook was never anywhere near the area, and MacMillan's "observation" being dead wrong, when he was verifiably on the very spot? Just another meaningless coincidence, I suppose.

[2] DR: Looks as if MacMillan was still dreaming that Crocker Land was just over the horizon.

Publisher D.Rawlins' View 2021/7/14 rev. 7/16

1 Based on the 1909 testimony and path-drawing by Cook's two 1908 Eskimo travel-companions, the Eskimo Map details Cook's 1908 movements after reaching Cape Hubbard. This 1909 map was unquestionably-vindicated — **beyond a document-verifier's wildest prayer** — via surprise later discovery that its placement and shape of hitherto-unknown Meighen Island were both so astonishingly accurate that the thereby-shock-refuted Cook Society never[1] — to its dying day — gave up suspicion that[2] both position's & shape's matches with reality MUSTA[3] been based on Peary having secretly discovered&mapped Meighen Island in 1906. This theory would require Peary hating Cook so much that he'd relinquish his career's only unchallengeable land-discovery — AND somehow knowing (years before their conflict) just where Cook would be in 1908. The theory is taken seriously in detail in *C&P* 1997 pp.1111-1112, where p.1111 says (emph added) "Meighen Island ***mysteriously*** appeared" on the EskMap.

2 *Lost* evidently no longer flirts with that extravagantly ornate (fn 12) theory — but replaces it with another: the fragile, delicately-induced Beaufort-Gyre-discovery theory (§21), which requires severely doubting the 2 Eskimos' distance-estimating ability until they (after returning to CTH) reach Meighen Island, when said ability suddenly materializes astonishingly precisely, as they map the island quite accurately. This division of the EskMap, into [1] the CookSoc-doubted-10 nmi-trip-off-CTH part vs [2] the rock-solid Meighen part, is required since the defender of Cook's c.100 nmi trip from CTH believes that a PORTION of the post-CTH content of ONE of Cook's North-Pole-fake notebooks is an accurate record of a genuine, risky trip, allegedly motivated in part by a self-educational purpose (§19 item [1]).

3 The crucial Eskimo Map ***unquestionably proves*** that ***NORTH*** Pole claimant Cook ***discovered Meighen Island due to his SOUTH-seeking*** post-CTH movements (§17) — a discovery which Cook deceitfully pretended never happened! To isolate the vindicated part of EskMap, *Lost* surgically divides the map in two (§2), arguing the pre-Meighen-discovery half showing Cook going only 10 nmi is a Peary-clique lie since Cook actually went from CTH c.100 nmi mostly west ***even though he believed he was heading northward*** (§28), thereby genuinely discovering something quite else[4] than Meighen Island, namely, the Beaufort Gyre's chaotically rough ice.

4 Over the Publisher's insufficiently firm protest (www.dioi.org/jL03.pdf fn 33), the c.100 nmi achievement's proposer unambiguously ***mapped*** that ***hypothesized*** WNW trip upon www.dioi.org/jL04.pdf Fig.2, based on a 6$^{\rm d}$ *section* (***all of whose dates are admittedly faked***: e.g., www.dioi.org/jL04.pdf §N1) of the 4$^{\rm th}$ of 5 diaries by which Cook sculpted (§10) a world-infamous hoax-trip to the North Pole.

5 The believer's extensive 2021/6/23 email (www.dioi.org/0br.pdf) entails Cook possessing [1] accurate distance-dead-reckoning but AWFUL direction-finding, heading mostly Westward though thinking he was [2] really going due Northward, while his 2 Eskimo companions had the inverse ratio of capacities: rotten distance-estimation, [3] misgauging 92or114 nmi as 10 nmi (!!!) since Eskimos are incompetent[5] distance-estimators, but accurately drawing the (amazingly-shrunk) c.100 nmi trip's ***direction*** as [4] to the NW.

6 Now, we need merely select [1]&[4], while de-selecting [2]&[3] — & we've got a preliminary brief for Cook having pioneered virtually the same round-trip that Peary's Donald MacMillan was fated to perform, 6$^{\rm y}$ later, in 1914.

7 *Lost Notebook* 2013 p.339 speaks of Cook's route from Cape Thomas Hubbard (CTH) as the "northwestern course . . . that he claimed in all of his accounts. . . ." But *Lost*'s case for Cook's drastic westwardness from CTH is based not on his but others' testimony:
[a] The EskMap does show Cook going NW (though just 10 nmi).
[b] But there's no mention of nontrivial westwardness in his notebooks or later works.
[c] RMB's new ms[6] on the 1914 Crocker Land expedition says [§1F]: "In a [1908/2/20] letter left for Knud Rasmussen . . . [Cook] wrote of a 'strong possibility' of finding much land west of Crocker Land. Clearly, Cook saw [NW] Crocker Land as a stepping stone[7] to the pole." Yet the actual letter (*Lost* p.99) muses that this "remote" possibility might offer a stepping stone to Alaska — not the Pole — hinting he saw it only as an aid for return ***from*** the Pole.

8 The 6/23 email's case for eliciting 4 marches (beyond CTH) out of testimony for 2 sleeps plus a 3$^{\rm rd}$-day stayover is weakened not only by being slip-vulnerably-verbal[8] (as against the pictorial clarity of the ***carefully-considered*** EskMap) but also by potential confusion of "sleeps" and marches (e.g., Herbert *Noose* 1991 Grafton ed, p.316). Further, there is the difficulty of knowing which cape RMB says Cook started from (www.dioi.org/j934.pdf §S3 [Cape Stallworthy] vs www.dioi.org/jL02.pdf §F1 [Cape Hubbard]), as well as from what point on that cape. The key, thankfully-unambiguous part of the Eskimo-testimony-based Peary quote cited by RMB's 6/23 email is (emph added): "After sleeping at the camp where . . . [native temps Inugito&Koolootingwah] turned back, Dr. Cook and the two boys [Etukishuk&Ahwelah] went in a northerly or northwesterly dirction with two sledges and twenty dogs, one more march ***when they encountered rough ice and a lead of open water. They did not enter this rough ice, or cross the lead, but turned westward***[9] ***to Heiberg Land. . . .*** I.e., Cook stopped at the land-ice edge, as soon as he saw the Arctic Ocean.
[c] The EskMap shows a (tiny) NW course, but there's not a hint of serious westward travel in Cook's Notes III ≈ notebook 4, documenting the alleged trip, nor is there any in its coverage 24$^{\rm y}$ ago at *Cook&Peary* (*C&P*) pp.969-975 which instead asks (p.969 emph added) "just how far ***north*** did Dr.Cook go in 1908?"

9 Both www.dioi.org/jL02.pdf §F2 and the 2021/6/23 email suggest that Cook's allegedly reaching 95°W longitude (after 50 nmi out from CTH) evidences west-aim. Yet how would someone who supposedly couldn't even handle latitude (adding westward miles to compute it!) find his way to a specified new longitude? And: when faking Notes III, what did Cook think CTH's longitude was? Presumably 95°W! (*What else explains how*

[1] Changing one's mind from learning of contrary hard data is one thing. Abandoning adherence to a fave theory on probability grounds is another, e.g., resorting to saying no absolute contrary proof exists is akin to asserting you're Galileo's resurrection and no one can prove you aren't.

[2] Meighen I. was the only remaining unknown land in the Arctic, so there's no question of confusion with another land.

[3] Did Cook react to EskMap's 1909/10/13 national publication by asking Peary to debate it? No. Instead, within weeks, he grabbed his lie-gotten profits and fled into hiding with them for a year, sculpting his eventual counter-exposure alibis just as he'd previously concocted his hoax-trip. But: did even excuse-fertile Cook ever suggest that Peary had discovered Meighen Island and kept it secret for years, just to later pounce on Cook?

[4] In early teens, DR saw the 1950 film *Rocketship X-M*, imagining the 1$^{\rm st}$ trip to the Moon: the space-ship goes "off-course" and ends up on Mars.

[5] An early Cook-apology by sportsman & Cook-helper H.Whitney is selected to cement that defense-useful verity: *Lost Notebook* [2013] pp.340&344, *C&P* p.856.

[6] "The Crocker Land Diaries: Fudging, Faking, Myths, & Murder in the Far North; New Discoveries About an Imaginary Place" — accessible on Bob Bryce's website.

[7] *Idem* also suggests that Peary should — if he really believed in Crocker Land — have used it in 1909 to reach the Pole. Yet, quite aside from the effort to reach it, one notes that his map with 1907's *Nearest the Pole* makes its northern terminus' latitude trivially higher than Cape Columbia's.

[8] E.g., Cook-defense notes that MacM reports one of Cook's Eskimo companions once mis-stated that Meighen Island was *northwest* of CTH. Obviously just a sloppy MacM typo. What is that and similarly inchoate junk worth, by contrast to the EXACT 1916-surprise-vindication of the EskMap by its placement of Meighen Island?

[9] On the EskMap the "westward" turn at CTH (really more south than west, to Cape Northwest [which is indeed west of CTH, contra 6/23 email] on Heiberg's west coast) occurred after encountering the land-ice edge, so Peary's earlier noting "one more march" beyond where Cook's extra Eskimos Inugito&Koolootingwah turned back, is saying that — however many sleeps or marches occurred — Inugito&Koolootingwah never got to that land-ice edge. ***And neither ever testified that they had***.

he dead-reckoned to 95°W? — even though he later [in *MAP* 1911] pretended he went west to 95°W intentionally.) Even if he originally had CTH's longitude correct (any evidence of that in Notes III?), then as remarked in DR's 2021/6/22 email (§A5) being on 95°W after 50 nmi would require a path no more than c.10° west of true North (not 60° west thereof!) In Notes III, his position at 95°W isn't described as aimed-at but seems meant by Cook to indicate he's been on a virtually northward path all along since leaving CTH. But even if he went for 95°W from 94°20′W: cosine 10° being 0.98, his simple addition of distances is valid [contra 6/23 email]. Notes III (out to at least c.200 nmi) provides not a single longitude after citing 95°W, when he claims 50 nmi north. You'd think a dead-reckoner moving mostly west might deadreckon a longitude or so along the way. But: nothing at all besides the lonely reference to 95°W, implying that his estimated longitude remained 95°W for Notes III's c.200 nmi, which would naturally be so if he's heading north. (Or even just thinks so.)

10 So "notebook 4" isn't a record of a westward trip that discovered the Beaufort Gyre & outdid unsmart MacMillan's observations out-west. To repeat: it's just a fragment of the 4th of 5 Cook notebooks recording evolution of the 520 nmi BIG ***North*** Pole LIE, by this notorious would-be thief of Peary's N.Pole (& of Hudson Stuck's Mt.McKinley), who took money from widows in the stock-reloading scheme that sent him to jail, after which he had the unprincipled brass (like Liberace) to torture with libel suits writers who simply told the truth re his fakes. So DR regards Frederick Cook as a low example of humanity. RMB sees him more broadly, as at *Lost* p.342: during 1908-1909 wintering at Cape Hardy (76°N) "Since Cook had a rich inner life & an infinite capacity for self-expression and embellishment of his already extraordinary experiences, he was no doubt content to have the winter to try out his [NORTH pole] story in his five unused notebooks and with each successive version, perfect the details he would tell the world upon his return."

11 The westish trip is unverifiable by any of 4 normal criteria, all readily-available in 1908 — but all ***deliberately*** prevented by Cook: [a] Literate witness.[10] Rudolph Franke: *Lost* pp.60&280. *Au contraire*, his 2 actual witnesses both told everyone Cook lied. Cook's failure to take a literate witness on the major part of his expedition is unique in classic polar history, but is nonetheless alibied at *C&P* pp.854f (as if Franke was the world's only dog-capable & literate potential witness). That Cook planned zero literate witnesses is attested by backer J.Bradley at *C&P* p.855.

[b] Sextant. Which Cook carried: *MAP* p.499, *Lost* p.52. To adduce his inability to use it as a DEFENSE of his westish-trip isn't going to impress scientists.

[c] Depth-sounding. (Which MacMillan bungled but at least tried.)

[d] Photograph. Below: §§13&15.

[e] Cairn-record. Cook deliberately left no written record at any place visited during his entire 1908 trip. (Possibly 1st noted in DR's 1973 book *Peary . . . Fiction* p.83?) The significance of this blank appears when RMB's ms (§8C) quotes from MacMillan's comments during his&Green's 1914 search for Peary's 1906 "cone-shaped rock cairn, such as all explorers build to mark the site of an important discovery, and beneath which a written record is left as proof [of] one's presence there which, incidentally is the only possible proof." Did Cook create such a cairn-record upon his "return" to land from the N.Pole? Of course not. After all, he'd never seriously left land in the 1st place!

12 Why attempt to alibi & thus make a genuine discoverer out of someone who ***arranges non-verifiability***? — not even offering sextant data, which are not only evidence but are ever-so slightly-helpful for navigating. The entire 100 nmi-beyond-Cape-Hubbard claim falls as did his McKinley&Pole claims because (*C&P* p.859): "he never offered anything in [their] defense beyond his bare word." However, *idem* then adds an anti-positivism: but this doesn't prove he didn't succeed! Which just allows[11] fabricators, thieves, & con-men

[10] Same for the lying Sourdoughs: *DIO 23* www.dioi.org/jn00.pdf §6F.

[11] Not even booster Izzy Bowman was that naïve: when he found from private expert scientific advice (*DIO 24* www.dioi.org/jo00.pdf §V9) that Peary's N.Pole claim depended on just his word, Izzy wrote

to rake in profits until disproof appears, at which point they scram with the boodle. (But what would this have to do with rich-inner-life Frederick Cook?)

13 Wait a minute! Granting a pass on verification-need to the most infamous exploration-liar in history?! When he claims he sledged hundreds of miles through the hummocky pressure-ridges of the Arctic Ocean but didn't think to photograph any travel except over the smooth flat ice parts (like that found near land), as with the photos at *My Attainment of the Pole* 1911-1913 opp pp.173, 204-5, 236-7, 268. Peary's 1910 *North Pole* had photos of his fight thru horrid rough ice at pp.215-216, 224-225, 240. So did MacMillan in 1914, when he photographed his own sledge fight against such ice: see cover of David Welky's 2017 book, *A Wretched and Precarious Situation* 2017 (or ms Fig.13). Yet re the 114 nmi trip, it's protested: *this blank doesn't PROVE Cook didn't travel thru rough ice when the camera wasn't looking*. Every travel-moment when the camera WAS looking, Cook just happened to be going along on smooth ice. Believing 2021/6/6&23 emails deem the trip "unwitnessed", thus ranking Eskimo testimony as worthless (fn 18) — except for the part that supports the desired hypothesis: §5 item [4]. But in the absence of DISproof, we must take the liar's tale seriously, even publicly, unqualifiedly mapping it as if real. Not exactly standard science, where (*DIO 21* ‡3 [2017] = www.dioi.org/jL03.pdf §D3):

The burden of proof is always on the claimant not the critic.

14 The published 1909 October EskMap, based on Cook's 2 Eskimo companions' witness, shows that Cook went only about 10 nmi from CTH before stopping at the 1st rough ice — and then turned southish ***taking care to stay always on the firm, smooth land-ice***, NEVER ONCE going onto the chaotic, hummocky, moving ice of the Arctic Ocean. But EskMap-skeptics have spent decades compiling an equally chaotic collection of disconnected fleck-snippets (e.g., fn 8) of contrary testimonies. (Consistent with no alternate coherent theory of the Eskimo Map's basis — just intended to lower the credibility of a document CookSoc has disbelieved for over 100^y, since it was distributed in 1909 by Peary's "minions": *Lost* p.344.) But the Eskimo Map places unknown Meighen Island at exactly the right place: 80°N, 99°W — extraordinary magic for allegedly distance-deficient (§5) Eskimos. So the defender says: OK, ***that*** part of the Eskimo Map has (undeniably) accurate distances (contra verbal snippetdom), but the part[12] that shows Cook going just 10 nmi to the NW (stopping at the end of smooth land-ice) is slightly misgauged by the natives: shrunk from an actual trip over 10 times bigger. They just forgot. Or massively misestimated distance. Or EskMap-supervisor-witnesses&minions Henson-Borup-Bartlett-MacMillan risked their reputations by conspiring to secretly erase the westish trip from a hypothetical original Eskimo Map, being willing to destroy a scientific record ***in order to fake the vital insult that Cook missed the Pole by 520 nmi instead of by merely 480 nmi.*** Or they intimidated the Eskimos. Or bribed them. Or something.

But we MUST get as complicated as it takes (e.g, §1), to reject the obvious option of just accepting the spectacularly surprise-vindicated (*idem*) EskMap.

15 By adducing a mass of (incoherently contradictory) flecks of inconsistencies by various parties (even suggestions that Peary secretly discovered Meighen I. before 1908 (e.g., *idem*) to explain the map's precision, traditional Cookite literature & recent emails have tried chipping away at the trustworthiness of the Eskimo Map. This is a classic case of varying evidence-weights. The lack of ANY photographs of Cook traveling through rough ice[13] is not just something one can brush-off as a trifle that doesn't PROVE he never

explicitly that it would never pass a competent exam, and so he hid the Peary records for the rest of his life.

[12] See Occam's Razor at *DIO 21* ‡3 [2017] = www.dioi.org/jL03.pdf §§D4-D5.

[13] Cook's exclusively-smooth-ice sledging photos are excused at *Lost* p.341 as: taken to CONFIRM the important discovery of how much easy ice he found. Never mind it's the rough ice that is alleged to prove his immortal Beaufort Gyre discovery (*Lost* p.340). And photos of such would dramatize & help sell his tale. Actually, the smooth photos do tell one tale: just as his Eskimos said, he did a land-ice trip from CTH on.

went onto the Arctic Ocean. (As CookSoc Pres Russ Gibbons passed off Cook's lack[14] of a photo of McKinley's top or its vista by claiming it doesn't ***prove*** Cook didn't climb McKinley! *NYT* 1998/11/28 p.1.) *To a scientist*, Cook's lack of rough-ice-travel photos (www.dioi.org/jL03.pdf fn 33) is far weightier than the random heap of inconsistencies combined, *because it backs up the Eskimo Map's direct witness that Cook never went onto the Arctic Ocean*. That's **coherence**. Which always outweighs scatter-shot incoherence.

16 Further coherences:
[1] Cook's 1908 turning-south at the very point when travel became tough is consistent with his 1906 turnback at the prospect of severely steep grades beyond the Gateway at McKinley's bottom.
[2] All 1906 camps were also on level ice: www.dioi.org/jL03.pdf fn 33.
[3] Due to his generally agreed-to inexperience with the sextant, Cook had no business risking his life hundreds of miles from land in any clime. There's no credible evidence that he had ever gone alone even 5 nmi over the calmest ocean. So it seems incongruous that he would debut his oceanic travel-talent in 1908 by going over ***100 nmi out*** onto the dangerous, frigid, pressure-ridged, shifting, sledge-breaking, Eskimo-terrifying, open-lead-riddled **ARCTIC OCEAN.**
[4] All these signs of incredibility are in the context of Cook's general record of fraudulence.

17 As for the claim (§5) that, after traveling intentionally West across Ellesmere, then NW to CTH, Cook lost his bearings by over 60° when he unintentionally traveled mostly west ***thinking he was going north***. But, as soon as he got back to CTH, he had an orientation-epiphany, suddenly refinding his North-South meridian, thence averaging close to due South (§3) for c.300 nmi until reaching Jones Sound & the chance of return to Greenland without crossing mountains.

18 Note there was nothing (§7) like a westish trip in the treatment (of the same Cook diaries) in 1997's *C&P*, which at p.975 credits him with (contra Stallworthy's subsequently-foundational 82°-limit testimony: §24) attaining "82°50′ North".[15] And Notes III, as summarized at *Lost* p.332, takes Cook not 92 nmi[16] but instead c.200 nmi from CTH: Cook added 7^d or 5^d further of "good progress" (c.100 nmi, figuring at 19 nmi/day: *Lost* p.331). All northward, thus well past 84°N, perhaps to 85°N. (*Lost* p.236 evidently distrusts post-114 nmi entries since print is smaller & they switch to past tense, though previous days' entries also have past tenses.) So we are supposed to trust the (sometimes re-written) polar notebooks from Greenland to 114 nmi out, but regard all beyond as lies.

19 *Lost* p.340 suggests Cook's purposes for the westish trip were to make his N.Pole hoax more credible by [1] getting a feel for Arctic Ocean travel so he could write realistically of such (as if a few miles wouldn't suffice), and [2] convincing Etukishuk&Ahwelah that they'd made a long trip (which worked so effectively that they reported it as 10 nmi). *Idem* also says that after the westward trip Cook, "turned back for Cape Thomas Hubbard." But none of his diaries ever say anything about turning around[17] — or about arriving back at CTH.

[14] No Cook fan ever recognized the import, to the Mt.McKinley flap, of Cook's lack of a single photo beyond the Gateway, but instead concocted remote excuses re bad film, etc: see R.Bryce's trenchant comments on invincible Cook-Believer Ted Heckathorn at www.dioi.org/j727.pdf fn 49. Given that the recent ms is offering Cook as discoverer of BG rough ice at his farthest, isn't there a never-say-die analogy to being pushed at this late date into accepting that discoverer Cook went west while doing north-math?

[15] Rounding of *C&P* p.973's latitude, "92 miles offshore, at approximately 82°52′ north latitude."

[16] *C&P* p.974: "about 95 miles offshore . . . the conjectural point where he turned back."

[17] Instead of recording a turnback to CTH, Notes III describes going on ahead (*Lost* p.332) for at least c.100 nmi more. But an advocate adding-in needed details to get a required result out of a document isn't new. See *DIO*'s study of the National Geographic-sponsored NavFou's fleshing-out "invisible" data in Peary's 1894/12/10 Greenland transit data, to convert them into a "time-sight" record: www.dioi.org/jL05.pdf §C8.

20 An 6/23 email responds to this point by arguing that N.Pole-fake-intent Cook isn't telling anyone (even a secret notebook) where he's really going, which seems at odds with the premis that Notes III recounts 92 nmi west to Beaufort. But maybe the idea is that Cook wrote up the W-to-Beaufort jaunt DISGUISED to Look-Like just another northward, no-return-until-Pole-reached link in his 520 nmi N.Pole hoax.

21 At c.100 nmi from CTH, Cook meets the hitherto-unknown Beaufort Gyre and so is its discoverer. There is no suggestion (yet) that it be renamed "Cook Gyre" — but *C&P* p.974 claims: "His description . . . corresponds almost exactly" with later-verified reality. (By contrast, EskMap's far more exact Meighen-later-verification is doubted in the same work: *C&P* pp.1111-1112.) *C&P* p.975: "All of these physical descriptions support every feature of these eight pages of Cook's diary." This sweepingly inclusive and highly enthusiastic 1997 interpretation of Cook's diary is for 92 nmi NORTH of Cape Thomas Hubbard [& *ibid* p.974].

22 But by 2013 the same Beaufort-Gyre-discovery (*Lost* p.340) becomes 92 nmi or 114 nmi mostly-WEST of Cape Hubbard. The trip is but an "unwitnessed[18] assertion" (2021/6/6 email), a hypothesis said (2021/6/23 email) to be neither provable nor unprovable, though here easily disproven (by, e.g., §29) and which is glaringly without verification by sextant, sounding, or photograph, and specifically denied by both his 1908 companion-witnesses. It was nonetheless mapped ***as fact*** on *DIO 21* ‡4 (www.dioi.org/jL04.pdf) Fig.2,[19] on the CENTERFOLD of a scientific journal whose Publisher is dedicated to careful, honest, & ever-corrective refereeing (which Bryce has been a central & invaluable — indeed amazing — part of for years: just count the curly brackets [signifying Bryce's corrections of DR] at *DIO 21* ‡3 = www.dioi.org/jL03.pdf). Urgings of the 92 nmi jaunt's removal from *DIO 21* ‡4 = www.dioi.org/jL04.pdf Fig.2 have met with the author's position that he continues to "stand by" (2021/6/23 email) the mapping.

23 How, between 1997 & 2013, did the huge discontinuity (switching from NORTH to WEST) happen to RMB's reconstruction of the initial $6^d$of Cook's post-CTH travel?
The oddity here (as DR is ashamed to admit he only realized 2021/6/23): Cook's revised path ends up nearly identical to MacMillan's 1914 westish trip — even though Cook thought it was northward and much longer.

24 The ***nakedly isolated origin*** of the idea that Cook went over 60° west of north for c.100 nmi invites close examination:
[1] Taking CTH's latitude to be 81°22′ (ms §11C), *Lost* p.339 makes overexact *to-the-**arcmin*** the rough guess — rounded *to-the-**degree***[20] — of Mountie (RCMP) Harry Stallworthy's guess that Cook turned back "APPROXIMATELY in latitude 82° North" (W.Herbert *loc cit* caps added) thus 38 nmi north of CTH (*Lost* p.339). Stallworthy's precision-lassitude corresponds to between 81°1/2 N & 82°1/2 N. So, instead of exactly 38 nmi, the guess-distance North of CTH is 38 nmi±30 nmi, or between 8 nmi & 68 nmi, which doesn't even conflict with EskMap's 10 nmi.
[2] To achieve a net northward-component of 38 nmi, an unverified & merely dead-reckoned 92 nmi of bee-line travel distance (alleged in one of the 5 diaries of a ***proven*** liar) must proceed in a direction about 24° north of west, since $\arcsin(38/92) = 24°.4$ or via sphtrig $\arccos(\tan\ 0°.6333/\tan\ 1°.533) = 24°.4$. (For 114 nmi, it's 19° north of west.)
MacMillan, Green, & Etukishuk went 85 nmi to their semi-well-located 1914/4/22 camp (MacM *Bull.AMNH 56*:393, 1928), 22° north of west from CTH. Pretty close to the hypothesized Cook trip's direction.

[18] The 1908 100 nmi trip was "unwitnessed"?? Despite all 3 of his 1906&1908 companion-witnesses denying he went anywhere near his claimed achievements?

[19] The depiction is based on the 1997 estimate of a 92 nmi trip — before it was decided in 2013 that "22" (*C&P* p.972) in the diary for March 22 wasn't the day of the month but 22 nmi — which was then added to 92 nmi to make the trip 114 nmi (*Lost* pp.331&344).

[20] Stallworthy could hardly guess 81°N instead, since that's south of CTH!

25 But Cook was never there. The alleged match is plainly just a coincidence,[21] based on selecting out — & making overexact (§24) as effectively 82°.00000 — one single part of Stallworthy's APPROXIMATE guess, while ignoring that [a] he never said anything about Cook traveling a considerable distance (and demonstrably reported that the 2 Eskimos told him otherwise: *idem* or Herbert *loc cit*), &
[b] he never had any notion that Cook traveled westward (since all Cook public post-CTH travel claims [which Stallworthy knew about] were Northward). By phone 1972/11/6, DR interviewed Stallworthy, who confirmed Cook's Eskimos told him that Cook went just a short distance: nowhere near 100 nmi.
[c] *Lost* inexplicably regards Stallworthy's 82°N as an **ENDLESS** NON-GREATCIRCLE of latitude, without considering Stallworthy's bases for his estimate, namely, his presumptions of [1] Cook's NORTHERN AIM & [2] SHORT TRIP. Both led to 82°N (§26). But both presumptions are contradicted at *Lost* p.339.

26 So the proposed geographical agreement of MacMillan's 1914 position with Cook's 1908 position is based on a *triangular internally-contradictory patchwork* mix of Stallworthy's SHORT (*idem*) northward-from-CTH distance with a liar's diary-claim of 92-114 nmi which directly contradicts the brevity of Stallworthy's report. To repeat: given said brevity, it's obvious that Stallworthy's APPROXIMATE 82°N was on the assumption of northward travel — so to swing that path ***over 60° to the west*** (which 6/23's email calls "somewhat west"), just to triangularly agree with Cook's unverified claim of a 92-114 nmi dash, is a mutilation of Stallworthy's testimony. He would be shocked at this: ***had he believed Cook went westish, his minor estimate of Cook's distance from CTH would have entailed a farthest-north-latitude considerably less than 82°N.*** That's why *Lost*'s use of a rigid, East-West-perpendicular, endless 82°N bound — which Stallworty plainly never envisioned — to argue for a long Cook trip, is unwarranted. And odd.

27 Yet the resulting alleged coincidence of Cook&MacM paths is being used to paint MacMillan's mistaken theory of the cause of rough ice at farthest west as inferior to Cook's at the same place, though Cook was never there. As for MacMillan (a mere geologist): the succession of condemnable fumblings&rewrites in his reports are further portrayed, at §39D in the new ms (based on Bryce's thorough examination of a mass of original documents) as similar to Peary's out&out lies, when MacMillan misestimates his distance from CTH (primarily due to companion-navigator Fitzhugh Green's frightened but mathematically competent computations) — though all MacM's off&on exaggerated distances simply increase the likelihood that Peary's Crocker Land doesn't exist, not news that would be well-received by Peary or AGS' Bowman!

28 The idea that Cook had no clue whether he was going North or westish is novel. So's the idea that this is why he added WEST-miles to find his NORTH latitude. This agile dodge of new data, that are contra the 100 nmi west hypothesis, would've been more convincing if 1997's *C&P* or 2013's *Lost* had recognized the 2 examples of said additions ere being informed of them years later, in 2021, which then **FORCED** instant resort to the (***hitherto-nonexistent***) *adding-West-miles-to-North-latitude alibi*, an alibi exclamatorily anticipated in DR's 6/22 email (www.dioi.org/brzum.pdf) at §A8, but as a joke too outré for anyone to adopt it. FORCED because: without it, the entire claim of Cook's 100 nmi westish trip (*right TO* MacMillan's 1914 spot, 6^{y} before MacM) would have to be abandoned.

29 Prior to DR's 2021 June investigation, no referee (***including DR***) had noticed or collated the double-perfect matches of Cook's navigational math:
[1] he placed Cape Thomas Hubbard at latitude 81°20′N (*My Attainment of the Pole* 1911-1913 p.194);
[2] both his conversions-to-latitude of miles-gained — simply adding the latter to 81°20′N — produced PRECISELY the latitudes he cites at *C&P* pp.970&973:
81°20′N + 50 nmi = 82°10′N latitude; 81°20′N + 92 nmi = 82°52′N latitude.

[21] Pure coincidences do occur in science, e.g., Percival Lowell's predicted Pluto orbit; J.Swift's semi-guess that Mars had two moons, one revolving faster than the planet spins.

30 Such new&precise info has produced no Beaufort-theory-adjustment. But the claim that Cook didn't know where he was going is curiously at odds with §29's competent math — precise, computationally-correct meridian-math deductions from accurate CTH latitude.

31 Actually, Cook said he went compass-south since he believed that near CTH the compass variation was around 180° (*C&P* pp.864f). In fact, it was rather less than 180°, to the west, so his compass-south procedure would've taken him a few degrees east of true North — nowhere near the 2013-proposed path, which is 60°-70° west of North.

32 Is the defense claiming that Cook ignored his compass? His 1906 records show he could take accurate compass data: www.dioi.org/j739.pdf fn 26. In 1908, en route to CTH from Greenland, he would have had repeated chances to observe the compass' direction at noon when the Sun is south. (The directions of noon&midnight are each pretty obvious: 1/2 way between those of sunset&sunrise.) The compass' direction wouldn't change drastically from day to day. So, by the time he got to CTH,[22] he'd know roughly how many degrees the compass deviated from south or north.

33 On the reconstructed CTH-departure date 1908/4/13 (*Lost* p.349), the Midnight Sun was 1° above the north point of the horizon. But if Cook was so disoriented that he headed 60° to the west (left) of actual North (thinking he was going due North), then when he was traveling Sunward (supposing it was Midnight), the Sun would be 5° above the horizon. Hard to miss a difference that's about 8 times the Sun's diameter.

34 And the noon Sun, culminating at 18° altitude — *noticeably 17° higher than the midnight Sun* — would be only 30° from (south of) due left of Cook's alleged course, instead of (as for northward traveling) behind him. I.e., he would be out in his clock-time by c.4^{h}. Yet never noticing. What explorer deserves to be implicitly accused of being such a dunce? (For a parallel defense, there's J.C.Adams' no-longer-accepted claim to Neptune's discovery, where genius Adams' #1 defender pleads him "like a very simpleton" to try saving his claim: www.dioi.org/j911.pdf §H12.)

35 However, that Cook *added* — as arcmin — his number of nautical miles allegedly traveled ***north*** onto his ***north*** latitude, would suggest to most scientific referees that whatever Cook's ethical shortcomings (§10), he possessed an elementary grasp of meridian navigation.

Why deny him the previously-unsuspected credit due?

[22] After the trip from Greenland to CTH, local noon (12^{h}) would occur c.$13^{h}1/2$ on Cook's timepieces had they been set for Annoatok local time.

Editor Bob Bryce replies 2021/7/14

I really don't want to waste more time on an issue that is not provable in the end. What's the use of arguing over an unprovable point? As I said I'm entitled to my opinion and you to yours. And I'm not interested in being "right" either, because there is no right or wrong in a case as this. I, at least, am willing to admit that your opinion might be correct, but I very seriously doubt it because of the fact that [it] is specifically contradicted by several eyewitnesses. As to your latest piece [the foregoing analysis], all I will say is *assumptions* are not *evidence*, and assumptions are far more in evidence in it than actual evidence; nothing in it changes my opinion that I have publicly stated several times already. Beyond that, at the moment I'm not inclined to spend hours and hours formally organizing and presenting another review of the actual evidence in favor of my opinion (which is quite substantial, though it could never be proof of an unwitnessed assertion), involving integration and examination of some that has never been published before,[23] and which, to be done properly, would necessitate additional research at sites holding needed documentation to augment my incomplete files.

[*DIO* looks forward to eventual materialization of that research, which we will of course publish or link.]

[23] DR: See Stefansson at www.dioi.org/jo00.pdf §Q1.

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