DIO
The International Journal of Scientific History

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—Accurate Ancient Astronomical Achievements: Badmathed by JHA and Isis
Download DIO 21, www.dioi.org/vols/wL0.pdf, to click on links.

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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 12.4 (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 1st 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!) followed by The Lost Island of Columbus: Solving the Mystery of Guanahani, http://columbuslandfall.com/ccnaw/author.shtml, a typically well-reasoned book on his prime life’s-project: where on 1492/10/12 did Columbus 1st 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 1st paper on the Columbus mystery appeared in DIO 4.1 2, www.dioi.org/vols/w41.pdf, in 1994, 23y 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

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.

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 dialogues, 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 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 as 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 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创新. 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抄写者

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’ 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 untrained 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 author 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 [BB.] 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 Copyists are corrupt. I made a mistake in one 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 notes appended to the end of this article: §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

F Even copyists need knowledge

F1 Copying from other non-fiction titles is one thing, but when an author indiscrimi­
nately 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 Verhoef was from St. Louis, rather than Louisville, shows Hen­
derson 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 children’s books that have no pretension to being truthful (apparently children
are in even less need of Truth than adults), like J. Alvin Kugelmann’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 expedi­
tions 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 Kugelmann’s book for children (recommended for “Grade 7 and
up” and penned 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 Kugelmann’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.
Robert M. Bryce  “A Hack Job”  2017 December  DIO 21 ¶1

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.

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.

1  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
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 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 without notation 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.


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 pagination. 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 copyst, 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 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.

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
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 humdrum 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 the 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.” [In *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 its 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 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.

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 acknowledgments, we are told “the resources [sic] in the photo section are labeled with [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 organization 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, biassed 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 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.”

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 “Dagtitorssoq and Inghruit: 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 bold-faced question), there is nothing in his paper that could change anything in that book, though the converse is certainly not true.

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 §6 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 and the coordinator of the program 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. 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 Cook’s [sic] claim to have traveled from Svartrevoeg to the North Pole.” [It should be noted that throughout his oral remarks and printed paper, Shoemaker uses Svartrevoeg for Cape Stalworthly (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 Stalworthly, 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 Svartrevoeg 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
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 [C3]) 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 xes 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 “we can’t get into that in a few minutes,” and added something that sounded impertinent, 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

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1FACS 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 its 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 its geography at a different site? (www.dj-webphotograph.com/wright/photograph/33-w93.pdf, DIO 9.2 §4 [Fig.5].)”

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 a magnetic Meridian method ‘liar’ 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 a better land to use such a method. The next question asked if the navigational 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 Heckathom 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 if 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 H “Ice Islands from the Ellesmere Breakoff: Was Cook’s ‘Bradley Land’ a Sighting?” below). What follows has absolutely 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 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.

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 theory is that: Cook was a liar — even though Peary said he was.”
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 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 had 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 §5§C&R] dealt with aspects of this paper at length. Apparently my letter to Heckathorn 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.

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 19th 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 “Erie”; 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’ “shag-la-hutte” (a huge liar) is now enshrined in print on p.77 of Report #18. (DIO 9.2 §4 L3.)

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 §1&2] which had 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 large 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 by 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, Athenaeum, 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 rôles 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 rôles 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
Robert M. Bryce  “Ignored” no more.  2017 December  DIO 21 ¶2

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 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/author­says­photo­confirm­s­mt­mckinley­hoax­in­1908.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.]

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 amnesia 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, non 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 inquiries 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, moral 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 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 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 informed 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 discussion. But the audience was not made up and took 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.” He 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.

N5 In that respect Russell Gibbons exceeded his expectations, and BPRC later made every effort to neglige 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, calling respect for Peary “a real blood and guts performance” and possessing 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.

N6 Each panelist was allowed a question for the others. Rawlins initiated a discussion of Cook’s gross errors in his published “observations” in MAP [§1 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 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 Last 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, 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 mic phones, 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.

3Brown was not an astronomer, so Cook’s refraction errors (§3 §C7) made no impression. See www.dioi.org/vols/w93.pdf, DIO 9.3 16 fn 18.
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 long-ouster. 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 §2 A1) but embarrass and harm innocent peripheral individuals 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 arenas). But some of us don’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 summed 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-sled journey that included roughly 1200 nmi of sea-ice travel, though his previous sea-ice experience (on the 1897-1899 Belge expedition, without dogs) was ordmag 1 percent of such a distance. No self-sustained dog-sled 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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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 role in the “theft” of the Yaghan 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 selection 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.
- Gilber: 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 conflicting 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 actually delivered a paper in October 1993: For: Cook-Dorough, Shoemaker, Heckathorn (by implication)

Against: Rawlins, Herbert, Malaurie

Neutral: Myerson, Gilber, 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.
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.3 Note: the records of Cook’s arch-rival, Robert Peary, are brimming with triangulation-trig 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. See8 DIO 1/4 and DIO 2/15 & 18.8B. Also Washington Post 1993/6/1 p.3, Science 260:1587, 1993/6/11 [& NYT 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 ([§§B4&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

7 My certainty [§9 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 sifs] 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).


10 Belmore Browne reports on one of his post-expedition encounters with Cook (Exp.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 impressed . . . . [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) one of the Club’s 1915 & 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 Exp.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 semipolular Explorers Club and

[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 During11 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 Club’s special committee investigating 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 learned12 (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 (Exp.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 flag13 into his rucksack — a flag which next appears in his “Top” photograph (Cook 1908 opp. p.227), evidently14 shot on 1906/9/12.

B7 Cook & Barrill reappeared15 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

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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: [a] not a geographical society (a point explicitly noted at the Peary Hearings), and [b] foreign.

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 risen 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 gentlemen preferred, however, to await the appearance of Dr.Cook’s book.” (By which time, Cook had departed civilization, to launch his McKinley fame: Freeman 1961 p.89. See also below at fn 30.) These four records were made during the 1909/10/12-12/24 Explorers Club sum. p.183 quotes item 12 (from NYT 1909/12/25:3-1), except for the last sentence.


13 Rawlins loc. cit. H.Parker reported (Exp.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: Exp.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 of Belmont 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 (hyp)ometer.)17 Barrill later confessed in detail18 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 & conspiracy20 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.”)25 But the Peary-power- clique's interest (interestingly enough 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 criticisms26 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 photo27 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 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 Cook's [1906] 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.178&283), allegedly devoted to “see [Peary] through” the Cook Controversy, is a fantasy based upon the oral recollection of octogenarian Cook-believer Clark Brown. (Cited ibid p.321 n.3.) This version of vast sums bestowed 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 had already unaccountably transferred attention from his own exploration-claims' shortcomings, taking every possible opportunity to spotlight Cook's instead.


21 See DIO 1.1 §4B.

22 See Barrill 1988, p.81, where the Fake Peak is clearly marked with an arrow. Arrow also [at www.dio.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 Okonow 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: [I2 [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 this remark to [Cook] that the eight peaks (including Mt.Grosvenor) on the other side of this point where I had been photographed the "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 taking his Arctic photos, too: Washburns 1989, p.90.]) The version of the "Top" photo published in Cook 1907 was cropped. (For similar case, see C68.) But the 1908 version inadvertently got published with the summit of Mt.Grosvenor showing at the lower-right corner of the picture.

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.

\(^{30}\) Hunt 1981 p.111. (Date at p.271 is a misprint, perhaps for 1909/12/29?)

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

\(^{31}\) Cook called 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.

\(^{33}\) I suspect that the photos opp. Cook 1911 pp.172&332 were taken at this place. Note that none of the photos in Cook 1911 show his party going \textit{up}over \textit{pressure ridges} or negotiating of obstacles. Cook says (pp.226-229) that all the 1906/09-18 camps were on pretty level ice. (Which, independently of Eskimo testimony adds confirmatory evidence that Cook never got far from land.) See p.122 of Washburn 1989.) Compare to Peary’s efforts during his hardfought 1909 drive to get as far north as possible, as shown in Peary 1910 via photos opp. pp.216, 224, 240, 306, 308, 309. [Further, Cook’s photos (Cook 1911 opp. pp.204&205, 236&237) show his sledge-dogs always widely fanned-out when pulling, since he’s always sledding over smooth ice, not negotiating a delile. (Peary’s dogs unfanned, apt to rough ice.) Contrary picture at Bryce 2013 p.339 (C&P p.882) is not a Cook 1908 photo [but an excellent 1912 drawing by W.R. Leigh]. Added 2020/2/12: Cook’s 1908-trip photos aren’t of known landmarks (e.g., Cape Hubbard) where shadows could betray his false timetable.

\(^{34}\) Cook said he hoped to travel poleward along a nonexistent “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).
C4 Cook’s eventual story has him passing by Crocker Land (at speeds which, though occasionally rather high, are more reasonable\(^3^3\) than some of Peary’s ludicrous 1906 & 1909 claims in this regard\(^3^4\) and then discovering his own nonexistent land, “Bradley Land”, farther north. Another mystery: since sledding along a land’s ice-foot is much speedier and less wearing than sea-ice travel, and since land may have game,\(^3^5\) 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 Cookeites report his regard 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) [\(^{[1][1]}\)]. 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 such land nowhere 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),\(^3^6\) 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\(^3^7\) 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 pre-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 the 288th 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.\(^3^8\)

\(^34\)See just irony at Freeman 1961 p.138.
\(^36\)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 Herlant Land.”
\(^38\)As before, perturbed planetary numbers, e.g., 2000 as a proxy for 1800.
\(^39\)Cook’s sestant was later recovered (& publicly displayed in Copenhagen), without artificial horizon.

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).\(^4^1\) For the alleged 1908/4/21 noon solar altitude, 12°, the correct\(^4^2\) refraction (at the reported temperature & atmospheric pressure) would have been 5° — but Cook 1911 (pp.292&302) instead plagiarized\(^4^3\) 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.)\(^4^4\) Moreover, Cook (1911 p.302) claims that he applied the same 9° refraction-correction to 7 pairs of such sextant observations (in 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” 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!\(^4^5\) The 1908/4&14 alleged observational data (both double-limb double-altitudes by sextant) appear in Fig.1 here, photographed 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 supposed to be taken with an artificial horizon,\(^4^6\) 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 — anywhere — could have made the [blunder responsible for this].”)\(^4^7\)

C8 In his book’s later editions, Cook “corrected” this slip (in 53 here & Fig.1) but without ever telling readers of his error or its patchup. [Same dishonest ploy by today’s toppy 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

\(^4^1\)Now found in most of the world’s navigational manuals, this simple format, for computing refraction \(r\) as a function of altitude \(h\), is: \(r = \alpha \cos(h + b/(h + c))\). 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) \(\text{ff}\) [\(\text{ff}\)] in 17 [\(\text{ff}\)] (subsequently refined by the late Keith Pickering).

\(^4^2\)By the 1992 Rawlins refraction formulae cited at fn 41, refraction \(r = 5.4\).\(^4^3\)Note that Cook 1911 p.292 [\(\text{photo opp.p.364}\) & p.245 also copies Peary’s [ultra-]minor error (1910 p.362) of applying refraction-correction after (not before) semidiameter-correction.\(^4^4\)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 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 pre-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.\(^4^5\)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 \(\{2\}\) 3I1] 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 \(\{S8\}\) 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.)
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 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 naive 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.

Cook frequently gives longitudes precisely to the arcminute even while closely approaching the Pole, though all navigators know the folly of this. 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 fn 18 in 31.)

In his first account (NYHerald 1909/9/21:1:4), 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, was placed at 90°.”

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

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 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., 1974/11/19; see also Rawlins 1973 p.80, years before the Court Papers’ unsealing) no more to be found than Cook’s alleged N.Pole navigational 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.

49See above at fn 34.

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

51Nonetheless, 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 ±3 meters (±1/2′)!

52See page 66 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.

53The 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, & 4/21-3, 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).
This matter goes to the heart of the Cook claim, and it ought to be faced by his defenders. I quoted Dunn & Parker earlier (§§B&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 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* for Cook’s friends (including even his lawyer, Henry Wellington Wack, who saw Cook frequently at this time). Loose reported 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 send Loose’s fakes to his Copenhagen judges. And he didn’t convince them.

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. (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.

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 Loose, describing his needs: “Svartevag [sic], start March 17-18. Strong wind — Haze. March 30 — obs. Lat & Long. daily observations to April 23.” Though the incident is highly suspicious no further comment is offered.

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54 As first uncovered in 1935 by grammarians 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.


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 does, 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.”

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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 whirldrift 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*; *Free- man* 1961 p.171; questioned by *C&P* p.910], *Cook* (*Rawlins* *loc cît*) “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.” 60 (For the evolution 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 nerve 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.)

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


60 In early version of *Cook*’s gelatinous account of the disposition of his records, he told Danish astronomer E. Strømgren 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.)

Cook claims 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.de.Long 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, see §4 8K1, there is another, equally-solid Never: the Cook claim will not & cannot be accepted in scientific circles.

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. [c] All Cook’s companions (1906 & 1908) were intimidated, misquoted, or bribed46 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.

But there is a much simpler theory which easily explains an otherwise ultracomplex 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.
65 See above at §§B8&D4.
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 first time I would read that book. I obtained a personal copy at an old
bookstore I visited to use when I lived near the ancient mill town of Elickcity. Mr. Doeds,
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 mendacious 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 unconvinc ed 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,
that said, 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 Polar Controversy, and the Peary named 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 decided as important that it had been kept in a safe deposit box along with Peary’s original North Pole diary, separated 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 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.

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

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 (12 §J5), and Henson’s long-anticipated 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.)


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
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 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.
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 Rigsharkivet, 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.I.” 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.II,” 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 (§4). 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 Rigsharkivet 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.
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 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 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 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, 94.9°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.

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 [Cåfion 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 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.

<table>
<thead>
<tr>
<th>Camp#</th>
<th>Date</th>
<th>1908 Camp Name Given by Cook (if any)</th>
<th>Miles</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Feb 26</td>
<td>On the ice of Smith Sound about 20 mi NE of Amnoatak</td>
<td>20</td>
</tr>
<tr>
<td>2</td>
<td>Feb 27-28</td>
<td>At Peary’s caboose at Payer Harbour on Pim Island</td>
<td>30</td>
</tr>
<tr>
<td>3</td>
<td>Feb 29</td>
<td>6 miles above Cape Rutherford on Buchanan Bay</td>
<td>18</td>
</tr>
<tr>
<td>4</td>
<td>Mar 1</td>
<td>Cape Koldewey near the Weyprecht Islands</td>
<td>20</td>
</tr>
<tr>
<td>5</td>
<td>Mar 2</td>
<td>On a small island off Cape Koldewey</td>
<td>6</td>
</tr>
<tr>
<td>6</td>
<td>Mar 3</td>
<td>In Flagger Bay off the coast of Knud Peninsula</td>
<td>18</td>
</tr>
<tr>
<td>7</td>
<td>Mar 4-6</td>
<td>A mile from the head of Flagger Bay</td>
<td>25</td>
</tr>
<tr>
<td>8</td>
<td>Mar 7-8</td>
<td>“Divide Camp” 20 mi into Sverdrup Pass</td>
<td>21</td>
</tr>
<tr>
<td>9</td>
<td>Mar 9-10</td>
<td>“Musk Ox Camp” 25 mi into Sverdrup Pass</td>
<td>5</td>
</tr>
<tr>
<td>10</td>
<td>Mar 11-12</td>
<td>“Storm Camp” 45 mi into Sverdrup Pass</td>
<td>20</td>
</tr>
<tr>
<td>11</td>
<td>Mar 13-17</td>
<td>“Glacier Camp” on east side of glacier blocking the valley</td>
<td>7</td>
</tr>
<tr>
<td>12</td>
<td>Mar 18</td>
<td>On the west side of the same glacier above Bay Fjord</td>
<td>19</td>
</tr>
<tr>
<td>13</td>
<td>Mar 19-22</td>
<td>In Irene Bay, 3 mi into Bay Fjord</td>
<td>23</td>
</tr>
<tr>
<td>14</td>
<td>Mar 23</td>
<td>“Bear Camp” 30 mi into Bay Fjord on the south shore</td>
<td>27</td>
</tr>
<tr>
<td>15</td>
<td>Mar 24</td>
<td>Near the junction of Bay Fjord and Eureka Sound</td>
<td>25</td>
</tr>
<tr>
<td>16</td>
<td>Mar 25-26</td>
<td>Near the north cape of Vesle Fjord</td>
<td>15</td>
</tr>
<tr>
<td>17</td>
<td>Mar 27</td>
<td>Northeast of Depot Point</td>
<td>35</td>
</tr>
<tr>
<td>18</td>
<td>Mar 28</td>
<td>Near the northeast cape of Sdlire Fjord</td>
<td>38</td>
</tr>
<tr>
<td>19</td>
<td>Mar 29</td>
<td>Near the junction of Eureka Sound and Greely Fjord</td>
<td>21</td>
</tr>
<tr>
<td>20</td>
<td>Mar 30</td>
<td>In upper reaches of Cannon Fjord NE of Cape Lockwood</td>
<td>40</td>
</tr>
<tr>
<td>21</td>
<td>Mar 31</td>
<td>Near Caledonia Bay on the eastern shore of Cannon Fjord</td>
<td>26</td>
</tr>
<tr>
<td>22</td>
<td>Apr 1</td>
<td>Near the location of Camp 20</td>
<td>26</td>
</tr>
<tr>
<td>23</td>
<td>Apr 2-3</td>
<td>“Berg Point” at the mouth of Greely Fjord</td>
<td>35</td>
</tr>
<tr>
<td>24</td>
<td>Apr 4</td>
<td>On the west coast of Schei Peninsula</td>
<td>35</td>
</tr>
<tr>
<td>25</td>
<td>Apr 5</td>
<td>At the cache site at the SW corner of Schei Peninsula</td>
<td>20</td>
</tr>
<tr>
<td>26</td>
<td>Apr 7</td>
<td>Near the cape of Stangs Fjord</td>
<td>40</td>
</tr>
<tr>
<td>27</td>
<td>Apr 7</td>
<td>Off Grant Land [north Ellesmere] near Stangs Fjord</td>
<td>26</td>
</tr>
<tr>
<td>28</td>
<td>Apr 8</td>
<td>Near the north cape of Otto Fjord</td>
<td>25</td>
</tr>
<tr>
<td>29</td>
<td>Apr 9</td>
<td>Near White Point</td>
<td>20</td>
</tr>
<tr>
<td>30</td>
<td>Apr 10</td>
<td>At the base of the Svartevoeg Cliffs</td>
<td>27</td>
</tr>
<tr>
<td>31</td>
<td>Apr 11-12</td>
<td>“Cache Point” near Cape Thomas Hubbard</td>
<td>22</td>
</tr>
<tr>
<td>32</td>
<td>Apr 13</td>
<td>Cook &amp; 4 Inuit depart Cape Hubbard, onto the Arctic Ocean</td>
<td>25</td>
</tr>
</tbody>
</table>
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.

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 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?

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.

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.
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 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.Peyre. A failed, amateurishly refereed attempt to snow scientists & public with Unimpeachable Expertise, resulting in a NavFou report which NGS still refuses 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 window-dressing 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 of research into the Amerigo Vespucci controversy, illustrating his points with a score of slides, as well as the planetarium’s reproduction of 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 DVD 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 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
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 Archeoscientific astronomical methods is a ‘first’.”

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°20’ or 1600 [nautilian] miles east of Davies’ deduced Brazil position, the longitude of Liberia (Africa). Since I made this little item public (1989/12/11), Davies has been asked by reporters about his paper, his longtime former (pre-Peary) pet “historical detective story” (DVC 10) project; but he refuses [like §3 §13] to answer press questions.

One isn’t accustomed to [seeing US Admirals departing] under fire.

1973 book Pearls at the North Pole: Fact or Fiction? was appraised similarly, e.g., by geographer Wm.Wantorz in the 1975/5 Annals Assoc Amer Geogr 65:179. “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 paper (which even 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 Ptolomy [in his crude delineation of China] calls the Cape of Cattegata [Geogr Dir 7.1.3.5: the Chinese anchorage, Kattigara: Καττηγαρα ορμος Σινων] (which is near the Great Bay [Geogr Dir 7.1.3.5] and c.11° E and 55° S) on the longitude of Liberia (Africa). Since I made this little item public (1989/12/11), Davies has been asked by reporters about his paper, his longtime former (pre-Peary) pet “historical detective story” (DVC 10) project; but he refuses [like §3 §13] to answer press questions.

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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 (and previously unknown) perturbation terms contained elsewhere 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 appointed12 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 non-events. But Tom Davies’ highly-expert, astro­of-the­art, 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&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 herefore 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 1899/12/11 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) omits the most elementary corrective 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 prefaches 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[. . . .].”

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 [NaviFou] “investigation” of Peary’s 1909 North Pole claim. Baltimore Mag flacks for Davies thumly (BM 1989/7 p.86): “The major difference between his [Davies’] investigation and all the previous ones is that his [computer] has 2 stellar altitudes (906 & 257) to perceive that in 1.25 hours of (say) 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 that at latitude 78°N (in the Arctic) would Betelgeux and (NF’s proposed 2°) BetDoc star Ras Alhague have such near-identical double-altitudes & corrected for dpk&pkf shown on the BetDoc, the former event 44° before the latter (as on BetDoc.]

14 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 36 [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: §B1, §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 Almagest 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 [Almagest] 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).

**almagast 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, 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 on 2 widely separated occasions (37° 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 §III-B4, Davies contracted events, separated by hours [In 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 unwise, for the real purpose of the whole work sounds suspiciously like the 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° 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.

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16Davies (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 (GeogrDis 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) 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.

17The 136 AD greatest evening elongation of Venus, which Ptolemy dates to +136/12/25 & +136/11/18 (Alm 10.1.2).

18For 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.
I might add that, 2 millennia ago, the best ancient Greek astronomers, by competent use of observational lenses (whether parallax doesn’t affect the time of the event) mapped longitudes to an accuracy of roughly a half degree. This is about 60 times better accuracy than that of Adm.Davies’ impressively attired “modern methodology” calculation.

I will also remark that over 5 years ago (Queen’s Quarterly 1984/12), I playfully criticized most modern astrolongers 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 astrolongers 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.)

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.)

Was Davies later apprised of his “Colossal error” (to quote a 1989 Davies attack upon a seemingly vulnerable quarry?) 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 not be given a copy. Davies’ excuse? — he hadn’t finished the paper yet. (No hint that the paper was grandly miscarried.) Question: was Davies worried about finishing the paper? — or about the paper finishing him?

C Vespucci’s Alleged Observation & Calculation

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

“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 . . . 23.


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

National Public Radio 1989/2/2. 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 computing 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.

25 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.iiii). (DVC 6 calls the letter’s genuineness “unquestioned”).

26. Reminds one of the prime issue raised by Mencken’s unstopperable Bath Tub 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; 1916. (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.

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 [Regionum], which was composed for the meridian of Ferrara [actually Nürnberg] correcting with calculations from the tables of King Alfonso [the ‘Alphonsine’ tables].

.. 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.

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.”

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°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.”

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 Portuguese 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
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) 870 leagues30 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 1/2).31 [c] It is impossible for the Moon to have moved nearly 4 1/2 in the reported 4 1/2 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.32 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 dissociate 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 Vespucci 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 Πόλης Ἑπετήμιον Heidelberg 1929 e.g., pp.52, 184, 189. Morison says that Vespucci “was fond of airings his classical knowledge, though it was a mere smattering” — some correct and incorrect Vespucci references to the classics are provided.) The text continues (iden): “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 Regionontanus’ tables, the lunar conjunction (angular distance from the Sun) for the wrong (1499/8/22-23) Nürnberg midnighth. (The time of Moon-Mars conjunction predicted in Regionontanus’ aspect-tables is the following midnighth: 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 reconstruction (DVD 8-9) of Vespucci’s math. (We have another example of NF reconstructions: 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: “I degree and some minutes”) and generously rounds it up to 1 1/4 (DVD 8), thus 2 1/2 of lunar motion (says Davies: DVD 9). The moonrise observation was reported by Vespucci as occurring 1 1/2 after sunset, which was at 18:03 (local mean time (LMT)) — Davies’ sole correct33 figure here. But Vespucci reported 18:00, which brings the observation to 19:30 LMT, 1 1/2 later. Thus, Vespucci’s hypothetical Moon-Mars conjunction time was 17:00 LMT (19:30 minus 2 1/2: DVD 9). Correcting for a (false) tabular 1 1/2 longitudinal difference between Monteggio’s meridian and that of Cadiz puts the former’s (predicted) midnight conjunction at 22:30 Cadiz local time, 5 1/2 greater than the observed LMT. Thus, taking the ephemers’ prediction as correct,34 Vespucci computed by Davies’

34 On 1499/8/23, at longitude 37°34’ W (near the Earth’s Equator), the refracted upper limit 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).

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 ephemeredes (based on mean time). But, since the EoT never exceeds about 20°, it was a serious problem only for the Moon’s rapidly changing position. (Ptolemy never bothers with the EoT for any other celestial body.) Davies no doubt knew this, but in his instance is that, at the time of the reported Moon-Mars conjunction, the EoT exceeded Mean Time by less than 1° — thus the EoT happens to be so tiny that we can ignore it here. Thus, 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 negated 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?)

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 — anyone) could have told him that the predicted 1499/8/23 Moon-Mars conjunction in fact took place some hours before the Regionontanus aspect-table’s predicted time (which was Nürnberg midnighth). Generalizing this point: someone knowledgeable in astronomical matters (as Vespucci pretend 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 ephemeredes were demonstrably 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 ephemeredes 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. I mean such phony writers as Thaddeus Schmidt (d.1600) who discovered & evaluated the large perturbative terms in the lunar orbit, terms which caused intolerable errors in all prior lunar ephemeredes (e.g., Ptolemy, Regionontanus) — errors sometimes exceeding a degree, which entails 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 making map, it would be possible to compare one’s at-sea observations against calculated lunar conjunctions (not with the unreliable predictions of the ephemeredes of the period but with acquaintances’ at-home
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° 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°/1 (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° east of it, while Cadiz (6°.3 W) is only 11° east of Cape St. Vincent (9°.0 W), both places being in the west Iberian peninsula. Thus, the Vespucci 5°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 it to 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 suppose37 that Vespucci merely equated hours of lunar motion with degrees of longitude as just simple calculations appearing on the surface — if he computed at all. I suspect that the whole 1499 report may be merely a muddled38 appropriation of another’s calculation. (I remark at fn 20 that the letterer’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 Lettres 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 ordnag a degree might be possible — but only after returning from the journey, not during it."

37 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 1835 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 equinocial 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° +)."

41 E.Brown in ibid, Markham 1894 p.81. And see ibid p.xxi. Markham reproduces 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 Portuguese archives, or in the contemporary chronicle of Damian de Goes. This remarkable silence points to the conclusion that if Vespucci was really in any Portuguese expedition he can only have filled some very subordinate post . . . .” (J.Farly 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] Hojeda 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 3°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 first40 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 times indicated by the intersection 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 1844 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 Vespucci’s 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 occurrence 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 landing . . . covered with trees and mangroves . . . 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 (1525) 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 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 Hermann Wagner (1917) and Duarte Leite (1958) have denigrated Vespucci without any 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. 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 America, 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, we will compute (below) for the terrestrial earth (as does Davies’ calculation: §D4) unless explicitly stating otherwise. (Testing shows that varying the observer’s geographical latitude ±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 Foundera

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 alitudinal parallax at the horizon is 57.36/1.7 radians or 56°. Since the event is near the meridian, the equatorial 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°/12°/6/1°/1°/1°/1°/1° = 1°37° = 97°. 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 ashcan the papers and Davies’ 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 in his artwork: DVD Fig.3. This is touchingly quaint, but (as noted above: fn 43) it simply reveals Davies’ inability to compute planetary planes on his own. Moreover, his DVD Fig.3 has Mars going in the wrong direction! At this time, Mars (an inner planet) was not observed at midnight, at magnitude −2. It was nearer to the 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.) Thus, as even an astronomical

48 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 1444 pp.64f, 218f.

49 Same identifications at DVC 7. But neither Wagner nor Leite were astronomers. Wagner was a German geographer & statistician; Leite, a Portuguese 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.)

45 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.

56 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 well-known book on Vespucci (Pohl’s) which computes its 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°3/2 N, 70° W: just north of Venezuela) and the datums Davies discards in favor of 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. . . .

51 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).

52 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 fairer: 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: rearward. But Rear-Admiral Davies’ Fig.3 shows Mars’ motion as direct (forward: positive slope) not rearward (negative slope). By contrast, the Retro-Admiral’s Fig.6 has Mars moving rearward at the very same time: negative slope. (Navigation Floundering?) Or, given the manipulation of possibilities possible under Lorentz transformation, do we have here yet another hint of the Davies paper’s curiously-undeclared relation to relativistic math? By misbehaving 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 (VB: Davies’ shadow-lines are his basis for supporting Peary) drawn & read correctly, he would find about 20:02. The 4° difference between this and the time Davies deduces (20:00; 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 3° in the different between the lunar & Martian mean celestial longitudes grew appreciably in just 25°, since the mean synodic positions of the Moon and Mars were both so well known. I have already cited above the probable cause of Regiomontanus’ large 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 astrogoler 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: in 20). There is no doubt that Regiomontanus accounted for parallax, as the most cursory Davies comparison of Oppolzer’s well-known Canon with Regiomontanus’ Tables in DVD 1494 would have shown: the eclipse times would have been 9.8 minutes earlier for Nürnberg, which is at 49°27’N, 11°04’E. Morison Admiral of the Ocean Sea (unabridged 2-vol edition) 1942 1.251, 262-3.35 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, opposition, square, trine, & opposition. (One sheaf of funereal calculations I possess.) Either Davies or the Reg that was used for the Red Book app of his day was 1.02 from the horoscope.) 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. . . .

Another possible explanation of Davies’ 4° error here: supposing the Gregorian-Julian calendar gap to be known, and assuming that the day had shown: these times in 1499 were 9.8 minutes later for Nürnberg, which is at 49°27’N, 11°04’E. Morison Admiral of the Ocean Sea (unabridged 2-vol edition) 1942 1.251, 262-3.35 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, opposition, square, trine, & opposition. (One sheaf of funereal calculations I possess.) Either Davies or the Reg that was used for the Red Book app of his day was 1.02 from the horoscope.) 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. . . .
“independently” produced & agreed to by various [hypnotized teamplayers] of Davies’ unvoice [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), 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 1/2 after sunset.) This 16° 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 2.5° (§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: 1.35/hr or 12°/eday. Thus, the correct amount of time it would take for the Moon to move 1.25 geocentric degrees would be 2°23′; so: an error here of 7° 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°/hr) = 2°23′ or 143°. Davies’ rounding here is dubious 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

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 1909/2/20 sounding. A successful 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 bathymetry but does not (near 55°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 hoax, has gotten Peary to within ordmag 10 mi of the Pole! As a knowledgeable science-newsmans has already commented: sounds like a classic case of high-precision-low-accuracy. 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 the same tolerances as the worst. In fact, since DVD 10 puts Vespucci at “37.75°W longitude, the formal factor is 3000!”

F2 Was systematic fudging 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 nal Vespucci longitude (37°3′). And it is remarkable that [a] he had 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 216 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 Joao 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° + 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

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′.

F7 Finally, I see that Davies includes no consideration of differential refraction. 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°/4 higher than that is: nearly 10°. This corresponds to about 5° of longitudinal difference, and this 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.

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 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-newsmans has already commented: sounds like a classic case of high-precision-low-accuracy.) 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 the same tolerances as the worst. In fact, since DVD 10 puts Vespucci at “37.75°W longitude, the formal factor is 3000!”

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Thus he is tacitly assuming that the observation was well clear of the horizon, in fact that in 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.

55 Nor does Davies admit that his first (1984/10/17, Fels Planeterium) 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. DVD 8 has moonrise at 19:33 LMT, 32° 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° more than 2.5 hrs (DVD 10; §E5). The net difference is 32° + 6° = 38°, 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.

56 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 who he’s supposed to be,” then: do manipulations in the “Navigation Foundation” analysis of the 1909 photos get Peary further north until he 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 miscalculations of Vespucci’s moonrise “observation”. We have 7 Davies errors: 3 cases of a nonspecialist’s ignorance, and 4 cases of easily-known nudging of Vespucci westward by clumsy miscalculation or arbitrary roundings. The 7 error-corrections here: [a] parallax (24° eastward: §E1); [b] retro-retrograde Mars-land over-corrected rounding of 1° eastward: §E2); [c] ET – UT correction (1° eastward: §E3); [d] miscomputed moonrise-time (4° eastward: §E4); [e] over-corrected time of lunar parallax 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° + 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,65 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 ecliptic 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 rotation60 (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 ecliptic 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, for 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 ecliptic 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 ecliptic longitude difference64 at moonrise would be 2°1/0 — 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, let us 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 (Melvilianly) 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

60One 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.


63This approach is assisted by the fact that differential parallax is null at the horizon.
beckons. How pathetically blind of lesser scientists not to have noticed any of this — until the Navigation Foundation’s insightful President faced them with irrefutable 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 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.”

Partial Bibliography:


NGD T.Davies National Geographic 177.1:44; 1990/1.


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 sober 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 kiltylitth 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 NYT Times 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 Cookieites’ impenetrable loyalty — ‘There will be a ‘Cook party’ to the end of time’ — by observing that:

There will be a Peary party too.

65When 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 prodcon 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 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 1989/9 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.

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67[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 1999 pp.5-4 and §G8.

68His equally sensational exposé, The Dark Side of Isaac Newton, will be at bookstores in 2018 November.

69During the last decade, National Geographic has evidently gotten sober 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 kiltylitth 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 N½ Times 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 Cookieites’ impenetrable loyalty — ‘There will be a ‘Cook party’ to the end of time’ — by observing that:

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John Bainbridge estimated its tail on December 3rd as being 45° in length, 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, “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°–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).

Father Grassi in Rome

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.” He estimated it had appeared in 1618 on November 28th in the constellation Libra, a few degrees above the ecliptic. Its point of origin (his own Nov.29 original 1st 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.28th Greg., 18th 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°.

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, also noted by Bainbridge.

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 6th, he found a parallax of 16′. At Innsbruck the comet was seen to pass by Arcturus on December 13th, 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, the absolute magnitude becomes brighter and the tail extends.

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According to Fig.1, the comet crossed the ecliptic on the 27th, at 16° Sco. Sarsi’s later 11°1/2 on the 29th does however concur.

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.

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 winged Virgo, its 1st 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 Zubenelegnubi (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 1st saw the comet. Zubenelegnubi & 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. Thence 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,” 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 stinted his view; however, within the Tyhonic 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. . . ?” 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.” His publication displayed a frontispiece of the three bees of the Barberini family, because Galileo’s friend the Tuscan poet Maffeo Barberini had that 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, 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.

Galileo was never burdened by undue modesty. These words were as Westfall observed a “provocative display of egotism.”

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 comets from Virgino 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 ailment 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.” None of this prevented him from composing his polemical riposte. As Il Saggiatore 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 mathematician.

### 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 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, 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.”

### 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.

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12Heidarzadeh, p 57; Controversy, pp.13-14.
13Heidarzadeh, p.60.
14Controversy, p.75.
17Controversy, p.189.
20Controversy, p.87.
21Heidarzadeh, 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 *Disputatio* from the Collegio Romano (in fact also by Grassi) re the comet, 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.”

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….” Not having seen any comet since that of 1577 in its 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 mathematicians, 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. 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.

1See *DIO* 22 (2018), especially §3 §5C-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/bb.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, the 10 commandments were carved into stone by god, not Moses.
3. When earthly god Jesus couldn’t dodge the Palestine bunko squad, he was executed, though said to have risen. (But hid instead of strolling the city, Resurrection 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's).

5. Hamlet & Macbeth were created by a loan shark 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 §B1.)
6. UK’s J.C.Adams discovered Neptune, though Frenchman Urbain Leverrier’s final publication prediction was spot-on to 1st while Adams’, lodged privately 2nd later, was 12th off.
7. The pope is infallible within certain boundaries, so voted in Rome, 1870, by the College of Cardinals. Themselves presumably possessing infallible wisdom as to persons & boundaries?
8. Robert Peary, who’d already (‡ §B5 in 1907) fantasized having discovered nonexistent land in 1900, was 1st to publish an uncorroborated claim in 2009, steering the American public in an unscientific direction. A deft edit of this paper, fully illustrated, appeared in Science 2018 August.
9. Richard Byrd was 1st to the Pole by air, 1926 May, sending his report to NatGeogSac in June, not to real (DIO 10.6.p6) science societies ‘till Nov., after stripping it of all raw data.
10. Ancient scientists were so unempirical and incompetent that they just copied prior work. Astroloner Ptolemy, who taught #4 & whose “observed” Sun-places were 50 times nearer to 280°-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 dept (idem), who got the post by teaching just such untested 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 weak Latinized Shakespeare’s uneducability at grammar school (www.dioi.org/shg.pdf, fnm 34&36) and cannot admit that they have, for over 400’s, missed dramatist and rebel 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 ago, have since gone the way of all flesh, undone by DIO at, respectively, Scientific American 2004 Dec, p98, 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 hoaxos. DIO vol.10, www.dioi.org/wals/wa0.pdf, ending Byrd’s polar claim, was co-published in 2000 by DIO and Cambridge University. Myth 410 finds itself 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, fnm 29&141.1) [2] Ignoring genuinely-great scientist Ariostos of Samos, [a] who was 1st to publicly announce heliocentrism (400’s) 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 ([§B2]); [c] whose universe’s volume exceeded geocentrists’ size a trillion times ([§K2]).

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’s (suiciding c.195 BC) Eratosthenes today has inspired enough fans to envy a rock-star, his apologist-army regularly launching article after article after article — Eratosthenes-Reconsidered-Reevaluated-Reconstituted-Revised-Rerigorated — 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 actually 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 rigidly 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’s.

It even convinced Columbus the Earth was so small one could reach Ptolemy’s Kattigara (Saigon: www.dioi.org/vols/w50/pdf, fnm 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 undeterably 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.
[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.
A 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 years, 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 (2nd-most enduring of the Seven Wonders of the World), have been used for the purpose? That obvious possibility is suggested by the double-coincidence that it was built at the very time AND place of Eratosthenes’ Earth-size estimate. (Similar potentially productive spacetime coincidences: ? [B #8, and Rawlins Peary’s Fiction.3 1973 pp.262-263].)

B2 As shown in 2008’s DIO 14 (www.dioi.org/vols/we0.pdf, p.2 fn 1 & 11 p.12), the Pharos’ height h = c.300 feet. If its designer Sostratos wanted a world-record lighthouses-talliness 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 southwest 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 1st realized in 2008, multiplying 40800 stades by 2r yields C = 256000 stades, exactly with the circumference extracted back in 1882 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 computing 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 unnecessarily 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 after Eratosthenes’. The delay’s likely cause: computing it required spherical trig (D.Rawlins Doubling Your Sunsets Am.J.Physics 47:126-129, 1979, Tables I & II), not available until the 2nd 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%.
Accurate Ancient Astronomical Achievements

History of Astronomy Cult: In-Denial and In-Decline
Journal for the History of Astronomy

by Dennis Rawlins

Accurate Ancient Astronomical Achievements

History of Science’s Persistent Mis-Depignification of Greek Science


A 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.)

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 (3rd century AD), ancients had noted a 4267 month eclipse-return cycle (nearly 345° long) which happened to coincide so nearly 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 126060°.10’. Dividing this by 4267 yielded 29.1244°03’.3. Sure enough, we find a monthlength of 29.1244°03’1/3 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 18.10°2/3, one 223° of which is 29.1244°03’2, agreeing to 1 part in 24 million with the “Babylonian” value, the difference just a rounding-imprecision. The Aristarchan month-length, 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 29.1244°03’1/2.

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°13’18”35’.1, just 1’ 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 –719/38/3 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°05’05”35’.0, too high by 1’. 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 equals 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°05’05”35’.9, off by but a fraction of 1’, the actual value then being 27°05’05”36’.6.

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’ 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 Zenith distance, or angular distance from overhead) at Local Apparent Noon (LAN) in Alexandria as 1/50 of a circle while at Aswan’s LAN, 5000 stades further south, the Sun was overhead (Z = 0°), so he computed that the Earth’s Circumference was 216000 stades (Strabo 2.2.2.). The latter was used in the Geography of astrologer Claudius Ptolemy (c.160 AD and no relation to the Ptolemies who ruled Egypt centuries earlier), which was dominant for 1000 years. But an unending succession of modern scholars have tried to justify it anyway, shrinking the stade (of which is 29.1203° explicitly attested on cuneiform text BM55555) or the stade (which happened to coincide so nearly 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 12600°0”10’. Dividing this by 4267 yielded 29.1244°03’3. Sure enough, we find a monthlength of 29.1244°03’1/3 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 18.10°2/3, one 223° of which is 29.1244°03’2, agreeing to 1 part in 24 million with the “Babylonian” value, the difference just a rounding-imprecision. The Aristarchan month-length, 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 29.1244°03’1/2.

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 years. 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.

C3 But, as §B5 [& 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 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 Eratosthenes’ 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 1000 years, navigation manuals have recognized that geometric calculation of the horizon’s distance needs adjustment by factor $\sqrt[6]{5} / 5$ (S.Newcomb 1906 p.203).

C6 Provocative coincidence: the wondrous Pharos lighthouse at Alexandria was built at the very timeplace of Eratosthenes’ C’s appearance (see www.dioi.org/shg.pdf, §G3 for Shakespeare—Marlowe parallel confluence), and the distance of its flame’s visibility could measure Earth-circumference C. (Ancestors 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 §C’s $\sqrt[6]{5}$ expansion-factor for $\sqrt[6]{5}$ 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 twixtsunsets time-interval (which is proportional to the horizon’s $\sqrt[6]{5}/5$-enhanced distance seen from the higher perch) requires division by its square (Rawlins 1979 eq.13). (See fun demo-by-extravces, 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 D1&D2, for which there is no evidence of existence (§F4 below) as early as Eratosthenes’ 180000).

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 task, as the solution now turns out to be physical not metricalogical, 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 Ordmg One Armin

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° 17’, fitting to c.1° these cities’ actual respective north latitudes, 31° 12’, 24° 05’, 16° 57’.

D2 Rounding-off? No, Greek astronomical latitude-fixes were really that reliable. Misled by the disgraceful ordmg 1° errors in astrologer Ptolemy’s Geographical Directory, scholars were unaware of this era 1922. But statistical investigations during that year, Rawlins 1982G n.17 (& Rawlins 1982C Table V’s solutions for $\gamma$), confirmed later by Maeyama 1984 & Brandt & Pujol 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 ordmg 1’ (1 nautical mile), pastereoscopic 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).

D3 Geographical Directory

D4 Which reminds us to the 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 gap reports by Kleomedes & Pliny are “badly overestimated”. His lethally central, truly astonishing errors (in the sole reference (Rawlins 2008Q eq.2) of the Earth’s radius results from squaring the flame’s visibility-distance in stades, which also squares §C’s $\sqrt[6]{5}$ expansion-factor for $\sqrt[6]{5}$ thereby multiplying the geometrically-calculated radius (idem) by 6/5.

E Ancient Longitude Accuracy by Lunar-Eclipse Comparison

History-of-Science’s Backfiring 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) ancienly tabulated Mediterranean longitudes, from the Roman-Carthaginian region east, were consistent with Greek scientists’ ordmg-1°-accurate longitude determinations by comparing lunar eclipses’ local times, assumign these had been ignorantly (Rawlins 2018A §F) multiplied by 4/3 or 7/5 by Ptolemy (or Marinus) 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-refered (Rawlins 2018A Postscript) attack on these solid findings as merely a “delusion”, an attack (Shcheglov 2016) revealing that both the author & Isis Editor H.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/iss2r.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”.

E3 Incringly, anyone reading the Isis author’s own citations (Shcheglov 2016 n.7), to esteemed historian-icon Neugebauer 1975 pp.669&844-848, watches Neugebauer overturn all of Shcheglov’s contents regarding “badly overestimated” eclipse-gaps, just as at Rawlins 2018A §D. So the data esteemed Isis carelessly, ignorantly threw at long-resented but maddeningly-invaluable 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 Helenic 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? Martians 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;
Hipparchos’ I35 BC Measure

G1 No explicitly attested ancienly adopted obliquity was close to the actual value in Hipparchos’ time, 23°42′/7. But, over 800 years ago, a world expert in ancient geographical miss, 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 solsticial method (Almagest 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 Hipparcian 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#f.html) demonstrated that Diller’s theory perfectly satisfies all 14 Strabo-attributed 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 paraplectic formula fit only c.1/2 Strabo’s data; or least-squares Jones 2002E, whose theory fit so poorly that he won’t even tabulate it. (And none among the JHAD-rabbiwars 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 & Obvious History


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, Euclidean, Kallippos, 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°) 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.Foad 267A, testifying to a Hipparchos —157/626 18° 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 §§Ks-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 vendications.) 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-learned robotic cultism.]

H3 Equinoxes were observed on the large public equatorial ring in Alexandria. The sole surviving one, from —145/3/24 11° (Almagest 3.1) was off by —4°, 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 Rhodes equinoxes show 2° or 2′ scatter. Their systematic error was 7° (Britton 1967 p.24, R.Newton 1977 p.78, Rawlins 2018U §B4), but all ordmag 1° of which was from non-observational factors (idem), none degradation 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 error(s) systematic errors are of virtually equal size but opposite sign, and so cancel each other.

H4 The accuracy (§H2) of papyrus P.Foad 267A’s −157 solstice isn’t isolated. Reconstructed was Aristarchos’, Kallippos’, Aristarchos’ Hipparchos’, solstices show errors of just 0°−3′ (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′ ago (Evans 1987, Evans 1998 p.259), citing 2 awful Hipparchan placements (Almagest 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 esteemed Editor of the “premier” & pseudo-recognized (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 3.53′. Is this credible? — given that the healthy human eye’s discernment-limit is better than 1′. 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. A key proof, of 2 magic century BC spherical trigonometry, is Hipparchos’ use of lunar parallax tables — still surviving at Almagest 2.13. (A collection of prime evidences for spherical trigonometry’s 2nd century BC currency is found at www.dioi.org/cot.htm#mmms.)

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 §3-saint? The needed data are easily acquired: Hipparchos would pre-calculate mid-eclipse-time from his manisolar 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 eq.6-8 & fn 22) drop to, resp. —2′, +1′, & +2′.

I6 Well, does anyone seriously contend that human vision was 10 times worse 2000′ ago than today? Though none of history-of-astronomy’s networks 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 §A’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 §9. JHA hopes never to admit DIO's unanswerable success in neat-fit-explaining (§15) all three Evans 1987-selected hitherto-mysteries observations. Saint Evans intends to keep HIDING (sneeaky) details at www.dioi.org/gj01.pdf, §A1 & fn 7), fleeing exposure of what he hope-imagines (since colleagues stay silent) is his private guilty secret. Which isn't.

17 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° (Earth-radii) away, affecting 10° 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° (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° (Rawlins 1991W eq.23); Aristarchos, Archimedes, & Poseidonios 10000° (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° of a radian; thus (Rawlins 1991W fn 272), diurnal parallax’s invisibility, given a parallactic baseline of ordmag 1°, would’ve been convincing evidence — esp. at Mars stationary points — that the Sun’s distance was at least 10000 Earth-radii, ordmagnly-right (since the Sun is 23000° away).

K Stellar Distances: Heliocentrist Trillionfold Universe-Expansion

K1 One reason for geocentrist’s resistance to Aristarchos’ 3rd 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 §3 for an astonishing collection of naive claims by prominent history-of-science archons that geocentricity-vs-heliocentricity remained an undetermined issue until the 19th century! The Church might relicefully 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 turncatedoed 1833.15

K2 Aristarchos replied to §K1’s doubts 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 geocentrist’s, & volume greater by ordmag 1000000000000 — a trillion.

K3 Connecting Aristarchos-Archimedes remotenesses — 10000° solar & 10000 AU stellar — to 1/10000°-radian eye-precision, is another original DIO discovery. (Ever uncomputed.) 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.)
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Robert Headland [polar research & exploration], Scott Polar Research Institute, University of Cambridge, Lensfield Road, Cambridge CB2 1ER, UK; tel (44) 1223-336540.
E. Myles Standish [positional & dynamical astronomy], Jet Propulsion Laboratory 301-150, Cal Tech, 4800 Oak Grove Drive, Pasadena, CA 91109-8099. Ret. Tel 864-888-1301.
F. Richard Stephenson [ancient eclipses, ΔT secular behavior], Department of Physics, University of Durham, Durham DH1 3LE, UK; tel (44) 191-374-2153.

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