

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

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

by

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

A Much hyped; poorly typed.

A1 One of the favorite lines of attack by the Frederick A. Cook Society (FACS) illustrating the supposed-bias in the writing of my book, *Cook & Peary, the Polar Controversy, Resolved*, was my failure to acknowledge the expert opinions of those who participated in the “watershed” symposium held at the Byrd Polar Research Center (BPRC) in October 1993 under the title “Frederick A. Cook Reconsidered: Discovering the Man and His Explorations.” From that time until the publication of my book in 1997, the symposium was never available except in an archival videotape format [although I was chided for being “uninterested” in the videotapes of the conference, the reader is told in the introduction to the proceedings now in hand that “the tapes proved difficult for those concerned with historic and geographic research”], even though FACS had announced the printed proceedings

would be forthcoming as early as 1995. While browsing a catalog from a polar book dealer in September 2000, I came upon a notation offering for sale "The long-awaited proceedings of the Cook Symposium held at Ohio State." Naturally, I hastened to acquire a copy.

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

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

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

B Editorial atrocities

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

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

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

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

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

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

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

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

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

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

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

C1 Dr. Myerson reprises the explorer's life with an emphasis on Cook's medical insights, especially in relation to his medical work on polar expeditions. In so doing, he gets a few facts wrong: Cook was the 5th of 6 children, not 4th of 5; the Cook brothers put runners on a boat, not a wagon to deliver coal during the Blizzard of '88; Cook moved to W. 55th St. after his wife died, not before; Cook's "medical prowess" was not "put to the test" by Peary's 1891 accident, because his broken leg did not even need to be set; Peary's *Northward Over the "Great Ice"* was published in 1898, not "1893"; Cook baked his Antarctic patients in front of a large stove, not a "bonfire"; you wouldn't eat "fresh walrus" in Antarctica because none live there; Cook did not receive "the gold medal of the Order of Leopold" but he did receive the white enameled cross of that order, and a silver medal from the Royal Belgian Geographical Society; it was a medical impossibility for Cook to have made a diagnosis of pernicious anemia in Peary in 1901, which is when Cook says he examined Peary, not "1904" (‡1 §E2); Cook took no gum drops with him on his polar journey, though he did say he took Nabisco cookies; Cook did not winter in a "cave," but a standard Inuit igloo;

Cook died on August 5, 1940, not "August 2"; and there is a lasting memorial to him as an explorer in Buls Bay, Antarctica, etc; etc. In the notes, Cook's 1897 paper (*Medical Record of New York*, vol.51 [June 12]: 833-36) is unattributed. And the citations of *Hampton's* and Peary's book are inaccurate. Every single one of the facts that Dr. Myerson presents, correctly or incorrectly in his paper, are to be found correctly stated in *Cook & Peary*. Therefore (in reference to §B6's bold-faced question), there is nothing in his paper that could change anything in that book, though the converse is certainly not true.

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

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

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

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

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

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

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

E1 I have already expressed my admiration for this paper [*DIO* 9.3 ‡4 §C4], which came to many of the conclusions I had already come to in my then-existing manuscript of *Cook & Peary*, of which I brought a copy to this conference. It's surprising that in his printed paper Mr. Harper failed to correct the erroneous authorship of the maxim that forms his title, because I cited it in the question period and sent him the exact reference soon after the conference. [The quip, "Cook was a gentleman & a liar, Peary was neither", was coined by Senator Chauncey Depew, not the usually-cited Peter Freuchen; it was quoted

in the *New York Times* March 31, 1910.] There are a few other factual errors, such as the statement that there is "no record" of Matt Henson being present at the 1909 interrogation of Cook's Inuit companions. Henson explicitly states in the July 17, 1910, *Boston American* that he conducted this interrogation for Peary: "I obtained for Peary the details of Cook's performance, which he afterward offered as his own investigation, that Cook was not once out of sight of land." Harper also states that "Theon Wright reports, on no quoted authority, that 'the Eskimo word for the North Pole is Tigi-su which means Big Nail' ". It may not be a reported authority, but Wright's "authority" is Dr.Cook, himself. On p.272 of *My Attainment of the Pole*, he uses the phrase "Tigishu-conitu" and translates it as "The Pole is near." Once again (see §B6 boldprint), there is absolutely nothing factual in this paper that does not appear in substantial detail in my book, and my conclusions are coincident to a very high degree with it on the questions Harper's paper seeks to answer speculatively.

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

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

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

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

additional details I had discovered in my research. After the conference, FACS published my detailed analysis of this same question as a separate paper. [*Dr.Cook and the Yahgan Dictionary*. Special Supplement *Polar Priorities* v.14, 1994, 12 pp.]

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

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

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

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

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

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

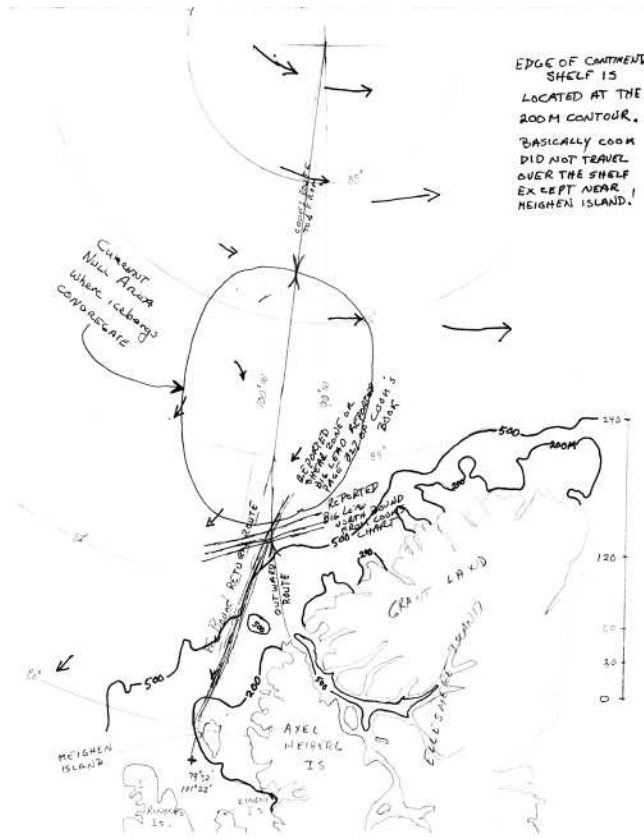


Figure 1: Captain Brian Shoemaker's Map Sent to the Author

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

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

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

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

G “Cook as Nondiscoverer: Demolishing the Mythical Attainments of 1906&1908,” by Dennis Rawlins [Paper’s full text is ¶3 below.]

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

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

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 his attainment of the Pole. But such information is not astonishingly specific. . . . if . . . Cook is vindicated because he reported a rough direction for drift. . . . then why isn’t he disconfirmed when he reports in detail — *and photographs* [www.dioi.org/vols/w93.pdf, DIO 9.2 ¶4 Fig.5] — a wholly mythical land? [Bradley Land]. . . . a neutral investigator will not [a] treat vague alleged positive evidences as vindicating Cook, while [b] treating all negative evidences as mere Problems or Paradoxes that prove nothing but the need for increased effort at dedicated Re-explaining.”

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

G5 A question period followed this paper. I asked if it was plausible that Cook might have attempted following his “Magnetic Meridian” in lieu of navigational ability. Rawlins logically rejected this because Cook reported and tried to present incorrect standard navigational data later [¶3 Fig.2], and Rawlins was of the opinion that it would be difficult to get back to land using such a method. The next question asked if the 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 Heckathorn asked if Rawlins had seen the original observation sheets among Cook’s papers at the Library of Congress, and said that there were also tables of refraction, lunar tables, and potential lines of magnetic declination besides, in his papers. Rawlins said that there may be tables, but there were [no sph trig calculations], which he assumed would’ve been presented by FACS by now if there were any [¶3 fn 7]. He added that he had seen copies of the alleged “original” [non-trig] observation sheets. There was also a question for Captain Shoemaker. He was asked when Cook observed the Beaufort Gyre did he realize what he was observing. Shoemaker replied that Cook had not, nor did he believe Cook ever knew it was there (thus contradicting the answer he seemed to have given to the question posed in the subtitle of his paper), but he (Shoemaker) had “interpolated this from what happened to him.”

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

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

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

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

H4 Fletcher started his presentation by apparently denying his introduction as “the first man to stand at the pole after Cook and Peary,” although this can’t be certain due to the

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

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

I “Admiral Peary and Doctor Cook:

New Insights into an Old Controversy,” by Ted Heckathorn

I1 I have already [www.dioi.org/vols/w93.pdf, ¶4 §§C5&K] dealt with aspects of this paper at length. Apparently my letter to Heckathorn² 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 “Eric”; Dr.Cook fled the country in November of 1909, not December; after a 1915 citation, Heckathorn says “meanwhile in Greenland, Danish explorers inadvertently uncovered the Peary ‘Channel hoax,’” when it was actually uncovered and widely published as early as 1908, thus discounting much of the paper’s misguided thesis. Apparently, Heckathorn remains blissfully innocent of the Eskimos’ evaluation of Dr.Cook. The embarrassing moment where the Eskimos called Cook “shag la-hutte” (a huge liar) is now enshrined in print on p.77 of Report #18. (*DIO 9.2* ¶4 §L17.)

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

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

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.]

²Idaho telephone 208-552-4718.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

N Summary Concluding Remarks and Panel

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

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

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

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

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

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

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 ¶6 fn 18.

N6 After the conference the annual meeting of FACS was held. I was invited with the idea that I might deliver an extemporaneous paper on Cook’s rôle in the “theft” of the Yahgan dictionary, but there was not enough time for me to do so. (It was subsequently published: see above at §E5.)

N7 Perhaps the reader can now understand how I felt at the end of the conference. The presenters seemed to be calling for just the thing I had already done, and I felt confident that what I had to publish would “get it right,” detail “the tactics of the Polar Controversy” and clear up many of the “mysteries” of this “man unknown,” Dr. Frederick Albert Cook, and thus resolve the Polar Controversy.

O The Presenters’ Conclusions

O1 Now to sum up and tally the conclusions of the presenters at this [§A1] “long-awaited” and “watershed exploration of the subject” of Frederick Albert Cook and hear their verdicts on his claim of having attained the North Pole on April 21, 1908.

- Myerson: no opinion expressed on the Pole claim, but Cook was an innovative doctor.
- Gilberg: no opinion expressed on the claim, but Cook was a better anthropologist than Peary.
- Harper: no opinion expressed on the claim, but you can never have the truth of what the Eskimos said about it.
- Shoemaker: if you assume Cook reached the pole, his field notes lend support to the claim, though there are major inconsistencies in his data compared with modern research. (During the oral presentation Shoemaker clearly said he believed Cook reached the North Pole, however.)
- Rawlins: Cook is a justly persecuted hoaxer; he did not reach the pole.
- Fletcher: no opinion expressed in his printed paper, since no paper was delivered or written. In his remarks during the conference, however, he felt it possible that new evidence “from Mother Nature” might throw further light on the matter.
- Heckathorn: no opinion expressed on the claim (though there is a strongly implied advocacy); but an answer should be possible when the primary evidence is thoroughly examined.
- Koryakin: Cook reached the pole because some of his narrative is supportive in terms of what is now known about the area of the Arctic through which he claimed to have traveled, although parallel conflictual evidence is discounted. But this paper was not delivered at the symposium and therefore I had no experience of it. It therefore could not have been “ignored” in *Cook and Peary*.
- Cook-Dorough: based on the evidence in his paper, Cook almost certainly did reach the pole.
- Herbert: In his printed paper, Cook faked his journey to the pole. But in his oral presentation he did not express any opinion about Cook’s polar claim at all.
- Malaurie: Cook faked his journey to the Pole; if he had actually gone there he would not have been able to return, but he was an exceptional personality.

O2 The verdicts of the nine presenters who actually delivered a paper in October 1993:

For: Cook-Dorough, Shoemaker, Heckathorn (by implication)

Against: Rawlins, Herbert, Malaurie

Neutral: Myerson, Gilberg, Harper

O3 This is really not much of a ringing endorsement when you consider that Cook-Dorough and Myerson were officers of the advocacy “Society” that bears Cook’s name, and Heckathorn was closely associated with the FACS (they published everything he wrote and bankrolled his jaunt to the Ruth Glacier to try to retrace Cook’s route up Mt. McKinley, though he was not a dues-paying member). Remove them from the list and Frederick A. Cook loses, hands down, by a three-to-one margin.

O4 And so this is the record of the symposium whose proceedings were so important, so crucial, and that so compromised my conclusions, that I studiously “ignored” them in writing *Cook & Peary*, a symposium where 75% of its non-FACS affiliated presenters who ventured an opinion concluded that Cook was a fraud. That was all a fine argument before the proceedings could be read. Now the solution is simple. Read *Cook & Peary*. Then read Report #18. Place the two records side by side. Compare them. I shall be satisfied with your decision.