Incontinental Drift: the “Davies Movement”
Snow Job and the Seven Dwarfs
National Geographic’s Unimpeachable Longitude Authority Proves Errant Brazil Kissed Africa, Under 500 Years Ago!
Plus Further Navigation Foundation HyperDiscoveries
Moon a Planet! Tachyonic Tectonics! Relativity a Hoax!

by Dennis Rawlins 1990/3/30

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

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

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

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

3 NG 2: “Peary . . . mercilessly pilloried by a vociferous minority”. (Reviewers have universally noted that Herbert’s 1989 book Noose of Laurels is exceedingly gentle regarding Peary’s sins. DR’s
199/8/23 proves the observer was at longitude e.37°.75 W. Thus, Davies says that his math vindicates Vespucci’s controversial claim to have reached Brazil in 1499, not to mention Vespucci’s priority in devising the historically crucial astronomical longitude-determination method known as “lunar-distances.” “I believe . . . our use of modern computer methods have [sic] shown Vespucci to be a credible navigator & innovator . . . . this application of Archeo[sic]-astronomical methods is a first.”4

However, when Davies’ work is corrected for various extraordinary astronomical-math howlers (one of them spectacular both for its size and for what it reveals of his NGS-advertised Expertise), his calculations prove instead that Vespucci was at least 27° or 1600 [nautical] miles east of Davies’ deduced Brazil position — an amazing detail in the longitude of Liberia6 (Africa). Since I made this little item public (1989/12/11), Davies has been asked by reporters6 about the Vespucci paper, his longtime former (pre-Peary) pet “historical detective story” (DVC 10) project; but he refuses [like §3 §B13] to answer press questions.7 One isn’t accustomed to [seeing US Admirals departing] under fire.

1973 book *Pearls at the North Pole: Fact or Fiction?* was appraised similarly, e.g., by geographer Wm.Wanztz in the 1975/3 *Annals Assoc Amer Geogr* 65.1/79. “Rawlins’ dismissal of the final Peary claim does not thereby mean that he does not understand Peary’s overall importance and his many earlier contributions. He notes and appreciates them. He writes with compassion and awe of the physical suffering endured. He recognizes Peary’s many virtues no less than his extraordinary frailties.” NGD 60: “on a personal note, we [the NF] cannot but hope that this marks the end of a long process of vilification of a courageous American explorer.” As the *Wash Post* headlined it (1989/12/12), in a Nixonian echo: the NF deems Peary “Not a Fake”. And, on the Vespucci observation (which even Vespucci’s defender Stein calls “fictional”); fn 47, Davies concludes that it probably “could not have been faked” (DVC 7; conditional satisfied at DVC 11). Davies goes so far as to justify the naming of the New World for Vespucci by stating (DVC 10): “even the erroneous Longitude of 1499 was enough to raise doubts that the new lands were off China.” (See also DVC 11. Davies knows better at DVC 14.4.14.) But in fact the very 1500/7/18 letter under discussion states (right in the sentence following that quoted by Davies at DVC 8, describing fresh water — which he supposes refers to the Amazon mouth): “it was my intention to see whether I could sail round a point of land, which Ptolomey [in his crude delineation of China] calls the Cape of Cattegata [Georgy Di.7.3.3; the Chinese anchorage, Kattigara: Καττηγαρα ομρος νυμφος] (which is near the Great Bay [Georgy Di.9.11.1]) 11°17’ east of the longitude of Liberia6 (Africa). Since I made this little item public (1989/12/11), Davies has been asked by reporters6 about the Vespucci paper, his longtime former (pre-Peary) pet “historical detective story” (DVC 10) project; but he refuses [like §3 §B13] to answer press questions.7 One isn’t accustomed to [seeing US Admirals departing] under fire.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

One isn’t accustomed to [seeing US Admirals departin...
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 somewhere else in the Davies-New-Astronomy: a remarkably fruitful & revolutionary universal-physics, which is about to provide us lots of other equally enlightening gems, below.

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

B1 At Philadelphia’s Fels Planetarium, on 1984/10/17, Adm.Tom Davies announced a remarkable revelation regarding Amerigo Vespucci (the Florentine merchant-banker after whom America is named). Vespucci, too, posed (§A1) as an expert on exploration and navigational astronomy, convincing Spanish royalty sufficiently that he was appointed 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, astro-theoretical, astronomical-computer-ephemeris-based analysis convincingly vindicated Vespucci as to both his truthfulness (below: §B2) and his supposed pioneer understanding of the important method of lunar distances (§D7). The final version (DVD) of Davies’ paper is “Amerigo Vespucci & the Determination of Longitude”.

B2 Davies quotes (DVD 7-8), analyses, & certifies (§§C&D§D here) Vespucci’s alleged sight (§ longitude-computation): a supposed 1498/8/23 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, 5, 10, 13; all 4 passages quoted here) is that he will use “the tools of modern science” (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) are information available only four centuries later, we have the opportunity to test his veracity” (DVD 6, emph added).

B4 Davies’ entire paper’s crucial math basis (a Nobel-Prize-winning discovery, if true! — DVD 6) are information available only four centuries later, we have the opportunity to test his veracity” (DVD 6, emph added).

B5 Davies prefaces his monumental discovery of widespread simultaneous lunar conjunctions by exhibiting (a 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 HSM Pinfare stuff is a thing of the past.

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

14 The NF’s months of inability (NG App.A) to solve its own Betelgeux Document (BetDoc) hypothetical time-sight problem reflect more than mere innocence of standard spherical trig and fundamental astronomy. For, in truth, one need not know the rigorous equations in Wm.Chauvenet’s Manual Sph & Pract Astron (1906 1:257f) to perceive that if one has 2 stellar altitude observations at a known time & 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.]

15 Davies prefaces his monumental discovery of widespread simultaneous lunar conjunctions by exhibiting (a 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”... . . . . .
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).

*almaest 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 §B3-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 upon, but instead he used the entailing the source from memory (or perhaps arguing from the. . . sin of scholarship: §1 §§D3&D-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 exceed 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.16

---

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 (GeogrDir 1.7.4) that Hipparchus (c.130 BC) found it 12°2/5 distant from the pole. (The bright star then nearest the pole was at declination 82°+: Kochab or β UMI, more than half again closer to the Pole than α UMI). Incidentally, Davies cites both these astronomers (at DVC 3) as if he has read them (though “their wording was somewhat obscure”, he knowingly judges); however, he gives the wrong century for both (also DVC 3), and we are now learning independently here ([B6]) that Davies is not quite the Ptolemaic scholar he poses as.


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

**B9** How often does a single paper augur discoveries in so many fields at once? (And more’s to come below.) It must be added that, had Davies consulted the chapter he cites (Alm 7.3) in either of the only two scholarly15 editions of the Almagest (K.Mantius 1912-3 or G.Toomer 1984), he would have found this chapter festooned with footnotes (catch irony of fn 19 here) discussing ancient astronomers’ calculations of lunar parallax. Indeed, by chance, Toomer (as he tells the reader in his n.77 to Alm 7.3) chooses the 283 BC Moon-Spica conjunction as the example he uses to illustrate (in great detail: Toomer 1984 App.A pp.652-653 Examples 9-10) just how the ancients computed the numerous lunaparallels that appear in the Alm’s text.15

B10 The obvious reason one must include parallax in any computation of the Moon’s observable non-zenith celestial position is that the Moon is so close to us: its mean distance from the Earth’s center is only 60.3 Earth radii (ER). Thus, when seen near the horizon, the Moon’s apparent position is lower (on the average) by 1/60.3 radians or 57.3/60.3 degrees (57 arcmin or 57°) as compared to an ephemeris’ computed position, which is invariably geocentric. (And when was the last time you observed the Moon from the Earth’s center?) For a tropical observation near the horizon (such as the Vespucci 1498/9/23 “observation” analysed by Davies), ecliptical parallax is nearly an entire degree. That is, the observer-centered or “topocentric” lunar celestial longitude will be roughly 1° higher than the celestial longitude given by an accurate ephemeris (geocentric); for tropical moonset, lower by same. This will seriously affect conclusions regarding time or the observer’s longitude (deduced via lunar distances): since the Moon’s mean celestial motion takes it about a degree every 2 hours,22 Adm.Davies’ “glaring error” (to quote the 1989/6 National Geographic on DR) in his deduced geographical longitude (caused by his omitting lunar parallax in a tropical moonrise observation like Vespucci’s) must be very roughly 2 hours or 30°! — which is the difference between Brazil and Africa. It is also comparable to the size of the error (45° of longitude) Davies asserts Vespucci made (due to poor tables), which Davies claims (DVC 10) to have “vastly” improved! (In truth, if Davies’ speculative §C8 reconstruction of Vespucci’s math is corrected for parallax etc., the resulting longitude is by chance much nearer Brazil than is Davies’ own modern-calculated longitude, similarly corrected. . . .) 19 Davies & National Geographic perversely condemn DR’s Peary at the North Pole: Fact or Fiction? (Washington 1973) for not being a scholarly work (e.g., not having a normal footnote system). This despite the fact that all the book’s professional reviews agreed that its evidence was convincing.

20 Similar Davies innocence interprets oversuively the Vespucci §C2 statement, that he was “correcting with calculations” from the Alphonsine Tables9, as merely referring to a 90° longitude shift from Regiomontanus’ meridian to Cadiz (a simple addition, hardly worth mention as plural and noteworthy “calculations”, especially since Regiomontanus’ own tables show about this time-difference between Spain and Nurnberg). Davies indicates (DVC 9) that, in order to check on this easy matter of the 90° difference, he has consulted “several variants” of the Alphonsine Tables. But did he not then notice that these include substantial tables for computing the vital longitudinal & latitudinal lunar parallax (cited Delambre Histoire de l’Astronomie du Moyen Age 1819 p.255-256)? The Vespucci §C2 passage [presumably taken from elsewhere, given his calculation’s innocence] makes much more sense if the reference is to parallactic corrections, which everyone (well, almost) knows are required when using lunar conjunction observations. A knowledgeable navigator would have realized that possibility.

21 “Celestial “longitude” is not the same as geographical longitude. Here, the former is virtually the position measured along the great-circle eclipse’s 360° degrees, the approximate path of the Moon’s motion (known as the “clock”), which is used as the basis of various time systems (see fn 25) the last measurement based on the lunar distances method.

22 In geocentric angular motion, the Moon is by far our fastest long-term celestial neighbor, with a mean sidereal motion (movement in celestial longitude, against the stellar background) of 360 degrees per sidereal month (27 1/3 days) or 13°.2/day or 33°/hour. Since the Moon’s mean diameter is barely 31’, a rule of thumb is that the Moon’s average geocentric motion is: its own diameter every hour. However, this average is seriously affected by the eccentricity and perturbations of the lunar orbit as well as by parallax; thus, for reliable investigations, one must compute precise absolute positions and use differential methods only with great care.
I might add that, 2 millennia ago, the best ancient Greek astronomers, by competent use of lunar observations (lunar eclipses, where parallax doesn’t affect the time of the event) mapped longitudes to an accuracy of roughly a half degree.

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 astrologers for computing horoscopes without including lunar parallax — especially since its omission will foul up the loveliest of all celestial conjunctions (conjunctions being astrologers’ meat), specifically: solar eclipses. (I also added that some few 20th century astrologers do include it: G. Noonan & G. Allen alias D. Bradly. 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 misprinted.) 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 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 Montereggo [Regiomontanus], which was composed for the meridian of Ferrara [actually Nürnberg] correcting with calculations from the tables of King Alfonso [the ‘Alphonse’ 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 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? [b] 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 that, on his 3rd reputed voyage, he had

Original now lost. The official Hakluyt Society collection of Vespucci’s output omits the letter on which Davies’ entire paper is founded, stating that it is one of “three spurious letters now so universally held to be forgeries, that they need not occupy our time” (Markham 1894 p.iii). (DVC 6 calls the letter’s genuineness “unquestioned.”) Remarks of one of the prime issue raised by Mencken’s unstoppable Bathub 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 [1990/5/1] 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.

23DVD’s Fig.6 caption notes this correctly. However, Davies’ 1988/9/18 memo on the Regiomontanus tables says: “Note: Regio’s time is at [Prussian] Koenigsberg, Longitude E 14°30.” [Actual K¨onigsberg: 50° 0’ N, 20° 54’ E.] This odd confusion arose because Johannes Müller (Regiomontanus) would sign his works (e.g., bottom of last page of Der Deutsche Kalendar des Johannes Regiomontan: same year and computational base as his Ephemerides): “M. Johan von k¨ongsperg.” But this has nothing to do with the meridian or the publication-place (which was in fact Nürnberg [fn 50]) of the Regiomontanus tables — rather it signifies that he was born (1436 AD) at the other K¨onigsberg (in Franconia: 50° 0’ N, 10° 34’ E.). In ancient times, one’s native town was commonly attached to one’s name. (By the way, how Davies arrived at a longitude of 14° 1/2 E is a mystery. Perhaps he couldn’t find this tiny town on a map but assumed that Frankfurt was in or near Franconia and so used it, but then got the wrong Frankfurt, an­der­Oder at 14°32’ E instead of am­Main at 8°41’ E.) Since Davies has sneered at DR’s minor (¢ not original) 1988 chronometer-number error as akin to confusing someone’s license-plate number with scientific data, I may as well point out a funnier, [original], and absolutely unique aspect to Davies’ 1988 misplacing of Regio’s meridian: it is well known (first sentence of his entry in DSB 11:348; 1975) that “Regiomontanus” is just a latinized form of Königsberg (King’s Mountain). Has any other self-advertised longitude expert ever confused someone’s name for his longitude?

24Text at Markham 1894 p.45. The attached Vespucci statement that he was thus only 17° 0’2 from the Antarctic Circle (23° 1/2 in radius around the terrestrial S.Pole) would put him nearby at latitude 49°S. The implicit Vespucci value of the obliquity in these statements appears to be 22° 1/2. Another Vespucci text quoted here (fn following) makes the obliquity 23°. The correct value in 1499 was 23° 30’.

25Davies’ 1989/9/2 Memo to DR refers to 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.
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 1/2 hours after sunset (not 1 1/2 h).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 Vesuvian faith.” U.F.Oologists, 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.

30Text 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 clima: see E.Honigmann Die Sieben Klimata und die Poleis Epishmoi Heidelberg 1929 e.g., pp.52, 184, 189. Many moderns believe that Vespucci “was fond of air ing 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 island, 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.)

31Leagues 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 miles (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.

32For 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 climation (angular distance from the Sun) for the wrong (1499/8-22-23) Nürnberg midnights (the time of Moon-Mars conjunction predicted in Regionontanus’ aspect-tables is the following midnight: 1499/8/23-24; DVD Fig.2.) Division by 15 for an approximate sunset-to-moonrise time-difference would yield about 1.6 hrs, virtually the amount reported in the alleged Vespucci letter (1 1/2 hrs). When dealing with midnight events, such 1-day computing errors are commonplace.

33See 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 reconstruction: the NF reconstructs a physically impossible solution [NG App.A], for Peary’s BetDoc, via: 4 invisible observed data [www.dioi.org/sict.pdf, §D2 item 1], a lengthy invisible sp trig calculation, plus [fn 54] an invisible star, all fantasies of utter & unambiguous [NavFou] miscomprehension: see also §E4.) Davies takes Vespucci’s reported ecliptical difference between the Moon’s center and Mars (§C3: “1 degree and some minutes”) and generously rounds it up to 1 1/4° (DVD 8), thus 2 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 correct34 figure here, put to him by Vespucci’s 18:00, which Davies claimed to have arrived at: 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 Montegerrius’ meridian and that of Cadiz puts the former’s (presumed) midnight conjunction at 22:30 Cadiz local time, 5 1/2° greater than the observed LMT. Thus, taking the ephemeris’ prediction as correct,35 Vespucci computed by Davies’

34On 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 DVD 8. Curiously, he later needlessly rounded this to 18:00 (DVD 10; §D4).

35It is perhaps unkind to sully this lone credit by mentioning that it is sheer luck, but that is the case. Davies shows no awareness of the fact that “mean time” was a mere abstraction in Vespucci’s era — which was long before the ubiquity of reliable day-round chronometers (much less portable ones). Thus, since well before Ptolemy, astronomers’ & navigators’ time of day was apparent time, not mean.

And, since a celestial body’s motion is a function of mean time (actually dynamical time), a correction ("the Equation of Time") had to be applied to apparent time before entering astronomical ephemerides (based on mean time). But, since the 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 writes on the 1499/8/24, 19°0’0” in his instance is that, at the time of the observation, the EoT would yield about 1° more than the correct value, which was about 1°-1 1/2° greater than the observed LMT. Thus, the taking the ephemeris’ prediction as correct,35 Vespucci computed by Davies’
reconstruction) that the observation was made 5°1/2 or 82°1/2 west of Cadiz (DVD 9).

C9 Since this longitude (actually in the Pacific Ocean — near the Galapagos Islands!) was impossible for a Brazil journey, critical historians have not been kind to Vespucci. Moreover, even Vespucci’s advocate J.Stein (who places Vespucci in Aruba on 1499/8/23) brings forth (1950 p.351) a provocative coincidence: 5° earlier, Columbus, from observing (at Saona Isle) the pre-dawn lunar eclipse of 1494/9/15 (other details below: fn 50), had celestialy deduced precisely the same longitude as Vespucci later pseudo-celestially found! — 5°1/2 (or 82°1/2). (And in 1500 AD, this was the only available Columbus astronomically-based longitude. Columbus’ 1504 longitude-estimate obviously hadn’t occurred yet.) Specifically, Saona (18°07’N, 68°42’W) was placed (by Columbus’ 1494 eclipse-based calculation) just 82°1/2 west of Cape St. Vincent (37°01’N, 8°59’W; near Cadiz which is at 36°31’N, 6°18’W). Note the coincidences that Saona is just north of Aruba (where Stein induces that Vespucci’s nonastronomical writings put him), only about 6th east of it, while Cadiz (6°3 W) is only 11th 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 to it but from it. The implications are too obvious to belabor here.

C10 Noting the striking equality of the degrees and hours (both 5 1/2 in Vespucci’s report, it is credible to suppose37 that Vespucci merely equated hours of lunar motion with degrees of longitude as appears in one of the latitudes — that longitude 82°1/2 was 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 letter’s reference to corrections from the Alphonsine Tables suggests that the hypothetical original computer perhaps took account of lunar parallax. If so, he must have been a sufficiently knowledgeable astronomer that he could not have committed the blunders & innocences so evident in the Vespucci rendition.)

C11 It is known that Vespucci transposed events from one journey to another: see [§C11 or Roy Geogr Soc President C.Markham Letters of Amerigo Vespucci (Hakluyt Society, London 1894 p.xxvii). Markham adds:39 “The investigation of Vespucci’s statements outdoor observations of the same conjunction. Correcting (both observations) for such matters as lunar parallax, deduction of a longitude accurate to ordnang a degree might be possible — but only after returning from the journey, not during it.]

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

38 Another Vespucci bungle that is inconsistent with his being expert at navigational math is found in the paragraph just previous to that quoted by Davies, when Vespucci states (C.Lester Life & Voyages of Am Vesp New Haven 1855 p.158): “we extended our navigation so far south, that our difference of latitude from the city of Cadiz was sixty degrees and a half, because, at that city, the pole is elevated thirty-five degrees and a half [the latitude], and we had passed six degrees beyond the equinocline 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°1,+).

39 E.Brown Astronomical Almanac 1894 p.81. And see ibid p.xl, which also contains the comments: “There is no mention either of Vespucci or of Giocondi, who is alleged to have brought him the invitation from the King to come to Portugal, either in the voluminous 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 . . . .” (I.Parry Discoveries of South America 1979 pp.99-102 suggests this might not be meaningful but notes at the same time that: [a] no Vespucci account is definitely genuine, and [b] the fourness of his alleged voyages suggests “a deliberate analogy with Columbus.”) Markham also notes (p.xx): “The feature in contained in the first and second [of his 4 alleged] voyages destroys all confidence in his unsupported word . . . .” Markham’s conclusion is (p.xxv): “The first voyage appears, both from internal and external evidence, to be imaginary. The second voyage is the first [1499] of [Alonzo de] Hojedo inaccurately told, while two or three incidents of the Hojeda voyage are transferred to the imaginary first voyage.” We note that the 1499/8/23 “observation” under review was reported from Vespucci’s account of his alleged second voyage.

C12 If Vespucci wrongly supposed that the Moon moved 1°/hr, then the 2 “observations” are in perfect geocentric accord — and, additionally, the striking juxtaposition of 5°1/2 and 83°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 Modern­Methodology, which is intended to lend science’s authority to his remarkable adventures (DVD 10-14; DR emphases added here & there):

D3 “Precision in dealing with the positions of celestial bodies at any time, the tabulation of which is the essence of an Almanac, is a relatively recent development. The first modern theory of the Moon, compiled by Brown in 1919, is still the definitive theory.41 The Jet Propulsion Laboratory has now completed a new numerical integration of all available data on the bodies for any desired time with remarkable precision. Using [these] data and the excellent computer model developed by Dr.Leroy Doggett of the US Naval Observatory, we can plot the positions of the Moon and Mars for the 23rd of August, 1499, with great confidence.42 Figure 3 shows a plot of the Celestial Longitude for these bodies, with the time, 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 vauenness. Not a single name of a commander is mentioned, and in the account of the two Spanish voyages [1497, 1499] there are not a half­a­dzen names of places.” (At pp vi­i­, 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 Admiral USN), whose veracity Davies also seeks to prove with Modern­Methodology.

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 Astronomical Almanac 1894 p.81. And see ibid p.xl, which also contains the comments: “There is no mention either of Vespucci or of Giocondi, who is alleged to have brought him the invitation from the King to come to Portugal, either in the voluminous 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 . . . .” (I.Parry Discoveries 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
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 tall grass, . . . covered with tall grass, . . . in the far northern coast today . . . the sea water was fresh within 25 leagues of the coast: Figure 5 is an Isoline chart of the area showing this phenomenon. . . . Continuing south and eastward . . . encountered an ocean current . . . so strong that they could not make headway against it, and so finally reversed course and headed back to the west and north. At the farthest east point Vespucci estimated latitude as 4 to 6 degrees south of the Equator. The sailing Directions for South America (1952) describes the Tidal Currents inshore (out to 10 miles) as running west (at ebb) up to 4 to 5 knots. Farther out they would have encountered the west-running South Equatorial current. [DVD 11-12]

D6 . . . there is such a surprising correlation of the data that it is hard to believe that these details of geography46 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 astronomers45 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.47 From this analysis I draw the following conclusions: 1) Vespucci

---

46Stein 1950 pp.349-350 notes that most scholars analysing Vespucci’s descriptive and geographical (not astronomical) accounts make Vespucci’s 1499/8/23 position to be off the coast of (not Brazil but) Venezuela, longitude roughly 70°W: fn 47. See also F.Pohl Amerigo Vespucci: Pilot Major 1944 pp.64f, 218f.

47Same 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.)

48I 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.

49Davies’ 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 his 1499/8/23 position (though not very accurately), alleging that it agrees with his writings in placing him off the coast of Venezuela. And a citation in another popular book (G.Arciniegas Amerigo & the New World 1955 pp.193-194) would have informed Davies that, in 1950, Vatican astronaut J.Stein checked Vespucci’s alleged location by celestial computations — finding it consistent with the isle of Aruba (12° 1/2 N, 70° W) just north of Venezuela. The result gives Davies his “in favor of his constellation” excuse for ignoring the “true early position” of Aruba (falsely identified as the “first” [that is, [D5] before the “second” (midnight) “observation”]. (Memorie della Societa Astronomico Italiana 21:345; 1950. Stein of course realizes one cannot fix position very exactly this way, so he attempts to locate Vespucci from his writings and then just checks how well the midnight observation agrees with this location.) Davies might also have been enlightened by Stein’s calculation of the Moon-Mars moonrise situation (the “observation” which Davies analyses at such length). Even Stein, an admirer of Vespucci and a defender of his integrity, declares the moonrise observation “fictional”. This for the very simple reason that, naturally accounting for lunar parallax, he finds (as do Kowal & I: [F4]), that, at 1499/8/23

---

E Rearward-Admiral’s Navigation Foundation}

E1 Omission of parallax is the most disastrous of the many reefs Davies’ math foundation upon: the distance of the Moon at the time of the reported Vespucci observation being 61.7 ER, the altitudinal parallax at the horizon is 57.3/61.7 radians or 56°. Since the event is near the terrestrial equator (as does Davies’ calculation: [D4]) one can make use of 56°.6/day, the time-error introduced by ignoring lunar parallax was then 12°.37’ (12°.37’). Thus, as even an astronomical observer would have recognized, and thereby decisively ashen the paper and Davies’ whole long-nurtured Vespucci-vindication-thesis, since 14°W longitude 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 lines50 in his artwork: [DVD Fig.3]. This is touchingly quaint, but (as noted above: fn 43) it simply reveals Davies’ inability to compute planetary places on his own. Moreover, his [DVD Fig.3] has Mars going in the wrong direction! At this time, Mars (an impressive ruddy spectacle at magnitude +1) was nearing closest approach 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.51 Thus, as even an astronomical

---

50The mean error at this time is about 3°. Pтолемей’s 150 AD tables were far better: mean error — 1°, with superposed error wave [of ordmag 1°] . . . . [At DVD 12 & 9, Davi suppose that such errors are

51Stein 1950 pp.349-350 notes that most scholars analysing Vespucci’s descriptive and geographical (not astronomical) accounts make Vespucci’s 1499/8/23 position to be off the coast of (not Brazil but) Venezuela, longitude roughly 70°W: fn 47. See also F.Pohl Amerigo Vespucci: Pilot Major 1944 pp.64f, 218f.
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 Flounderation?) Or, given the manipulation of possibility possible under Lorentz transformation, do we have here yet another hint of the Davies paper’s curiously-undeclared relation to relativistic math?) By miscalculation in the usual direction (the direction that will get Vespuci 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 (20:06; DVD 10) from Fig.3 provides yet another 1° of westward error.

due to scarcity of historical Mars data and to the fact that the Regiomontanus tables were published in 1474, thus the 1499 places represent “a 25-year extrapolation, with consequent accumulating errors.” Lacking access to Regiomontanus’ entire tables, I have not checked the matter directly, but I doubt that the error in the difference between the lunar & Martian mean celestial longitudes grew appreciably in just 25°, since the mean synodic intervals 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 1891 pp.157-159. 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 unfulfilledly 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 astronomer Ptolemy (whose “observations” were fictional and whose tables were simply appropriated from prior observing scientists: Rawlins A J Physics 1987/3) was, as a conceptual astronomer, superior to Davies. There is some suggestion that Vespuci 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 Argentina shows: those eclipse times would have coincided times when the Earth was 9,770 miles away from Nürnberg, which is at 49°27’N, 11°04’E. Morison Admiral of the Ocean Sea (unpublished 2-vol edition) 1942 I:251, 262-3 n.35 correctly remarks that Regiomontanus’ tables were really issued for workers of horoscopes, not navigators. Regiomontanus invented a still-popular astrological house-division system. (I see that Davies’ caption to DVD Fig.2 confuses houses with zodiacal signs.) We note that Regiomontanus tabulates not just conjunctions but all five of the aspects astrologers live by: conjunction, opposition, square, trine, & opposition. (Thought provoking: the number of fundamental aspects was 50 in the French; Davies used only 10 tables to compute.)

None of the others would be of serious navigational use to a sailor of 1500 AD, except for those occasional Sun-Moon oppositions that were near enough to a lunar node to result in visible lunar eclipses — which Regiomontanus does tabulate. Columbus says he used Regiomontanus’ predicted eclipses for longitude-determination. The Regiomontanus ephemerides’ error for the 1494/9/14-15 eclipse (15) as seen by Columbus at Saona (18°07’N, 68°42’W) was +2°9’ (7:45 Nürnberg Apparent Time = 4:52 UT predicted vs. 4:32 UT actual). Columbus was said to have used the same lunar eclipse in Jamaica. (Both for longitude and for impressing the natives. Vespuci’s astronomy likewise impressed the natives. Back home [in 12.] On this occasion, Davies’ error was +2°7’ (1:36 Nürnberg Apparent Time = 1:02 UT predicted vs. 0:35 UT actual). These mistakes caused westward errors in Columbus’ deductions: longitudes: 5° (1494) and 7° (1504) — though both his eclipse-based longitudes’ total westward errors are far larger. I note that the rms lunar-eclipse-time error of Hipparcos’ well-known 146 BC lunisolar tables was 16°, an error later reduced by him to just 10°6’ [www.doi.org/vols/w11/l.pdf, DIO 1.1 & eq.32]. So the accuracy of Regiomontanus’ eclipse predictions appears to be not quite as good as Hipparcos’.

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

Alm 1988/9/18 memo notes that the 1499 Venus ephemerides to check 1492-3 lunar conjunctions with planets, finding six: 1492/8/7 Jupiter, 1492/9/19 Venus, 1492/9/24 Venus, 1492/9/30 Jupiter, 1493/2/15 Jupiter, 1493/2/26 Saturn. Only one of these is correct: 1492/9/24 Venus. Davies’ memos consistently confused Jupiter for Saturn and vice-versa — an error which suggests Davies didn’t know that Saturn is more distant than Jupiter, since Regiomontanus’ aspect angles exhibit the phenomena in columns, one for each planet, ranked in order of decreasing distance (sample aspect-table at right side of DVD Fig.2). The alleged 1492/9/19 conjunction with Venus (only 5 days before Davies lists another Venus conjunction — some trick!) is really a lunar sextile-aspect.

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

E4 Using the Sun-Moon Right Ascension-difference (cited §D4), Davies also miscomputes the time of moonrise, presumably by consulting the lunar Right Ascension for a time previous to the event. We recall that Davies speculates-reconstructs Vespucci’s supposed computed conjunction time as 17:00 local time (DVD 9: §C8); had Davies used this time to compute the Sun-Moon Right Ascension difference from the Greenwich time positions given him by the Naval Observatory, he would have found this Right Ascension difference to be 1°05’ — just as required for his stated result (§D4) that the equatorial moonrise-sunset difference equals 2°05’. But this hypothesis requires that Davies forgot to convert Greenwich time to local time when performing the computation. Traces of a possible occurrence of the same NavFou-Snafu55 glimmer through in the “patently absurd” 1906/2/27 Cape Hecla pseudosolution [in §54 of the Peary Betegelux Document, 1942/11/30 by him that indeed his hypothesis required that Davies forgot (or TDT, I make the ET minus TDT difference to be +4°). Most specialists in this area would call it about the same or even a little higher. So we have yet another Davies error of innocence. Again: 1° westerly mistake.

53 Another possible explanation of Davies’ 4° error here: supposing the Gregorian-Julian calendar gap is correct, then the 1499 result is correct: the moonrise-sunset difference equals 2°05’. But this hypothesis requires that Davies forgot to convert Greenwich time to local time when performing the computation. (Traces of a possible occurrence of the same NavFou-Snafu55 glimmer through in the “patently absurd” 1906/2/27 Cape Hecla pseudosolution [in §54 of the Peary Betegelux Document, 1942/11/30 by him that indeed his hypothesis required that Davies forgot (or TDT, I make the ET minus TDT difference to be +4°). Most specialists in this area would call it about the same or even a little higher. So we have yet another Davies error of innocence. Again: 1° westerly mistake.

54 A reporter recalls a question being raised at the 1989/2/1 NGS pressconference (Annapolis, on the Betegelux Document (BetDoc), with respect to the NF’s assurance that the BetDoc was a time-sight pair (almost certainly taken at Cape Hecla on 1906/2/27). This unanimous NF time-sight interpretation, of whose truth there could be “no doubt”, according to the 1989/6 National Geographic, is now determined. But the NF’s supposed time-sight pair of Peary’s lunar-sight computation on the wrong Gregorian equivalent date (for Julian 1499/8/23), 1499/9/5 (actual equivalent 9/1). A reporter recalls a question being raised at the 1989/2/1 NGS pressconference (Annapolis, on the Betegelux Document (BetDoc), with respect to the NF’s assurance that the BetDoc was a time-sight pair (almost certainly taken at Cape Hecla on 1906/2/27). This unanimous NF time-sight interpretation, of whose truth there could be “no doubt”, according to the 1989/6 National Geographic, is now determined. But the NF’s supposed time-sight pair of Peary’s lunar-sight computation on the wrong Gregorian equivalent date (for Julian 1499/8/23), 1499/9/5 (actual equivalent 9/1). A reporter recalls a question being raised at the 1989/2/1 NGS pressconference (Annapolis, on the Betegelux Document (BetDoc), with respect to the NF’s assurance that the BetDoc was a time-sight pair (almost certainly taken at Cape Hecla on 1906/2/27). This unanimous NF time-sight interpretation, of whose truth there could be “no doubt”, according to the 1989/6 National Geographic, is now determined. But the NF’s supposed time-sight pair of Peary’s lunar-sight computation on the wrong Gregorian equivalent date (for Julian 1499/8/23), 1499/9/5 (actual equivalent 9/1).
"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 1¹/2 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: 31.5/hr or 12°/day. Thus, the correct amount of time it would take for the Moon to move 1.25 geocentric degrees would be 2⁵/₂₃°; 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°/(3¹/₂/hr) = 2⁵/₂₃° or 143°. Davies' rounding here is doubly peculiar because we have textual proof that he originally did not round the Moon's motion: his first version of this paper used the precise (and wrong) value 0°.48/hour (DVC 8). Once he subsequently realized that this was an incorrect lunar motion (DVD Fig.3), why did Davies then so round the right value (0°.525/hr) that he could still keep Vespucci well west of where Davies' own figures should put him?

E6 Davies' rounding of Vespucci's "one degree and some minutes" to equal 1° 15′ (§(C8) is questionable. I believe that most of us would take Vespucci to mean something nearer 1°05′. A 10° difference is worth about 5° of deduced geographical longitude: and, yet region (a spur stretching nearly to the 70°W meridian) on the NF digital-terrain-model bathymetric map (reproduced at NG 122-123 & NGS 49) is ultimately based on the 1909/3/20 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 data (even when accepting the NF model: NG 122-123). The NF claims (NG 120) that its modifications of the 1985 map had no effect on its evaluation of the Peary claim; however, the NF could not defend the 567 meter 1909 sounding only by altering the 1985 Navy map: stretching its 500 meter isobath far west of its original western bound, as a comparison (NG 119 vs. NG 122-123 or NGS 49) readily shows.

E7 I ignore dip throughout these analyses, since we don't know the observer's height; its effect would be small and would apply nearly equally to all parties' analyses. Incidentally, if one changes the geographical latitude from 4°S to the Equator, the LMT when the Moon cleared the horizon is still 20.21. Note that, throughout, I use this implicit definition of moonrise (lower limb touching horizon), because the 1499 observation's purpose was supposed to be to gauge visually how far past conjunction (of the lunar center and Mars) the Moon had traveled, and this could hardly be done until the entire lunar disk rose.

Curiously, Davies contradicts this figure at DVC 1: "The precision of the methods of modern astronomy are such that we can extrapolate backwards to 1499, and say with great confidence [see fn 42] that the moon rose that night, one hour and twenty minutes after sunset." Since Davies has just quoted Vespucci as calling it 1¹/₂ hrs, he implicitly regrets (DVC 1) this near-agreement as one of his verifications of Vespucci's accounts — a "verification" that had evaporated by the time Davies published DVC (which recognizes that the correct figure is over 2h; §(E4)). How Davies grossly miscomputed this figure is a mystery. The most likely explanation is a simple slip of 1 hr: since the correct figure is 2⁵/₂₃°, a 1° error would explain DVC 1's 1°2²₂° figure almost exactly. However, this only produces another mystery: when Davies finally discovered this error, why did he then round 2⁵/₂₃° (virtually correct) down to the conveniently false result 2⁰5⁴° (§(E4))? — an alteration that was critical to vindicating Vespucci's S.America claim. Without this forced 4° error, Davies' placement of Vespucci would have been at a longitude less than 34°W; that is, 1499 mi east of S.America (§(F2), out in the Atlantic.

again, Davies' arbitrary rounding decision takes Vespucci to the west, towards S.Amercia. However, in order to allow for easier comparison, and so as not to make Davies' problems any worse, I will generally (in the calculations that follow: except at §(G3) adopt his value of 1°15′.

E7 Finally, I see that Davies includes no consideration of differential refraction. Since this is the only Davies error which helps his argument (moves Vespucci's location westward when corrected, I will do so. The difference in mean refraction between the lunar center at Moonrise (when the lower limb touches the horizon) and any object apparently 1°1/4 higher than that is: nearly 10°. This corresponds to about 5° of longitudinal difference, and this omission moves Vespucci to the west, 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 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 own very 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 a hundred times worse. In fact, since DVD 10 puts Vespucci at "37.75° W longitude, the formal factor is 3000!"

**F2** Was systematic fudgery at work in Davies' Vespucci analysis? Consider: whereas Davies did not think to correct for parallax, ET—UT, or differential refraction, he did find 4 numbers (§E2 & §E4—§E6) which produced his final Vespucci longitude (37°3/4°). And it is remarkable that [a] had he computed these 4 numbers correctly, Vespucci would have been placed by Davies' math at a point out in the Atlantic Ocean, hundreds of miles east of Brazil (this independently of the massive parallax gaffe, note); but, by a felicitous coincidence (one chance in 2¹⁵ 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) about to 31°W. But the easternmost point in South America (near Joao Pessoa, Brazil) is at longitude 35°W! So

F3 Momentarily forgetting parallax, ET—UT, and differential refraction (since Davies did): even dropping his arbitrary rounding-up of 1° + to 1°25′ (§(C8), §E6), Davies' accumulated errors (the math in all cases easily done correctly by a scrupulous analyst) nonetheless come to: 1° + 4° + 2° = 7° — a total of about 400 miles. (Including the effect of Davies’ odd §(C8) decision to round the Moon-Mars 1° gap upward would roughly 12°/day...
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 sinking a Davies onto a case where his wealthy employers desperately seek a legend’s exoneration — and you’ve just visualized the Davies-NGS report on Peary’s Pole claim.)

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

F5 Let us next perform a rough differential summing-up of the effects of correcting Davies’ extensive series of creative miscomputations of Vespucci’s moonrise “observation”. We have 7 Davies errors: 3 cases of a nonspecialist’s ignorance, and 4 cases of easily-knowable nudging of Vespucci westward by clumsy miscalculation or arbitrary roundings. The 7 error-corrections here: [a] parallel (24° eastward: §E1); [b] retro-retrograde Mars-flounderation and graph-misreading (1° eastward: §E2); [c] ET—UT correction (1° eastward: §E3); [d] miscomputed moonrise-time (4° eastward: §E4); [e] overrule rounding of lunar speed for time elapsed since conjunction (2° eastward: §E5); [f] up-rounding of 1°+ to 1°1/4 (roughly 5° eastward: §E6); [g] differential refraction (5° westward if Moon on horizon, much less otherwise: §E7, §G2—G3). Adding up all but [f] (for which I mercifully opted at §E6 to compute largely with Davies’ dubious up-rounding), we find a required total eastward longitude shift of 24°+1°+1°+4°+2°+5°-什么呢。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, but only if cautiously & competently handled.)

G The “Davies Movement” & Admiral Rehab’s New Kissmology

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

G2 And note that, realistically, it is improbable (as Davies implicitly agrees: §E7) that such an observation would be made with the Moon just clearing (lower limb touching) the horizon; if the Moon is instead assumed to be a few degrees up, then the solution is moved eastward both from [a] the rotation63 (of the observer along the terrestrial Equator) required to raise the event’s altitude and from [b] the attendant sharp decrease of differential refraction. Repeating the same problem (1499/8/23, Equator, 1°15′ visible Moon-Mars ecliptical longitude difference), but asking that the lower limb of the Moon be seen not on the horizon but at an apparent altitude of 1°, the deduced geographical longitude of

Vespucci is 8°W (20:22 LMT); for 2° up, 6° W (20:26); for 3° up, 4°W (20:30); for 4° up, 3°W (20:34). (Naturally, one computes the longitudes & times more exactly than displayed here, but the precision is meaningless in the context of a naked-eye report: §F1. So I round to the nearest degree of longitude and minute of time.)

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

G4 Any likely member of the foregoing families of solutions would put Vespucci well into Africa’s Gulf of Guinea (also astronomer Kowal’s solution, roughly 2000 mi east of Brazil. Even the most generous (to Davies) of our calculated geographical positions (0°N, 11°W: §G1), puts Vespucci well into African longitudes (just south of Liberia) — way east of the westernmost point of Africa (Dakar, Senegal: 17°1/2 W). But since the easternmost point of S.America (Brazil) is at 35°N (as noted: §F2), there is no chance that the purported Vespucci observation (which Davies’ 14 pp DVD paper has carefully “proved” was Brazilian) could have been made as far west as S.America now resides. In fact, if we put Vespucci at Davies’ location (37° 45′W tropical), the observed Moon-Mars ecliptical longitude difference48 at moonrise would be 2°10′ — roughly a degree larger than Vespucci’s reportedly observed value (“1 degree and several minutes”). Thus, a skeptical type might say that the report is altered, faked, or so inaccurate as to be worthless. But we will instead follow mentor Davies — to see where trust in Vespucci will transport us.

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

G6 Note that Davies is extremely convincing and [Melvillian] 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

62This is the same regardless of whether one uses null latitude (Equator) or (as Davies posits) about 4°S.
beckons. How pathetically blind of lesser scientists not to have noticed any of this — until the Navigation Foundation’s insightful President faced them with ironclad proof: proof that “will hold water with any scientist anywhere” (quoting Davies’ pre-publication reference to his 1989/12/11 Peary report for NGS: _BaltoMag_ 1989/7 p.86).

**G9** Many of the greatest geniuses of science are modestly unaware of their brilliance. Happily, the discoverer of the “Davies Movement” is not in the dark on this subject, either. As our Retro-Admiral Rehab has often reminded us (§B12), his clear superiority on navigational matters is based on “experience” (gruff, deepvoiced military-authoritative 1989/2/1 putdown) and “familiarity with normal practices of navigation” (NG App.A, e.g., p.2: emph added). As one may see even from Davies’ first published paper (_US Naval Inst Proc_ 1937/2): an unwarrented 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 saner on the Peary case, as hope-predicted at the end of _DIO_ 9.3 (1999) §6 fn 70. The foregoing 1990 paper is published here less with NGS in mind than with the thought of enlightening those who still kitlytitter 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](http://www.dioi.org/EMS-facts.pdf), CalTech’s Standish (fn 41). On the 100th anniversary of Peary’s claim, the _NYTimes_ Science page reported (see internet citation at §A1 above), that NGS officially still held with the NavFou report! (though no longer publicly defending its 1909 N.Pole embarrassment: §4 §K1) — so the _NYT_ writer found it irresistible to spoof an oft-repeated 1909/12/30 (_Independent_ magazine) comment on Cookites’ impenetrable loyalty — “There will be a `Cook party’ to the end of time” — by observing that:

“There will be a Peary party too.”

---

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