

‡5 **Incontinental Drift: the “Davies Movement”**  
**Snow Job and the Seven Dwarfs**  
**National Geographic’s Unimpeachable Longitude Authority**  
**Proves Errant Brazil Kissed Africa, Under 500 Years Ago!**  
**Plus Further Navigation Founderation HyperDiscoveries**  
**Moon a Planet! Tachyonic Tectonics! Relativity a Hoax!**  
 by  
**Dennis Rawlins 1990/3/30**

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

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

**A1** The uncontrollable urge, of “Navigation Foundation” [NavFou] President Tom Davies (Rear-Admiral USN), to exonerate dubious explorers via equally dubious scholarship, is here examined in a little-known 1988 incarnation<sup>1</sup> 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*<sup>1</sup> of research into the Amerigo Vespucci controversy, illustrating his points with a score of slides, as well as the planetarium’s reproduction<sup>2</sup> 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](http://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](http://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.Léite “spends many pages running down Vespucci” [quoting Leite: “a fatuous person not capable of innovative thinking, amateur astronomer, navigator only average, cosmographer who repeated concepts of others, false discoverer who appropriated the glory of others” (similar to ‡3 §A1)]. Davies’ all-too-evident [rather religious] distaste for skeptics here is prototypical of his current hagiographic Peary reports.)<sup>3</sup> Davies’ astronomical-navigational computations contended that Vespucci’s purported tropical observation of Mars’ lunar distance on

<sup>1</sup>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.]

<sup>2</sup>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.

<sup>3</sup>NG 2: “Peary . . . mercilessly pilloried by a vociferous minority”. (Reviewers have universally noted that Herbert’s 1989 book *Noose of Laurels* is exceedingly gentle regarding Peary’s sins. DR’s

1499/8/23 proves the observer was at longitude  $c.37^{\circ}.75$  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'."<sup>4</sup>

**A2** However, when Davies' work is corrected for various extraordinary astronomical-math howlers (one of them spectacular both for its size and for what it reveals of his NGS-advertised Expertise), his calculations prove instead that Vespucci was at least  $27^{\circ}$  or 1600 [nautical] miles east of Davies' deduced Brazil position — at  $11^{\circ}$ W, the longitude of Liberia<sup>5</sup> (Africa). Since I made this little item public (1989/12/11), Davies has been asked by reporters<sup>6</sup> 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.<sup>7</sup> One isn't accustomed to [seeing US Admirals departing] under fire.

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

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

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

<sup>6</sup>*Washington Post* (1989/12/11 & thereafter) and *Washington Times* (1990/2/12).

<sup>7</sup>Davies will say only that the paper is in his private files and wasn't (as initially reported) delivered to the SocHistDisc on 1985/11/16. (Davies protests he was abroad at that time.) But he has repeatedly refused to answer the *Wash Post*'s queries as to where he *did* deliver the Vespucci paper. Davies' artfully selective responsiveness could impart the false impression that his Vespucci analysis was never presented in public. But the paper was given publicly (and the printed text distributed), repeatedly. (On 1990/2/27, the Ass't Director of the Fels Planetarium confirmed that Davies had indeed delivered his Vespucci paper there. DVC 11 itself refers to the 1984/10/17 presentation as a "lecture".) And this point is far less important than the following facts: [a] The paper was submitted by Davies to National Geographic for publication. [b] According to delightedly enthusiastic NGS Senior Assoc Editor Joe Judge (1989/12/11 morning), publication of Davies' Vespucci researches was being quite seriously

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

considered as late as 1989/12/11 — right up until DR revealed the paper's epochal implications [at a press conference later] that day, at NGS. (See *Wash Post* 1989/12/12 & *Wash Times* 1990/2/22.) [c] Judge's preening 1989/12/11 expansiveness also included the information that Davies' Vespucci paper was the direct cause of Davies' happy selection (by Judge, who refused to share that supreme credit with the US Naval Observatory) as NGS' Peary investigator (§A3). (Judge & Davies both reside in fashionable Potomac, MD.) My two favorite Joe Judge statements are neatly juxtaposed by Kelly (*Wash Times* 1990/2/22): "this [Vespucci] article . . . so impressed . . . Judge that he saw to it Mr.Davies was brought in to oversee the [NGS] Peary project. . . Mr.Judge dismisses the Vespucci [analysis] dubiousness]. 'What the hell has that to do with Peary?' he asks." (And see §A3.) The precious rareness of National Geographic's knack for entertaining the public remains unquestioned.

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

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

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

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

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

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

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

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

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

**B4** A classically perfect case of Dangerously-Little-Knowledge. [Which sadly undoes Davies' earnest try at *adding to knowledge* — unlike *DIO 22 ‡3's* zoo of *subtractors*.] I recently gave the Davies analysis to one of the world's best known and most respected astronomers, Charles Kowal (Chiron's discoverer; now at Space Telescope Science Institute). His amusingly incredulous appraisal of this keystone paragraph: it's based on an error "any freshman astronomy student wouldn't make" (*Wash Post* 1989/12/12; see also *Wash*

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

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

*Times* 1990/2/22). Kowal independently also finds that Vespucci's 1499/8/23 "observation", correctly computed, puts him near Africa (§G4), not Brazil. As is self-evident to [any positional astronomer]<sup>14</sup> a conjunction of the Moon with another celestial object (planet or star — or the Sun, as in a solar eclipse) will in general NOT occur "simultaneously"<sup>15</sup> at widely separated points on the earth." If such a fantasy were in fact able to [materialize], then solar eclipses would be seen the same way and at the same time for all observers (who could see the eclipse at all), the world over! [The same elementary confusion of use of solar & lunar eclipses in an establishment attack on DR occurred as recently as late 2016 in *Isis*, History of science Society's flagship journal: www.dioi.org/vols/wm0.pdf, *DIO 22 ‡1 §D*.] Most of us, from professional astronomers to highschoolers, have heretofore believed otherwise. But it seems we all have to catch up to Davies' revolutionary New Astronomy: solar eclipses are henceforth to be seen simultaneously & at the same magnitude all over the entire sunward side of the Earth. Thus, the Moon's solar-eclipse shadow does not move over the Earth's surface — and it has no locale. (Understand: this is the [NGS-quarter-million-dollar-remunerated] Expert — demonstrably innocent of the behavior of the best known shadow in astronomy, the solar-eclipse umbra — who on 1989/12/11 assured the public that his Navigation Foundation has competently analysed [on NGS grounds] the shadows and spatial relations in Peary's photos and thus concluded that he got to the Pole.)

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

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

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

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

one who had ever opened the *Almajest* would know that it is divided into 13 “Books”, not “Volumes”. It happens that DR has published at least 4 scholarly analyses discussing this very [*Almajest*] chapter (*Publ Astr Soc Pacific* 94:359, 1982, end of App.A; *Isis* 73:259, 1982, n.17; *Vistas in Astronomy* 28:255, 1985, §3; *Amer J Physics* 55:235, 1987, n.14). So, when I read Davies’ citation of it, I knew instantly that Peary was not the only US Admiral who faked when he pleased. Yes, *Alm* 7.3 contains observations of lunar conjunctions with Spica, two (not one) from Alexandria and one from Rome. The catch is that the Alexandria observations of Moon-Spica conjunctions were in 294 BC & 283 BC (by Timocharis), while the Rome observation of a Moon-Spica conjunction was in 98 AD (by Menelaos) — **almost 4 centuries later**. This large time-interval is the heart and basis of Ptolemy’s entire discussion here (his demonstration, by lunar-conjunction data from different centuries, of the reality of precession,<sup>16</sup> 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<sup>17</sup> on 2 widely separated occasions (37<sup>d</sup> apart). But now we have a new champion in the scholarly pretense department: an expert who has discovered that “the same celestial event” was seen from 2 different terrestrial places at times nearly 400 years apart! (Back in §B3-§B4, Davies contracted events, separated by hours [fn 15], to simultaneity — now he’s compacting whole centuries! Is it unfeeling to pull the magic carpet out from underneath such delightfully accelerating science fiction. . . ?)

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

<sup>16</sup>Davies (who obviously did his navigation at sea out of standard cookbook-style Navy tables, with uneven comprehension) is as innocent of precession as of parallax. In his first Vespucci paper, we find (DVC 2-3): “we must divert again for a discussion of navigation and navigational methods of the 15th century. . . . The navigators of the northern hemisphere have always had an easy way of determining their Latitude roughly. The star called Polaris lies less than a degree from the point in the sky around which the celestial sphere (or sky) appears to rotate. Measuring its altitude above the horizon, which gives Latitude, was done by various means from the earliest times.” Polaris or  $\alpha$  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<sup>o</sup>.4 from the true pole in 1499 AD. Ptolemy notes (*GeogrDir* 1.7.4) that Hipparchos (c.130 BC) found it 12<sup>o</sup>2/5 distant from the pole. (The bright star then nearest the pole was at declination 82<sup>o</sup> +: Kochab or  $\beta$  UMi, more than half again closer to the Pole than  $\alpha$  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.

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

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

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

**B10** The obvious reason one must include parallax in any computation of the Moon’s observable non-zenith celestial position is that the Moon is so close to us: its mean distance from the Earth’s center is only 60.3 Earth radii (ER). Thus, when seen near the horizon, the Moon’s apparent position is lower (on the average) by 1/60.3 radians or 57.3/60.3 degrees (57 arcmin or 57’’) as compared to an ephemeris’ computed position, which is invariably *geocentric*. (And when was the last time you observed the Moon from the Earth’s center?) For a tropical observation near the horizon (such as the Vespucci 1499/8/23 “observation” analysed by Davies), ecliptical parallax is nearly an entire degree. That is, the observer-centered or “topocentric” lunar celestial longitude<sup>21</sup> will differ from the *geocentric* position by roughly a degree. So, for tropical moonrise, the observed (topocentric) celestial longitude will be roughly 1<sup>o</sup> 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,<sup>22</sup> 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<sup>o</sup> of longitude) Davies asserts Vespucci made (due to poor tables), which Davies claims (DVD 10) to have “vastly” improved! (In truth, if Davies’ speculative §C8 reconstruction of Vespucci’s math is corrected for parallax etc., the resulting longitude is by chance much nearer Brazil than is Davies’ own modern-calculated longitude, similarly corrected. . . .)

<sup>19</sup> 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.

<sup>20</sup> Similar Davies innocence interprets oversurely the Vespucci §C2 statement, that he was “correcting with calculations from the Alphonsine Tables”, as merely referring to a 90<sup>o</sup> longitude shift from Regiomontanus’ meridian to Cadiz (a simple addition, hardly worth mention as plural and noteworthy “calculations”, especially since Regiomontanus’ own tables show about this time-difference between Spain and Nürnberg). Davies indicates (DVD 9) that, in order to check on this easy matter of the 90<sup>o</sup> 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.

<sup>21</sup> Celestial “longitude” is not the same as geographical longitude. Here, the former is virtually the position measured along the great-circle ecliptic’s 360 degrees, the approximate path of the Moon’s motion — which is the “clock” (see fn 22) being used by the lunar distances method.

<sup>22</sup> 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<sup>o</sup>.2/day or 33’/hour. Since the Moon’s mean diameter is barely 31’, a rule of thumb is that the Moon’s average geocentric motion is: its own diameter every hour. However, this average is seriously affected by the eccentricity and perturbations of the lunar orbit as well as by parallax; thus, for reliable investigations, one must compute precise absolute positions and use differential methods only with great care.

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

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

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

## C Vespucci's Alleged Observation & Calculation

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

**C2** "As to longitude, . . . I was put to great pains to ascertain the east-west distance that I had covered [since leaving Cadiz 1499/5/16]. . . . I found nothing better . . . than to . . . take observations at night of the conjunction of one planet with another, and especially

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

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

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

<sup>26</sup>Original now lost. The official Hakluyt Society collection of Vespucci's output omits the letter on which Davies' entire paper is founded, stating that it is one of "three spurious letters now so universally held to be forgeries, that they need not occupy our time" (Markham 1894 p.iii). (DVC 6 calls the letter's genuineness "unquestioned".) Reminds one of the prime issue raised by Mencken's 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; 1918. (DVC 4 says there is archival evidence that Hojeda returned in 1500 June. And J.Hitt tells me [1990/3/1] that there is said to be similar proof that someone with a name similar to Vespucci's sailed with Hojeda.) For another problem noted in this review, see fn 31.

<sup>27</sup>Slightly misdated as 1500/7/15 at DVD 11 but correctly dated elsewhere by Davies.

of the conjunction of the Moon with the other planets, because the Moon is swifter in her course than any other planet. I compared my observations with the Almanac of Giovanni da Montereggio [Regiomontanus], which was composed for the meridian of Ferrara [actually Nürnberg]<sup>28</sup> correcting with calculations from the tables of King Alfonso [the 'Alphonsine' tables].

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

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

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

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

<sup>28</sup>DVD's Fig.6 caption notes this correctly. However, Davies' 1988/9/18 memo on the Regiomontanus tables says: "Note: Regio's time is at [Prussian] Königsberg, Longitude E 14-30." [Actual Königsberg: 54° 42' N, 20° 29' E.] This odd confusion arose because Johannes Müller (Regiomontanus) would sign his works (e.g., bottom of last page of *Der Deutsche Kalendar des Johannes Regiomontan* — same year and computational base as his *Ephemerides*): "M. Johan von Königsberg." But this has nothing to do with the meridian or the publication-place (which was in fact Nürnberg [fn 50]) of the Regiomontanus tables — rather it signifies that he was born (1436 AD) at the other Königsberg (in Franconia: 50° 05' 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, an-der-Oder at 14° 32' E instead of am-Main 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?

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

reached 50°S. But latitude (unlike longitude) is so easy to determine astronomically that there can be no Davies-rehab by “modern” recomputation of celestial data. In a similarly Münchhausenesque vein, Vespucci claims on his supposed 1<sup>st</sup> voyage to have gone along an American coast (starting at 23°N)<sup>30</sup> 870 leagues<sup>31</sup> to the NW — over 3000 miles! (I use Vespucci’s 3.6 mi leagues: see fn 31. Markham 1894 p.xxvi comments: “Such a course and distance would have taken him right across the continent of North America into British Columbia.”) [b] Off the coast of Brazil on 1499/8/23, the Moon rose about 2 1/3 hours after sunset (not 1<sup>h</sup>1/2).<sup>32</sup> [c] It is impossible for the Moon to have moved nearly 4°1/2 in the reported 4<sup>h</sup>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.<sup>33</sup> I recommend the observation of Harvard’s S.Morison *European Discovery of America: the Southern Voyages* Oxford 1974 p.309: “Let it be remembered that Vespucci lived until 1512 and thus had plenty of time to disassociate himself from the Soderini and the Medici printed letters, had he chosen to do so.” On p.308, Morison comments: “you cannot convince anyone who has the Vespuccian faith.” UFOlogists, Pearyites&Cookites, [Strats&Oxfordians] are no different. Morison generously credits Vespucci with 3 journeys (albeit in a trivial rôle), but also regards him as a repeatedly exposed “Liar”: p.297. And pp.294-295: “We regard all the pretentious apparatus of celestial navigation in Vespucci’s writings as so much dust thrown in the eyes of important Spaniards and leading Florentines. . . . It would weary the reader to pick out every inaccurate statement made by our genial faker. His distances are palpably wrong . . . . His claim to have used lunar distances to find longitude is fantastic.”

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

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

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

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

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

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

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

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

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

reconstruction) that the observation was made  $5^h 1/2$  or  $82^\circ 1/2$  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 celestially deduced precisely the same longitude as Vespucci later pseudo-celestially found! —  $5^h 1/2$  (or  $82^\circ 1/2$ ). (And in 1500 AD, this was the only available Columbus astronomically-based longitude. Columbus' 1504 longitude-estimate obviously hadn't occurred yet.) Specifically, Saona ( $18^\circ 07'N$ ,  $68^\circ 42'W$ ) was placed (by Columbus' 1494 eclipse-based calculation) just  $82^\circ 1/2$  west of Cape St. Vincent ( $37^\circ 01'N$ ,  $8^\circ 59'W$ ; near Cadiz which is at  $36^\circ 31'N$ ,  $6^\circ 18'W$ ). Note the coincidences that Saona is just north of Aruba (where Stein induces that Vespucci's nonastronomical writings put him), only about  $6^m$  east of it, while Cadiz ( $6^\circ .3 W$ ) is only  $11^m$  east of Cape St. Vincent ( $9^\circ .0 W$ ), both places being in the west Iberian peninsula. Thus, the Vespucci  $5^h 1/2$  longitude "deduction" is a virtual replication of Columbus' result. (Note that, though his result was poor, Columbus' chosen astronomical method for finding his longitude at sea was fairly reliable: fn 15, fn 50. Vespucci's was not: fn 36.) Even the sympathetic Stein (1950 p.351) concludes that longitude  $82^\circ 1/2$  must therefore be a "presupposed value". Which would require that Vespucci (or someone) did not compute to it but from it. The implications are too obvious to belabor here.

**C10** Noting the striking equality of the degrees and hours (both  $5 1/2$ ) in Vespucci's report, it is credible to suppose<sup>37</sup> that Vespucci merely equated hours of lunar motion with degrees and thus made just as simple a calculation as appears on the surface — if he computed at all. I suspect that the whole 1499 report may be merely a mizzled<sup>38</sup> 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.<sup>39</sup> "The investigation of Vespucci's statements

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

<sup>37</sup>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.

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

<sup>39</sup> See Las Casas at, e.g., Markham 1894 p.81. And see *ibid* p.xi, 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 . . ." (J.Parry *Discovery of South America* 1979 pp.99-102 suggests this might not be meaningful but notes at the same time that: [a] no Vespucci account is definitely genuine, and [b] the founess 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  $1^\circ$ /hr, then the 2 "observations" are in perfect geocentric accord — and, additionally, the striking juxtaposition of  $5^\circ 1/2$  and  $5^h 1/2$  is also explained; this simply & immediately yields the result:  $82^\circ 1/2$  longitude.

## D Admiral Rehab's "Surprising Correlation"

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

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

**D3** "Precision in dealing with the positions of celestial bodies at any time, the tabulation of which is the essence of an Almanac, is a relatively recent development. The first<sup>40</sup> modern theory of the Moon, compiled by Brown in 1919, is still the definitive theory.<sup>41</sup> 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.<sup>42</sup> Figure 3 shows a plot of the Celestial Longitude for these bodies, with the time of conjunction at Greenwich, 20h 06m, indicated by the intersection<sup>43</sup> of the two curves. From this evidence we can determine for ourselves what Vespucci's real longitude must have been, regardless of his calculations. [DVD 10]

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

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

<sup>40</sup>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.

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

<sup>42</sup> Whenever Davies says anything about Vespuccian astronomy "with great confidence", he's reliably mistaken. See also fn 57.

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

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

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

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

**D7** "What conclusions can be drawn from this use of our *modern data and methods of analysis*? The literature includes several earlier but incomplete analyses of this incident: while such distinguished critics as the astronomers<sup>45</sup> Hermann Wagner (1917) and Duarte Leite (1958) have denigrated Vespucci without any<sup>46</sup> 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.<sup>47</sup> From this analysis I draw the following conclusions: 1) Vespucci

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

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

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

<sup>47</sup> Davies' pretense here (see also above fn 3) that he is familiar with "the literature" is just one more of his scholarship-poses. Had he actually searched the literature on this conjunction, he would swiftly have found a wellknown book on Vespucci (Pohl's) which computes his 1499/8/23 position (though not very accurately), alleging that it agrees with his writings in placing him off the coast of Venezuela. And a citation in another popular book (G.Arciniegas *Amerigo & the New World* 1955 pp.193-194) would have informed Davies that, in 1950, Vatican astronomer J.Stein checked Vespucci's alleged location by celestial computations — finding it consistent with the isle of Aruba (12 1/2 N, 70°W: just north of Venezuela), using the very datum Davies discards (in favor of the impossible moonrise "observation"), namely: the second (midnight) "observation". (*Memorie della Società Astronomica Italiana* 21:345; 1950. Stein of course realizes one cannot fix position very exactly this way, so he attempts to locate Vespucci from his writings and then just checks how well the midnight observation agrees with this location.) Davies might also have been enlightened by Stein's calculation of the Moon-Mars moonrise situation (the "observation" which Davies analyses at such length). Even Stein, an admirer of Vespucci and a defender of his integrity, declares the moonrise observation "fictional". This for the very simple reason that, naturally accounting for lunar parallax, he finds (as do Kowal & I: §G4), that, at 1499/8/23

understood and attempted to apply the method of lunar distances to the determination of longitude well before the 1514 [discovery] date ascribed to Johannes Werner, 2) the location of Vespucci on the 23<sup>rd</sup> of August, 1499, has been *reasonably established* as on the northern coast of South America, somewhere in the modern state of Ceara (in Brazil). *The [Davies] calculations are in accord with the [Vespucci] recitation of the geographical details of the voyage.* [DVD 13]

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

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

## E Rearward-Admiral's Navigation Founders

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

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

moonrise on the north coast of S.America, the Moon-Mars separation was well over  $2^\circ$ , not barely  $1^\circ$  as Vespucci reported (Stein 1950 pp.350-352). Yet again we find: had Davies made even a modest effort to examine the very literature he consistently pretends to have consulted, he would have learned of Stein's warning, as well as the need for including the lunar parallax correction (repeatedly noted by both astronomer Stein and even Pohl 1944 pp.68, 219 n.12) — an item on which one would not have supposed an expert required education. . . .

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

<sup>49</sup>Based on a few discrete, well separated positions provided by the Naval Observatory.

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

novice would instantly discern, Mars was obviously proceeding in retrograde: rear-ward. But Rear-Admiral Davies' Fig.3 shows<sup>51</sup> Mars' motion as direct (forward: positive slope) not rear-ward (negative slope). By contrast, the Retro-Admiral's Fig.6 has Mars moving rearward at the very same time: negative slope. (Navigation Floundering?)<sup>52</sup> Or, given the manipulation of velocities possible under Lorentz transformation, do we have here yet another hint of the Davies paper's curiously-undeclared relation to relativistic math?) By miseyballing in the usual direction (the direction that will get Vespucci westward to S.America, where he "belongs"), Davies finds a too-high conjunction-time from his graph. Were the graph's lines (**NB**: Davies' shadow-lines are his basis for supporting Peary) drawn & read correctly, he would find about 20:02. The 4<sup>m</sup> 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<sup>y</sup>, since the mean synodic motions of the Moon and Mars were both so well known. I have already cited above the probable cause of Regiomontanus' large Mars errors for 1499/8/23: §E2. For a discussion of his precession, see Wagner 1918 pp.157f. Precession errors would of course not affect times of conjunction.) Note that Ptolemy's mean synodic Mars tables (probably based on ones issued at the outset of Kleopatra's reign) are still — in 1989 AD! — off by only 0.4. Incidentally, Ptolemy unfaithfully included lunar parallax in his work: see *Alm* 2.10-13 for math analyses, plus pages of tables for its computation. So it appears that even astrologer Ptolemy (whose "observations" were fictional and whose tables were simply appropriated from prior observing scientists: Rawlins *Amer J Physics* 1987/3) was, as a conceptual astronomer, superior to Davies. There is some suggestion that Vespucci or his source computed with parallax (above: fn 20). There is no doubt that Regiomontanus accounted for parallax, as the most cursory Davies comparison of Oppolzer's wellknown *Canon* with Regiomontanus' solar eclipse times would have shown: these times are all corrected with parallax for Nürnberg, which is at 49°27'N, 11°04'E. Morison *Admiral of the Ocean Sea* (unabridged 2-vol edition) 1942 1:251, 262-3 n.35 correctly remarks that Regiomontanus' tables were really issued for workers of horoscopes, not navigators. Regiomontanus invented a still-popular astrological house-division system. (I see that Davies' caption to DVD Fig.2 confuses houses with zodiacal signs.) We note that Regiomontanus tabulates not just conjunctions but all five of the aspects astrologers live by: conjunction, sextile, square, trine, & opposition. (See right page of Regiomontanus sample reproduced at Fig.2 of DVD.) None of the others would be of serious navigational use to a sailor of 1500 AD, except for those occasional Sun-Moon oppositions that were near enough to a lunar node to result in visible lunar eclipses — which Regiomontanus does tabulate. Columbus says he used Regiomontanus' predicted lunar eclipses for longitude-determination. The Regiomontanus ephemerides' error for the 1494/9/14-15 eclipse (§C9) seen by Columbus at Saona (18°07'N, 68°42'W) was +20<sup>m</sup> (7:45 Nürnberg Apparent Time = 6:52 UT predicted vs. 6:32 UT actual). Columbus also used the 1504/2/29 lunar eclipse in Jamaica. (Both for longitude and for impressing the natives. Vespucci's astronomy likewise impressed the natives. Back home [fn 12].) On this occasion, Regiomontanus' error was +27<sup>m</sup> (1:36 Nürnberg Apparent Time = 1:02 UT predicted vs. 0:35 UT actual). These mistakes caused westward errors in Columbus' deduced longitudes: 5° (1494) and 7° (1504) — though both his eclipse-based longitudes' total westward errors are far larger. I note that the rms lunar-eclipse-time error of Hipparchos' wellknown 146 BC lunisolar tables was 16<sup>m</sup>, an error later reduced by him to just 10<sup>m</sup> [www.dioi.org/vols/w11.pdf, *DIO 1.1* ‡6 eq.32]. So the accuracy of Regiomontanus' eclipse predictions appears to be not quite as good as Hipparchos'.

<sup>51</sup>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.

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

**E3** Davies' Fig.3 provides 1499/8/23 ecliptical longitudes of Moon and Mars graphed vs. "Greenwich Mean Time" or Universal Time. But Earth's gradual spin-deceleration renders UT not apt for celestial mechanics, thus ephemerides are computed with terrestrial dynamical time (TDT) or (a few years ago) Ephemeris Time. (The difference between TDT and ET is trivial in this context, so I will speak of the traditional ET below.) On close examination, I found that my own ephemeris program gave positions in neat accord with Davies' Fig.3 if I used ET where he speaks of UT. But UT was less than ET by several minutes in 1499 AD. So I checked with the writer (the astronomer Davies cites: §D3) of the Naval Observatory program Davies uses and was told (1989/11/30) by him that indeed his program is for ET or TDT, not GMT (or UT). (I make the ET-UT difference to be +4<sup>m</sup>. 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 miscalculates the time of moonrise, presumably<sup>53</sup> by consulting the lunar Right Ascension for a time previous to the event. We recall that Davies speculates-reconstructs Vespucci's supposed computed conjunction time as 17:00 local time (DVD 9: §C8); had Davies used this time to compute the Sun-Moon Right Ascension difference from the Greenwich time positions given him by the Naval Observatory, he would have found this Right Ascension difference to be 14<sup>h</sup>05<sup>m</sup> — just as required for his stated result [§D4] that the equatorial moonrise-sunset difference equals 2<sup>h</sup>05<sup>m</sup>. But this hypothesis requires that Davies forgot to convert Greenwich time to local time when performing the computation. (Traces of a possible occurrence of the same NavFou-Snafu<sup>54</sup> glimmer through in the "patently absurd"<sup>55</sup> 1906/2/27 Cape Hecla pseudosolution [fn 54] of the Peary Betelgeux Document,

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

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

<sup>55</sup>Borrowing diplomat Davies' gloriously malapropos language (regarding DR) at NG 116 n.2: "The [Rawlins F11] notion that the [Peary 1909] soundings are still on the [modern hydrographic] charts is patently absurd in view of the vast of [sic] amount of bottom data available at the time Rawlins was writing (1973).]" In fact, all of the bottom-touching 1909 Peary soundings are on the chart CP7 GEBCO C1 (1966/12) published by the International Hydrographic Bureau; also on provisional 1967 Canadian Hydrographic Service chart #896. I possess not only copies of both charts but a thick file of 1967-1973 correspondence with three national hydrographic offices (US, Canada, UK) discussing in detail the advisability of retaining such loosely-placed soundings on modern charts. (There was never any question that the Peary soundings were on the charts — that point was obvious and undisputed by all parties.) Davies believes (NG 121) that, for what it's worth (so far from the Pole), modern bathymetry has at least vindicated the longitude of Peary's 1909/3/20 sounding: 310 fathoms or 567 meters at c.85°.4 N, supposedly near 70°W longitude. If the Peary soundings' retention on modern maps infected the NF's work, it's possible that a key west-extended spur in the NF's 500 m isobath in this

“independently” produced & agreed to by various [hypnotized teamplayers] of Davies’ univice [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),<sup>56</sup> the local mean time (LMT) when the lower limb of the Moon was seen clearing the horizon was 20:21 LMT, not 20:05. (DVD 10; previously, DVC 8 had it 19:33, by accepting Vespucci’s false report that moonrise occurred 1<sup>h</sup>1/2 after sunset.)<sup>57</sup> This 16<sup>m</sup> 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<sup>h</sup>.5 (§C8); but the Moon’s actual geocentric motion (see slope in his own Fig.3) at this time was distinctly faster than 1/2 degree per hour: 31°.5/hr or 12°.6/day. Thus, the correct amount of time it would take for the Moon to move 1.25 geocentric degrees would be 2<sup>h</sup>23<sup>m</sup>; so: an error here of 7<sup>m</sup> or about 2 degrees of longitude (moving Vespucci towards the west — as usual). To assume that Davies didn’t know this is to assume he can’t do grade-school arithmetic: 75’/(31°.5/hr) = 2<sup>h</sup>23<sup>m</sup> or 143<sup>m</sup>. 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

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region (a spur stretching nearly to the 70°W meridian) on the NF digital-terrain-model bathymetric map (reproduced at NG 122-123 & NGD 49) is ultimately based on the 1909/3/20 sounding. A 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* profile, but does not (near 85°.4 N, 70°W) agree with the NF model (NG 122-123). The NF claims (NG 120) that its modifications of the 1985 map had no effect on its evaluation of the Peary claim; however, the NF could defend the 567 meter 1909 sounding only by altering the 1985 Navy map: stretching its 500 meter isobath far west of its original western bound, as a comparison (NG 119 vs. NG 122-123 or NGD 49) readily shows.

<sup>56</sup>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.

<sup>57</sup>Curiously, Davies contradicts this figure at DVC 1: “The precision of the methods of modern astronomy are such that we can extrapolate backwards to 1499, and say with great confidence [see fn 42] that the moon rose that night, one hour and twenty two minutes after sunset.” Since Davies has just quoted Vespucci as calling it 1<sup>h</sup>1/2 hrs, he implicitly regards (DVC 1) this near-agreement as one of his verifications of Vespucci’s accounts — a “verification” that had evaporated by the time Davies wrote DVD (which recognizes that the correct figure is over 2<sup>h</sup>: §E4). How Davies so grossly miscomputed this figure is a mystery. The most likely explanation is a simple slip of 1 hr: since the correct figure is 2<sup>h</sup>21<sup>m</sup>, a 1<sup>h</sup> error would explain DVC 1’s 1<sup>h</sup>22<sup>m</sup> figure almost exactly. However, this only produces another mystery: when Davies finally discovered this error, why did he then round 2<sup>h</sup>22<sup>m</sup> (virtually correct) down to the conveniently false result 2<sup>h</sup>05<sup>m</sup> (§E4)? — an alteration that was critical to vindicating Vespucci’s S.America claim. Without this forced 4<sup>h</sup> error, Davies’ placement of Vespucci would have been at a longitude less than 34°W: that is, ordmag 100 mi east of S.America (§F2), out in the Atlantic.

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

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

## 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<sup>59</sup> the nonspecialist reader frankly (what is obvious to an astronomer): since the observational basis is obviously uncertain by ordmag 10’ (e.g., §E6), the deduced result, even if it were correctly computed, must be uncertain by roughly ±5° of longitude or 300 miles. (Davies, hired by National Geographic to act as its allegedly neutral judge, overseeing its investigation of its very own Peary hoax, has gotten Peary to within ordmag 10 mi of the Pole! As a knowledgeable science-newspaperman has already commented: sounds like a classic case of high-precision-low-accuracy.<sup>60</sup> Indeed, we notice that the DVD analysis of Vespucci carries this ideal to extremes: Davies’ nominal precision is a quarter-degree, while his accuracy is roughly 30°: about a hundred times worse. In fact, since DVD 10 puts Vespucci at “37.75” W longitude, the formal factor is 3000!)

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

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

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

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

<sup>60</sup>Compare to the attitude of Stein in fn 47; see also §G2.

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

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

**F5** Let us next perform a rough differential summing-up of the effects of correcting Davies’ extensive series of creative miscomputations of Vespucci’s moonrise “observation”. We have 7 Davies errors: 3 cases of a nonspecialist’s ignorance, and 4 cases of easily-knowable nudging of Vespucci westward by clumsy miscalculation or arbitrary roundings. The 7 error-corrections here: [a] parallax (24° eastward: §E1); [b] retro-retrograde Mars-floundering and graph-misreading (1° eastward: §E2); [c] ET–UT correction (1° eastward: §E3); [d] miscomputed moonrise-time (4° eastward: §E4); [e] overcrude rounding of lunar speed for time elapsed since conjunction (2° eastward: §E5); [f] up-rounding of 1° + to 1° 1/4 (roughly 5° eastward: §E6); [g] differential refraction (5° westward if Moon on horizon, much less otherwise: §E7, §G2–§G3). Adding up all but [f] (for which I mercifully opted at §E6 to compute largely with Davies’ dubious up-rounding), we find a required total eastward longitude shift of 24° + 1° + 1° + 4° + 2° – 5°, which comes to about 27°. This moves our Retro-Admiral’s impressively computed longitude solution from 37° 3/4 W to about 11° W — quite close to the correct result, directly (nondifferentially) computed (§G1). (The foregoing breakdown of errors shows that differential methods — of the sort Davies tries — can work,<sup>61</sup> 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).<sup>62</sup> So this is the actual solution to Davies’ problem *as he himself posed it* (not the first time he’s required such assistance: see fn 14 & fn 57) — roughly 27° or about *sixteen hundred miles distant* from the Vespucci (Brazil) location Davies has deduced.

**G2** And note that, realistically, it is improbable (as Davies implicitly agrees: §E7) that such an observation would be made with the Moon just clearing (lower limb touching) the horizon; if the Moon is instead assumed to be a few degrees up, then the solution is moved eastward both from [a] the rotation<sup>63</sup> (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

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

<sup>62</sup>Moon: geocentric 13° 41’ (celestial longitude), –4° 42’ (celestial latitude); topocentric unrefracted 14° 32’, –5° 03’; topocentric refracted 14° 03’, –4° 51’. Mars: geocentric 13° 08’, –4° 54’; topocentric refracted: 12° 48’, –4° 46’.

<sup>63</sup>This approach is assisted by the fact that differential parallax is null at the horizon.

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

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

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

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

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

**G7** And so Rear-Adm.Davies has led us to the door of a much more revolutionary realization than anyone expected to come out of his heretofore unjustly neglected legacy 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-hubboggery 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

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

beckons. How pathetically blind of lesser scientists not to have noticed any of this — until the Navigation 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 unwarranted attack upon mathematician E.Willis, from his youthful, pre-Nobel period: he has long been aware that mere professors are not nearly as smart as he is.<sup>65</sup> (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:

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

### Afterword [2017]:

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

We see from ‡4 §K1 here that, during the last decade, National Geographic has evidently gotten saner on the Peary case, as hope-predicted at the end of *DIO* 9.3 (1999) ‡6 fn 70. The foregoing 1990 paper is published here less with NGS in mind than with the thought of enlightening those who still kitty-litter 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 100<sup>th</sup> 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.

<sup>65</sup>When Davies 1<sup>st</sup> (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.