

‡5 Unpublished Letters

A Banned in England: Another Astronomer-Royal Suppression

In reaction to our publications on the Neptune affair (*DIO 2.3 ‡9* & *DIO 4.2 ‡10*), some have responded with disbelief that Astronomers Royal would suppress material. This is a peculiar reaction, considering that it is a matter of record that the greatest of Astronomers Royal, George Airy, suppressed key parts of [a] his own 1846/7/9 letter to Jas. Challis (*DIO 2.3 ‡9 §B2*), and [b] Challis' 1846/10/12 letter to Airy (*ibid §D7*).

A1 I've recently found (Cambridge, 1996/9/20) a letter by Airy's successor as Astronomer Royal (from 1881, after J.C.Adams refused the post), Wm. Christie, exhibiting the same penchant. (My thanks to St. Johns College archivist Elizabeth Quarmby Lawrence for assistance with my exam of the file containing this find: Adams mss Box 17.)

A2 I quote the entire letter (merely adding an occasional comma for clarity):

To: [Dr. Donald] MacAlister 1893/4/24

From: W. H. M. Christie, Royal Observatory, Greenwich, London, S. E.

A3 Before sending you the copies of the letters you asked for, I submitted them to Mr. W. Airy and enclosed is a copy of the letter he has written to me after going carefully through the whole correspondence. There are some other letters besides those to [Cambridge's Adam] Sedgwick which, I think, should not be published without some excisions — those of Leverrier in particular.

A4 As I am to a certain extent responsible in the matter, would you mind letting me see what you propose to publish, when the time comes? Leverrier's letters seem to me to require rather delicate handling, as he was evidently very angry with [John] Herschel when he wrote, but you will, I have no doubt, judge discreetly as to what should be published.

A5 The §A3 contemplation of censoring Airy-Sedgwick letters should be of particular interest to our readers, since *DIO 2.3 ‡9 §A6* specifically stated in 1992 that the remains of the Airy-Sedgwick 1846 Neptune correspondence indicated to DR that it had been protectively censored. *DIO's* full 1992 comment: "This is part of a series of Neptune ms disappearances suggesting systematic suppression of documents, a situation encouraging some otherwise unthinkable speculations." As for modern bans: 150th anniversary pieces appeared (1996/9) in *S&T* (Patr. Moore), *Astronomy* (Sheehan&Baum), & *Sci. Amer* (O Gingerich); all omitted the trifle that the key RGO file walked in the 1960s (*DIO 4.2 ‡10*). *Astronomy's* lawyeresque 1996/7/29 plea: its article was on the 19th not the 20th century!

A6 In pleasant contrast to such discouraging patterns: I wish to credit the Brit Astron Assoc for being unafraid — indeed proud — to welcome heresy: the BAA invited DR to give a 1996/9/21 lecture before its annual National Astronomy Week meeting (Birmingham) on his long-unorthodox view of the Neptune scandal. (The 150th anniversary of the discovery was 1996/9/23. Both the BAA & the audience were more than fair. I.e., I was not tarred&feathered — not even after asking why *England* was celebrating National Astronomy Week from 1996/9/23 to 9/30, this being the 150th anniversary of *precisely* the week [1846/9/23-30] during which England was the only nation in northern Europe that did **NOT** know where Neptune was.) Heavy post-lecture feedback reflected evaporation of old myths; e.g., DR had just dropped an unevadable bomb: Brit-hero Adams' final Neptune solution (Hyp X: *DIO 2.3 §§B4, E8, F3, Tables 1&2*) wasn't on *any* Berlin Starchart.

A7 While in England (1996/9/19, after being taken out of earshot of anyone else), I was privately briefed by an insider (who prefers anonymity) regarding the odd behavior of the chief suspect (a former high RGO official and confidante of the then-Astronomer Royal) in the disappearance of the Royal Greenwich Observatory's Neptune file: when

contacted many years ago (while RGO archivist Philip Laurie was alive) by leading British officialdom, as to the whereabouts of these mss (which he was the last to use) he simply did not reply! I know from two sources that he has, now that Laurie is dead (since 1983), begun claiming that the texts out of the letters he published from the missing file (years after its disappearance) were in notes given him by Laurie. I therefore immediately proposed (to both sources) that he be requested to produce these alleged notes, in Laurie's hand. (I would write him myself, but he will not answer my communications.) We now await the next chapter of this ongoing tale.

A8 I wish to add that virtually all British astronomical officials are innocent of the circumstances of the Neptune file's disappearance — and are (as if this needed to be said) involved in no conspiracy. They are as dismayed as DR at the continuing unavailability of a crucial documentary record of one the grandest chapters in the history of astronomy.

A9 It is a pleasure to reveal here an unexpected credit to the RGO in the Neptune affair. In 1847, Harvard's Prof. B. Peirce besmirched (by a misbegotten public attack) the mathematical legitimacy of the eternally-glorious predictive discovery of the 8th planet. (Since Harvard-trained physicist DR's detailed 1970 laying of this matter to rest [*Mon. Not. Roy. astr. Soc.* 147:177], Peirce's case is now taken seriously only by those less fortunate than ourselves mathematically, e.g., Harvard's Prof. O. Gingerich at *Sci Amer* 1996 Sept p.181.) As noted at *DIO* 2.3 ‡9 fn 5, John C. Adams eventually (1876) published a learned discussion of the fallacy in Peirce's reasoning. But who first discerned the error privately? Answer: the very party who (in popular mythology) has been most frequently & ignorantly abused as an idiot in celestial mechanics — no other than Astronomer Royal George Airy!

A10 The following letter¹ was found in the same file as the above Christie note.

To: J. C. Adams, Esq. 1847/4/29
 From: G. B. Airy, Royal Observatory Greenwich
 ... I was astonished to see Prof. [Peirce]'s remarks about the equation depending on $n - 2n'$ [i.e., the Uranus-Neptune 2-1 resonance]. Such are necessarily of long period, or, even if they constrain the orbits to exact commensurability, they do not produce sensible² disturbances in one revolution.

A11 Having for years (e.g., *DIO* 2.3 ‡9) defended Airy from the uncritically repeated charge that he was (*Scientific American* 1963/3) a "school-bright, hapless donkey" & "unusually conceited", I am gratified to find such positive proof of his intuitive expertise in the Neptune context. As for "conceit": well, the reason we have explicit evidence of his initial overskepticism towards the solubility of the Neptune problem (his 1834/11/23 letter to Thos. Hussey) is that *Airy himself published it* in 1846, along with the wry remark³ that, "It will be readily understood that I do not quote this letter as a testimony to my own sagacity". A final remembrance of Airy as a decent human being (far from the popular image of unfeeling machine):⁴ though he called James Challis (history's chief fall-guy for

¹The same letter also shows early motion in the process whereby British almanacs eventually (under Adams' direction, I believe) ceased calling Uranus "The Georgian" — a reconsideration born (of a sudden in 1846-1847) out of the desire to keep Neptune from being called "Leverrier" — as the Paris Observatory was then urging.

²[Note by DR.] I know this from practical experience in such problems. If the enormous 2-1 perturbation is completely ignored in the theories of the motion of Uranus and Neptune, and the disturbed elements are then re-determined on this basis, the resulting theory will track either planet (esp. Neptune), for several centuries, to an accuracy finer than the residuals which Leverrier & Adams were dealing with, when they solved the Uranus mystery.

³*Mem.RAS* 16:385-459 (p.389), or *Mon.Not.* 7:121-152 (p.125). Both 1846.

⁴Airy even attempted poetry. (His is probably even less known than Abe Lincoln's more impressive efforts in that direction.) See Airy's sincere memorial to the uplifting deeds and depressing end of James Cook (quoted at p.138 of Dava Sobel *Longitude* NYC 1995).

Britain's Neptune disaster) censurably inconsistent in his defenses of Adams (see *DIO* 2.3 ‡10 §D2) and "perfectly dreamy" in theoretical areas (*ibid* fn 28), he was sympathetic regarding the attacks (continuing to this day) upon Challis' miss of Neptune, which we have revealed (*ibid* §B4) was actually due primarily to Adams' wide range of disparate solutions (*ibid* Table 2) to an enormously difficult problem. (A problem Adams had the admirable courage to challenge even before being fully conversant with the relevant science: *ibid* fn 4.) When M.Lalande's 1795 near-miss of Neptune was discovered by Sears Walker in 1847 (the US' first internationally recognized contribution to astronomy),⁵ Airy's comment (same 1847/4/29 letter cited above) was perfect: "Let no one after this blame Challis."

A12 Postscript. Indeed, Challis is *less* blameworthy than either Galileo or M.Lalande, who missed finding Neptune (1613 & 1795, resp) despite *explicitly recognizing positional discrepancies* between two nights' observations of it. Note: all 3 men were looking for solar system objects, though the planet-discovery ambition of Michel Lefrançois Lalande (whose uncle J.Lalande sponsored & published the *Histoire Céleste* project) seems generally unknown to historians. Lalande's intent is obvious from his transparently consistent references⁶ to Uranus as "Herschel". After mapping the heavens down to 9th magnitude stars, he intended to remove⁷ the sky within a few degrees of the ecliptic, which would reveal the upcoming planet "Lalande" by its motion. So, what-could-possibly-go-wrong with this surefire plan? [a] The 2nd (follow-up) survey died.⁸ (Funding expired?) Later, it re-flickered to life briefly. Note the unpublished 1804/6/13 record which, with luck, could have included Neptune.⁹ [b] The Lalandes made the mistake of expecting success only by a lengthy, methodical mass-search. (Same as Airy & Challis assumed. And this before either was born.) It didn't occur to Lalande in 1795 that a planet might turn up in only *one* survey; but, by chance, Neptune lurked in the tiny sliver¹⁰ of overlap of the 1795/5/8 and 5/10 zones, so it could have been found *without* the followup-survey-that-never-matured. . . .

⁵Full story excellently told by J.Hubbell & R.Smith in the *J. Hist. Astron.* 23:261-291 (1992).

⁶*Hist Céleste* pp.217-229 (1796/3/15-4/15). Note that analysis of this flock of Uranus observations could either confirm or disconfirm the suggestion that the still-persistent Lalande 1795 Neptune residual is due (in nontrivial part) to the effect of planetary nonpunctuality.

⁷The original mss of the *Histoire Céleste* are in the Paris Observatory archives: (A.)C.5^{bis}. At vol.33, p.303, atop the 1800/10/25 start of sweep#2, a hitherto-unpublished note in M.L.Lalande's hand: "This project has been conceived in fructidor year 8 [1800/8/19-9/17] by le franç. [Lalande] & Burkh. [Johann Burkhart] to discover a planet beyond Herschel, if there exists one. . . ."

⁸The *Histoire Céleste* mss were officially presented to the National Institute on 1800/11/2. (See Delambre's handwritten note on p.321 of vol.33.) However, hope melted (after the 1800/10/25 enthusiasm) into a few nights devoted to filling in (mostly non-zodiacal) sky-areas previously skipped: 1800/11/13, 1801/1/8, stopping on 1801/1/15. (All 4 days of observations were published in 1801 in the *Histoire Céleste* pp.570f.)

⁹See mss vol.36. When Lalande recorded the 3rd wire of Gemma at 21:59 Paris Mean Time, Neptune was a few timesecs from transiting the 1st wire, had he immediately shifted the telescope down half a right angle. But not until 1/2 hour later did he move south into the zodiac (re-sweeping some of his 1798/5/22 area), starting with ν Sco — at virtually the same ecliptical latitude as Neptune, and only 8° of ecliptical longitude past it. Lalande's last chance was gone. (Speculation: Did he belatedly wonder whether the disparate 1795 observations were of a planet? If so, did he briefly take a stab at chasing it down in 1804, unfortunately assuming too rapid a motion during the 9th past?)

¹⁰When mowing a lawn, one deliberately arranges a little overlap. Neptune happened to be in the 1795/5/8&10 sweeps' slim overlap — which is the only reason the planet, exceptionally, got observed twice. Naturally, the two positions exhibited Neptune's motion — but Lalande assumed that the discrepancy was just due to a blunder; therefore (and this *was* a blunder — which cost him eternal astronomical fame), he avoided the labor of further investigation by just suppressing the 5/8 Neptune place, while publishing the 5/10 one with a mark of doubt (":") beside it. See *Comptes Rendus* 24:666 (1847/4/19), *Histoire Céleste* pp.156&158, & original mss vol.23 pp.8 & 27. Mercifully, M.Lalande died in 1838, without ever knowing what he had (just to save a few minutes of checking-time) let slip through a net he spent 10 years scrupulously weaving.

B CoveringUp the CoveringUp of Princetitute Amateurishness

B1 Our “Black Affidavit” (*DIO* 1.3 ¶10) noted that we have exposed several botched (even faked) calculations in ancient-astronomy researches emanating from the Princeton Institute-Muffia. Though these results are known¹¹ to Muffia & Princetitute personages, not one of the errors (see also *DIO* 6 ¶1 §H4)¹² has ever been acknowledged. Standard archonal integrity. The following will add yet further material to that which the Princetitute will simply tuck under its increasingly Himalayan rug. Similarly, its courageous Muffia’s desire for rational discourse may be gauged by its habit of attempting to hide from DR the date & location of all its snug little get-togethers [e.g., its 1994 Dibner Inst. symposium].

DIO requests that we be informed of future ancient astronomy conferences.

B2 From a DR letter to R.Newton (1985/9/12), part of our astonished monitoring of the perpetual Hist.sci sales-pretense that the *Almajest* is a marvel of accuracy for its time.

There is a [Mercury] station of $-264/11/16$ observed on 11/15 & 11/19 by the Dionysians . . . accidentally preserved . . . [at *Almajest* 9.10]. To Neugebauer’s credit, he [recognizes the station] (*HAMA*, pp.166-167; also Toomer’s 1984 *Almajest*, p.464 n.99), though he does not remark the revealing fact that Ptolemy (who regards stations as worthless, *Almajest* 9.2) hasn’t any idea of why these data were [valued] by the Dionysians. ([Stations] provide the empirical basis [*DIO* 2.1 ¶3 fn 17] of the *Almajest* mean motion of Mercury . . .) Aaboe’s 1980 *Centaurus* paper, p.27, similarly protects the reader from understanding Ptolemy’s ignorance in this fundamental connection.

B3 Neugebauer’s diagram (*HAMA*, p.1254, Fig.152) is useful but misleading. He remarks (p.167) that the data’s agreement with Tuckerman is “almost perfect” if we shift them for precession error. Not so. . . . [For Mercury’s net geocentric motion in the 4^d], Ptolemy has . . . an error by a factor of over two [1°/4 claimed, vs. 0°/6 actual] — huge by the measuring unit (lunar diameter) specified. N says (p.166) that pt.10 on Fig.152 corresponds to Mercury’s place on $-264/11/14$, “one day before Ptolemy’s first observation; [and point] No.11, for Nov.19, coincides with the second observation.” These points are taken directly from Tuckerman (who uses 5^d intervals for Mercury). But N has forgotten¹³ that . . . Tuckerman’s places are all for . . . 6 PM Alexandria [a rather unconventional hour for observing Mercury when it’s west of the Sun!] Thus, pt.10 is 1^d/2 (not 1^d) before 11/15.25, and pt.11 is 1^d/2 (not 0^d) after 11/19.25. . . . if we follow N’s error and compare evening positions, the position shift in the 4^d interval grows to [0°/9], which disagrees even worse with [Ptolemy’s 1°/4]. The discrepancy here is *larger than Ptolemy’s measuring unit* — namely, the lunar diameter.¹⁴ Finally, the sharp point (extending Mercury’s alleged path nearly a full degree to the right of pt.10) at the station on N’s Fig.152 is [pure Princetitute] imagination. Mercury only went about 1°/4 [13′] beyond pt.10 before station [at 212°43′].

¹¹E.g., *DIO* 4.2 ¶7 §B28 (& §B14).

¹²Just another instance of Princetitute-biggie Neugebauer so slavishly copying Ptolemy that all his ancient mentor’s errors become his own. A longtime colleague of Neugebauer has confided his realization of precisely such routine Neugebauer behavior. But, because of Neugebauer’s long connexion to the Princetitute, no science-history publication besides *DIO* can mention it publicly. (By admirable contrast, the *American Journal of Physics* permitted DR to point out a posegay of such errors [by Neugebauer & claque] in n.30 of his 1987/3 *AJP* paper.)

¹³An equally ethical archon’s similar confusion: *DIO-J. Hysterical Astronomy* 1.1 ¶8 §E4. David Hughes’ response to these & other cometic errors? (Of up to c.30°!) No response.

¹⁴And c.1 moonwidth is the error in the 11/19 datum at p.411 of Muffia-circle archon O.Pedersen’s *Survey of the Alm* (Odense Univ 1974); more on this minefield: *DIO* 1.1 ¶5 fn 6 & *DIO* 6 ¶3 fn 9.

B4 Both §B3 Princetitute foulups achieve the delicious distinction of exceeding the size of the entire measuring stick for the problem! (Reminiscent of other bloops by Ptolemy & his equally honest defenders: see *DIO* 1.3 fn 288.) The Princetitute’s repeatedly botched and sales-sculpted (*DIO* 4.3 ¶15 §F1) effusions on ancient astronomy are perversely over-rated in academe primarily because no journal (other than *DIO*) will criticize Princetitute-Hist.sci super-archonal behavior: pathetically amateurish science, plus censorial attitudes that do an artistically faithful imitation of raw fear. (Only at absolute-zero pinpricklessness can “arrogant gasbags”¹⁵ survive. So, an uncritical environment *has* to exist.) I.e., Hist.sci won’t criticize archonal misbehavior, reserving (p.2 fn 3) criticism instead for he who does. (See, e.g., *DIO* 1.2 fn 30 & fn 96.) Thus, discussion of suppression is itself suppressed.

B5 My 1987/3 *Amer J Physics* paper (n.24) noted that the *Almajest* 9.10 celestial description puts the $-264/11/15$ Mercury longitude at c.213°/1 (*idem* or *HAMA* p.166), disagreeing (by over 10′) with the stated longitude (213°/1/3), but closely accordant with the *Canobis Inscription* orbit. So I proposed: when founding the prior *CanInscr*, Ptolemy performed his usual pseudo-orbit-establishment math-proof (identical to *Almajest* 9.10 **except**: using *CanInscr* orbit elements). Later, when he adopted the *different* elements of the *Almajest* theory, he had to wrench this longitude up to 213°/1/3 so that when he repeated the same pseudo-proof (*Almajest* 9.10: *intimately dependent upon these now-mutated elements*), it accorded with exactly the same mean motion already announced in the *CanInscr*.

B6 But I only recently (1997/1/22-23) noticed a stark — and precise — independent trace of this procedure: in the other longitude ($-264/11/19$) given at *Alm* 9.10, Ptolemy places Mercury at longitude 213°/6, which does not match (even within 10′!) the *Almajest* Mercury orbit’s position (213°48′); *however*, it neatly matches¹⁶ the position (213°37′) given by the *CanInscr* orbit — the same orbit which DR’s *AJP* paper suggested *ten years ago* (on quite independent evidence: §B5) was behind this entire *Almajest* 9.10 math-charade. In exact sciences, dishonesty often (¶1 §G2) leaves a slimy trail.¹⁷ So we should be all the more grateful for the rare true giants of ancient astronomy, one of whom will be the subject of a reconstructive appreciation in an imminent *DIO*.

¹⁵Disgusted 1997/1/15 appraisal, by a wellknown veteran observer of academic pretense.

¹⁶Self-evident reason the original $-264/11/19$ longitude survived: this Mercury “observation” is not used in Ptolemy’s *Alm* 9.10 math; thus, it wasn’t worth laboriously recomputing. I.e., Ptolemy lazily left the 2nd datum (11/19) as it was; but, noting that his new value for the 1st longitude (11/15) was now merely 1°/4 (not 1°/2, as previously) west of the 11/19 longitude, he merely took a moment to alter his report (of 4^d differential longitude-motion), to make it agree. Sloppy. (For another instance of such *precisely* revealing Ptolemaic sloth, see *DIO* 1.1 ¶6 §H5.) Resulting hybrid gap: 213°37′ (*CanInscr* for $-264/11/19.25$) minus 213°/1/3 (*Almajest* 9.10 for $-264/11/15.25$). This equals 17′ — which is indeed half a moonwidth, as Ptolemy reports (hitherto inexplicably) in his *Almajest* 9.10 discussion of these data. Note: if we do not accept some such hypothesis, we must believe that this already doubly-suspect (notoriously discrepant [Toomer *loc cit*] and altered-orbit-ensnared) observation-pair report had a 4^d-motion-error (1°/4) that *just-so-happened* to match the difference between the *CanInscr* and *Almajest* theories here. This, when there is no question that Ptolemy kept constant his Mercury mean motion, allegedly math-based upon the $-264/11/15$ longitude (pseudo-proof at *Almajest* 9.10) — *despite* alterations (prior *CanInscr* vs. later *Almajest*) of the underlying orbital parameters of this math, which *required* a 1°/4 alteration in the $-264/11/15$ longitude in order to ensure that his math would still promote exactly (and I mean *exactly*) the same mean motion in both works. (Above, §B5.) See discussion of this alteration at Rawlins *Amer J Physics* 55:235 (1987) p.236 [item #5] & n.24. For the great mathematician van der Waerden’s delighted appreciation of the finality of its bearing on the Ptolemy debate, see *DIO* 1.1 ¶6 fn 37. The Princetitute-Muffia’s typically honorable reply, to this thoroughly refereed & very prominently published lethal proof of Ptolemy’s Mercury hoax? Ten years of silence. While profitably peddling Ptolemy to academe as a brilliant and **highly ethical** scientist.

¹⁷As slick a trail as any is Ptolemy’s Mars orbital eccentricity of 0.10000! — allegedly (*Almajest* 10.7-10) based on observations, but so overneat it’s actually funny. (See R.Newton *Crime of C.Ptolemy* Johns Hopkins Univ 1977 pp.319-320, or Rawlins *Amer J Physics* 55:235 n.25.) For a crushingly clear proof of Ptolemy’s fraudulence here, see mathematician H.Thurston at *DIO* 4.2 ¶6.

C Hysterical Velikovskians Flee Own Frankenstein-Mongoose!

To: DIO 1996 . . .
From: Ellenberger, 3929A Utah Street, St. Louis, MO 63116 c.leroy@rocketmail.com

C1 It may merit a very sweet ironic smile that turncoat and apostate Leroy Ellenberger, until 1983 one of Velikovsky's most active defenders,¹⁸ and since considered by some his "most unrelenting critic",¹⁹ was barred from a Velikovsky-retrospective meeting in Portland, OR, 1994 November 25-27, co-sponsored by Kronia Communications and the equally Velikovskian organ *Aeon*. The meeting, "Velikovsky, Ancient Myth, & Modern Science", was actively promoted on UseNet's talk.origins newsgroup as open to the public.

C2 The ban of Ellenberger was stipulated by at least two speakers, Charles Ginenthal²⁰ and Prof. Lynn Rose (Philosophy, SUNY Buffalo). Rose is — rather ironically in the present context — author of "The Censorship of Velikovsky's Interdisciplinary Synthesis".²¹

C3 The organizers established a veneer of intellectual respectability by including on the program, as "call girls",²² Dr. Victor Clube (Physics, Oxford), co-author of *The Cosmic Winter* (see fn 33); Dr. Henry Bauer (Science Studies, Virginia Polytechnic Inst & State Univ), author of the highly-acclaimed (though not by Velikovsky partisans) *Beyond Velikovsky* (Urbana 1984); and astronomer Dr. Tom Van Flandern (author of numerous able professional papers in mathematical & observational astronomy, and now publisher of the *Meta Research Bulletin*)²³ — all of whom are friends of Ellenberger — as well as several academic Velikovskian camp-followers, including sociologist Gunnar Heinsohn (Univ. of Bremen), classicist William Mullen (Bard College), and anthropologist Roger Wescott (prof. emeritus, Drew University).

C4 Van Flandern was so upset upon learning of the ban on November 21 that he initiated a conference call with the organizers and the censoring speakers; but they would not relent. The organizers decided that letting Ellenberger audit the sessions by an audio feed to his hotel room violated the spirit, if not the letter, of the ban.

C5 The ostensible reason for barring Ellenberger was his threat as a disruptive influence (after an incident at Haliburton, Ontario, during the previous August's annual meeting of the Canadian Velikovsky study group), which might interfere with the videotaping of the Portland proceedings for a documentary. However, the organizers' concern over the possibility of Ellenberger's attending was apparent before August (in June), when Reichian watch-dog Joel Carlinsky imparted what he had learned during a May visit in Portland with one of the organizers of the upcoming November meeting.

¹⁸ See *Zetetic Scholar* Nos. 3-4 (1979) and No. 5 (1979), 1980 May-June *Bib. Arch. Rev.*, 1980 Oct. *Astronomy*, 1981 April *Physics Today*, and 1983 May *Science Digest*.

¹⁹ So described in the program for the 1990 August conference on Velikovsky in Toronto, Canada. *Aeon's* 1992/4/15 subscription renewal-form noted, "The abrupt about-face of Leroy Ellenberger, combative secretary of *KRONOS* [sic, see §C12], and hitherto a devoted supporter of Velikovsky, has likewise provided fuel for those who would relegate the author of *Worlds in Collision* to the dustbins of history." See, e.g., Ellenberger: "Falsifying Velikovsky", *Nature* 316:386 (1985/8/1); "A lesson from Velikovsky", *Skeptical Inquirer* 10:380-381 (1986 Summer); "Immanuel Velikovsky 40 Years later: Not to Be Taken Seriously", *New York Times* 1987/5/16 p.14; "Velikovsky Revealed", *Venture Inward* (1990 Jan-Feb) p.49; book review, *J. Sci. Explor* 10.4:561-569 (1996); and H. Bauer, "Velikovsky" in G. Stein (ed.) *Encyclopedia of the Paranormal* (Prometheus Buffalo 1996) pp.781-788.

²⁰ Founding Editor-in-Chief of *The Velikovskian*, and compiler of *Carl Sagan and Immanuel Velikovsky* (1990, 1995), and contributor to *S. J. Gould & I. Velikovsky* (1996).

²¹ *Pensée* 1:29-31 (1972); reprinted in *Velikovsky Reconsidered* (N.Y.City 1976), whose fallacies-perpage count is estimated in "Applied Philosophy of Science 101 — The Annotated Rose: A Propaganda Piece Analysed", distributed at Toronto "Reconsidering Velikovsky" Conference, 1990/8/17-19.

²² After Arthur Koestler's coinage of the term in *The Call Girls* (N.Y.City 1973).

²³ *Meta Research Bulletin*, PO Box 15186, Chevy Chase, MD 20825-5186, phone 202-362-9176. TVF sincerely contends: some V-ists are as openminded as centrists, and the 1994 ban was atypical.

C6 Actually, Rose and Ellenberger broke off relations in 1983 when Rose refused to concede that the omissions and self-serving misinformation in Velikovsky's *Stargazers and Gravediggers* were material and important.²⁴ In 1990, Rose refused an invitation to debate Ellenberger on the Greenland ice cores as a crucial test of *Worlds in Collision* (see fn 35) at Milton Zysman's "Reconsidering Velikovsky" Conference in Toronto. Ellenberger's antagonism with Ginenthal began in 1984 when the latter's letters to *Kronos* were sent to the former for reply and Ginenthal tenaciously resisted any scientific explanation that contradicted pro-Velikovsky dogma.

C7 At Haliburton, Rose was upset by Ellenberger's stream of pregnant questions from the audience, following Rose's rebuff of a simple request for a clarification while remarking *sotto voce*: "I do not take questions from that source."²⁵ Irving Wolfe, Prof. of English (Univ. of Montreal) and arch-relativist, as attendee, tried to quell Ellenberger's interrogatories, contrary to the meeting's established format which encouraged audience participation. This was in distinct contrast to Wolfe's posture as moderator in 1992 when he allowed Ginenthal to lead two audience rebellions — i.e., bullying by outnumbering — against keynote speaker Ellenberger's explanation of the bearing of the Pioneer and Magellan missions' results upon Velikovsky's claim that Venus is young.

C8 Thus, Ellenberger, who had never truly disrupted a meeting,²⁶ was banned from Portland, while Ginenthal, who actually had previously (§C7) been a disruptive influence, was on the program — demanding Ellenberger be barred.²⁷ Ginenthal had boycotted the 1994 Haliburton meeting as a protest against Ellenberger's attending.²⁸

C9 Since the organizers for Portland did not believe Ellenberger would actually attend, they did not take seriously his several expressions of intent that ended posts on talk.origins in August, September, and October and, therefore, did not communicate the seriousness of their concern for barring him. Not even when he volunteered to replace astrodynamacist Victor Slabinski, who had declined his invitation in late September. This concern, which had been apparent since June when Carlinsky talked with Ellenberger, was subject to jokes at Haliburton. Under those circumstances, by late October, Ellenberger had arranged to attend as a reporter for *Skeptical* magazine, *Skinq* not having been interested.

²⁴ The Ellenberger-vs-Rose schism-spatfight is summed up in M.Gardner's *The New Age: Notes of a Fringe-Watcher* (Prometheus Buffalo 1988) pp.70-71. Detailed delineation in the section "Dénouement" of Ellenberger's invited memoir, "Of Lessons, Legacies, and Litmus Tests: a Velikovsky Potpourri", whose Part 1 appeared in *Aeon* 3.1:86-105 (1992). Part 2, containing "Dénouement", plus a sweeping appreciation of the lofty scholarly merits & intellectual stature of the Velikovsky movement's leadership, was cancelled by the humorlessly enraged editor, against the staff vote. (These merits are manifested most prominently in the scholarship & openmindedness of L. Rose, whom Ellenberger has occasionally needed with such choice flattery as: "α-class epigone".) After its suppression at *Aeon*, Ellenberger's "Dénouement" was instead posted on talk.origins in a longer 1994/6/20 message titled "Ellenberger Contra Cochran: The Second Reply & Talbot, Too". It is archived and can be retrieved at <http://abob.libs.uga.edu/bobk/cle/cle-contra-cochrane.txt>; see, too, the file cle-talbot.

²⁵ So perturbed was Rose that he blurted out a reference to such world-class scholars as P. Huber and R. Parker collectively as "the jerks". (But, in fairness, one notes that Rose is not in the least perturbed at ending up simultaneously maintaining two contrary historical chronologies!)

²⁶ Ellenberger participated previously without incident in meetings at Princeton (1980 & 1981), San Jose (1980), Toronto (1990), and Haliburton (1992).

²⁷ Previously, in 1993 June, Ellenberger had been barred as a contributor to *Aeon*, as a condition of L. Greenberg joining the staff, at the same time he was told the publication of his memoir had been cancelled. The cancellation was a surprise since the last word from *Aeon* concerned the need to polish up the section "Legacies". Ironically, D. Patten, author of several fundamentalist books invoking interplanetary collisions à la Velikovsky, was also on the program at Portland, despite having been barred from *Aeon* in 1991.

²⁸ As, too, C. Whelton, who joined Rose and Ginenthal in the Portland ban, but at the last minute could not attend. Velikovsky's daughter Ruth V. Sharon had also conditioned her promised attendance upon barring Ellenberger, but she was a no-show, too.

C10 By the time Ellenberger was informed he would be *poisona non grata*, after leaving (1994/11/20) a message on superVelikovskian²⁹ Dave Talbott's answering machine, he already had a non-refundable airline ticket; so he went to the conference, socialized in the hotel's public areas discreetly selling various "Velikovskian's right!" paraphernalia,³⁰ delivered pre-prints of an invited paper by Slabinski³¹ that was not published before the meeting (as promised), and sold anonymously both his Macmillan first printing of *Worlds in Collision* (with dust jacket) and other collectible Velikovskian publications at the conference book table. Rose chastized former *Kronos* staffers seen fraternizing with Ellenberger. Talbott made clear *Skeptic* could have any reporter in the world, except Ellenberger, when editor-publisher Michael Shermer tried to get Ellenberger reinstated. Talbott was so intent on barring Ellenberger that he made the ludicrous threat in a telephone conversation to have Ellenberger ejected from the hotel if he tried to claim his reservation!

C11 While the ostensible reason for barring Ellenberger was his alleged potential disruptiveness, a more likely contributing cause was the animosity between true believers and a turncoat. The aftermath of the 1994 Nov meeting was hashed out on talk.origins in early December. Copies of the major postings plus "The Annotated Rose" (see fn 21), "Dénouement" (see fn 24), and "Magnetism, Dynamos, & Neptune" (see fn 35), are available from either of the addresses at the head of this letter. (Or by telephone: 314-773-0329.)

C12 Esprit d'Escalier. Some background to Ellenberger's rôle in the Velikovskian cult:

[1] In late 1978, when Marcello Truzzi (E. Michigan University sociologist) was organizing the "Dialogue on Velikovskian" for his admirably open journal, *Zetetic Scholar*, Ellenberger accepted the invitation to participate, despite being aware that *Kronos* staff were boycotting it. When the "Dialogue" appeared in mid-1979, Ellenberger was on the *Kronos* staff and felt strongly that a rebuttal to the critics was in order despite Senior Editor Rose's desire that *Kronos* continue to ignore Truzzi's project. Disobeying Rose's injunction, Ellenberger submitted a rebuttal. When it appeared, it merited Rose's compliments. Ellenberger's services to *Kronos* led to his being rapidly promoted and named "Sr. Ed. & Exec. Sec'y.", the only dual-titled staffer, in mid-1981. Later that year he was awarded the Macmillan first printing mentioned above (§C10). This perk, and more, in spite of his many initiatives and memoranda to staffers that conflicted with the Editor-in-Chief's need for control. Ellenberger resigned from *Kronos* in 1986 December and terminated his duties as back-issue order-filler in 1987 November, a hold-over task he had continued at the request of the Editor-in-Chief.

[2] Now, as a Velikovskian disbeliever, Ellenberger has been transformed from hero to exile. In private, his former colleagues consider him "a barbarian unfit to be in polite academic society",³² as ex-Ed.-in-Chief L. Greenberg wrote a mutual correspondent in 1991

²⁹Talbott organized Kronia Communications in 1987 and was publisher of *Pensée*, 1972-1974, author of *The Saturn Myth* (N.Y.City 1980), and founding editor of *Aeon*, 1987-1991. His *idée fixe*, following a hint by Velikovskian, is that, during the "Golden Age" ruled by Kronos-Saturn, a seasonless Earth orbited the Sun in close proximity to Saturn, which loomed immobile over the N. Pole: rich entertainment for those who give priority to lethal falsification vs. ambiguous confirmation, and to the laws of physics vs. interpretations of mythic imagery, whose meaning is arguable to say the least.

³⁰Velikovskian gave Ellenberger permission to market "Velikovskian's right!" t-shirts in 1979 June.

³¹V. J. Slabinski, "A Dynamical Objection to Grubaugh's Polar Configuration", *Aeon* 3.6:1-10 (1994) (answering 1993 *Aeon* 3.3:39-48). Ellenberger performed all numerical analysis and computer simulations and prepared "Appendix B" (pp.8-9).

³²The feeling is mutual. When Greenberg published an *ad hominem* reaction (*Aeon* 3.2:82-88) to Ellenberger's memoir in *Aeon* 3.1 (see fn 24), part of Ellenberger's response was a 1993/6/15 postcard, whose closing read: "With a mongoose's respect for a cobra." In *Kronos* 12.3 (1988), polite unbarbarian Greenberg kissed off Ellenberger as "a disaffected zealot who long ago drifted beyond the pale of rational objectivity." Further with respect to "polite academic society": when R. Davis, Emer. Prof. of English, Columbia Univ, in *The New Leader* (1977), panned V's *Peoples of the Sea*, mirrorless

July. But Ellenberger's behavior & tactics are no different now than they were between 1977 & 1983 when he learned the rules of *engagement* under L. Greenberg's tutelage and was golden. Only the objects of his fulmination have changed: instead of V's critics and alternate catastrophists,³³ his targets are his former colleagues. Now he is a pariah, protesting the hypocrisy manifested by those who fail to follow the same standards of scholarship to which they hold their critics accountable³⁴ and who obdurately give Velikovskian's hypotheses & intuition priority over physical evidence & the laws of nature. Witness, e.g., Velikovskians' hard-core reactions to ice-core evidence against *Worlds in Collision*.³⁵

[3] In an effort to "de-program" Velikovskian cultists, Ellenberger distributes informative memorandums & postcards, intended to alter mind-sets — efforts that are not appreciated by old guard opinion-formers who dominate Velikovskian publications & conferences.

C13 Epilogue by DR. After the Portland conference (1994), Shermer invited a 2-stage exchange,³⁶ to begin a winding-down³⁷ of *Skeptic*'s involvement in the Velikovskian debate, on which *DIO* will also publish nothing further.³⁸ We are, significantly, giving Ellenberger our last word on the matter. Primary reason: counterbalancing the Portland ban. (NB: the entire public success of the Velikovskian movement has been based upon its being seen as a *victim* of censorship. So: where does its own 1994 Portland behavior leave its credibility?) But we share with *Skeptic* an implicit awareness: the Velikovskian debate — such as it was³⁹ — is long since over,⁴⁰ in even half-serious scholarly circles. And the skeptics have won.

Greenberg protested to *NL* that Davis had "departed the world of reality never to return".

³³ See, e.g., S. V. M. Clube and W. M. Napier, "The microstructure of terrestrial catastrophism", *Mon. Not. R. Astr. Soc.* 211:953-968 (1984); S. V. M. Clube, "The dynamics of Armageddon", *Spec. Sci. Tech.* 11:255-264 (1988); D. J. Asher, *et al.*, "Coherent Catastrophism", *Vistas in Astronomy* 39:1-27 (1994); S. V. M. Clube, "Hazards from Space: Comets in History and Science", in W. Glen (ed.), *The Mass-Extinction Debates: How Science Works in a Crisis* (Stanford 1994), pp.152-169; and V. Clube and B. Napier, *The Cosmic Winter* (Oxford and Cambridge, MA, 1990).

³⁴E.g., Greenberg & Rose, "L. Sprague de Camp: Anatomy of a Zetetic", *Kronos* 3.1:45-67 (1977).

³⁵ Contra Rose in *Kronos* 12.1 & 12.2, see S. Mewhinney, "Ice Cores and Common Sense", *Catastrophism and Ancient History* 12.1:5-33 & 12.2:117-146 (1990), and Ellenberger, "Litmus Tests in the Ice", section in "... a Velikovskian Potpourri, Part 2" (see fn 24), distributed at C. S. I. S. Meeting, Haliburton, Ontario, 1992/8/25-26. Further on Ginenthal's unique gifts as physicist, see Ellenberger, "Magnetism, Dynamos, & Neptune", section deleted from "... a Velikovskian Potpourri, Part 1" (see fn 24); later posted on talk.origins, 1994/4/25.

³⁶E. Cochrane, "Velikovskian Still in Collision", *Skeptic* 3.4:47-48 (1995). Ellenberger, "An Antidote to Velikovskian Delusions" [web <http://abob.lib.uga.edu/bobk/velidelu.html>], *Skeptic* 3.4:49-51 (1995). Followup by each: *Skeptic* 4.2 & *Skeptic* 4.3; the latter is the 1st public treatment to take the discussion of Velikovskian beyond *Worlds in Collision* and Sagan's flawed analysis, which organized skepticism has unthinkingly over-worshipped. [See *Kronos* 3.2 (1977), S. F. Kogan (letter), *Physics Today* 1980 Sept pp.97-98 (sponsored by Freeman Dyson for Velikovskian's older daughter in the interest of fair play); and Ellenberger (letter), *Physics Today* 1991 April p.72.] Preferable to Sagan is D. R. Moorcroft's unpublished "Taking a Leaf from Velikovskian & Examining It", available from Ellenberger (see §C11).

³⁷Now concluding with imminent *Skeptic* 4.4 [p.107] (1997) reviews of *Sagan & V* and *Gould & V*.

³⁸Those who wish to hear the Velikovskian side of these issues (and-or to learn of errors and omissions that may have occurred here from human fallibility) are encouraged to consult the several pro-Velikovskian publications cited above — or to contact the cultists themselves at 800-230-9347. Addresses: Kronia Communications, POBox 5215, Aloha, OR 97006. *Aeon*, 601 Hayward, Ames, IA 50014. Chas. Ginenthal (718-897-2403), c/o 65-35 108th Str, Suite D15, Forest Hills, NY 11375.

³⁹Post-carnage-mop-up historians who wish to enjoy Ellenberger-as-Letterman may contact him for his handy one-page "Top Ten [or is it Eleven?] Reasons Why Velikovskian Is Wrong About Worlds in Collision" — plus a fuller version of the present article, including physical evaluations of V's theories (discussions which are mostly outside the realm of *DIO*).

⁴⁰See ¶4 fn 21. *DIO* 4.3 ¶14 asks an oft-overlooked question: "if even the most logically & evidentially one-sided controversies are ... indefinitely irresolvable, then — why investigate anything?"