

¶15 Naked Came the Arrogance

Shunfight at the 0G Corral:¹ Portrait of an Archon The Reality Behind Academe's Free-Discourse Pose The Muffia at the Dawn of the Ptolemy Controversy

A The PRIMO Principle

A1 At Harvard and Boston University, while earning degrees in physics, I sought the grail of truth by (primarily under the guidance of Robert Cohen) additionally partaking of the scientific-philosophical wisdom of, among others: A.Whitehead, H.Weyl, S.Stebbing, E.Schrödinger, B.Russell, H.Reichenbach, K.Popper, H.Poincaré, E.Nagel, E.Mach, I.Kant, M.Jammer, G.Galilei, M.Faraday, D.Hume, N.Hansen, A.Einstein, R.Descartes, M.Cohen, M.Born, N.Bohr, D.Bohm, G.Berkeley, A.Ayer.

A2 My inductive ability bears a debt to their wisdom which I cannot adequately measure, let alone express. But, remarkably, not *one* of these eminent thinkers' publications ever mentioned the PRIMO truth-determination principle — a principle that over-rides ALL the refined-scholarship rules and guides which naïvely earnest university students (such as DR) have foolishly spent years of labor (and thousands of dollars) to learn.

A3 This precious principle is marvellously simple & clear — and it positively resolves any controversy. Instantly. No matter how complex the facts may be or seem to be. The PRIMO-principle is simply this: the side that's telling the truth is the one with the most money. (Note that the PRIMO-principle is indistinguishable from the way TV 'snews decides truth for us: whoever's selling Truth best is telling it best.)

A4 History-of-science's Princeton Institute-Muffia-O.Gingerich [PRIMO] combine² has been teaching this principle to DR for two decades. (A kindness which is due to the combine's recognition that, in a soft field [*DIO* 4.2 ¶9 fn 46] like History-of-science, the PRIMO-principle is the only principle.) But, unfortunately for this distinguished cult, it is dealing with an invincibly-unteachable pupil: a political moron.

A5 And Muffiosi have their own learning difficulties. Even aside from inability to recognize compelling discoveries by those who dissent from Muffiathought, they also have yet to perceive the unattractiveness of gang-bully cohesiveness. There is an admirable Biblical saying (Exodus 23.2), long a favorite of Bertrand Russell's family (& of DR):

Thou shalt not follow a multitude to do evil.

B Ringo Newton, There Ain't Room Enough on This Here Planet for the Two of Us

Few followers of the Ptolemy Controversy have the faintest idea just how long ago the Muffia began hating and slandering the work of the highly capable and respected physicist

¹ You know you're getting on in years if these are your first identifications: [a] Ringo was an OK Corral-gunfight baddie, [b] Madonna refers to Jesus' stage-mom, [c] the biggest Aug.16 celebrity-death was Babe Ruth's, [d] Sitzkrieg was the 1939-1940 "phony war". (You don't know what started *Animal House's* food-fight?)

² Readers not familiar with the Muffia are referred to §§F1-F2.

Robert Newton (RN), Supervisor of the Space Sciences Division of the Johns Hopkins University's Applied Physics Laboratory (Laurel, MD). Fortunately for historians of this affair, RN imparted to DR a copy of his written record of his very first contact with Hist.sci's anti-heresy inquisition. (The inquisitor: O.Gingerich, later head of Harvard's Hist.sci Dep't.) It is entitled: "Notes on a Telephone Conversation with Owen Gingerich". The (largely typed, partly script) document's text follows, starting with RN's handwritten marginal note:

Written on either Dec.3 or Dec.4, 1968. That is, either on the day of the call or the next day. This note was added Feb.21, 1969.

B1 Gingerich tried to call me on Monday, Dec.2, 1968, the same day that I received the peculiar letter from *Nature* rejecting my paper about the eclipses of Caesar and Stiklestad. We did not actually make contact until Tuesday, Dec.3. The conversation lasted about half an hour. I certainly cannot reconstruct the conversation precisely, however I am rather sure that the notes which follow represent the spirit of it faithfully.

B2 He started by finding out whether I was interested in extending my work on ancient astronomical observations beyond the paper that is now in progress, and it was established that I was, although I did not intend to extend the present paper itself, except for the completion of Part II.

B3 He then stated that the subject was very complicated and no business for an amateur. [Note by DR: on OG's expertise, see §C12.] From this point, I shall not attempt to follow the conversation chronologically, but only to summarize the main points.

B4 My amateur status was shown³ by my ignorance of the literature. One cited instance of my ignorance was the work on the Babylonian "astronomical diary" by Neugebauer, which is not even finished yet, much less published. According to Gingerich, this contains a large number of observations, particularly of lunar occultations. The other cited instance was a doctoral dissertation on Ptolemy's data done at Yale about two years ago. This dissertation has not been published and, according to Gingerich, almost surely never will be [vs. fn 4], because the author [J.Britton] has gone into stock-brokering or some such non-scientific field. Gingerich did not know the author's name.

B5 Any attempt on my part to publish anything about Ptolemy will be fiercely resented by the Department of the History of Science at Yale, because of the existence of this unpublished (any chance it is unpublishable?)⁴ dissertation. Yale and Brown (Neugebauer) cooperate closely, and my attempts to publish would certainly mean that I will be denied any access to the Babylonian work.

B6 [Note by DR. As at *DIO* 1.1 ¶1 fn 20, we pause to appreciate OG, who — despite his remarks, quoted there and here, showing his complete awareness of the Muffia cult's suppressiveness — called DR "exceedingly paranoiac" for "suggesting that a cabal has been suppressing the consideration of [R.]Newton's work" on Ptolemy. Despite *DIO* 1.1 ¶1 fn 20's 1991 publication of OG's demented lie-slander, OG has not seen fit even to withdrawn his libel, much less express regret for it. See here at §H10 item [1]. (Note added 2009. RN's heresy [that Ptolemy lied] is now vindicated. Archonal vanity's hilariously transparent antidote? See www.dioi.org/fff.htm#mhwx!)]

³ [Note by DR.] Muffiadum's history is a study in the need (whether careerist or psychological) to use an Enemy's supposed slips to portray him as someone whose output should be burned at the stake and forgotten forevermore. *DIO* has chronicled repeated seeee-he-is-so-worthless assaults against RN (most of which crashed). These stabs hugely tickled Muffiosi's sadistic sides. Until the approach got turned around on them. (See, e.g., "Muffia Muff-Catalog: the Incompetence-Chargers' Competence", at *DIO* 4.1 ¶4 §A.) An earlier *Little Caesar* wondered at those who can dish it out but can't take it. (Translation: you can't have Saddy without Massy.)

⁴ [Note by DR.] See *DIO* 1.2 fn 170.

B7 (The force of this threat was largely negated later in the conversation. It appears, still according to Gingerich, that Neugebauer is feeding the Babylonian occultation data to a graduate student at Yale as fast as he establishes it. Thus I would not have any access to the data before the astronomical analysis of it is published in any case. Once it is published, I cannot be denied access, whatever access may be worth at that point. I refrained from pointing out to Gingerich that this information negated his threat.)

B8 In sum of this part of the conversation, the Gingerich-Yale-Brown axis intends to exercise control over my publishing in the field of ancient astronomical observations. It was not clear to me whether I was being forbidden to publish only about Ptolemy or whether I was being forbidden to publish at all.

B9 The conversation closed with reference to my claim that there are errors in the [D.Menzel-O.Gingerich] Preface to the [1962] Dover edition of Oppolzer's *Canon*. In my opinion, he continued to show the same lack of comprehension [§C12] of the subject matter in conversation that he did in the Preface. He tried to maintain that Fotheringham's big paper (1920) on solar eclipses is full of references to the *Canon*, and that he knew this because he had just finished rereading the paper. A look through Fotheringham's paper shows that Gingerich's claim is false.⁵ He tried to maintain that the whole line of work of Fotheringham and others on finding the secular accelerations was inspired and made possible by the *Canon*. He seemed to be ignorant of the work in the field before the *Canon* was published, such as Newcomb's 1875 paper, and he also did not know that Martin at Yale is re-analysing the occultation data used by Newcomb. He felt that the error about the saros has been made so often that it has become correct, and that it is beyond all question that he was correct in his usage of the word. [Note by DR: I side with OG on this one.]

B10 (According to the biographical sketch in *American Men of Science* (does this list any women?), Gingerich has been a lecturer in the history of science at Harvard several different years. This accounts for his feeling that he is an expert on the subject.)

B11 The timing of Gingerich's phone call with respect to the [1968/11/25] letter from *Nature*, and the uncompromising nature of his call and of that letter, suggest an hypothesis: Gingerich, or a friend of his, blocked publication in *Nature* by writing a letter to them warning them that my papers on the subject are no good. He received notice of their action, and knew that I had received and been "softened up" by the rejection, and chose that time to call.

B12 I am trying to test whether Gingerich indeed speaks for the Yale department by a letter I have written asking for information about the dissertation.

B13 Since Gingerich is at SAO [Smithsonian Astrophysical Observatory], I would expect that he can and will block publication in the *Smithsonian Contributions to Astrophysics*. Further, Duncombe will almost surely turn to this axis (such as Clemence at Yale) for review of the big paper [see §B16], so there is a good chance that it will be rejected by the Naval Observatory. I must probably find an alternate source of publication and try to manage for review by [Sir Harold] Jeffreys . . . or some other charitable soul.

B14 RN later added the following comments, entitled "Explanation of the Accompanying Notes on a Telephone Conversation with Owen Gingerich", dated 1980/11/17.

⁵ [Note by DR.] The paper in question is found at *MonNotRAS* 81.2:104-126 (1920/12/10). While other Oppolzer work is cited, there are only a few references (at its pp.117-118) to Oppolzer's *Canon*.

I have just reread the accompanying notes of a telephone conversation I had with Owen Gingerich on December 3, 1968. As the handwritten note suggests, I did not date the notes originally, and when I did date them on February 21, 1969, I was unsure whether I wrote the original notes on December 3 or 4. In any case, they were written almost immediately after the conversation.

B15 The paper about the "eclipses of Caesar and Stiklestad" is a short paper I prepared with the intent that it be a letter to the editor of *Nature*. It was rejected twice by *Nature* and never published as such, although the information in it appears in *Ancient Astronomical Observations* [R.Newton 1970], pp.81-86 and pp.70-73.

B16 The paper mentioned [at §B2], and the "big paper" mentioned [at §B13], both refer to what [eventually] became *Ancient Astronomical Observations* [R.Newton 1970]. At one time I expected to publish it as a paper in the *Astronomical Papers Prepared for the Use of the American Ephemeris and Nautical Almanac*, but that never worked out. I do not know whether that was for the reason hinted at or not; I never actually submitted the paper to the Naval Observatory, because I was discouraged from doing so although I was originally encouraged to. I prepared *Part I* of the book [R.Newton 1970] and distributed it in preprint form before I started *Part II*. I think this must be why the subject of Ptolemy came up, since I showed in *Part I* [R.Newton 1970 p.20 & Table II.2] that Ptolemy's solar observations were fabricated.

B17 [At §B11] I suggest an hypothesis which I tested by writing to Yale asking about Britton's dissertation. I did receive an answer identifying the dissertation and its author, and I subsequently got a copy from [University Microfilms]. I also did another test:

B18 The paper sent to *Nature* was originally rejected on the ground that it was so short that it was incomprehensible, and it needed to be lengthened before it could be considered. I revised it slightly, lengthening it by half a dozen lines in the process, and resubmitted it. Again it was rejected, this time because it was too long! At the same time, I got another call from Gingerich, with suspicious timing. He reminded me of the earlier call and wanted to assure me that he meant business. I consider that the hypothesis is established.

C God Save the Grand Inquisitor

C1 At *DIO* 1.2 §11, it was recounted that, on 1976/11/12, a few days after DR mailed *Science* the first précis⁶ of his least-squares analysis of the Ancient Star Catalog, a man phoned c.8PM for DR from Cambridge (Mass) and — finding DR not at home — grilled DR's wife for details regarding his person & plans. The caller did not leave his name. However, in light of the foregoing and the common factor of post-submission timing (see §§B1, B11, & B18): is anyone now incapable of inducing the caller's identity?

C2 Since there are those who wish to blame the unpleasantness of the Ptolemy Controversy upon DR, the reader should be reminded that the foregoing shunning-obscurity (§B) occurred several years before DR had even entered the Controversy. (See also the reception of DR's occasional attempts at amiability: *DIO* 1.1 ‡1 fn 20 & ‡3 fn 7, *DIO* 2.1 ‡3 fnn 20&31. [Note especially the 1st reference & here at §B6: the almost comical perversity of professedly religious OG's record-setting apogee in vileness-space. No self-respecting fiction-editor would permit such behavior even in a storybook character.]

⁶ Dated 1976/11/1, the document was the first to reveal DR's by-now-wellknown absent-error-waves test — which flunks both Ptolemy & the Muffia: see fn 34.

C3 Another pre-Controversy happenstance: around 1974-5, I became interested in the modern Media-hustled revival of astrology and other occultist garbage (ESP, UFOs, etc). CSICOP's Ray Hyman warned me that learning the particulars of astrology would be useless since believers were invulnerable to evidence. And Ray was right. But the excursion turned out to be a twofold godsend when I later got into the Ptolemy Controversy. [a] I had learned that astrologers need only three manuals: [i] astronomical tables, [ii] geographical⁷ tables, and [iii] an interpretational rulebook. When I later noticed that Ptolemy's prime works were just these three (*Almajest*, *Geographical Directory*, & *Tetrabiblos*), I gained an unobvious clue as to his goals & priorities. [b] It was helpful to have experienced, firsthand, just how remotely far-out cranks' evidence-immunity can go; so, fortunately, my later encounters with the Muffia were something less than a complete shock.

C4 But, for me, this history actually starts in 1965-6, when I first got to know the Johns Hopkins University Hist.sci Dep't and its history-of-physics professors Kargon & Spencer; the latter often repeated (even on the athletic field) his favorite saying:

We don't want the history of physics to be written by senile physicists.

The proprietary nature of this guiding principle afforded useful later insight (§G9) into the actual psychological mechanics underlying the purported reasons for Hist.sci resentment of the terrible bottom line of the Ptolemy Controversy: mere physicists, not Hist.sci specialists, discovering some of the prime secrets of ancient astronomy.

C5 [Always friendly in a personal way with the JHU Hist.sci crowd in those days, I later played alot of softball with Kargon on the Hist.sci team. In 1984, about the time I'd become a political untouchable in the eyes of the *JHA* Editor, Kargon suddenly concluded that my facile switch-hitting habits were "obnoxious". (His newfound partiality was especially weird since Kargon was the first Hist.sci person who had warned me of the peculiar bigotries of the Muffia, and had described Hoskin to me as just a power-operator who was trying to get his then-young journal off the ground.) Considering the sharpness of language Kargon had routinely used when discussing other scholars (e.g., Aaboe;⁸ and see *DIO* 2.3 ‡6 §F1), I was not especially surprised at the tone of his disapproval of DR. (Another possible factor in his alienation: I had around this time informed Kargon that the Ptolemy Controversy was becoming a lesson-rich war of two worlds — scientists vs. Hist.sci. From this time, a one-sided chill settled into our relations.)]

C6 The first detailed R.Newton papers proving Ptolemy's fakery were published (1973-1974) in the *Quarterly Journal of the Royal Astronomical Society*. (All Fellows of London's Royal Astronomical Society receive its house journal, the *QJRAS*, as a perc of RAS fellowship. That is how I first heard of the Ptolemy Controversy.)

C7 Without reading the articles carefully at the time, I presumed that there must be something to the exposure. But about the only specific datum I initially absorbed from the material was the information that R.Newton worked nearby. (I lived in Baltimore, less than an hour's drive away.)

C8 On 1974/11/15, I met⁹ with the wellknown Harvard & Smithsonian astronomer & historian Owen Gingerich at Goddard right after he had delivered a talk there, and since JHU-APL is very near Goddard, I asked OG (as we chatted in the right half of the audience area of the hall) whether R.Newton had also come to hear the talk. At the sound of R.Newton's name, OG reacted with a strange look, replying: no, but-why-do-you-ask? I

⁷ Astrologers' geographical manuals are not (like, e.g., Strabo) concerned with descriptive geography, but are mere lists of longitudes and latitudes. Ptolemy's "Geography" (*Geographical Directory*) is of the latter type: c.8000 sites' longitudes & latitudes, by far the largest such compendium surviving from antiquity.

⁸ The criticism of Aaboe was that his *œuvre* was inappropriately slim. This represents a failure to understand that Aaboe's work is largely inductive (not merely slapping together a biographical piece on someone: the standard Hist.sci project); thus, a huge amount of mental effort will inevitably produce just a few pages of results, but such results may represent much more novelty & intelligence than the Hist.sci norm.

⁹ I had first met OG on 1974/2/11.

mentioned the *QJRAS* papers. At this, a visibly agitated OG burst out with an unqualified assurance that R.Newton's analyses of Ptolemy were worthless & disreputable scholarship, completely unaccepted by the establishment — adding that a top Yale expert (unnamed at the time but clearly Asger Aaboe, Hist.sci, Yale Univ) was preparing a devastating response. (Which has never appeared.¹⁰ Under Aaboe's name, at any rate.)

C9 Later that day, I joined OG at a purely social gathering of scholars. During the conversation, I brought up some suppressive (& overly personal)¹¹ behavior by Editor Jos. Ashbrook of *Sky & Telescope*.¹² OG listened with obvious interest but said nothing about the matter in front of the group.

C10 The next day, we talked by phone, at which time OG only now (privately) volunteered that he had had a parallel experience with Ashbrook (who years earlier had for ordmag a year unjustly condemned OG as misinformed for having made certain "errors" which in fact were based on JA's own use of an obsolete source). Naturally, I was interested in finding someone who knew that my account of an utterly weird JA encounter was credible. (I would not have believed it myself. Until it happened.) But when I asked what one could do about it, OG said that nothing could be done: JA is just "like that". OG added that he presumed that I would not do anything, but added that if I did react to JA: "You'll only hurt yourself."

C11 This translates: you can't fight City Hall. What I didn't know at the time was: Gingerich *is* City Hall. One is reminded that many scholars who cite Sherman's war-is-all-hell appraisal fail to add that he made it so.¹³

C12 OG was a wellknown & (I then thought) highly respected figure. I inexcusably permitted these (at best) irrelevant factors to persuade me — and so stupidly accepted (for many months) what OG said about R.Newton's charges against Ptolemy. (In retrospect, I am puzzled that OG's opinion is so overrated. His errors [*DIO* 6 ‡1 fn 66] are less telling than the fact that he remains virtually the sole regular participant in the Ptolemy Controversy who has never¹⁴ made an important original discovery in ancient astronomy. I doubt he even claims to have done so. This is quite aside from his judgements upon others' discoveries, a department where he also exhibits little discernable talent, though RN suggested that he may serve as a handy reverse weathervane: if O Gingerich takes a strong position, the contrary will usually turn out to have logic in its favor.)

¹⁰ If it is under 20 pp, *DIO* would be happy to publish Aaboe's attack on R.Newton. (This suggestion is not purely libertarian; I would expect Aaboe's comments to contain worthwhile material & reflections.)

¹¹ Still a sickness at *S&T*, I'm sorry to report. On the other hand, it must be said: whatever Joe's oddities, he was a marvel of astronomical lore, and I miss his "Scrapbook" entries in *S&T*. (See Sam Goldstein's 1987 appreciation in *JHA* 18:147. I even agree with Sam's choice of Joe's best entry: on solar oblateness.) It has been a comfort to me that, at the time of his justly-lamented death, we were back on good terms. (Reconciliation was triggered by a funny incident — cited at *DIO* 1.1 ‡3 §A3 — which Joe took in just the right spirit. We remained friends from then until his death.)

¹² DR had criticized *S&T* for its ludicrously-superfluous censorial policy of refusing ads for the nutty Velikovsky journal *Pensée*; so an enraged *S&T* had tried to wreak vengeance by rushing into print a review (1973/9) implying that DR's new polar book, *Peary . . . Fiction* was unreliable. (To JA's dismay, no one paid the slightest attention to his piece. *Fiction* is now regarded as a standard in its field, cited even in *Encyclopedia Americana*'s Peary entry.) *S&T* then added to its glory by: [1] suppressing DR's reply to the review, and [2] refusing (for all time subsequently) to besmirch *S&T*'s pages by citing DR's work. E.g., *S&T* has for nearly a decade suppressed the fact (probably known to *S&T*: its letter of 1987/3/2) that the famous supercompact refraction-correction format (now found in top astronomical & navigation manuals the world over) was invented & (1982 April) first published by DR. (See Rawlins 1982C eqs.8a&8. For improved *DIO* refraction & extinction formulas [of professional-level accuracy], see *DIO* 2.1 ‡4 fn 17 & other fnn there cited.) In 1986 July, *S&T* (whose Editor co-publishes with Gingerich) instead credited (*S&T* 72.1:70) the format to two later scholars. Apparently, in certain archaical circles (not exclusive to Cambridge MA), if one simply doesn't like an inventor, no consciences are bothered when his due credit is stolen for a decade. (If now independently confronted with DR's priority, I suspect that agile *S&T* will go in another direction: losing interest in the invention's worth.)

¹³ Sherman also offered (1865/4/19) the most admirably generous peace in the history of warfare.

¹⁴ But see *DIO* 1.3 fn 223 for citation of a key observation (which I believe should be credited to OG) that helps to narrow options for alibiers of Ptolemy's fakes. [See also where OG was right against an incorrect DR theory: *DIO* 11.2 p.30, even if evidently by politically-inspired accident: *DIO* 1.1 ‡1 fn 9.]

D Censorship Encountered

D1 Over a year went by with no involvement of myself at all in the Ptolemy matter. That changed (faster than) overnight due to an incident that finally smelled odd enough to alert me that something seriously wrong was going on. The 1976/2 *Sky&Telescope* carried a virtually ad-copy review by CUNY's Janice Henderson of *A Survey of the Almagest* (Pedersen 1974), authored by a prominent Hist.sci prof who then ran *Centaurus*, a journal which has carried a number of pro-Ptolemy papers but never one by the center of the skeptical contingent. (Still the case — 20 years later! Same for the equally captive reviewing journal, *Sky&Telescope*: fn 12. After 2 decades of such utterly one-sided promotion of Ptolemy, both journals now have accumulated an enormous face-investment in continuing to suppress the truth about the greatest faker in astronomical history.)

D2 What struck me right away was that the *S&T* review did not even mention the *existence* of R.Newton's work, simply noting that there used to be doubts about Ptolemy but that "Pedersen's book goes a long way toward reestablishing¹⁵ the reputation for integrity that Ptolemy deserves."

D3 Henderson 1976 was not merely an ad for Pedersen's book. It was an even more enthusiastic ad for Ptolemy's *Almagest*: "the greatest astronomical treatise of antiquity, a compendium of all mathematical astronomy known at the time", and (quoting Neugebauer 1957) "one of the greatest masterpieces of scientific¹⁶ analysis ever written".

D4 When I learned that Henderson was a Yale protégé of Aaboe, I realized that a revealing re-write of OG's 1974/11/15 confident assurance had since taken place: OG had said at that time that Aaboe would publish evidence showing R.Newton was wrong; but, over a year later, Aaboe had published nothing (& never has in the 2 decades since) in reply to RN; instead, his personal henchperson was scoffing at RN's position while entirely ignoring RN's evidence — not even telling *S&T*'s readers where they might consult it. I did not yet realize that noncitation had already for *seven years* been standard policy among Ptolemy's defenders, being just about the only weapon they felt comfortable with. (Besides private slander of dissenters — which needn't be accurate, since the party discussed is never on hand to correct errors.) Nor did I realize that attack by protégé-proxy was equally standard.

D5 Question in passing: would a clique with confidence in the evidence for its position behave this way? Would it for 20 years evade repeated suggestions of face-to-face panel discussion or debate? (The most public pre-DIO debate challenge is published right in the *AmerJPhysics*: Rawlins 1987 p.236.)

D6 Two decades ago, O.Gingerich was involved in the AAAS scientists-vs.-Velikovsky bearbaiting episode (which, for courage, matched the US invasion of Grenada); thus, we conclude that he is not unwilling on principle to endorse or even arrange such debates. (Likewise, O.Neugebauer published, in *Isis*, a review of Velikovsky — but would not answer R.Newton.) Or, perhaps it would be more accurate to conclude (from OG's approval of debate with V but not with RN & DR) that OG is not unafraid of debate *when he knows the evidence is in favor of orthodoxy*. (One recalls that during the Watergate impeachment days: those who knew the law were openly saying that the Nixon defense lawyers' strategy only made sense if the defense believed its client was guilty.) Another question is raised by this contrast: does OG believe that Velikovsky is worth more attention than the leading world controversy in ancient astronomy? [The Ptolemy controversy's heatness-primacy was later

¹⁵ From the discussion of Pedersen 1974 pp.255-258, this strikes me as an accurate induction from the book (p.371 raises the thought of fraud but then backs off on p.372 by calling the evidence simply a mystery). However, Pedersen announced during a 1983/6/4 symposium that he had known all along that Ptolemy faked observations and felt that readers of his book ought to have understood this. I leave it to Pedersen & Henderson to work that one out. During a 1987/2 *JHA* review of Toomer 1984, Pedersen repeated in print this appraisal of Ptolemy's observations. (He did not mention RN or DR.) So that is his position for now. [This suggestion ("for now") was originally written in 1988. I refer the reader to *DIO 1.2* fn 99.]

¹⁶ Whatever its pretensions, the *Almagest* is actually not a scientific work but a mathematical one: *DIO 2.1* ‡2 fn 11.

publicly acknowledged by OG-sucophant B.Schaefer: www.dioi.org/fff.htm#rsgs.] Or is it rather simply that: until his opponents become prominent enough in the popular press (thus a palpable danger to orthodoxy) OG won't debate them? (Similarly, Ptolemy's top [Muffia] defenders — who do not include OG in their inner sanctum — did not publicly attack RN until the press began noticing him in 1977: §F4.) If so, then: [a] this implicitly makes the press a large factor — almost an arbiter — of what are important issues in academe; and [b] one must conclude that power-academics who attack scholars' attempts to create press sensations are simultaneously & revealingly reacting only to the very same low-appeal approach which they profess to scorn; thus, they are effectively encouraging that proscribed behavior — and are penalizing those who prefer logical discourse to popular-media p.r.

D7 It was on 1976/2/25 that I first read & photocopied Henderson 1976. I was so perplexed (§D2) by Henderson's review that on that very day I made two unsuccessful attempts to reach her by phone. I then phoned R.Newton (same evening). We had never previously had any communication. I asked him right off about the Henderson review's noncitation of his papers. It didn't seem to surprise him: he said this had been going on for 7 years, since R.Newton 1969! The stench of rottenness was getting stronger.

D8 He mentioned O.Gingerich's having warned RN (§B) that he would no longer be permitted to publish such charges. (This would not be the last time OG tried throwing his weight around: §H9.) So RN had had to send his Ptolemy-doubting papers to the *QJRAS*, because that journal was deliberately seeking opinion papers.

D9 This first (2/25) chat did not persuade me of much regarding Ptolemy, but the arrogant centrist treatment of heterodoxy made a powerful impression. I was on the verge of beginning a long experiment, which I have since found increasingly intriguing: testing the extent to which a small band of mutually backscratching scholars is able to control opinion in a specialty area — entirely regardless of incoming evidence's strength. The secret of such success is the same as one of the keys to the Peary North Pole fake: just as Peary was in 1909 geographically isolated, far out on the Arctic Ocean ice (and thus protected from scrutiny), so any modern specialty academic establishment is intellectually isolated by its very specialization. What external sanctions on misbehavior can apply, when outside observers are given to believe that they cannot even understand the matter under discussion? The implicit insulation from checks & balances is the core ingredient of a recipe for corruption. Moreover, there is no deputed academic court of appeal for such matters (§J3) — no recognized mechanism for dealing with cases of abuse. Isn't anyone out there even a *little* concerned that modern academe — with all its genuine lofty intellectual merits & credits — has come to such a low, degraded state in the vital area of freespeech? (See §J4.)

E The Awful Stakes

E1 Decades ago, a small group of History of science scholars rejected centuries of knowledgeable astronomers' consensus¹⁷ that Claudius Ptolemy — Mr.Geocentricity of antiquity — had faked data. They did so while slandering numerous prior scholars in this area as fools. (See, e.g., Neugebauer 1957 p.206. And Neugebauer 1975 pp.16, 274, 331 n.6, 334 n.10, 350, 734 n.14, 935 n.7, 937, 942 n.1, 959, 965 n.6, 976. And Toomer 1975 p.201 & *DIO 1.3* §R2.) [For a happy contrast, see Neugebauer at his best at *DIO 6* ‡1 fn 100. Also the admirable remarks of Neugebauer 1975 p.vii.] Insofar as hypercriticism represents commitment to high personal standards of scholarship, this peculiarly rude approach isn't a pure negative. But the Muffia is itself not quite genius-level. Thus, with respect to aggression-tactics: for this gang to savage others' scholarly acumen is about as smart as starting a poison-gas war when your self-created Enemy is upwind. (Didn't

¹⁷ E.g., Tycho's star-catalog preface (1598), Delambre 1819 pp.lxvii-lxix, Peters & Knobel 1915 p.15, C.Payne-Gaposchkin *Intro to Astron* 1954 p.266.

WWI Germany actually do this?) As a conspicuous tactic of the Neugebauer cult, attacking others' alleged incompetence appears to be one of a number of manifestations of what the psychologists call "projection".

E2 The attempted shift of opinion in Ptolemy's favor has never really taken hold outside Hist.sci; but even the partial success was a definable Achievement of the Muffia, which made it feel Important. Since there was never any strong evidence for Ptolemy, the cult went far beyond reasonable conclusions from evidence, in order to prove its case. It is possible that an early conscious realization of a key fiscal & careerist reality played a rôle in the sham this group has perpetrated upon the scholarly community. That reality: how can one raise funds for research in a field where the central document is a clumsy¹⁸ fake? And, even assuming the best initial Muffia motives, the awful problem lingers nonetheless: when a Cultleader & followers have for decades damned all dissenters as fools & knaves, the cult acquires a huge stake in making its position look valid.

E3 [Paragraph added 1995.] Thus, there is no way that such instinctively-turf-sensitive, gang-warring parties can independently choose to make peace. E.g., at the very moment when the *JHA* was making the potentially-fruitful (if insultingly handled [*DIO* 6 ¶3 §C5]) gesture of creditably acknowledging *DIO*'s math-correctness on a single point, the Hist.sci Society re-opened hostility (in the HsS's *Isis* 86.2:309; 1995/6) by using¹⁹ nonmathematician Muffioso Alan Bowen (who co-vetted the very *JHA* 1991 elementary-school mis-math which the 1995 *JHA* was now correcting!) to pass off, as virtually worthless, Cambridge University-trained mathematician Hugh Thurston's valuable,²⁰ gentle, openminded, & highly capable Springer-Verlag book. (Which has won well-deserved praise from the Royal Society's D.King-Hele at *Nature* 370:339. [We are happy & obliged to here thank *Nature* for listening to *DIO* 1.2 fnn 12&96.]) Bowen's unprovoked (§F4) denigration of non-Hist.sci-cultist Thurston naturally has no relation to: [a] Hist.sci's professional-survival-instinct desire (§C4) to insist that scientists can't do history as well as the Hist.sci cult. [b] Springer's recent dropping of Muffia-capo G.Toomer's valuable 1984 *Almajest* (an event which, despite the book's flaws [& Toomer's seething, irrevocable loathing of DR], *DIO* genuinely deems regrettable). [c] Thurston's temerity in defying a private Muffia warning never to cite DR [*DIO* 6 ¶1 fn 7 & ¶3 §E2]. (Understand: Thurston's text only mentioned two DR results, while repeatedly and respectfully citing numerous Muffia achievements. But 99% assent is not enough. Just as no medical procedure can permit mere 99% sterilization. The book-burner mind won't tolerate ANY heresy.) Not to mention: [d] Thurston's *JHA* note [*DIO* 6 ¶3 §C] correcting a Bowen-vettee's error.

E4 The unbreachable barrier that prevents any progress towards compromise here is that, at this late date, if Ptolemy's fraudulence is *even seriously considered*²¹ by academe (much less generally accepted), the loss of face by the Muffia cult would humble its precious pretense to expertise. And it knows it. Indeed, the effect on the viability of the whole highly insecure field of Hist.sci could be unpleasant. And Hist.sci knows it.

E5 Reality: if a couple of rogue physicists (RN & DR) could be right after all, while the cream of Hist.sci experts could have been (even *might* have been) adamantly & slanderously wrong for decades, well — Hist.sci archons don't need to be told what questions will immediately surface. Each of the several (very few) major universities that have established Hist.sci departments could start asking why the university's reputation need be dragged

down by association with such embarrassments.

E6 Therefore, if Hist.sci promotion of Ptolemy as "the greatest astronomer of antiquity" is a horrifically ironic mistake (whether originally by wellintended folly or no), the awful truth *cannot be admitted* (at least on the record). The stakes are too high. Important faces are too deeply into potential egg. No matter how clear & potent the still-incoming evidence may get, it cannot be allowed that the very astronomer repeatedly puffed by the Hist.sci establishment as The Greatest of all ancient astronomers is in fact the biggest faker of the lot. The irony is too gross. Too perverse. Too hilarious. Too damaging to the whole Hist.sci business, whose top²² Ptolemy expert decreed that R.Newton 1977 "tends to bring the whole topic [of Ptolemy's manipulation of his material] into disrepute" (Toomer 1984 p.viii). But, the evidence has gotten so unfriendly to the defenders that many now content themselves with an increasingly watered-down version of the former position: Evans, Włodarczyk, & O.Gingerich have essentially, though not as explicitly as acknowledged homeopaths, been reduced-diluted just to arguing that the manifold evidence convicting Ptolemy is not utterly conclusive. That is a long way from the confident Gingerich 1976 assertion that Ptolemy was the best thing that ever happened to ancient astronomy. Such transformations are among the nuttier features of The Controversy — the nuttiest being that many Muffiosi will deny there has even been a transformation.

E7 This and indeed all of the dementedly illogical, inconsistent,²³ & burgeoningly dishonest history recorded here is the inevitable consequence of this nightmarish vise — this tarbaby commitment to a short-term-smart course of trying arrogantly to stonewall out of a mess. A mess created in the first place by, primarily: arrogance.

F The Muffia & its Godpop

F1 The above-cited notoriously contentious & turf-possessive Muffia clique was fathered by (& long godfathered by) the late Otto Neugebauer, of BrownU & the Princeton Institute for Advanced Study, who for decades Princetited his own talents & his colleagues' reps to the curious cause of glorifying the most transparent faker of ancient science, C.Ptolemy. (The common factor in this & ON's other odd fixation, Babylonian math astronomy as source of Greek [*DIO* 6 ¶1 fn 27], is: sell-convincing modern academe's grant-dispensers that ancient astrologers' texts are high-science, which thus merit high-funded, high-expert-Muffia analysis.) ON managed to breed & plant at prominent universities a small but virulent clique of clones: Asger Aaboe (Yale), Bernard Goldstein (UPitts), & Gerald Toomer (Brown, Harvard). Each of these breeder-reactioners has in turn attempted to clone further grad students.²⁴ (Yes, it sounds like bad science fiction. And it is.)

²² Toomer has been called *the* Ptolemy expert. (See *DIO* 2.1 ¶2 §H21.)

²³ See *DIO* 2.3 ¶8 §C25.

²⁴ [Note added 1995.] Those (incl Rawlins 1991W fnn 172&236) who are unawed by Princeton socialite-scholars & the MacArthur Foundation shouldn't miss J.Hitt's highly perceptive (& funny) inside-look at MacArthur winners. The PU connexion (Hitt 1995 p.96): for years, "it didn't hurt to be a professor at Princeton (where foundation president Adele Simmons once served as a dean). In fact, five of Princeton's many MacArthur geniuses . . . all live on Hartley Avenue." (This doesn't sound much like the original MacArthur intent of aiding needy nonestablishment geniuses: Rawlins 1991W fn 236.) Systematic catering, to the tenets of archon-angels who arrange such manna, is a Princetitution-plague whose side-effects include a lingering inability of Hist.sci (with its high Princeton Inst contacts: Rawlins 1991W fn 172) to distance itself from Princetitude-god Neugebauer's obsessive fantasy that pre-Ptolemy Greek astronomy was primarily Babylonian in origin. The reader may judge for himself whether these social factors had any relation to Neugebauer-protégé N.Swerdlow's 1988 MacArthur, a \$285,000 prize for (Hitt 1995 p.98): "technical analyses of the works of Ptolemy & Copernicus [which] have led to a greater understanding of the development of astronomy". (For other views&facets of Swerdlow, see *DIO* 2.3 ¶8 §C, Rawlins 1991W fn 167, & Dicks 1994.) If there simply *had* to be a Muffia-circle MacArthur prize, it might have been slightly wiser to choose John Britton, who continues *seeking* & occasionally making (fn 31) genuine discoveries and who does not have the academic seniority of Swerdlow (also productive: ¶13 §B8). It would be good to see Britton free to pursue ancient mysteries fulltime. (Note: there is no difference between JB & NS on the subject of *DIO*; they both loathe it.)

¹⁸ See *AmerJPhysics* 55:235 (p.236 item 4) or *DIO* 4.2 ¶7 §B23 item B ii!

¹⁹ Designating a Muffioso to review Thurston is about as innocent as deputing Wilberforce to review Darwin.

²⁰ See *DIO* 3 §L8 & *DIO* 4.2 ¶6 fn 1.

²¹ I.e., decades of uncompromising Muffia archonal bulls have infallibly established the orthodoxy that Ptolemy was The Greatest. Thus, the Muffia's high priests have locked themselves inescapably into rejecting even mild agnosticism on this subject. Consequence: religiously-obsessive inability to compromise with either heretics or heresy; i.e., the Muffia's years of unbending decrees (& cumulatively dishonest pretense that heretics contribute nothing) have forced it to continue striving for complete & utter *expurgation* of dissent (& all disobedient dissenters) on Ptolemy. (Such poison infects even a productive scholar like Britton: §I3.) Meanwhile, Muffiosi slanders (e.g., *DIO* 1.1 ¶1 §C7 & ¶3 §§D2-D3) paint the skeptics as the rigidly nutty side of the Controversy!

F2 This seething clique has made genuine contributions to knowledge — including my own knowledge. For this I have for over a decade repeatedly published²⁵ my appreciation (entirely unreciprocated);²⁶ and I shall naturally continue a policy of evenhanded citation, since that is the heart of honest scholarship. It is on this point of policy (citation of those with whom one simply disagrees) that the Neugebauer clique most blatantly exhibits both its scholarly & temperamental shortcomings — and reveals a cohesiveness of purpose which I will unfashionably but quite justifiably refer to as conspiratorial. (Written confirmation of this assessment will appear below,²⁷ from an unexpected source: the Neugebauer clan's most loyal public toadie, O.Gingerich.) The group's other characteristic is its innocence of the techniques & openminded attitude of modern science — a precious ferity which has led it into so many entertaining howlers, omissions, & muffs in matters of astronomy, mathematics, & statistics, that readers will understand why *DIO* regularly refers to this gang as: the Muffia. (See 45-item catalog of Muffia muffs at *DIO* 4.1 ‡4 §A.)

F3 (I do not mean to imply by the foregoing that the Muffia's historical accuracy is a whole lot better²⁸ than its science. The clan's greatest scholarly strength — especially Toomer's — is probably bibliographical. But Muffia affinity for deliberate noncitation of heretics is obviously fatal to one's trust even in that.)

F4 The Muffia currently explains its reluctance to cite heterodoxy by complaining of DR's behavior. (Classic self-fulfilling prophecy: my criticisms are entirely *in reaction to* the censorial behavior now justified thusly.) The problem with that alibi is obvious: R.Newton never reacted as forcefully as I have to Muffiosi. But a Muffia citation-blackout (Rawlins 1991W §D4) was applied to him for about a decade — abandoned only when R.Newton 1977 (*The Crime of Claudius Ptolemy*, Johns Hopkins Univ) became widely noted in the popular press. (See §D6 & *DIO* 2.3 ‡6 §E3.) [Remark added 1995.] Likewise, gentlemanly Hugh Thurston is now attacked, shortly after he committed the heinous offense of publishing the irresistible [*DIO* 6 ‡1 fn 137] heresy that DR had solved the source of the Babylonian System B yearlength: §E3 [& *DIO* 6 ‡1 fn 16]. Earlier polite scholars, who became victims of Muffia anti-heresy assaults, are listed at *DIO* 1.1 ‡1 §C5.)

F5 So explaining years of Muffia noncitation of RN will require a *different* ad-hoc excuse, a situation that creates an Occamite problem which we will also encounter in the evidential pros&cons of the Ptolemy Controversy itself: Muffia attempts to answer the charges against Ptolemy (invariably appearing in arenas where reply is highly unlikely to be published — e.g., book reviews) always concentrate largely upon just 1 single sector of the evidence. Thus, a reader will not realize that the unlikely alibi generated for this special part of the Ptolemy problem is inapplicable to Ptolemy's other fabrications. Each ad hoc scenario (e.g., Evans 1987) seems less grossly implausible in isolation; for, only when one sees, all at once, the whole crazyquilt of unattested Muffia hypotheses (applied to the task of resuscitating Ptolemy), does one discern just how unlikely they are in conglomerate.

F6 The scholarly acumen of O.Gingerich (Harvard & Smithsonian) is not much respected by these Muffiosi capos, but OG's unrequited loyalty [Gingerich 1976, *DIO* 2.3 ‡6 fnn 17-18] is such that I will loosely speak of him as an adjunct member anyway. (Has OG imagined that backing Muffiosi might land him a permanent Princetitude post? [Contra Rawlins 1991W §B3's jest: not even Inst critics think its standards have come to this.] Note in passing: those familiar with the hidden politics of the Peary affair will find a parallel

²⁵ See Rawlins 1991W fn 16 & fn 174 for extensive lists of examples of *DIO*'s recognition of & gratitude for Muffia contributions and discoveries. [See also *ibid* fn 2.]

²⁶ [Note added 1995.] The 1995 *JHA* has finally acknowledged a few corrections [*DIO* 6 ‡3 §A1], but there is still (*ibid* §§G-H) no Muffia admission of *DIO* contributions. Indeed, despite the collapse of lead-paper-billed "proof" that Greek trig orbits couldn't be fit to Greek solar data, the Muffia still coherently stands behind its incoherent (*DIO* 1.2 fn 209) Babylonian *arithmetic* purported solution of the trios A&B data of the *trigonometrician* Hipparchos (*ibid* fn 75). The mental requirements here are as obvious as absent: common sense, testability, & generosity. I.e., nothing essential has changed.

²⁷ See §H8 item [5]. Also §§B5-B6, B8, G13-G14, *DIO* 1.1 ‡1 §A8, *DIO* 1.2 §D4 [& *DIO* 6 ‡1 fn 7].

²⁸ See *DIO* 1.2 fn 92 & fn 116.

between V.Stefansson's rôle in it, and OG's rôle in the Ptolemy Controversy.)²⁹

F7 OG is #2 editor at *J.Hist.Astr.*, whose Editor M.Hoskin (Cambridge Univ) & Hoskin's best friend Olaf Pedersen (Univ Aarhus; long an Editor of *Centaurus*) have also aided the Muffia tactic of destroying dissent on Ptolemy. Thus, I include them & theirs under the Muffia banner, as well. (That does not imply that the central Muffia exactly reveres Hoskin or Pedersen. See, e.g., *DIO* 1.1 ‡5 fn 6.)

G Fear & Dissembling

G1 On 1976/3/2, a few days after my first talk (§D7) with R.Newton, I again tried & failed to reach Henderson at CUNY (Queens). On the evening of 3/3, I phoned OG about the review. He told me that Henderson was a protégé of Aaboe, himself a protégé of the ultimo in these matters, Otto Neugebauer, whose name I knew quite well and for whom I had at the time the highest regard. OG acted not sympathetic to the noncitation approach, one of a number of comments he made which had me for quite a while hoping that he was trying to encourage free debate on the Ptolemy issue.

G2 Later that evening, I finally reached Henderson by phone in Queens and asked her: [a] why she hadn't cited the R.Newton papers I'd seen in *QJRAS*, and [b] how she accounted for the suspicious agreement which R.Newton had demonstrated between Ptolemy's allegedly outdoor "observations" & his indoor tables, especially³⁰ for the Sun. (E.g., was this correlation being explained away as due to Ptolemy's data-selection?) We also discussed the fact first pointed out by R.Newton 1973-4 (p.14): that the framing instrument allegedly used by Ptolemy for measuring lunar zenith distances (*Almajest* 5.12-13) would instantly reveal that the Moon's distance from Earth did not vary by a factor of nearly two (!), as the Ptolemaic lunar theory required. (Neugebauer actually believed Ptolemy knew of this error: §G12.) And I raised an amazingly revealing bit of evidence, which though long known, has been insufficiently emphasized by skeptics, and which is to me by far the strongest proof that Ptolemy never observed: he didn't even know his own latitude (error -14'). No regularly observing astronomer who (as Ptolemy claims at *Almajest* 3.1&7 and 7.3) made numerous observations for years on a transit circle (*Almajest* 1.12), could possibly be so far off. That is obvious to any scholar with the slightest understanding of observational astronomy (a class which I increasingly came to realize did not include alot of Hist.astron persons).

G3 But it was equally obvious from Henderson's replies that: she didn't even pretend to know the answers; i.e., *she* didn't know what the justification for *her* ignoring R.Newton was. Surreal. All she could say in response to questions of astronomy & statistics was that: her mentor Asger Aaboe knows the answer; let me give you his phone number! (It all reminded me of a longago conversation with a young nun whose defenses of the deity were entirely: speak to my priest.)

G4 It is worth adding that, given O.Gingerich's good relations at this time with *S&T*, it is likely that he was involved in the selection of this cipher as a reviewer.

G5 I spoke to RN again 3/3. When I brought up the possibility of a verbal debate, he was not sanguine about the idea, largely because he had been earlier invited to a gathering where he was to be the sole skeptic and his paper was to be shown to the other side without reciprocation! He also had been quite ill at the time. (Curiously, the next time he was invited by OG to speak, 1984/6, he also happened to be ill. Though he was [in the event] able to talk, no debate was scheduled — despite my 1984/4/6 urging of this to OG. By contrast, I am always healthy; and OG has never invited me to talk anywhere.)

²⁹ But whereas VS wanted to be known as a freethinker, OG prefers posing as a practicing Christian.

³⁰ See *DIO* 4.2 ‡7 §B23 [B] or Rawlins 1987 p.236.

G6 RN mentioned that John Britton (another Aaboe protégé, who had since become a stockbroker)³¹ had invented a way to explain Ptolemy's erroneous obliquity (Britton 1967,³² Britton 1969); but Britton's theory didn't answer the question that applies throughout the *Almajest*: why did Ptolemy's "observation" of the obliquity perfectly agree with an erroneous value, pre-established long before (by Eratosthenes, in this case), while disagreeing with the real sky by an amount far larger than Ptolemy's own (*Almajest* 1.12) estimate of the measurement's error (the same situation as for the solar observations)?

G7 On 3/9, after another chat with OG, I phoned Aaboe and asked some of the same questions I had put to Henderson. No problem here, regarding shyness: he filled me in with the greatest of confidence and ease. Superficially, he was far more impressive than RN (whose voice was very quiet and nervous). Aaboe was conversant with all of the texts and with the entire Ptolemy Controversy. A pleasure to listen to as he rolled along.

G8 However, aside from his confident air, he had no convincing answers to the question of the agreement of Ptolemy's allegedly outdoor astronomical "observations" with his indoor tables. Aaboe spoke of errors of observation, instruments, calculation, & rounding. Perhaps these dodges would work with a neophyte science reporter, but they of course could not persuade physicist DR, since they did not address the central point (§G2[b] & §G6).

G9 When I then asked Aaboe about R.Newton (wondering why mere disagreement had led to such bad feeling), he said that RN's research was: "incompetent work in my realm". The proprietary message (§C4) outweighs even the arrogance. (I hope that a lasting achievement of *DIO* will be the establishment of a forceful public reminder that: those who banish scholars are *gambling*; they are gluing their reputations unremovably to the inevitably risky evaluation-prediction that the exile is utterly worthless *and will forever remain so* — that is, he will never make a single valuable discovery throughout his entire career. [Thus, if he does, the banishers **must forever fake** its worthlessness: *DIO* 4.2 †9 §T, *DIO* 6 †3 §B2.]

G10 Three days later, I attended a Hist.sci conference at Johns Hopkins and there met a Muffia protégé, whom we will call L. I also supped with a leading mogul (Hist.sci, Yale Univ), who defended W.Heisenberg's infamous attempt to build an atomic weapon for Hitler with a novel line of exoneration: well, Heisenberg had to do *something*. This archon steadfastly refused to own that Heisenberg had done anything amiss. (Other than fail.)

G11 A week passed, and yet another archon (since deceased) from Yale's Hist.sci Dep't visited the JHU campus for a Hist.sci event. I asked him (skeptically) about Ptolemy's astrological work *Tetrabiblos*; he replied that at least it was astrology at a high level.³³

G12 Ten days later, another Muffia protégé — whom we will call W — joined me (1976/3/29) in a clandestine visit to R.Newton (at his Silver Spring home), whom neither of us had ever met. It was an entirely amiable chat. W asked about a single RN slip (cited at *DIO* 1.3 §O3) which has become a Muffia favorite (since so few other slips can be found in his Ptolemy work). RN openly discussed it (as also at R.Newton 1977 p.130). Later in our chat, he enlightened both his visitors by pointing out an astounding error of arrogance by O.Neugebauer. (See *DIO* 1.3 fn 284.) The 3-way conversation lasted from about 20:30 EDT until around midnight.

G13 As we left and got into my car, W said that I must *never* tell Aaboe of our visit to RN. (*DIO* will continue to protect W's identity until receiving clearance to do otherwise.)

G14 Soon after, I phoned shy W's colleague L (who had already known of the visit before it occurred) to find out what the problem was. I was told, in so many words, that W

³¹ Despite his Muffia inclinations, it is fortunate that Britton continues his customarily-high-quality work. A pearl is his induction that the Babylonian tablet-fragment BM 55557 is a collection of integral 4th-power calculations.

³² Later published as Britton 1992.

³³ But, in fact, the *Tetr* is as nutty as any other astrological work. See excerpts & DR's comments thereon in the *Skeptical Inquirer* (*Skinq* 2.1:62 [1977] pp.70-71). Ptolemy's promotion of exploitive superstition tells us what he really was. (Just as Jesus' magic-show "miracles" tell us what he really was. [The utterly unremorseful 1913 *Catholic Encyclopedia* 3:17 states that Giordano Bruno was imprisoned & condemned by the Inquisition primarily for drawing attention to this common-sense view — & alleges it was *purely coincidental* that Bruno was also a proponent of the Church-proscribed heliocentric astronomy. He was burned alive at the stake, Rome, 1600/2/17.]

simply wished not to lose little things like: publication, grant-funds, & conference-invites.

G15 The reality was now out of the bag. The course & nature of the modern Ptolemy Controversy was determined. I.e., an ancient scandal was irrevocably doomed to become a modern one.

H Referee Anonymity Abuse: Backshooting-Slander as Peer Review

H1 Soon after, DR learned that *JHA* Editor MAHoskin had called, for early 1977, a meeting at the Royal Astronomical Society, where R.Newton's work was to be attacked — but to which RN was not invited! Upon learning of this, DR immediately sent MAH (in two pieces) the full version of his analysis (§C1) of the Ancient Star Catalog. It was not read nor even noted at the meeting. (Nor did the *JHA* referee it. In fact, Hoskin did not even acknowledge receipt — until after DR inquired at the Roy.Astr.Soc.) Indeed, the high-handed nature of the paper's treatment probably accounts for the stolidity of the PRIMO-clan's demented, even dishonest³⁴ refusal (to this day) to acknowledge the force of its arguments. After treating the paper so badly, to now admit its value would implicitly reveal how untrustworthy the Muffia's pretended expertise can be. (This is not to imply that the Muffia's work & judgement is worthless. Far from it.³⁵ But its suppressive policies are as unreliable as they are unethical & transparently motivated, particularly the skewed policy of citing heretics' supposed errors, not their useful contributions [*DIO* 6 †3 fn 6].)

H2 Next, DR — realizing that Hist.sci was simply incapable of evaluating his star-catalog statistical arguments — submitted the paper to a real science journal: the *Publications of the Astronomical Society of the Pacific*, which — after long delays due to OG&co — published it in 1982. (Meanwhile, the paper was strongly endorsed³⁶ by the great Bart van der Waerden, author of a classic Springer-Verlag statistics text.)

H3 One of the anonymous *PASP* referees chosen was none other than O.Gingerich. Now, most scientists believe that a referee's job is to analyse the technical aspects of a paper. But OG didn't bother with any of that. What follows is the full text of OG's referee report, which was expected to remain forever secret from DR & the rest of the academic community:

To: *PASP* 1977/7/8
From: Owen Gingerich
Professor of Astronomy & the History of Science, Harvard University

H4 It is rather awkward for me to be a reviewer for the paper by Dennis Rawlins. I have had long telephone arguments with him about Ptolemy's integrity as a scientist. He sent a version of this paper to be presented at a session of the IAU in Grenoble last summer, and because it was so badly written and unclear³⁷ I did not read it. And as a result he believes that there is a conspiracy to suppress [sic] all criticisms of Ptolemy.

H5 I do not wish to be seen as part of a conspiracy, but I must agree with your previous reader that the message is almost incomprehensible. I think that his two arguments are sound as far as they go, but in both cases I believe that legitimate alternative interpretations could be proposed. [DR: none mentioned; likewise at fn 41.] I think that it would be a disservice to his own attack on Ptolemy to publish arguments so obscurely framed.

³⁴ See *DIO* 2.3 †8 §§C10-C15. [Cause of *JHA*-HAD lockjaw-catatonia: www.dioi.org/cot.htm#rksw.]

³⁵ See fn 25.

³⁶ The private letters of van der Waerden were highly supportive & helpful. See also his *Die Astronomie der Griechen* Darmstadt 1988 p.175.

³⁷ Astronomers may judge the DR 1976 paper's clarity & value by an excerpt from its most technical section, published at *DIO* 1.1 †7 §F3 (as part of DR's 1990 lecture, delivered by invitation to the Amer Astron Soc).

H6 Mr.Rollins [sic] has almost become paranoid about this, so if you turn it down in its present form I hope that you will be extremely careful in framing your letter.

H7 Gosh, what a nice guy. So, even back in 1977, just as soon as JHA's O.Gingerich saw DR's horrible absent-error-waves test (later Rawlins 1982C) and before he even understood it, he was already stating privately in writing that (though he claimed to find no factual or technical error in it [note added 2009: its validity is not even controversial anymore]): there MUST be an alternate explanation. And, he of course helpfully added that DR was crazy — a desperate (failed) ploy to prevent DR's critical discovery from ever being published.

H8 DR's private reaction follows.

This Referee Report uses libelous personal statements to cast doubt upon the validity of scientific conclusions which are embarrassing to the Referee's own repeated public stance. In this way, he avoids dealing with the technical content at all — which is perhaps fortunate, considering that not one factual statement in the Report is accurate. . . . Literally. (Incredible? — not if you know the Referee's record re fluffs, in both astronomy and gossip.)

[1] I have never had a verbal argument with O. [Several friendly chats.]

[2] He has confused two papers of quite different date, subject-matter, math-level, and method of analysis.

[3] The Star Catalog tests were conceived and executed entirely subsequent to the IAU section on Ptolemy at Grenoble, so their alleged obscurity cannot possibly have been the cause of the first paper's non-presentation, which OG's own 1976/9/15 letter to me ascribes to lack-of-time (he ought to get his stories straight). The earlier paper was 3pp. and its basic equation was arithmetic. No-time? Not-clear to OG? Well

[4] OG's failure to present the first paper at Grenoble (1976/8/31±1) can hardly have been the cause of my criticisms of ancient-astronomy-historians' cohesive non-reply (to R.R.Newton's demolitions of Ptolemy) for the very simple reason that my censuring of this behavior was right in the addenda to the Grenoble paper! (Mailed to OG, 1976/8/19)

[5] It is amusing and ironic that OG should now attempt to brand as mere Conspiratorial Dementia these conservative appraisals of historians' systematic peculiarities re Ptolemy (and re RRN, whom they then refused to discuss in print, while regularly reviling him behind his back — still the case with Neugebauer and his disciples Aaboe & Toomer). The fact is that *everyone* familiar with this cult's introverted ways was aware of the realities (and freely discussed them, privately). For example, the scholar who first (*Science* 1976/8/6) broke the Ptolemy-defenders' 2 year public silence (re RRN's 1973/12-1974/6 *QJRAS* papers) later wrote me (1976/9/15): "So far the Neugebauer camp has not been heard from. Perhaps my merely mentioning Newton in a review of Neugebauer has placed me beyond speaking terms." The identity of this, my [fellow paranoid]? Owen Gingerich. . . .

I am reminded of the exasperation of a recent critic of other irrepressible Independent Thinkers who have plagued astronomy (generally from the outside) — he notes wearily how much time, labor, and page-space are required to set straight only a paragraph of pseudoscience mis-statements.

I [had] taken similar pains once previously with OG; his reply, unable to refute a single one of a long string of demonstrations of factual screwups, instead simply went off in other directions — and launched a whole new thicket of misinformation!

And I always thought it was we Paranoids who were out of touch with reality.

H9 About the same time, OG answered an invitation to debate DR by sending (to the party offering to publish the exchange) another private charge that DR was a nut. (Text quoted at *DIO* 1.1 ‡1 fn 20.) When this gross letter was sent to DR, OG got mad at the transmitter! — and, to this day, he has not recognized any fault of his own in the matter. Yes, archonhood does affect brains. And their remains.

H10 We end this section by citing two striking realities, each of which will usefully educate the uninformed, regarding the true current state of US academic freedom:

[1] O.Gingerich will never acknowledge his misbehavior in any of the here-noted episodes (§§B5-B6, H6, I1).

[2] Not *one* archon will criticize OG's behavior. In public, at any rate. (Which explains the broad scope of one of this article's sub-headlines. [See *DIO* 2.1 ‡3 fn 15.] Academe has finely-honed senses of proportion & propriety: *it bans not its criminals but their exposers.*)

H11 Similarly: not one scholar has ever (publicly) criticized Hoskin's 1983/3/21 banishment of DR. When I say that alot of modern academics live under a reign of terror, it's realities like this that I'm thinking of.

H12 These realities — as well as the fact that the most basic common sense *can be suppressed for decades on end* — recommend a mild warning to the public: one must be just a little bit wary of academe's pronouncements. Even though I have no (or am not aware of having an) argument with most academic orthodoxies (especially in the science arena, which is by far the sanest of them all), the problem is that, when controversies arise, the public can be kept uninformed about even the very fact that there *IS* a controversy. So, since one cannot (without laborious private checking) know which of academe's consensuses are the ones which are reared upon rotten foundations, it is wise to remain mildly skeptical about all of them — *until open-court mechanisms for settling controversies are finally established.* (See §J3.)

I Ivy League Grab: Mean Motions & Spirit

I1 On 1980/4/13, DR sent a fateful letter to OG, imparting the shocking information that all the Muffia's top snobsters had mis-stated the mathematical sources of the *Almajest* planet mean motions. (Full citations at *DIO* 2.1 ‡3 fn 38. See also *DIO* 1.2 fn 56. On Muffia amnesia about this: see *DIO* 2.1 ‡3 fn 47.) Those who had published mathematically incorrect equations: O.Gingerich, O.Neugebauer, & O.Pedersen. (See also G.Toomer's misconception: *ibid* §C15.) I.e., all Muffia capos had mathematically screwed up the solutions of these 5 mean motions, and DR's letter bore mathematically fitting solutions for all 5. [Two historically false: see *DIO* 11.2 (2003) p.30.] (Letter's text at *DIO* 2.1 ‡3 §C5. Planet-by-planet Muffia-vs-DR fit-comparisons at *ibid* §C3.) Muffia's pristine record: [a] no (explicit) retractions, & [b] 1984 publication (Toomer 1984 App.C) of most (the three³⁸ undeniable ones) of DR's five solutions (this after referee OG had suppressed 1983 publication by DR! — see *DIO* 2.1 ‡3 §C7) — but without the slightest credit to the discoverer, or to R.Newton 1982 (pp.103&108 n.11) where RN published the same three solutions for DR, two years ahead of Toomer 1984. (In the more than ten years since, not one Muffioso has acknowledged the undeniable publication-priority here. Is this the sort of scholarly integrity which Harvard Univ and the Princeton Inst wish to be involved with?) I.e., the very cult that for years consistently contradicted the solutions is now implicitly pretending to have invented most of them, while not crediting the scholar who actually did recover [most] of them. This is as extreme³⁹ as academic brass gets.

³⁸ Mercury, Venus, & Saturn — whose [valid] solutions' identical factors are right in Ptolemy's own preface (*Almajest* 9.3) to the tables. The other two [nonhistorical: §I3] solutions were first published at Rawlins 1987 p.237 (Mars) & fn 29 (Jupiter).

³⁹ But academic archons will do nothing about such sham. (See §I2.) Why? Because a scandal makes academe *look* dishonest. (If you ever questioned whether there are natural comedians, this almost perfectly perverse situation should dispel any doubts.)

I2 On 1994/5/8, when publicly challenged by DR from the floor, at the M.I.T.-Dibner Inst meeting, neither Toomer nor OG would claim these discoveries for themselves. And DR proposed that, *after 14 years*, there should be public acknowledgement of the identity of the true discoverer of solutions important enough to be featured by Toomer in a special final Appendix C to the *standard modern edition* of the *Almajest*. This reasonable suggestion has met with silence since. DR has requested⁴⁰ leading Hist.sci persons to inquire of Gingerich about the matter (to push OG to make up his mind as to whether he wishes now to claim nonreceipt or subsequent misplacement of the 1980/4/13 DR letter to him). None has reported doing so. [No Muffiosi asked; but in 2003, D.Duke asked for a copy, and OG sent one. (To DR, not Duke; but we'll assume here that OG is now acknowledging receipt.)] This, in spite of *DIO 2.1 ‡3 §C*'s extensive publication of the historical record of this appropriation.

I3 OG's 1983/7/23 reaction to DR's §I1 planet-mean-motion solutions? Same ploy⁴¹ (echoing §H5): there's-got-to-be-an-easy-alternate-explanation. (Question: why has *Muffia* output, e.g., §B4, not been suppressed on similar grounds? I.e., the alternate-solutions gambit is an argument for *skeptical discussion*, not for *suppression*. And this conveniently-broad alibi can kill *any* paper an editor dislikes: in the entire history of science, no one who found a solution to an empirical problem has ever been able to pre-guarantee that another solution [whether valid, plausible, or just an Occam-nightmare contraption] won't fit the same data. Should one have suppressed I.Newton's corpuscular optics just because the wave-theory provided an alternate explanation?) [Note added 2003. Though the OG & Moesgaard Mars solutions were indeed false (& grossly nonfitting), DR's perfectly-fitting Mars solution has also turned out to be invalid, and the Jupiter one as well: see A.Jones' true solutions at *DIO 11.2 p.30* and ‡4 eqs.31&45. Jones' finds prove positively that all 5 motions are based on period-relations (as DR was 1st to propose: 1980/4/13), so there is no longer any doubt (contra Toomer 1984 p.672) that the revolutionary 1980/4/13 DR solutions for the other 3 planets are correct. And, along with most scholars in the field, Gingerich appears (if *DIO 11.2 p.30* item [a] is true) to have at least implicitly acknowledged that the above (§H5) alternate-theory alibi against DR's Ancient Star Catalog analysis is dead.] Comments: [a] Does occultist-level invincible-unfalsifiability alibi-wriggling-out get any funnier than the *there-must-be-an-answer*⁴² bedrock of OG's faith in the mere-

⁴⁰ E.g., *DIO 2.1 ‡2* fn 25.

⁴¹ See *DIO 2.1 ibid* §§C7&C18-C19; also *DIO 1.1 ‡1* fn 9. Likewise, a K.Moesgaard 1983/12/15 referee report discouraged publication of DR solutions to a quite different Ptolemaic problem by adopting the automatic alternate-solutions-are-easy mantra. However, again, in the decade since, he has yet to produce any of these allegedly-easy alternatives. Instead, the "alternatives" proposed are either lousy fits (*DIO 1.2* fn 129 & *DIO 2.1 ‡3 §C4* [note: former ref misrendered at latter's fn 25 in 1st printing]) or solve a completely different problem. Moesgaard — an extremely able scholar, sadly trapped in a careerist world — has been the most irrepressibly-volunteering fount for such exercises in *Muffia*-suppression-alibi artistry. (So far.) See the amazing Moesgaard 1987 noncitation remarked at *DIO 1.2* fn 56. He has made no comment on this or several other oddities: *ibid* fnn 126, 129, & 170, and *DIO 2.1 ‡3* fn 23. [Note 40% DR mis-fire here: *DIO 11.2* [2003] p.31.]

⁴² C.S.Lewis *The Problem of Pain* 1940 Chap.9 ("Animal Pain") attempts to slither out of one of the several fatal internal contradictions of Christianity: why god visits pain upon animals who have no souls (to purify by deserved adversity) and who do not share Original Sin. Thus hideously cornered, Lewis actually suggests that animal pain might be merely an illusion! (This is where one ends up if one thinks about it. Less scrupulous religionists' usual solution: don't think about it.) Do not miss a theology-on-the-rack followup 1950 exchange regarding animal pain (reprinted: Lewis *God in the Dock* 1970 pt.1 chap.20) between Lewis & C.E.M.Joad (both dep't heads at Camb Univ! — see *DIO 1.1 ‡8* fn 13). Joad gives a deliciously nimble pseudo-evasion of the Problem of Evil, inadvertently accusing god of [i] unlovable virtuous automatonhood, and [ii] brooking no competition for that rôle. Lewis then gives his precious overview-answer to the problem of animal pain (pp.167-168 & 170 emph added): "if God is good (and I think we have grounds for saying that He is) then the appearance of divine cruelty in the animal world *must be a false appearance*. What the reality behind the false appearance may be we can only guess. . . . What really matters is the argument that *there must be an answer*: the argument that if, in our own [as distinct from the animals'] lives, where alone (if at all!) we know Him . . . then in other realms where we cannot know [much about] Him . . . — then, despite appearances to the contrary, He *cannot* [DR: vs. above *if-at-all*] be a power of darkness. For there were appearances to the contrary in our own realm too; yet . . . they have somehow been *got over*." To watch this theologian (the world-renowned "apostle to skeptics") get so desperate as to seek refuge in semi-solipsistic agnosticism (in order

illusoriness of all apparent contradictions in Ptolemy-worship? [b] If OG was certain *from the outset* that there **MUST** be equally convincing alternate explanations — even though he can't find them! — for all evidence that seemed to contradict his views, then he must have known positively *from the outset* that his scholarly position was correct. So we all await impartation of the precious secret (possessed also by hero Ptolemy — as well as by a fellow specialist in Christian apologia & other fiction, C.S.Lewis: fn 42) of how to **KNOW** the answer to a problem *before investigating the evidence*. (See Princeton Institute-Muffia-godpop's 3rd-grade-math-level screwup-of-prejudice [*DIO 6 ‡1 §H4*]. See also Princetitute-funded Britton 1992 p.xvi's regrets that R.Newton's accursed "work has come to represent a counterview of Ptolemy's contributions *which has proven difficult to dislodge*" [emphases added] — a *Muffia* phraseology which inadvertently gives us a glimpse within the Inquisitional mind, whose aim is not openminded pursuit of truth but rather: the Extirpation of Sacrilegious Evil. [See *DIO 6 ‡1* fn 5.]) This is the key to all *a priori* geni⁴³ throughout history: they treat incoming evidences not as the ultimate arbiters of truth but as low, subsidiary junk — which acquire significance only when they are finally [if ever: *DIO 6 ‡3* fn 6] fit to the pre-known Higher Truth. Final question: Why are faith, slander,⁴⁴ & misreportage⁴⁵ ho-hum-tolerated in the US' top **scientific** councils? [See, e.g., www.dioi.org/fff.htm#pgrs.]

J The Rule of Law: a Fading Memory

J1 Most current scholars cannot remember the day when science-journal refereeing was not anonymous. However, a half-century ago, nonanonymous refereeing was common procedure for the American Physical Society.

J2 And what is the standard modern archonal excuse for today's norm of star-chamber refereeing? Answer: referees must be promised anonymity if they are to comment frankly upon papers authored by those *who can affect their careers*. Not only is this a classic band-aid approach to a deep-rooted problem, but it is frying-pan-to-fire-hilarious — in the clumsiness of its devastating implicit admission of the central reality, namely, young scholars are terrified that unseen archons could sever their lifesblood (§G14): the grant-funds & publication they must have to survive in an era when even tenure doesn't mean much anymore. (Another reason DR must be stamped out: if his defiance succeeds, this could

to evade an out&out proof that his entire structure is without foundation) is alone worth the price of the book. (One sees why an inventive fictionist is best-suited to this sort of thing.)

⁴³ Indeed, it's this that convinces OG that Ptolemy was a genius. See OG's catch-all alibis at Gingerich 1980 p.264, which are *required orthodoxy* in much of the curious Hist.sci community (echoed also, e.g., at Graßhoff 1990 pp.205&215). OG: fudging observations to accord with theory isn't fraud, it's absolutely MAHvellous; "Ptolemy, like many of the brilliant theoreticians who followed him, was perfectly willing to believe that his theory represented Nature better than the error-marred individual observations of the day. As one of America's Nobel laureates remarked to me, any good physicist would do the same today. [DR: (a) Why does [our ever-more-cornered-&-delusional alibi-artist] not name said putative Nobelist? (b) Is Congress reading this?!) . . . It is *marvelous* to find these foremost theoreticians so clearly voicing their belief in the primacy of theory over observations. . . ." (Emph added.) Unfortunately for OG, none of the scholars he cites ever says (clearly or unclearly) that it is better to force observations to fit theory. Thus, he must cite alleged verbal exchanges and their alleged implications. Which shows how wonderful it is that we have [MAHoskin-cult geni & jourmii] — without whose discernment (of what great scientists *really* thought), we might never have figured out that scientific immortals believe data-faking to be a superior form of intellectual activity.

⁴⁴ See §§H4-H6 for OG's behind-the-back 1977/7/8 referee report, in which he offers no criticism of DR's science (other than OG's occult belief that alternate-explanations-must exist) — but instead says DR is insane. See also *DIO 1.1 ‡1* fn 20.

⁴⁵ At p.76 of OG's American Physical Society-published *Eye of Heaven* (1993-revised: n.3 at p.79) which updates his earlier papers, he avoids public admission that horrible DR unquestionably solved [most of] the *Almajest* planet mean motions. He instead **unrevisedly** repeats the claim (*DIO 2.1 ‡3* fn 19) — which his own 1983/7/23 *QJRS* referee report acknowledged the falsity of — that Ptolemy's Mars mean motion is based upon his alleged arc/time ratio (*Almajest* 10.9). DR sent OG a [perfectly-fitting, though also historically false] Mars solution as long ago as 1980/4/13, and OG's 1988 *JHA 19:142* note acknowledges awareness [of the solution's superior fit. Detailed account]: *DIO 1.2 §H3* & *DIO 2.1 ‡3 §C2-C7*. Does the American Physical Society simply not care?

encourage scholars to believe that one doesn't *have* to cower to archons. An intolerable vision.)⁴⁶ The upshot is: overarching, ever-present fear of making highplaced enemies⁴⁷ — thus, reign-of-terror-freezing of free discourse and equitable evaluation of scholarly output.

J3 (The *Wash Post*'s prize-winning science reporter B.Rensberger notes that, for years, there has been discussion of the idea of establishing a court for settlement of academic disputations — but archons keep killing the proposal, claiming that academe is so honest that there is no need for such a body. OK, while we're at it, let's just scrap the US court system, too, since US society is so trustworthy & fair that: there's just no need.)

J4 The close of Mill's more-revered-than-read 1859 *On Liberty* (emph in orig):

The worth of a State, in the long run, is the worth of the individuals composing it; and a State which postpones the interests of *their* mental expansion and elevation . . . [and] which dwarfs its men, in order that they may be more docile instruments in its hands even for beneficial purposes — will find that with small men no great thing can be accomplished; and that the perfection of the machinery to which it has sacrificed everything, will in the end avail it nothing, for want of the vital power which, in order that the machine might work more smoothly, it has preferred to banish.

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⁴⁶ [See *DIO* 6 ¶1 §J6.]

⁴⁷ Despite numerous obvious non-parallelisms, I am nonetheless reminded of the impassioned closing pages of E.Eyck's *History of the Weimar Republic* (Harvard Univ 1963, Sci Ed 1967), summing up the suicidal events culminating in Hitler's Chancellorship (emph added): "The attempt by the German people to rule themselves had failed. A time now came when Germany *ceased to be a state based on law*. This was the time when German judges allowed their courtrooms to be overrun by [the cult] who drove out the people whose noses they did not like; when the judges saw their independence and security abolished and *their professional advancement become dependent on the way their decisions pleased the ruling party*"