

‡1 Prologue: by Dennis Rawlins

A Countdown

A1 It has been leftly remarked that a free press exists only for the person who owns one. But the onsets of photocopier & computer have lately assuaged this traditional plaint, by bringing publishing costs within reach of the nonwealthy, at least for scholars in a small field. Curiously, the Gutenbergian potential for avoidance of (if not outright rebellion against) overproprietary & overconfident authority has heretofore been little realized.

A2 An attractive opening in this regard is afforded by the field of ancient astronomy, an area with which *DIO*, the periodical here launched, will be intimately (though far from exclusively) involved. As will be evident following this prologue (p.12), I intend also to enjoy the eclecticism & fun of an upfront general commentary column, “Rawlins’ Scrawlins” (nonPascalian title credit to my mutually-deflating friend Quiglet). But the more specific purpose of the journal will be scientific history. (As against “history of science”.)

A3 *DIO* is fortunate that the figure who has been the most able¹ of the world’s scholars in ancient astronomy, the great mathematician & statistician Bart van der Waerden, concludes his capstone book on the subject, *Die Astronomie der Griechen* (van der Waerden 1988 p.307), by passing to *DIO* the flickering torch of openminded and mathematically competent critical analysis in this exquisite field.²

A4 Early numbers of *DIO* will present various newly recovered details of the ingenious & refined astronomy of the legendary ancient figures: Kallippos, Timocharis, Aristarchos, Aristyllos, Hipparchos³ — and still other highly creative ancient Hellenistic figures whose magnificent work can be reconstructed but whose names are lost to us. And we will be proud to publish the final academic contributions of 2 of the world’s most gifted and wellknown ancient science specialists. One appears in this premier issue of *DIO*: ‡5. (Other scholars who may wish to contribute papers to *DIO* are asked to read this entire Prologue carefully.)⁴

A5 For the last 2 decades, by far the warmest controversy in the ancient astronomy field has been that surrounding the cascade of revelations of pervasive fakery⁵ in Claudius

¹ It is typical of van der Waerden that (in a 1988/12/20 letter to DR) he denies the charge — instead attempting to convince me that O.Neugebauer is the most respected of such scholars. I’m sure Neugebauer would agree. But I disagree with both men, regarding van der Waerden as the better scholar on at least 3 grounds: [a] mathematical facility (where his superiority would not be denied even by Neugebauer), [b] openness to new evidence, [c] advice & assistance to scholars entirely regardless of personal agreement or academic politics. Whatever Neugebauer’s former contributions, he has long since become the don-of-the-dead to a mob of truth-possessors (borrowing Pat Truzzi’s incisive phrase), while van der Waerden has become an inspiration to truth-seekers.

² However, van der Waerden is in no fashion responsible for the sometimes journalistic tone of *DIO*, and he is far too gentlemanly to approve of *DIO*’s blunt exposures of those who took advantage of his kind & retiring scholarly disposition by publishing wild attacks on a few of his historical papers. I should add that R.Newton, BvdW, & DR all have serious disagreements with each other on a wide array of subjects — disagreements which have never for a moment affected the amiability of our relations.

³ See ‡6 here for Hipparchos’ admirable final discovery. In the gratifying context of rescuing this treasure, it’s worth remarking (for contrast) the necessarily critical or “negative” thrust of so much of the best work that rewrites history. (After all, nobody says, e.g., he didn’t reach the Pole when he actually did.) Nonetheless, one can find numerous DR redemptions (some already published) of unjustly treated figures: e.g., Aristarchos, Aristyllos, LeMonnier, Morton, Papanin, Amundsen, Ellsworth, Nobile, Plaisted, Diller, van der Waerden, R.Newton, & others. See also (fn 24) DR’s macro-defense of the previously-misunderstood majority of ancient scientific scholars.

⁴ My first (nonfacetious) recommendation will be, simply: start your own journal! But if you instead wish to send a paper to *DIO* for consideration, then: [1] mail a *spare* xerox (not to be returned) to [a] *DIO* referee, & [2] phone him in 40 days]. *DIO* will publish very few papers not by DR’s acquaintances. [DR 1998 note: original 1991 policy has long since broadened. Contributors are asked to consult the most recent *DIO*’s inside-back-cover publisher’s statement. *DIO* also] welcomes readers’ notices of [a] errors & [b] prior publication of matters presented here as new. And its departments of Unpublished Letters and Referees Refereed seek input from all quarters.

⁵ Britton 1967 is now much cited by the O.Neugebauer-Muffia’s capos (though never in this connection, before R.Newton’s arrival on the scene) to prove that the Muffia knew all along that Ptolemy’s outdoor “observations” were in strangely consistent agreement with his indoor tables. Certainly the Muffia knew. So why wasn’t the public told? (And why has the proprietary Muffia become so enraged when alien nonMuffiosi publish the obvious implications?) Instead,

Ptolemy's *Almagest*,⁶ which is the central text of the entire field of ancient astronomy. (Ptolemy's dishonesty⁷ has been openly suspected for at least 1000 years, most notably before recent times by the great astronomers Tycho, Delambre, and Peters.)

A6 The revelations which have so scandalized Ptolemy's censorial modern cadre of defenders were first published by Robert Newton (especially R.Newton 1977), while he was Supervisor of the Space Sciences Division of the Johns Hopkins University Applied Physics Laboratory. The other central modern developer of such evidence is myself. These analyses have appeared in some of the leading science journals of the world since 1969.

A7 Nonetheless, during this time, the seemingly most apt "centrist" historian-dominated journals, the extremely handsome *Journal for the History of Astronomy (JHA)* and *Centaurus*, have systematically suppressed the skeptical side of the Ptolemy dispute, providing their readers with only a minimal, controlled, and warped glimmer of what has transpired in this, the most critical ancient astronomy controversy ever.⁸ (Revealingly, Ptolemy's defenders have fled every suggestion of public debate, e.g., the challenge issued by DR in the *American Journal of Physics* 1987/3.) *DIO* will provide an antidote to this skew. Those unfamiliar with science journals may not know my output, so they may understandably conclude that I am starting a journal simply due to Reputable journals' rejection of my papers. Actually, I cannot recall having a refereed paper rejected in over 7 years — not since 1983/7/23, when *QJRAS*-appointed referee O.Gingerich of Harvard (Ptolemy's #1 p.r.-man) evaded his way out of a written judgement. (However, this dearth of rejections is no doubt due in part to my refusal since 1984 to send anything to Hist.sci journals, for reasons explained below — often in overlarge footnotes, which the reader oughtn't to have to be explicitly urged to skip, during first readthrough here!)

A8 Since OG's embarrassingly indefensible flipflop,⁹ I have issued a stream of discoveries in ancient astronomy through: *Queen's Quarterly* 1984 (invited); *Vistas in Astronomy* 1985 (invited: Greenwich meridian centenary symposium); *American Journal of Physics* 1987; *Bulletin Amer Astron Soc* 1990 (invited). These publications have been possible only due to the courageous assistance of a few decent, highly placed scholars who are not

Britton 1967 was not published — except for the sole portion (Britton 1969) that seemed to exculpate Ptolemy! (The Muffia later unsuccessfully tried a similar ploy with DR regarding Rawlins 1999: fn 11.) By contrast, Britton 1967 was uncited even in Neugebauer 1975, Toomer 1975, & Toomer 1984H — though Toomer 1984 now suddenly brings forth Britton 1967 repeatedly as the best discussion available of these matters! (See Toomer 1984 pp.viii, 135 n.12, 138 n.21, 253 n.58, 334 n.64.) Convincing sleight of hand isn't a Muffia strong suit. (See also ‡5 fn 7.)

⁶ From the Arabic "almajasti" (Toomer 1984 p.2). So RRN asks: why does Ptolemy's modern alibi-contingent object to DR's spelling *Almagest* as *Almajest*?

⁷ *DIO* will frequently examine shady scholarship. Whether this is seen as showing that the journal has a critical tendency or is founded upon an ethical base, will depend upon the observer. (See fn 3.)

⁸ Not one of the dozens of new evidential findings against Ptolemy has first appeared in a Hist.sci journal. (Most debuted in journals of astronomy & physics, an inappropriate burden on them.) This impressively pristine record only deepens Hist.sci's stake in now discrediting skepticism of Ptolemy. [But see Jones' huge success (noted at *DIO* 11.2 ‡4 §F3) and Graßhoff's thorough vindication by D.Duke at *DIO* 12 ‡2.]

⁹ Evidently, the hypothetical inner OG never really did reverse his enthusiastic (if miltimed) initial written 1983/1/14 praise for the later-rejected material. (OG's original 1/14 reaction: mailed just before his *JHA* co-Editor's 1983/3/3 rage necessitated DR's ostracism from *JHA*&co: fn 25 & ‡6 fn 15.) For, without a word to me, OG in 1985 encouraged another astronomer, Sam Goldstein (UVA), to publish this same paper's central equations (solutions of the *Almagest*'s mean motions) under Sam's name in OG's *Journal for the History of Astronomy* — a venture that never came off because Sam (an admirably honest and knowledgeable scholar) fortunately contacted me (1985/9/21) before proceeding. These new equations were originally sent privately to OG by DR on 1980/4/13 ultimately published in the *Amer J Physics* (Rawlins 1987). . . . OG spent 6 months (1983/11/4-7/23) piling up heavy reams of computer readout (proudly shown DR 1983/6/4) . . . [yet OG's] 1983/7/23 [*QJRAS*] referee report could find no computational errors anywhere in the paper's 91 equations. . . . [and] acknowledged (privately) that the *Almagest* mean Mars motion [a] had received a [non-fitting] solution published by OG in the very same journal (Gingerich 1981 p.41-2), while [b] the DR paper's Mars solution [fit & was accurately computed] OG's negative verdict on DR did not prevent OG's 1985 attempt to publish the very same Mars equation in *JHA* (SG's ms, Table IIa, Mars' mean motion). [Note added 2008. This adds yet another irony to the odd 1980s Mars-mean-motion history: when headline-crediting OG (*DIO* 11.2 [2003] inside-front-cover), for correctly suspecting an alternate possibility for DR's 1980 Mars solution, DR had forgotten that OG had in 1985 tried to publish the identical unhistorical DR solution under another's name. I.e., OG's objection was not intellectual but personal.]

personal friends but who privately acknowledge that the behavior of the eminent "prima donnas" of this field has been "horrible", adding that there is no question that certain Hist.sci archons are trying to "blackball" DR. (Hardly the chosen sport of scholars busy enjoying their own intellectual creativity.) But they also acknowledge that it could be unhealthy careerwise if they came forth publicly. Thanks to them, I have since 1984 had access to an assortment of non-Hist.sci journals; however, none among these nonhistorical journals can be expected to publish a long succession of papers outside their field. Thus, to continue along this laborious path requires one new journal after another to consider and evaluate subjects alien to immediate areas of expertise. The aggravations of pursuing such a nomadic publication circus are hardly much more than that of starting my own periodical. I do not thereby bar my contributions from other journals' use, since any scholarly journal has *DIO*'s permission to re-publish, verbatim, portions or all of articles published in any issue of *DIO*, so long as: [a] *DIO*'s name & address are printed with the excerpts; [b] it is stated that replies to appended comments will be published in *DIO*.

A9 During the same period, I have also had papers accepted at *Archive Hist.Exact Sci.* (several), *Isis*, & *Archives Int.Hist.Sci.* *AHES* is a very high quality journal, run by someone exceedingly competent in science, but whose editing priorities are unshared by DR. The *Isis* & *AIHS* acceptances included conditions barring certain statements in the papers. (And *Isis* exceptionally added that if my conclusions were attacked, I would get no reply space.) Since these demands were politically motivated censorship, I could not honorably comply and have published much of the same material instead in *QQ* & the *Amer J Physics* (Rawlins 1984A & Rawlins 1987). On 1987/7/14, I was invited to write a feature article for *Sky&Telescope*, but the proposal was not attractive, given that journal's wellknown editorial record.¹⁰

A10 More than once, I have had the stirring experience of sending a new academic discovery to an editor or journal and then later finding the same result published under another scholar's name — sometimes in the very journal I'd sent it to. (In 1984, I entirely ceased sending material to Hist.sci people. By coincidence, no subsequent DR discoveries have mysteriously diffused.)

A11 So I am starting *DIO* at least in part because I prefer to publish without negotiating the perils of [1] censorship & [2] finding my results in print but with my name randomly misspelled as "B.Goldstein" or somesuch odd anagram.

B Originality

B1 I am prepared to believe that many cases of apparent plagiarism actually involve nothing worse than innocent intellectual osmosis. (A simple test: after any publication

¹⁰ As noted in Rawlins 1984N, a fresh Neptune-Scandal theory (namely, the critical import of J.Adams' 1845-6 calculations being sequestered by a tiny UCambridge clique, until after Neptune's 1846/9/23 discovery in Berlin from U.Leverrier's published math) was included in a DR review for *Sky&Tel* in 1980 but then suppressed. Soon after, much the same theory surfaced without citation in a speech by R.Smith (protégé of *JHA* Editor-for-Life, whose co-editor, O.Gingerich, is close to *S&T*). Smith's exceedingly valuable 1989 *Isis* article ("The Cambridge Network in Action: The Discovery of Neptune") nowhere mentions DR in the main text in this key connection. The Smith paper's thanks & notes acknowledge access to (& nonevidentially reject the secrecy-aspect of the theme of) Rawlins 1984N & a DR 1966-1972 Neptune ms (basis of 1966/5/11-20 presentation at Johns Hopkins); but few readers will know that the Smith article's central (titular!) thesis is essentially the keystone of both the 1966-1972 ms & Rawlins 1984N and was pioneered by DR (running counter to all other pre-1980 modern discussions), and that Smith's hard-earned new evidence consistently confirms DR. (As does vacation-bound Cantab Airy's 1846/8/6 parting advice to Cantab Challis, Camb Obs Nept file item #6: "write to Mr.Main [Airy's RGO ass't] who is fully in my confidence and understands the position of the whole matter.") In the 1989 Smith paper (& in Smith's intimately related 1983 *JHA* paper), Liverpool's Smith thanks longtime friend (& 1984 *JHA* co-author) R.Baum of nearby Chester for comments; Baum was the only scholar on Earth who possessed the 1966-1972 DR ms (since 1972/6/16) before Smith found the clique-silence key to the Neptune affair. As for the 1980 DR ms (containing this discovery) written at *S&T*'s request: *S&T* has lost its entire file. Anyone wishing to lodge an original find in the pages of *S&T* (or a similar journal), might ponder these events: [a] The discovery is not published. [b] It soon reappears elsewhere. [c] The prior transaction vanishes; thus, no certifiable copy of the original submission survives — or so it may have seemed.

of your findings without proper citation, give the author or journal the opportunity to acknowledge the actual order of priority. The reaction will indicate the degree of guilt involved.) However, there is also no question that original scholars have had to contend with intellectual piracy since antiquity. In the First Century AD, Pliny (Preface 21) remarked that verbatim plagiarism was practiced by the *majority* of the *best known* writers, adding (*ibid* 21, 23):

it is a pleasant thing and one that shows an honourable modesty, to own up to those who were the means of one's achievements Surely it marks a mean spirit and an unfortunate disposition to prefer being detected in a theft to repaying a loan

B2 Internationally respected U.Minn. astronomer Willem Luyten's 1987 *Autobiography* (collected, prefaced, & published by my late friend, the courageous scientist-explorer Rob't Lillestrand, with Anton LaBonte) notes at pp.115-8:

I can recall something like 20 occasions where another astronomer "discovered" a star (previously discovered by me) with some unusual properties and where he announced his "discovery" in an accredited scientific publication. In several cases these investigators had received support from the National Science Foundation. NSF took a particularly dim view of my critical pronouncements because they cast a shadow on the recipients of their research grants. In all of these cases I had published a description of these stellar objects many years earlier, so the issue of concurrence did not exist.

. . . the outright unwillingness of many scientists to give credit to an earlier discoverer even though [the discovery] is already published. . . . seems to border on deliberate intellectual dishonesty and is far more pervasive than most people believe. Also, this characteristic is not restricted to the lesser Achilles of astronomy.

I suspect that many of these professors are so accustomed to taking ideas from their graduate students and research assistants that they don't even regard this practice as dishonest.

. . . I have done . . . my best to stick to the truth. In some cases this has made life difficult for my colleagues, in other cases it has made life difficult for me, but in every case it has been basic to my life.

C Evaluating the Evaluators

C1 Who are the academic-businessmen-politicians that control Hist.sci journals and thereby assume god-like prerogatives both as censors of information flow and as arbiter-bestowers (upon the Less Fortunate) of the "prestige"¹¹ that is said to attach to publication in their incestuous forums? These gentlemen allegedly evaluate incoming manuscripts. But: who evaluates the evaluators? Are these editors and-or their referees capable in the very disciplines where they pretend to measure others? Even in high school math?

¹¹ On 1983/6/6, *JHA* #2 Editor O Gingerich urged me to accede to the sudden late attempt of the *JHA* Editor-for-Life, Lord Hoskin, to excise the sole, brief pro-R.Newton section of a paper long previously accepted (even advertised in the 1982 March *Isis*) — a section which included, e.g., the little-known information that Ptolemy's solar "observations" agreed 50 times better with his indoor tables than with the outdoor sky. OG explicitly recommended I tolerate Lord H's censorship (typical of that which has prevented *JHA* readers from knowing the truth of the Ptolemy situation) because publication in the eminent *JHA* would enhance my "prestige" in the field. For the record: the dirty business surrounding this affair (see fn 25 & ‡6 fn 15) is what led directly to the inception of *DIO*. I'm sure establishmentarians everywhere will be grateful to OG & Lord H for that achievement.

C2 Well, one of the rôles of *DIO* will be the investigation of precisely these matters, to which *DIO* will devote a regular supplement. I believe these little forays will enlighten, perhaps surprise, & certainly entertain *DIO*'s readers. The supplement will be called the *Journal for Hysterical Astronomy (J.HA)*.¹²

C3 This is the right place to state that DR is an apt publisher of scientific folly, since his own 1988 release of R.Peary's Betelgeux Document was inexcusably careless and stupidly overconfident.¹³ Especially for one who insists on high scholarly standards, in his own work above all.¹⁴

C4 Please note that criticisms & satires in *DIO*'s supplemental *J.HA* will be primarily aimed not at the small or the powerless, but at [1] the lordliest archons of academe (largely reviewing their Hist.sci effusions), and-or [2] he who tries to kiss these lords' brains, by attempting (safe in their captive journals) to bully-trash dissenters' creativity, though himself being not especially original or infallible.

C5 I have in mind most particularly the O.Neugebauer cult's ongoing war (examples:¹⁵ ‡3 §D; ‡6 fn 6) upon the discoveries of such civil, gentlemanly scholars as R.Billard, D.Dicks, A.Diller, W.Hartner, R.Newton (the bravest of all Greek astronomy analysts), & B.van der Waerden. The cohesiveness, vitriol, & accuracy-quotient of Neugebauer-clique slanders is such that I have taken to calling it The Muffia. (For samplings of truly epic Muffiosi struggles with the mysteries of elementary arithmetic, see DR's exposures in the *American Journal of Physics*: Rawlins 1987 nn.30 & 35. Previously suppressed by Hist.sci: ‡6 fn 4. See also Captain Captious' Muffia math at ‡5 fn 7.)

¹² A prime cause of the poor interdisciplinary communication discussed above has been numerous Hist.sci professionals' doubtless unbiassed conviction that mere scientists are ill equipped to contribute to the field. As we shall see, some among these superior Hist.sci folk can indeed be class entertainers when attempting, e.g., astronomical calculations. (See below: §C5.) However, *DIO* shall nonetheless appreciate their talented facets, as well as their occasional contributions to our knowledge. (E.g., ‡6 fn 35.) In defying the gods of the field, I have no wish to join them (in either power or omniscience). *DIO* is being launched to enhance knowledge, not the writer's political influence. Thus, at least initially, most *DIO* copy will be generated internally, except for occasional pieces by friends (as well as the dep'ts of Unpublished Letters & of Referees Refereed, both of which specifically seek others' input), whose appearance in *DIO* carries no implication of anything *but* friendship. I.e., *DIO* is operating just as numerous journals do, but is being upfront about it. Indeed, DR is not even calling himself "Editor" of the journal.

¹³ Regarding DR's original 1988 BetDoc error and his unqualified retraction (*Wash Post* 1989/2/16) just 2 weeks after evidence against it appeared: DR saw the experience as principally a test of character, and attempted (under an intense and frequently hostile spotlight) to set an example of rigorous integrity and severe self-censure. I was gratified that the scientific community responded by itself setting an admirable example, treating DR with fair criticism and balanced attention to the full range of evidences bearing on the Peary Controversy. (See *Science* 1989/3/3 & 1989/12/22, *Scientific American* 1990/3 & 1990/6.) The result has been a joint behavior-model which one hopes will henceforth encourage other temporarily mistaken scholars (regardless of the prominence & depth of their previous commitment) not to fear frank retraction when the weight of evidence turns out to be against them. (DR's openness so infuriated primo-Peary-apologist & National Geographic chief G.Grosvenor 2, that G2 has even publicly fanned the flattering rumor that DR's error was intentional. With enemies like Grosvenor, who needs friends?)

¹⁴ DR's restoration on this issue was accomplished by: [a] Total DR retraction (previous fn) of his egregious 1988 error. [b] DR's surprise announcement (1989/12/11) of the BetDoc's correct solution (1894/12/10 Betelgeux & Vega 3-wire transit data observed at 77°40'N), along with detailed demonstration of the impossibility of the elaborate "time-sight" solution published in NGS' 1989/2/1 pressrelease (also overconfidently promoted in *NatGeogMag* 1989/6), *unanimously* validated by NGS' experts. The NGS was mistaken on virtually every detail: observation-type, altitude-type, altitude-purpose, instrument, orientation, unnamed star, date, place. (The truth of DR's solution and the falsity of NGS' has been unqualifiedly certified by several expert astronomers.) [c] DR's release of numerous independent evidences, including some startling finds in the Peary Papers (US National Archives) showing that Peary's 1906 discoveries and 1909 N.Pole fable are riddled with contradictions and data-alterations that render these claims scientifically unacceptable. (See in this *DIO*: ‡4.) [d] Perhaps most important: the courageously open intercession (on the skeptical side of the Peary Controversy) of the famous astronomer Chas.Kowal at the very time the massive NGS' p.r.-blitz was trying to stampee the press into unquestioning acceptance of its inept hired consultants' 1989/12/11 verdict. [e] DR's photogrammetric demonstration (22-unknown least-squares fit) that Peary's 1909/4/6-7 position was about 100 mi (3 standard deviations) from his claimed N.Pole (Amer Astron Soc 1990/10/22 presentation).

¹⁵ As one may see from these quotes, the most frantic missman for the Muffia has been its Captain Captious: N.Swerdlow. Two decades of similar output have helped earn historian Swerdlow: [a] a prime seraphic place directly beneath the oscufied throne of O.Neugebauer, [b] a professorship in the U.Chicago Dep't of Astronomy & Astrophysics, [c] a MacArthur Foundation grant, & [d] a place on the board of no less than the *Journal for the History of Astronomy*.

C6 The most curious aspect of these violent attacks is that (unless they represent a conscious effort to save the faces of Muffia archons' precommitted reputations and/or to hog all power & grants in the field as the exclusive property of a restricted clan),¹⁶ they appear to be inspired by nothing more than *disagreement* over scholarly questions. Before observing the Muffia at work, I had mistakenly supposed that the idea that error was sinful had somewhat declined since the Dark Ages.¹⁷

C7 When describing those who doubt that Ptolemy observed outdoors (a class which has included some of the finest astronomers in history: §A5), Muffia-circle folk use such pleasantries as: "incompetent"* , "crank", "silly"* , "unreliable"* , "absurd"* , "disreputable"* , "insults the intelligence of the most naïve reader"* , "pipsqueak", "Velikovskian", "con-man", "crazy"* , and . . . well, you get the drift. Just the sort of terms rational & intellectually secure scholars use to describe persons with whom they merely happen to disagree.¹⁸ Oh, I forgot one other Muffia term applied to a skeptic: "abusive".¹⁹

C8 Are we dealing here with an absolutely precious unselfconsciousness — or with a sense of humor even more warped than my own?²⁰

C9 Note that this behavior must be just fine with — often useful to — certain archons of academe, since most of the abusive scholars quoted here (§C7) have advanced to prominence, while some among the polite opposite numbers mentioned above (§C5) have not done so well politically. In controversies embarrassing to entrenched institutions, baseless high-archon slander (e.g., §C7) is freely employed to defend orthodoxy. Note: [1] The perpetrators pretend to eschew such abuse, and seek to punish any who speak against their own

¹⁶ An academic clique's members can achieve prestige, regardless of scholarly ability, just by loyally promoting each other, counting on naïve onlookers never to sense the circularity of the proceedings. Auxiliary tactics: [a] Discredit and attempt to utterly destroy all competitors, as threats to inevitably finite fiscal resources. [b] Most observers cannot understand technical details well enough to tell who's right in a disagreement, so forget evidence and concentrate on *ad hominem* attacks. [c] A critical argument is without effect if its expounders are not heard. The natural issue of such approaches might be expected to resemble the pack snarls of the Muffia quoted at §C7. (General principle: a clique attempting to kill, starve, or isolate an intellectual opponent, betrays inward fear of that party's evidence.) It would be unfair and libellous to make comparisons here to the hyena, which is known for its intelligence, good spirits, & pleasant laughter.

¹⁷ W.Lucky *History of . . . Rationalism in Europe* 1865 Chap.4 (1873 NYC ed., 2:26-28, emph added): in the 4th & 5th centuries AD, "the pagans were deprived of offices in the State, . . . the entire worship condemned . . . [though their leaders] had exhibited a spirit of tolerance . . . [this in decent contrast to the orthodox's] doctrine of exclusive salvation, and the conceptions of the *guilt of error* and of ecclesiastical authority."

¹⁸ The published bouquets are starred. Sources: Toomer 1975 p.201, Gingerich 1984P (echoing Swerdlow ‡3 §D2), HamSwerdlow 1981, Toomer 1984 p.viii, *Balto Mag* 1989/7 p.80. The unpublished expressions are from private communications of 1976/3/9, 1978/11/30, 1979/2/7, & 1983/6/2 (‡3 §D). (Equating topflight physicist R.Newton with bigtop zany Velikovskiy is tophole something-else, even for the Muffia.)

¹⁹ O Gingerich 1983/8/26, referring to DR, whose prose is admittedly not *quite* so staid as that of Diller & RRN. But the catch with blaming Muffia rage on DR is that §C7-style Muffia treatment of dissent had been going on for about 7 years before DR entered the Ptolemy Controversy in late 1976. Indeed, Diller received a similar Neugebauer letter in 1934, reviling Diller 1934's seemingly unoffending discovery of Hipparchos' obliquity. ON's comments were published at Neugebauer 1975 p.734 n.14 and were soon proved to be as valid as they were polite (see ‡6 fn 21). (I have long tried, not always successfully, to apply abusive remarks only to my own mis-scholarship. Strong self-criticism encourages scrupulous investigation.)

²⁰ When first involved in the Ptolemy Controversy, I attempted amiably to encourage O Gingerich's feeble attempts to refute R.Newton, since Neugebauer's clones were pursuing a policy of noncitation. (They've never cited DR. Up to now.) This policy's reality was freely acknowledged by all parties. An earnest Muffian grad student joined me when I first met RRN (at his home 1976/3/29) but later told me NEVER to tell the Muffia about that heinous indiscretion. Neugebauer himself defended the freeze-out of R.Newton to me 1976/8/14, even alibiing his having attacked Velikovskiy (in *Isis*), but not R.Newton! I spoke openly of this shameful policy and continued trying to bring out the putative best in OG, but then learned to my amazement from a number of scholars that OG was, behind my back, slandering me by characterizing such common-knowledge (which OG privately shared) as symptomatic of paranoid insanity. For instance, OG wrote (1978/2/2, alibiing his rejection of an editor's invitation to debate DR) that an "exceedingly paranoid" DR has been "suggesting that a cabal has been suppressing the consideration of [R.]Newton's work" on Ptolemy. OG omitted to quote another cabal-inventing nut, who wrote (to DR 1976/9/15, commenting on Gingerich 1976; emph added): "So far the Neugebauer camp has not been heard from. *Perhaps* my merely mentioning [R.]Newton in a review of Neugebauer has placed me beyond speaking terms." The identity of this, my partner-in-paranoia? O Gingerich! Stand aside, Machiavelli. [More at DIO 4.3 ‡15.]

self-deified persons, even though [2] The unmighty's occasional reactive slanders are [a] not secret and [b] of featherweight concern compared to potentially lethal (often-clandestine & evidentially unsupported) institutionalized gossip-judgement that a given scholar is Impossible or Not-Reputable. (A famous & able now-deceased US Antarctic explorer was often falsely so vilified, which may explain his omission on the recent USPS polar stamps that included some lesser figures.) Such an evaluation, uncheckable in 2 senses, can spread like an invisible cancer, throughout the ill body of an institution detached from reality, triggering the customary self-fulfilling-prophecy action-reaction circles.²¹ (The mass of scholars fear power-operators' editorial or fiscal revenge, and so are cowed into tacit or even active assent to archons' misbehavior. Perhaps *DIO*'s independent voice will rekindle once-cherished ideals in at least a few among those ashamed of silent acquiescence in tyranny.)

C10 Similarly, Muffia public attacks exhibit all the legendary courage of the hit&run driver, since they are attempted exclusively in forums where reply is not believed to be possible, thanks to the protection of power-priority editors whom these attacks often serve. As noted (§A7), suggestions of face-to-face debate are routinely ignored. Likewise the case for the first *eighty two years* of the long Peary Controversy — now finally to be debated on 1991/4/19 at the US Naval Institute, Annapolis. (The 5 panelists: T.Davies, W.Herbert, W.Molett, R.Plaisted, & DR.) Note that no university has ever sponsored a debate on the Peary 1909 North Pole claim, which is now widely suspected of having been the most successful science fake of the century. (Debates' outcomes cannot be rigged so easily as captive, politician-edited journals' contents & reviews; thus, power operators abhor debates as wildcard threats to the Conventional-Wisdom fantasy worlds they prefer to promote in more controlled fashion.) And all academic forces that matter may be counted upon never to call anyone to account for this pathetically transparent record of behavior — meanwhile advertising academe to the public as an entity that thrives on intellectual openness.

C11 For contrast, we may note that [a] the AAAS in 1974 held an official session to debate Worlds-in-Collusion²² Velikovskiy (whom the AAAS regarded as a nut); [b] the Muffia's late expert cuneiformist A.Sachs debated Velikovskiy 1965/3/15 face-to-face at Brown Univ (though Sachs' admirer Toomer says Sachs wasn't sincerely debating but merely trying to make V look ridiculous); [c] debates pitting biologists against creationists are fairly common; [d] astrologer-vs-astronomer debates are just as routine (e.g., *Nightline* 1988/5/3, with the otherwise pre-occupied and thus ineffectual Dr.Squareza representing skepticism; see ‡8 §A6). For some scholarly groups, kicking mental cripples' crutches from under them is evidently preferable to dueling with forces intellectually capable of defending themselves on at least equal terms.

C12 Muffia tactics against Robert R. Newton & DR are worth comparing to the implications of some satirical articles that may occasionally adorn the *J.HA*. The Muffia's essential attitude is that RRN & DR are not *ever* right. (See fn 17 & §C7.) By contrast, the *J.HA* will merely show that Muffiosi are not *always* right. I recommend careful attention to this distinction. (Though, admittedly, I am not denying the tenuous possibility that the inverse of these statements is nearer the truth.)

C13 When any of his subjects opposed his high-handedness, Boss Tweed used to scoff: "What're you going to do about it?" It was a humorist (artist & cartoonist Thos.Nast) who answered the question. Some of modern academe's dispensers of patronage (grants, publication, review-treatment, review assignments, conference-invites) also operate by fear & intimidation. The predictable upshot has of course been classic Lord Acton. An

²¹ E.g., secretly calling someone paranoid (fn 20) indicates a remarkable insensitivity to irony, even aside from the ethics of the matter. DR happens not to be an ultimate casualty of such warfare since: [a] I've stayed pretty well informed regarding slanders about myself, and [b] I have primarily certain snakebit detractors to thank for handing me a legitimate measure of world fame (e.g., *NYTimes* front page 1989/12/12, editorial 12/15). But things do not always come out so well, and scholars who are simply trying to discern & proclaim the truth oughtn't to have to waste effort on such trivia, nor to tolerate strains which some cannot survive and shouldn't have to.

²² Credit: Ira Wallach.

unexpected but equally inevitable upshot is *DIO*.

D Unearthing the Unearthly

D1 The new periodical's name, *DIO*, is a merging of numerous themes. Dionysos was the god of fertility. Dio can mean twoness: apt for a journal attempting to fuse competent science and competent history into a progressively more accurate & just understanding of the precious period at the birth of science, when predictive intellect was first achieving and revelling in astoundingly correct & ingenious success. (These earthly raptures were first made possible by the inherently controlled & virtually frictionless mechanism of the heavens.) Dio Cassius was a valuable ancient historian. Diogenes sought an honest man. Bart van der Waerden's longtime nickname for me is Dionysios (Greek for Dennis).

D2 Also, both van der Waerden & DR believe that the 365^d 1/4 Dionysios calendar²³ was founded by ancient scientists who had unseated the Earth from the center of their universe (van der Waerden 1984-5 p.130).

D3 Future issues of *DIO* will unearth the precise orbital parameters of a vital, well established ancient heliocentric astronomy: admirably accurate, mathematically sophisticated, and improving over at least 2 centuries, between the epochs of Ptolemy II and Cleopatra (the end of Greek rule at Alexandria).²⁴ Pre-eminent among these heliocentrists was Aristarchos of Samos (c.280 BC), who defied the threat of prison or worse (as we are told at Plut *Mor* 923A) to broaden the vision of men infected with that intellectual narrowness & sterility which so often pairs with intolerant, ungenerous arrogance.

D4 Aristarchos' book, one of the most important in the history of human intellect, is lost. (Not even a textual excerpt survived classical antiquity.) But his orbital data and their empirical bases are, by amazing good fortune, largely reconstructable. And his central truth was inextinguishable. A publishing scholar could not wish for a finer model.

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²³ I propose that this calendar (epoch 285 BC) was probably named by Aristarchos (fl. 280 BC) for his elder contemporary, the courageous heretic Stoic & philosophical hedonist, Dionysios the Renegade (c.330-250 BC), [a] devotee of the poet Aratos (who authored the great contemporaneous astronomical poem, Φαινόμενα), [b] schismatic opponent of Cleanthes (who asked for Aristarchos' arrest), & [c] pupil of his fellow Heraclidian, the famous geomobilist Heracleides of Pontos (fl. c.360 BC; temporarily head of Plato's Academy). Which suggests that Dionysios was a link connecting Heracleides & Aristarchos, part of a precious heliocentrist chain that appears to go back at least to the time of Plato. The Dionysios connection suggests a philosophical bent in Aristarchos, of which we previously had no hint.

²⁴ It has long since become Conventional Wisdom to accept that the ancient Greeks were poor empirical scientists. (The belief that Ptolemy was a mainstream scientist of his era is an important founding father of that general misconception.) A consistent theme of *DIO* will be the undoing of this long un-re-examined blanket libel of ancient scientists. See also Rawlins 1982G, Rawlins 1987, & here at ‡5 fn 13.

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²⁵ Paper recovers several revealing, longlost ancient Greek & Babylonian values of the yearlength; ms accepted in entirety for publication by *JHA* Editor-for-Life (1981/9/17, and several times thereafter in response to pointed DR inquiries following repeated arbitrary deferrals of publication). Publicly stated to be forthcoming in 1982 (*JHA* ad: 1982/3 *Isis*), and material cited in Moesgaard 1983 (p.57). Failing in sudden late attempt to rush DR into agreeing to bowdlerization of paper (to Muffia specifications: fn 11), EFL then became furious & threatening in response to accurate 1983/2/9 criticism of EFL's disastrous over-riding of *JHA*'s own referees (for a non-DR paper published in *JHA*, later retracted & recomputed): ‡6 fn 15 & ‡8 §G6. EFL did not allege problems with Rawlins 1999's validity (already approved by both *JHA*-appointed referees, K.Moesgaard & W.Hartner). Nonetheless, *JHA* severed correspondence with DR 1983/3/21. Paper not in press. Will not be withdrawn.