

## ‡2 Correspondence: Reaction to *DIO* 1.1

### A Fantasies

**A1** Absolutely delighted with *DIO*. I've long said that what this backward nation needs is a good three-letter abbreviation for junk-mail. You have my warmest congratulations for finally supplying it. [Signed] The Malignant 1

**A2** I lost no time reading your magazine. [Signed] DMcC.<sup>1</sup>

**A3** *DIO* is inimitable. I simply cannot put it down. Having not chosen to pick it up. Ahead of the garbage man. [Signed] N.Coward

**A4** You are a genius. I love you & long to bear your child. [Signed] Spike

**A5** *DIO* makes the libellous implication that I am a litigious dictator, which is a damned lie. Retract or I shall sue. [Signed] Lord Thinskin, Deity-for-Life.

**A6** Kill it before it metastasizes. [Signed] The BrownU Crew.

### B Realities

**B1** OK, now to subside from dreams-of-gory to: the real letters. Before dipping into that barrel, though, I must note that numerous unwritten verbal reactions were received. Several professional sociologists expressed gratitude for the raw data that *DIO* is placing onto the public record. And, while a number of readers said the footnotes' print was eyestrainingly tiny, at least one *DIO* subscriber reads *mainly* the footnotes. (See fn 10.)

**B2** *DIO* 1.1 drew encouraging missives from so many quarters that not all can be included here. Some are too kind to quote, such as the letter from my Gilman School physics teacher Bill Porter (who, at the instigation of DR's lifelong friend & advisor Ludlow Baldwin, introduced me to the wonders of calculus<sup>2</sup> & Newton's Laws, when I was only 16<sup>y</sup> old). But I will always treasure them privately.

### C Honesty & Belief

To: *DIO* 1991/5/21

From: Louisa Fitzgerald Huber, Harvard University

**C1** . . . glad to find someone who among other things makes a case against stupid or downright damagingly dishonest reviews. . . .

**C2** It's funny: the people who at the bottom of it all don't believe in much of anything are the ones who care the most for honesty and accuracy.

### D Alkaline Reaction

To: *DIO* 1991/5/14

From: Kristian Peder Moesgaard, University of Aarhus, Denmark

**D1** I hope that soon you may even succeed in diluting the acid component of the ink<sup>3</sup> and thus keep the innocent paper free from contamination.

<sup>1</sup> This was a favorite response of poet David McCord (Harvard Alumni Fund), longtime genial boss of my dear mother-in-law, Sylvia Reynolds.

<sup>2</sup> Now living within easy driving distance of BrownU, Bill is available for tutoring. Muffios take grateful note.

<sup>3</sup> A delightfully artful reference to *DIO*'s back-inside-cover statement.

**D2** [Your] Hipparchos paper [*DIO* 1.1 ‡6] clearly falls within my field of interest and competence. It reveals an important new stage-setting for the interplay between Babylonian and Greek-Hellenistic astronomy.

**D3** . . . I find a lack of [consistency] in using [ $Y_J$ ] . . . for the [UH solar] mean motion instead of [ $Y_B$ ] . . . Also the [Meton-Hipparchos solstice-pair] already includes epoch-longitudes, so why speculate further on this as [at . . . §D7f]? . . . the argument for the reconstruction of the three Hipparchian positions of the Sun would gain by employing consistent elements, even if the results should miss the mark by a minute of arc.

**D4** My central question is concerned with the interpretation of the year-length  $Y_B$ , tropical or Metonic, i.e., 235/19 times synodic month? . . . [DR] takes it to be tropical and accordingly “the best of a rather poor lot of surviving ancient estimates” [§B8]. But interpreted as Metonic it is clearly inferior to 365;14,48<sup>d</sup> [ $Y_J$ ] which the author (therefore?) brings into play for the demonstration of the success of the UH orbit in imitating eclipse events.

**D5** *DIO* believes no extended responses are necessary, for those who have read the original DR paper with care. Three brief comments: [a] The on-the-nose agreement displayed at §H5 of the DR paper shows that the proposed UH mean motion indeed matches Hipparchos’ final mean motion tables to 1′. [b] Though  $Y_B$  (eqs. 1&4) revealed [to *DIO*] Hipparchos’ raw recorded time for the 135 BC SSolst, it is obvious from *Almajest* 3.1 that Hipparchos’ contemporaneous solar tables instead<sup>4</sup> used yearlength  $Y_J$  (DR eq. 7). [c] The same Babylonian tablet ACT 210 contains both  $Y_B$  and  $M_A$  (*ibid* eq. 10), and these 2 numbers are obviously not in the ratio 235:19 — though  $Y_J/M_A$  does very closely approximate 235/19 (as Moesgaard has often justly emphasized, contra R.Newton). Therefore, we know that  $Y_B$  was (wisely & boldly) not founded in any way upon lunar data or considerations. (I.e., the tropical year was not falsely identified with 235/19 months — Meton’s 432 BC scheme, subsequently followed in some degree by Kallippos, Aristarchos, Hipparchos, & Ptolemy.) Which is precisely *why*  $Y_B$  was the most accurate tropical year we have inherited from antiquity.

**D6** I warmly commend the broadmindedness of Moesgaard (an internationally respected expert in this area) in acknowledging the essential validity & watershed import of the DR paper. [Note added 1993: Unfortunately, Moesgaard has failed to put this encouragement into public effect. See *J.HA* 1.2 fn 170.]

## E Ringside Chuckles

To: *DIO* 1991/10/13  
From: David Fowler, Math Inst, Univ Warwick, Coventry CV4 7AL, UK

**E1** Many thanks for *DIO* & *J.HA* . . . I’ve wondered how to thank you . . . am enclosing one of my last offprints (for they were fabulously expensive) of a recent obscure article [*Archive Hist Exact Sci* 41.3:189 (1991)] on continued fractions — a topic close to your heart . . . Also a little preprint [“Unit Fractions Again”] in which I loose off a few strident opinions. Had I enjoyed your journal less, I might have sent more reprints. (The old chestnut: “First prize: a week at Blackpool. Second prize: two weeks.”)

**E2** I work outside most Muffias, cliques, schools, and the like —

**E3** So I stand way outside most of the issues that you are dealing with, and treat these arguments and fights as high class entertainment.

<sup>4</sup> Moesgaard’s helpful observation is not mere carping. He & I are aware that the UH orbit fits the data regardless of whether  $Y_J$  or  $Y_B$  is adopted.

## F Penguins Under the Ecliptic Pole

To: *DIO* 1991/9/12  
From: Keith Pickering, 10085 Country Rd. 24, Watertown, MN 55388

**F1** . . . I was struck by your remark<sup>5</sup> that Aldebaran and Antares “can never be seen simultaneously from the Mediterranean area”. . . This led me to investigate the conditions under which Aldebaran & Antares are above the horizon at the same time.

**F2** The [great circle] angular separation of the 2 stars is 169°.96, just over 10° from complete opposition. This implies that it would be possible for each star to be a comfortable 5° above the horizon simultaneously, provided that the median point of the 2 stars is at the zenith. . . The median point turns out to be (epoch 2000.0) . . . less than 0°.5 from the south pole of the ecliptic, which makes perfect sense when you realize that the 2 stars have opposite ecliptic longitudes and each is at about −5° ecliptic latitude.

**F3** But now we are faced with a problem: if we stand with the median point overhead (near the Antarctic Circle), . . . the [solar center on the ecliptic] can be no more than 0°.5 below the horizon. Correcting for refraction, this means that at least half of the solar disk would be visible at all times of the year for any observer who has the median point of the 2 stars at his zenith.

**F4** The way to save the situation is to rotate our horizon frame around the Aldebaran-Antares axis. Or, more precisely, around the axis joining [the two points on the ecliptic at the 2 stars’ longitudes]. Each star will be slightly “above” this axis for our hypothetical southern hemisphere observer. The rotation will tilt the lowest point of the ecliptic well below the horizon, while lowering the altitudes of the 2 stars only slightly. The tilt angle must be at least 6° (which would put the sky at the end of civil twilight at the right time of year) . . . This tilting creates what we might call a [fairly narrow] “line [locus] of zeniths” perpendicular to the Aldebaran-median-Antares line, under which we must stand to see both stars simultaneously.

**F5** Projecting this line on the equatorial-coordinate sphere, we find that it passes through its lowest declination at −68° 1/2 . . . This implies that Aldebaran and Antares are both visible above the horizon on at least one day a year for all observers in the southern temperate zone, but observers travelling north of the Tropic of Capricorn will find it increasingly difficult to see both simultaneously. If we extend the tilt all the way to 90°, the line of zeniths reaches declination 8°, which latitude<sup>6</sup> may be regarded as the absolute northern limit from which the simultaneous observation can be made [ignoring refraction]. Observations from the Antarctic [south of c.74°S] are precluded by Aldebaran’s northerly declination.

**F6** Now here’s a tougher test: is it ever possible to observe Aldebaran, Spica, Antares, & Deneb Algedi simultaneously above the horizon? These four stars all lie just south of the ecliptic in four different quadrants of the sky. Putting the south ecliptic pole near your zenith would allow all four to be above the horizon, but only during daytime. The horizon tilting strategy used above will not work, since at least one of the four will be rotated below the horizon along with [half] the ecliptic. The 4-star simultaneous observation is therefore impossible.

<sup>5</sup> This question arose because *DIO* 1.1 (‡ 6 fn 30) had noted that the longitudes of Aldebaran & Antares differed by exactly 180° (within 1′) from c.300 BC to 1200 AD — and that the Ancient Star Catalog (whose longitudes are generally expressed to precision 10′) placed these 2 bright stars exactly 180° apart. Neugebauer 1975 p.960 notes that this same stellar opposition was stressed by Kleomedes (4th century) & Rhetorios (6th century).

<sup>6</sup> A crude check is obtained from  $\arctan(\tan \epsilon \cos \lambda) = 8^\circ 1/2$ . For about 300 BC, the same calculation would find that the limit was nearer to the Tropic of Cancer than to 8°N latitude.

**F7** There is, however, one “miraculous” exception to this: for an observer at the correct latitude (the Antarctic Circle), and at the correct Local Sidereal Time (6<sup>h</sup>), the south ecliptic pole will be overhead and the [refracted] sun will be just above the horizon. If at the same time there were a total eclipse of the sun, all four stars could be visible simultaneously.

**F8** The enterprising photographer who uses an all-sky lens to capture Aldebaran, Spica, Antares, Deneb Algedi, the eclipsed solar disk, and up to 5 bright planets, all ringing the horizon in a single frame, would certainly qualify for the Astrophoto of the Millennium Award. Such a photo would best be taken from a high-flying aircraft, because of: [1] lack of suitable land along the Antarctic Circle; [2] generally bad weather in the region; [3] dip of the horizon due to altitude; [4] reduced atmospheric extinction.<sup>7</sup>

**F9** Questions for *DIO* readers with eclipse-predicting software: When and where is the next time a path of totality will cross the Antarctic Circle at 06:00 LST? And when was the last time?

**F10** P.S. I keep seeing references to “DR” in your magazine. Does this stand for “Dennis the Renegade”?<sup>8</sup> . . . .

Comments by D-the-R:

**F11** I enjoy the footnote-to-a-footnote aspect of the foregoing. (Various DR research finds have started out as such.) So the following thoughts are 3<sup>rd</sup> order footnoting.

**F12** The scene painted by Keith reminds me of one of my old favorite astrologer-hassling ploys: ask a horoscope-caster to do a birth for the Arctic Circle at Local Sidereal Time (LST) 18<sup>h</sup> or the Antarctic Circle at LST 6<sup>h</sup>. (Any longitude will suffice.) Slight unstated problem: in either situation, the ecliptic is coincident with the horizon, so the astrologer’s critical “Ascendant” point becomes nonexistent and thus uncalculable.

**F13** Forgetting relatively dim Deneb Algedi (the Seagoat’s tail), we note that all the 1<sup>st</sup> magnitude zodiac stars’ longitudes fall in one precise semicircle: Aldebaran, Castor, Pollux, Regulus, Spica, Antares. When the Sun is well into the opposite zodiacal semicircle, there will be at least a few minutes every night when all 6 of these famous stars are visible simultaneously in the southern hemisphere’s temperate zones, something that was never possible for the northern hemisphere ancient astronomers who first cataloged these objects.

**F14** I note that the current celestial latitude of Pollux is +6°.7 (and slowly increasing); this is well outside the Moon’s usual (5° amplitude) range of wander off the ecliptic. In ancient lunar-planetary observational work, Pollux was traditionally (though see *Almagest* 9.7) not regarded as being quite of the import of Aldebaran, Regulus, Spica, & Antares, the famous 4 stars which were occultable by the Moon. However, a lunar occultation of Pollux is almost possible from the deep southern part of the Earth, since the Moon’s latitude can be as great as +5°.3, and the gap of 1°.4 (between this figure and Pollux’s latitude) is nearly covered by 1°.3, the maximum possible sum of the Moon’s horizontal parallax & semidiameter. But not quite. So such an occultation is now impossible to see from the Earth’s surface.) However, in ancient times, the latitude of Pollux was less. (It was +6°.4 in King Tut’s time.)<sup>9</sup> So occultations of it were occasionally visible from the Antarctic regions. The only catch is that no literate persons were there in those days to see the event. (Yahgans & penguins may have been luckier.) By the time civilized man had penetrated to the deep southern reaches of the Earth, the latitude of Pollux had so increased that the shadow (of Pollux’s light), cast by the Moon, grazed the Earth no more.

<sup>7</sup> Deneb Algedi would be dimmer than 6th magnitude for a sea-level observer under the S. ecliptic pole.

<sup>8</sup> See *DIO* 1.1 ‡1 fn 23.

<sup>9</sup> See p.146 of the neatly arranged (also highly accurate & competent) tables of K.Moesgaard & L.Kristensen *Centaurus* 20.2:159. Pollux’s secular increase of latitude since ancient times has been largely due to the decrease of the Earth’s obliquity in the interim.

## G Footnote-Bifocalling

To: *DIO*

1991/5/8

From: Christopher Walker, British Museum, Dep’t W.Asian Antiquities

**G1** Your kindly sending me a copy of the first issue of such an interesting journal as *DIO* & *J.HA* deserves an extended letter in reply, since you touch on many matters of interest to me; having recently invested in a pair of bifocal spectacles to read cuneiform inscriptions, I may even be able to cope with the footnotes. . . .<sup>10</sup>

**G2** I am currently working a little on the archival evidence for the adoption of the Metonic cycle in Babylonia — but even getting reliable data on that one point takes a lot of checking.

**G3** Considering that Walker collaborates with Muffia scholars from time to time, I especially appreciate the good spirit of his letter.

## H The Looking Glass

**H1** The following multiply-enlightening exchange occurred between *DIO* and MIT statistician Peter Huber. Huber’s humor & high intelligence make his letters a good read, quite aside from the Ptolemy context. But the correspondence also usefully typifies the Through-the-Looking-Glass quality of Muffiosi effusions regarding Ptolemy’s behavior. And it suggests an analogy to embryonic growth’s rapid replication of evolution: in a few letters, we see Huber go through the same shifts & alibis which it had originally required years of Muffia ingenuity & sinuosity to refine into a bulwark-fence against admitting that an alleged scientific Immortal was dishonest. (Evaporating Myth One: Ptolemy was not a scientist at all. He was perhaps<sup>11</sup> a skilled mathematician. But he was, first&hindmost, an astrologer.)

**H2** Huber is the sole scholar who wrote *DIO* to defend the otherwise silent Muffia (who have been publishing Huber’s ancient-astronomy work, e.g., in the recent Abe Sachs memorial volume). We recall that Muffia don Otto Neugebauer calls Ptolemy “The Greatest Astronomer of Antiquity”. Huber’s good friend O.Gingerich on the other hand calls Ptolemy “The Greatest Astronomer of Antiquity” (‡3 fn 28). Ironically, as we will see below, Huber inadvertently — while attempting to defend both these promoters’ logic — demotes Ptolemy (about correctly)<sup>12</sup> to the level of a mere textbook writer. Huber condemns R.Newton for

<sup>10</sup> Another irresistible reaction to *DIO*: sociologist Scott Halupka (my Johns Hopkins softball-partner-in-ringerdom) says he skims everything *but* the footnotes. He finds the academic fighting the best part of the show. Suspecting that other *DIO* 1.1 readers also got overmuch into our footnotes (much as seasoned *MAD* magazine readers get addicted to panel-backgrounds), DR will attempt to move some of the same sort of weird stuff into the main text in this & future issues. Besides integrity, fun, & novel discovery, I intend that the only predictable aspect of *DIO* will be: unpredictability. E.g., given the long timegap between past *DIO* issues, Muffiosi seem to have hoped that no response is necessary since *DIO* & *J.HA* have perhaps died. Several typically-subtle high-level Hist.sci probes have revealed a lot more Hist.sci interest in this question than in citing the discoveries revealed in *DIO*. Standard. Pathetically standard.

<sup>11</sup> One interpretation of the Ptolemy scandal might be: the *Almagest* is what happened when mathematical ability overwhelmed scientific ability. (For a different slant on this question, see P.Huber’s thoughts at §H22.) By this statement, I am not at all subscribing to the amusingly ubiquitous modern myth (e.g., Neugebauer 1975 pp.108, 367, 643, 667, 938) that mainstream Greek scientists were non-empirical. (See *DIO* 1.1 ‡1 fn 24 & *DIO* 1.2 §E3 and the highly recommended analysis of R.Newton *Crime of Claudius Ptolemy* Johns Hopkins Univ 1977 pp.350-354.) There are some gorgeous mathematics in the *Almagest*. But, since Ptolemy is a multiply-convicted plagiarist, it is hard to know how responsible he is for this. Similarly: much of the astronomy of the *Almagest* is of a higher level than what remains of, say, Hipparchos’ work. But the same caution is recommended: we have no firm evidence that the *Almagest*’s astronomy is Ptolemy’s personal contribution — while there is plenty of indication that large parts of it are due to others.

<sup>12</sup> The long-standard edition of the *Almagest*, that of K. Manitius, calls it a mere *Handbuch*. (Manitius’ is still the preferable edition of the *Almagest* in some respects, e.g., sound judgement.) See DR *Queen’s Quarterly* 1984 p.973.

trivia, while excusing Ptolemy for every kind of cheating — and Huber is “neutrally” (§H21) undisturbed by Muffia suppression of discourse. The implicit double-standard is evidently so unconscious that Huber probably won’t be aware of it — until [a] seeing it dissected in these *DIO* pages, or [b] looking (at his responses) in a mirror.

**H3** It is false & mass-defamatory to suggest (as at §H13, below) that ancients approved of plagiarism more readily than moderns. I have elsewhere<sup>13</sup> quoted Pliny’s blunt opinion (modern in 77 AD, anyway) that plagiarism is theft. (See also Synesios: *J.HA 1.2* fn 154.)

To: *DIO* 1991/8/12  
From: P. Huber, Dep’t Math, M.I.T., Cambridge, MA 02139

**H4** Bill Tuman has shown me a copy of *DIO* [1.1] and I would appreciate getting a copy of my own.

**H5** Incidentally, while I deplore the mud-slinging between Swerdlow and RR Newton, I believe RRN is wrong on several points. First, his statistical analysis of medieval eclipses is affected by a hidden selection bias bad enough that his conclusions cannot be trusted. When I tried to confront him on that issue at a meeting, I believe in the early 1970s, he shut up and refused to discuss as soon as he realized that I was a professional statistician. How should one interpret such a behavior? I then felt it would be a waste of time to read his works!

**H6** Second, it seems to me that RRN approaches Ptolemy with a thoroughly post-Gauss frame of mind. Ptolemy did not have least squares! . . . I know that it was accepted practice among Babylonian observers to report observations not as they were, but as they should have been under good weather conditions, sometimes (but probably not always) with a footnote stating that the event had not actually been observed.

To: P.Huber, MIT, 2-334, Cambr, MA 02139-4307 1991/8/23  
From: DR

**H7** You may not realize that I have no opinion on R.Newton’s eclipse-discrimination, having long ago opted for a method (of determining mean Earth-spin-deceleration) which quite side-steps the need for selecting among the motley ancient eclipse reports. (See *DIO 1.1* ¶5 n.11 & ¶6 n.5.) Nonetheless, I will be glad to receive your evidence that RRN’s work in this area is infected by a “hidden selection bias”. I know that our mutual friend van der Waerden also rejects RRN’s eclipse sampling, but this never prevented him from appreciating RRN’s considerable contributions to the scientific history of ancient astronomy.

**H8** You speak of mud-slinging “between” Noel Seraph [Swerdlow] & RRN, implying shared responsibility. The record is sufficiently clear on the point that you may wish to reconsider this sentence’s expression. (Seraph has attacked in repulsive fashion even so gentle and esteemed a scholar as van der Waerden. See *DIO 1.1* ¶6 n.6 and the 1973 *Isis 64*:239 Seraph review there discussed.)

**H9** The enclosed offprint (*American Journal of Physics 55.3*:235; 1987) reveals a wholly new, crucial-experiment argument<sup>14</sup> showing that Ptolemy already possessed the *Almajest* mean motion of Mercury *before* he fabricated (obviously using that very mean motion!) the 139/5/17 “observation” which

he claims<sup>15</sup> this mean motion was computationally based upon. See item [5] (pp.236-237). (Note also item 4!) Curtis Wilson, van der Waerden,<sup>16</sup> & others regard this as an important contribution to the Ptolemy Controversy. (Your friends in the Neugebauer clique, including its satellite O Gingerich, have not deigned to take note of it.)<sup>17</sup> I look forward to your comments [upon this & the enclosed *DIO 1.1*].

To: *DIO* 1991/9/6  
From: P. Huber, Dep’t Math, M.I.T., Cambridge, MA 02139

**H10** . . . About **data faking**. Consider the following:

**H11** At present, most statistics<sup>18</sup> texts use fabricated or fudged data, thinly disguised as real data . . . . The usual reason given is that the focus is on methodology, not on the subject matter, and that it is much easier to teach the methodology by using “clean”, small data sets, with distracting details<sup>19</sup> stripped away. . . . I myself do not like fudging and think one should use it only as a last resort, if one cannot find . . . real data sets to make the point without adulteration.

**H12** It is standard practice today in the physical sciences not to report raw observational data. . . .

**H13** Customs can vary widely. Compare for example our current attitudes with regard to copyright and plagiarism to those prevailing among medieval authors and, more close to our days, among Singers of Tales . . . .

**H14** I do not contest that Ptolemy fudged his observational data [DR’s §H9]. I suspect that the following two forces were acting: first, as above in [§H11], his emphasis is on methodology: to explain his theories and so show how to derive parameters from the data. He did so very well and comprehensively . . . . If you illustrate the method of deriving parameters with a data example, you either have to fudge the result . . . or to fudge the observational data. Doing either explicitly detracts from the argument. Please note that it took Gauss and the Method of Least Squares to give a rational basis to dealing with *slightly*<sup>20</sup> [emph added] inconsistent observations. If Ptolemy had seen anything wrong in what he was doing, I guess he would have added slight perturbations<sup>21</sup> to his fake data (as present-day students do in lab courses. . .). Second, I surmise that Ptolemy reported the observations not as they were,

<sup>15</sup> [Fn in orig letter:] *Almajest* 9.10; Toomer’s 1984 *Almajest* p.467. (Toomer n.104 says a discrepancy of ordmag 10’ isn’t negligible, oblivious to Ptolemy’s 5° blunder for the same four-century interval!) See also the prejudice-inspired, undeniably-false Neugebauer-gang claims (regarding *Almajest* planetary mean motions) listed at n.30 of the enclosed *AJP* offprint. I suggest that you [Huber] request from O Gingerich a xerox of my 1980/4/13 letter to him (discussed in *DIO 1.1* ¶1 n.9), which revealed for the 1st time the math behind all five planets’ *Almajest* mean motions. [Details of this math are given here at ¶3 §C3. Text of original letter: ¶3 §C6.]

<sup>16</sup> [Fn in orig letter:] See *DIO 1.1* ¶6 n.37.

<sup>17</sup> Swerdlow 1989 (*JHA 20*:29) makes no mention of this new argument, though its truth is obvious, and it is *highly* relevant to the subject of Swerdlow 1989.

<sup>18</sup> Huber’s missive adds: “RRN has fallen prey to the old Emperor-of-China paradox (I believe it is due to the physicist Kapteyn): You are supposed to estimate the height of the emperor. Unfortunately, he lives in the Forbidden City, and the few people who see him won’t talk to you. But there are 600 million Chinese. So you simply go and ask every one of them what he or she thinks the height is. Then you average and get a fantastically accurate estimate (with a standard error of the order of 0.001 mm!).” Reminds one of the wisdom & trustworthiness of [a] ESP research results (see pp.79-80 of D.Rawlins *Skeptical Inquirer 2.1*:62-83; 1977), and [b] US democracy’s elective choices.

<sup>19</sup> This is less than relevant to Ptolemy, who piled on such distracting details as: [a] descriptions of instruments, and [b] repeated assurances that he had personally used these instruments to make the dozens of alleged outdoor observations we now know were actually faked indoors. See discussion at R.Newton 1977 pp.350f.

<sup>20</sup> Slightly?! See R.Newton 1977 p.188.

<sup>21</sup> Ptolemy *would have* added perturbations to hide his sins? In fact, he *did* precisely that. See ¶4 fn 19 for this and a later historical example of a similar ploy.

<sup>13</sup> E.g., Rawlins 1982C and *DIO 1.1* ¶1 §B1; Huber had just written to thank *DIO* for receiving the latter.

<sup>14</sup> [Fn in orig letter:] Previous accusations that Ptolemy had computed his “observations” from his tables had to be defended in statistical fashion. But the *AJP* argument’s unexpected confirmation of said charges: [a] is entirely independent of prior skeptical findings, and [b] is not statistical.

but as they SHOULD HAVE BEEN: you calculate predictions, and if your admittedly imprecise observations agree within reasonable bounds, you stick with the theory. [DR's added caps. Muffia's most-avoided question: from whom came the theory by which Ptolemy pre-calculated what he "SHOULD" observe?! — i.e., what was the original empirical base for the theory which gave the "right" answers?] Especially so if you do not trust your own talents as an observer! All the available evidence seems to indicate that Ptolemy was not a first rate observer, so his course of action may have been the prudent one. The same argument applies to the fixed star catalog. — As I mentioned in my previous letter, I have some antique evidence from Babylonian sources for the second part. For example, it is well known that the dates of solstices and equinoxes always agree with the known computational scheme (which was not very accurate), but they are reported just like observations. [DR: reported with Ptolemy-style discussions of the instruments allegedly used?! Hardly.]

**H15** I think RRN misjudges the cultural context. His claims of fraud presuppose malicious intent, and I fail to see any basis for that. [DR: Note that in the previous paragraph, Huber himself refers to Ptolemy's too-faithfully-computed "fake data". So "fake" data don't imply malicious intent, while "fraudulent" data do?? And, no less an authority than the Editor-for-Life of the *Journal for the History of Astronomy* says that a scholar who uses such terms as "fabrication" places "a barrier" between himself and "the writing of history" (¶3 §B2). What a show . . .]

**H16** . . . **Mud slinging.** I stand corrected: RRN slung at Ptolemy, and NS [Swerdlow] slung back at RRN. . . .

**H17** I was especially intrigued at this last piece of implicit logic, which is: no matter how strong the evidence for fraud by historical figure *P*, if scholar *N* mentions it, then scholar *S* is (according to Huber: §H16) no worse (than *N*) if *S* then accuses scholar *N* of fraud — *no matter how nonexistent the evidence for the latter allegation*. Another lesson in Muffia Morality. (No wonder fraud is so common in science — if those who object to it must endure Muffia-style-vengeance.) DR understandably kept his next letter brief:

To: P.Huber, MIT, 2-334, Cambr, MA 02139-4307 1991/9/23  
From: DR

**H18** Thanks for yours of the 6<sup>th</sup>.

**H19** I have a question: are you sympathetic to the idea of promptly scheduling a 2-sided symposium on the Ptolemy Controversy? — to be held before an audience of professional astronomers, the panel comprising: myself & preferably (but not necessarily) another skeptic or two, plus yourself (if you like), O Gingerich (in case he's willing), presumably the cynosurae Swerdlow and-or Toomer, backed up by as many other leading Ptolemy-apologist scholars as the Neugebauer cult is willing to exhibit for live crossexamination before competent scientists.

**H20** The obvious expectation that the proposed panel's composition will give Ptolemy's defenders a strong advantage (at least numerically) creates no deterrent on my side.

To: DIO 1991/10/1  
From: P. Huber, Dep't Math, M.I.T., Cambridge, MA 02139

**H21** . . . I am neutral with regard to your Ptolemy symposium. In 1974, I participated in a Velikovsky panel. . . . neither side in any emotionally loaded controversy will ever bother to look at the evidence. The scientists

were as bad as the Velikovsky fans.<sup>22</sup> Thus, a Ptolemy symposium might be fun, but I am not at all sure whether it would achieve any useful purpose. . . . At present, Toomer is **the** expert on Ptolemy, and he simply must be on the panel, whether willingly or by coercion, if there is a symposium.

**H22** . . . . I am pretty sure that the ancient astronomers . . . must have derived their parameters by trial and error from rather inadequate sets of observations. But I am equally sure that for a self-respecting Greek scientist the only legitimate scientific paradigm was the deductive one. This adds still another facet to the Ptolemy controversy: how to publish results found by unsavory methods!

**H23** I must add that I happen to be sensitized to such issues, because back in the 1960's I had made myself a name in statistics by legitimizing the field of robust statistics in terms of the then prevalent paradigms. That is, I had been able to invent and solve a mathematically rigorous optimality problem in a murky area shunned by most academic statisticians, and I had done so in full awareness of the fact that the mathematical rigor I was injecting was nothing more than window-dressing, needed to make robust methods acceptable.

DR has several reactions to the foregoing:

**H24** If we defend plagiarism by pointing to other instances, this is effectively just *broadening the indictment* of the academic community. Curious line of defense.

**H25** Huber begins his rejoinder to *DIO* by attacking RN for being a bad statistician (§H5). Yet, when it transpires (§H9) that the case that Ptolemy faked & plagiarized is airtight anyway, Huber does not cede confirmation to RN or DR — but instead increasingly shifts (§H11-§H15) to contending that these are not such grave sins!

**H26** Huber's comparison (§H23) of his 1960s behavior to Ptolemy's is irrelevant. (Was I. Newton a fraud for dressing up calculus-attained results in geometric garb? Come now.)

**H27** Huber condemns a single *verbal* nonresponse. Hell, I've been dealing with 15<sup>y</sup> of repeated nonresponse to written (e.g., 1984/4/30 to O Gingerich), even published (Rawlins *Amer J Physics* 1987) suggestions of the very item (face-to-face discussion at a scientific gathering) which Huber damns RN for allegedly evading on one solitary occasion. (Further details: fn 24.) The Muffia's long & consistent evasion Huber "neutrally" ignores — arguing that encounters wouldn't do any good. (Why is Huber not excusing RN's alleged longago silent encounter by supposing that his responding to Huber wouldn't do any good?) Hmmm. Huber refused to read RN's work after their noncommunication. So, will Muffia avoidance of debate cause Huber to cease reading Muffia output? . . .

**H28** Huber's long letter of 1991/9/6 manages (during pages of opinions, guesses, generalities, and analogies) to avoid<sup>23</sup> responding to all the parts of DR's 1991/8/23 letter which show his apologia for Ptolemy & the Muffia to be inadequate, e.g., [a] Noel C. Seraph's vicious, unprovoked attack on gentle BvdW (brought to Huber's attention at §H8), and [b] O.Gingerich's laborious, persistent, devious attempts to deny credit to DR for finding the numbers behind the *Almajest* planet mean motions. (See fn 25; ¶3 §C3 & §C15; Rawlins *Amer J Physics* 1987/3 n.30; *DIO 1.1* ¶1 fn 9, *DIO 1.2* fn 56.)

**H29** From Huber's 1991/9/6 letter we learn that: [a] Nothing at all reprehensible has been done by Ptolemy (who Huber says MERELY deceived his readers and faked data) or by the Neugebauer Muffia (whose sole possible indiscretion is Swerdlow's regrettable-

<sup>22</sup> DR asks 2 questions: [a] Where has DR failed to listen to the other side? (Muffia works are cited almost nonstop in relevant *DIO* output — admiringly when appropriate. Indeed, DR has been converted by occasional able Muffia findings: see, e.g., *DIO 1.1* ¶6 fn 35. Meanwhile the Muffia refuses to cite *DIO* at all. Why is Huber equating these 2 behavior patterns?) [b] Is Huber seriously claiming that scientists are as impervious to evidence as the Velikovskians? Such a judgement is more extreme than anything DR has ever said or written.

<sup>23</sup> Huber's letter is responsive & specific regarding the attacks on RN's use of eclipses. (I am happy to note that Huber's adopted Earth spin-acceleration is almost identical to that independently deduced at *DIO 1.1* ¶fn 5.)

but-of-course-purely-defensive countermudslinging). [b] No, it is R.Newton who is to be damned; he is unhistorical & evasive, and it would be a waste of time to read his works.<sup>24</sup>

**H30** All of this will surely seem perfectly reasonable to any gathering of competent scientists. (I can't imagine that anyone could for a moment imagine that there are double standards operating here, or that such hypothetical bias could have anything to do with Huber's friendship with O.Gingerich.)<sup>25</sup> Therefore, I urge that a Ptolemy symposium be held (as proposed at §H19), to ensure that RN's (& DR's) vile position be given its thoroughly deserved evidential quietus. If the Muffia continues to remain silent to this proposal, I can only quote the 1991/8/12 words of someone Huber admires (§H5: appraising an alleged RN evasion of confrontation): "How should one interpret such a behavior?"

**H31** Stepping back from trees & twigs here, to discern the forest: we may, by analogy to the foregoing, envision the path down which Hist.sci ethics promise to take academe. During an exam at a large university, a hypothetical jock freshman copies answers from an adjacent honor student. The professor, insufficiently enlightened by Hist.sci morality, reports the incident. The jock-cheater responds by wisely hiring Muffia legal talent and submits, to the President of the university, a beautifully typed letter, from which we excerpt the following wisdom:

**H32** . . . Our firm [Otto, Zero, Seraph, Tumor, Hooberdamn, & Cloneez] has drafted the following statement and transmitted it to the accusing professor:

Given that the said Mr.Jock had, previous to this test, never heard of a certain Kraut-4-eyes named Karl Gauss, and given that the said Mr.Jock is accused merely of copying all of Mr.Honor Student's answers faithfully and without alteration (we know that a dishonest student would deliberately get a few answers wrong in order to cover his tracks), and given that Mr.Honor Student's answers were (if, for the sake of argument, one admits the charge) evidently regarded by Mr.Jock as 'theoretical constructs' which Mr.Jock — with admirable intellectual modesty — had good cause to believe were more accurate than his own work, we allege that no dishonest intent has been proved. We therefore ask for our client's immediate relief from imputations of unethical conduct. Until further evidence is produced than mere moralizing, dressed up with sociologically-inappropriate application of currently-fashionable postGaussian paradigms, we continue to regard Mr.Jock as the Greatest Intellectual on Campus.

<sup>24</sup> RN allegedly made a statistical misjudgement regarding medieval eclipses, and Huber claims that RN refused to respond after receiving certain verbal information (Huber's statistical expertise) at a meeting c.20<sup>y</sup> ago. Comments: [a] Huber raises this to me only many years later (now that RN is not able to comment). [b] Even trusting Huber's recollection of this encounter, other interpretations (than fear of his brilliance) are faintly possible. (This is, after all, just a sample of one. Is Huber proud of damning a scholar's entire corpus on such a basis? I hope Huber will re-read & re-think his astonishing 1991/8/12 claim.) [c] Huber dislikes RN's nonreaction to him on a single ephemeral occasion. By contrast, the Muffia's avoidance of DR is consistent over 15<sup>y</sup> of the written public record; and it is not subject to misinterpretation, especially since its reality has been directly attested through multiple sources (*DIO 1.1* ‡1 §A8). I have repeatedly urged a public discussion or debate; see, e.g., the 1978 challenge cited in *DIO 1.1* (‡1 fn 20) or the suggestion in *Amer J Physics* 1987/3 (an offprint of which was sent Huber 1991/8/23). No takers. None of this Muffia behavior (documented in detail in *DIO 1.1*) appears to lead Huber to any conclusions or misgivings, while a single instance of RN's shyness (and he was shy in person) causes Huber to reject *all* RN's work — and to portray RN to Tuman and to me as a coward, which is a particularly amazing tag to attach to such a bold explorer & scholar. (In fact, RN did not avoid contrary scholars' work but replied in extensive detail in his 1979 book.)

<sup>25</sup> My 1991/8/23 letter vainly suggested (see above, fn 15) that Huber ask OG for a xerox of DR's solutions for all the *Almajest* mean motions, sent to OG on 1980/4/13. (Math details of letter quoted below at ‡3 §C6.) Instead of giving credit to the discoverer, 0 and his colleague Toomer have done all in their power to deny it. (See also *DIO 1.2* §D4.) If Huber is going to maintain that his friends have done nothing especially amiss, he should not be afraid to see relevant raw evidence. (Silent OG has now had years to work up an alibi for his behavior. Let's urge him to try it out. In the open.)