‡5 The Scholarly Integrity of Book Reviews

by Robert Russell Newton[1]

A Introduction

A1 Appearing in the *Journal for the History of Astronomy*, a review of one of my books reads, after a few preliminary sentences: "The object of this book is to determine whether the rate of the rotation of the Earth is subject to long-period variation independent of the retardation produced by lunar tidal forces." I will cite and discuss both the book and the review later in this paper, but first I want to call attention to the quoted sentence.

A2 It has been known for a long time that the rate of rotation of the earth "is subject to a long-period variation independent of the retardation produced by lunar tidal forces." For that matter, the rotation of the earth is also subject to short-period variations independent of the lunar tides. The oddity in this context is that the book in question has nothing to do with the forces responsible for the variation in the earth's rotation. Instead, as its preface clearly states and its [very] title implies, the object of the book is to enquire whether the force of gravitation (including the modifications due to general relativity) is sufficient to account for the observed orbital motion of the earth and the other planets, or whether there may be other effects that affect the orbital motion at the present level of observational accuracy.

A3 If the reviewers can make such an outstanding error in even understanding the object of the book (and their review shows many other failures of understanding as well), it is clear that they are not competent to review the book. The question then arises: how can such an incompetent review appear in a scholarly journal? This paper will be concerned with documenting the incompetence of the review in question, and with suggesting a method of improving the quality and integrity of book reviews.

A4 I should point out one editorial matter in this introduction. I identify a reference by giving the name of the author or authors, followed by the year of publication, in square brackets. When necessary, I follow the year of publication by a specific point of reference, such as page number or a section number. For example, the book in question will be cited as [Newton, 1976]. If I need to refer specifically to page 532, for example, I add "p.532" after "1976" within the brackets. If the author's name occurs naturally in the text, it is not put within the brackets; otherwise it is.

B The Integrity of Research Papers

B1 Most scholarly journals have done a pretty good job of maintaining the scholarly value and integrity of the research papers that they publish. This applies both to journals published by professional societies and to those which are published independently of professional societies. The same cannot be said of the book reviews published in many scholarly journals.

B2 Let us look at the method of handling research papers. In the usual procedure, a paper submitted for publication is sent to the editor or to his office. From there it is sent to two referees who submit independent reports to the editor. The contents of the paper are kept confidential between the editor and the referees at least until the paper is accepted for publication, and all parties are in honor bound not to discuss even the existence of the paper with any other person until this happens. A few journals disclose the identity of the referees to the author but most do not.

B3 If both referees recommend that the paper be published, the paper is published as soon as possible, with due attention paid to earlier accepted papers that are still awaiting publication. If both referees reject the paper, the editor returns all copies of the paper to the author, with a note that it has been rejected. If one referee recommends publication while the other recommends rejection, practice varies, but we do not need to go into this complication.

B4 When an editor returns a rejected paper, he does so along with copies of the referees' (or referee's) reports that caused rejection. Several options are now open to the author, of which we have room to discuss only two. First, he may rewrite the paper to conform to the objections that have been raised, and resubmit it for publication. Second, he may write a letter of rebuttal to the referees, send the rebuttal, adverse referees' reports, and the original paper back to the editor, and request that all this material be sent to a third referee for another opinion.

B5 So far as I can discover, no journals, even those of the utmost probity, take any such safeguards with regard to the integrity of book reviews. Before I discuss this situation, I want to describe the book and the review in question.

C The Book

C1 The book being reviewed is *Ancient Planetary Observations and the Validity of Ephemeris Time* [Newton, 1976]. The term "Ephemeris Time" is well known to astronomers but is probably not known to the general reader. The need for ephemeris time arises from the variation of the earth's rotation, but the measurement of ephemeris time does not require measuring the earth's rotation.

C2 Suppose for the moment that the motion of the solar system is completely governed by gravitation, within the accuracy of present observation (about 1 part in 10^8). We can then establish a time scale based upon, say, the orbital motion of the earth (or the apparent motion of the sun). If the entire motion of the solar system is dominated by gravitation, the same time scale can be used to describe the motion of all the planets. This time scale is known as ephemeris time.

C3 However, it has been known for more than a century that gravitation is not sufficient to account for the orbital motion of the moon. It is believed that friction in the lunar tide is the only cause for the deviation from gravitational motion, at the present accuracy of measurement, but this is only a belief which there is no current way to test.

C4 If there are forces other than gravitation which affect the motion of the moon (at the level of about 1 part in 10^8), it is natural to ask if there are forces other than gravitation which affect the motion of the planets about the sun. There is a way to answer this question that is simple in principle but that is difficult to carry out because of the accuracy required. In order to describe this way, it is necessary to discuss the variations in the rotation of the earth.

C5 At the same time that friction in the lunar tide affects the orbital motion of the moon, it changes the rate of rotation of the earth. We do not know enough about the tide to calculate its effect upon the orbital acceleration of the moon or upon the acceleration in the earth's rotation, but we can calculate accurately the ratio of the two accelerations. (The

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ratio depends only upon such matters as the moment of inertia of the earth and the mass of the moon, which are known with reasonable accuracy.) The ratio of the two accelerations shows clearly that there are forces other than friction in the lunar tide which affect the rotation of the earth. On the whole, the length of the day is increasing and the number of days in the year is decreasing.

C6 However, until quite recent times, the rotation of the earth (the length of the day) was used in astronomy to furnish the standard of time. I shall use "solar time" to denote the time defined by the length of the day, because this kind of time was measured by observing the transits of the sun across the observer's meridian, or by some equivalent observations. Now if the number of days in the year is decreasing, and if the length of the day is taken as the standard of solar time, it is clear that the sun has an acceleration with respect to solar time. The planets must also have an acceleration with respect to solar time in their heliocentric motions. Further, if the concept of ephemeris time is valid, the acceleration of the planets with respect to solar time must be in the same ratio as their heliocentric mean motions.

C7 To see if there are forces other than gravitation which affect the motion of the planets (that is, to see if the concept of ephemeris time is valid), we simply calculate the heliocentric acceleration of each planet and see whether the accelerations are in the same ratio as the mean motions. In the book under review I do this using only surviving observations up to the year 1019. Unfortunately, the surviving observations do not let us find the planetary accelerations with enough accuracy to apply this simple test. However, I found another way to test the validity of ephemeris time, but one that does not allow us to infer the planetary accelerations.

C8 The investigation was based upon old observations; I actually used observations dating from -567 to $+1019$. During this period, the planets beyond Saturn were unknown. Further, the mean motions of Jupiter and Saturn are small, and the expected accelerations are also small, so I decided not to estimate their accelerations. As a result, I confined the immediate purpose of the book to studying the accelerations of Mercury, Venus, Earth, and Mars. I did so by using timed measurements of the position, such as the times of equinoxes and solstices, the rising and setting times of the planets, and the times of conjunctions of the planets with other celestial objects.

C9 Unfortunately it is not always a straightforward matter to extract the data from the ancient and medieval sources. To start with, we cannot always read the calendar used by the observers. The Babylonian months were determined by the time when the moon first became visible in the evening after passing the sun in longitude. Islamic writers used two different fundamental dates for the start of their calendar. Thus, even when Babylonian or Islamic dates are explicitly stated in their own calendars, we are not always able to translate the dates into unique dates in our calendar.

C10 Most of the sources have not come down directly to us but have come to us only through the medium of many copyists. Thus large errors which have probably been introduced by copyists, are frequently present in the sources. In some sources, c.1/5 of the observations contain scribal or copying errors so large that they are useless. In other cases, one astronomer used the work of another and forgot to record where the original observation was made. We cannot automatically assume that the observation was made where the writer worked, and sometimes it takes considerable research to establish the site of an observation.

C11 And, unfortunately, there are some outright forgeries, hoaxes, and fabrications. As an example, the Islamic astronomer Abu Sahl al-Kuhi claims to have made a thorough study of the solstices around the year 990. This study comes to us through al-Biruni [1025], and I have discussed it in some detail [Newton, 1976, pp.226ff]. al-Kuhi claims to have gotten exactly the same value for the obliquity of the ecliptic as Ptolemy [ca.150] although, as al-Biruni points out, all other contemporary measurements give a result that is about a quarter of a degree smaller. al-Biruni concludes that al-Kuhi's claimed study is a hoax, and I agree with him. There are many other fabrications in the ancient astronomical literature, particularly in the Greek.

C12 Thus it frequently takes considerable historical research in order to decide which ancient and medieval observations can be used in modern astronomical research.

D The First Part of the Review

D1 The review in question is by Hamilton and Swerdlow[2] [1981], which I shall denote by *HS* for brevity in the rest of this paper. I have already pointed out the enormous error that *HS* make in understanding the purpose of my book. Shortly after they make this error, *HS* write of my results: "The results found are not positive, nor are they negative, nor are they inconclusive; they are simply meaningless. . . . the standard deviations of the estimates are so large as to make all the numerical estimates, including those for the Sun, without value."

D2 This shows that *HS* fail to understand either the object of the book or its results; since they fail to understand the object, it is probably not surprising that they should fail to understand the results. It is simply not true that the numerical estimate for the acceleration of the sun, for example, has such a large error that it is without value. My final result for the acceleration of the sun is (page 534 of the book)

$$2.52 \pm 0.35 \tag{1}$$

seconds of arc per century per century. This was probably the best estimate that had been made of the solar acceleration at the time of of the book, and it may still be the best estimate available.

D3 Shortly before the quotation just given, *HS* make the peculiar remark that "the accelerations of Mercury and Venus are, when compared to the solar acceleration, far too low, the acceleration of Mars is too high. . . ." This again shows a failure to understand the object of the book. If the accelerations of Mercury and Venus are too low compared with the solar acceleration, while the acceleration of Mars is too high, then the planets are subject to forces other than gravitation at a measurable level. As I said in the second paragraph of this paper [§A2], answering this question is the main object of the book. Results showing the quoted property would not be meaningless, as *HS* imply. Instead, they would be of the utmost importance for the theory of the solar system, and my results would be highly meaningful.

D4 As an aside, while I cannot be certain what *HS* had in mind, their writing about meaningful results suggests that they share a miscomprehension about statistical results that is common among scientists and many other people who have to deal with statistical results. To illustrate this miscomprehension, let us take an example from Table XV.1, page 532 of my book. There I find from Babylonian measurements of the times of conjunctions of Venus with other objects that the acceleration of Venus is

$$1.45 \pm 3.39 \tag{2}$$

seconds of arc per century per century. Here, as in the acceleration of the sun just given [eq. 1], the first number is the "central value" or "best estimate", while the second number is the standard deviation. The combination of the two numbers means that, with a probability of about 2/3, the acceleration of Venus lies between $+4.84$ ($1.45 + 3.39$) and -1.94 ($1.45 - 3.39$). That is, the uncertainty[-range] in the acceleration of Venus is $2{\cdot}3.39$, or 6.78.

D5 Many people think that a result such as this is meaningless because the standard deviation exceeds the central value. This is not so. If there were no measurement of the acceleration of Venus, the uncertainty in the acceleration would be ∞. With the quoted measurement, the uncertainty has been reduced to about 6.78. Surely, reducing the uncertainty from ∞ to 6.78 is a meaningful accomplishment.

[2] Note by DR: No doubt aided by the sort of attack (upon dissent from Neugebauer-Muffia orthodoxy) here under dissection, Swerdlow (Hist.sci member UChicago astronomy dep't) has ascended to the board of the extremely handsome *Journal for the History of Astronomy*, the very journal in which *HS* appeared.

D6 It is true that the uncertainties in the accelerations found for Mercury, Venus, and Mars are too large to let us simply use the ratios of the planetary and solar accelerations in testing the validity of ephemeris time. This does not make the results meaningless in the sense used by *HS*; it merely makes them inconclusive (contrary to the statement made by *HS*). For example, the acceleration of Venus should be 1.6255 times the solar acceleration if ephemeris time is valid. Using the solar acceleration given above [eq. 1], then, the acceleration of Venus should be 4.10 ± 0.57, while the range found for Venus [eq. 2] lies between -1.94 and $+4.84$. The central value 4.10 does lie within the range found, but the range of uncertainty found is so large that the result is inconclusive. This is not the fault of the book, however. It is a consequence of the data available, and it is useful to know that the available data cannot give us a conclusive answer by simply using the ratios.

D7 As I have already mentioned, I found a way to test the concept of ephemeris time in spite of this difficulty. *HS* do not mention that I found a way to test the concept, perhaps because they do not understand that this testing was the purpose of the book.

D8 After their incorrect comment about the accelerations, *HS* go on to say: "But more must be said for the book also seems to be intended as a contribution to the history of astronomy in that the author evaluates ancient Babylonian and Greek, and medieval Arabic observations. . . ." (I think the reader can understand this sentence more easily if he will put a comma after "said". Without this comma, it may take several readings to understand the sentence.)

D9 Again *HS* show their failure to understand the nature and purposes of the book. As I have already explained, there are many historical problems in trying to extract the valid astronomical data from the ancient and medieval sources. There are problems in trying to tell which statements represent [outdoor] observation and which represent only [indoor] calculation. There are a number of innocent fabrications of data in the literature, as well as a number of outright hoaxes, which have been taken as serious observations in earlier astronomical literature; these must be detected and eliminated from the corpus of accepted observations. There are many scribal and copying errors in the existing forms of the old literature, in some cases amounting to about a fifth of the total observations. There may also be problems in determining the time and place of an observation.

D10 Thus of course I had to spend quite a bit of space in evaluating "ancient Babylonian and Greek, and medieval Arabic observations" in order to carry out the main purpose of the book which, to say it again, was to test the validity of ephemeris time. However, I did only the minimum amount of historical research necessary to find a body of valid data. If I could not establish the validity of a datum after a moderate amount of effort, I dropped the datum from my body of observations. I did not necessarily search the historical literature for the most recent historical analyses which might admit other valid observations; I merely tried to find a body of observations, of reasonable size, which I could accept as valid observations with reasonable confidence. I am sure there are many research works on the history of the old sources which I did not consult and which contain much information that is important to the historian. Such a failure does not impair the value of my book, which merely required a reasonable body of valid observations.

D11 Thus my book was not intended as a contribution to the history of astronomy and any criticicm of it as such a work is automatically invalid. To be sure, it is valid to criticize the historical research that I had to do, but such criticism must be within the framework of the book and its purposes, and not upon the basis of a professional historian being criticized by other professional historians.

D12 Of course I have tried to be accurate in every historical statement that I have made, but I have not tried to give a complete scholarly discussion of every historical subject that has come up. I have only tried to give evidence for the acceptance of valid data. When I have rejected data, I may or may not have explicit reasons for doing so. I do not need such explicit reasons for rejecting data; for my purposes I should reject or omit data if I cannot find explicit reasons for accepting them.

D13 *HS* then really start to play hardball; up to now their comments have been mild compared with what is to follow: "Newton's work is careless and unreliable to the point that it can be recommended, if at all, only to the reader who is prepared to examine every source of observations and check every computation, *i.e.*, to do all the work over again, and this is far from easy as Newton's exposition is often far from clear. In order to defend this admittedly harsh judgement, we can do no better than to give examples of Newton's understanding and use of sources in making the crucial decisions about whether a report represents an observation or a computation."

D14 *HS* then give two examples of places where I have presumably failed "in making the crucial decisions about whether a report represents an observation or a computation". Both their examples are interesting. They concern matters of controversy, but *HS* do not mention this point. Instead, they choose one side without mentioning the other side, and then show my alleged lack of understanding by demonstrating my differences from their viewpoint, which they present as established fact. [See fn 15.]

D15 I shall now take up their examples in order.

E A Babylonian Text Dated to the Year −424

E1 Kugler [1914, pp.233-242] gives a thorough analysis of a Babylonian text which he describes as "a text allegedly originating from the middle of the second millenium B.C." The text is (or at least was in 1914) in the museum of the University of Pennsylvania, and is identified as CBS 11901. The text applies to the Babylonian months IV through IX of some year that is not identified. For these months, it gives the number of days in each month, the dates of the full moons, of the first and-or last visibilities of the planets and the star Sirius, of the summer solstice and of the autumnal equinox, and of one solar and one lunar eclipse. It also says that the lunar eclipse began 40 minutes after sunset.

E2 Before the work of Kugler, CBS 11901 had been dated at about −1500, for reasons that I have not studied. Kugler noted, however, that, because of the precession of the equinoxes, the date of the first or last visibility of a star (in this case, a first visibility of Sirius) moves steadily throughout the solar year. In the text CBS 11901, the summer solstice comes on day 1 of the month called Duzu and the first visibility of Sirius comes on day 20 of the same month.[3] After paying due attention to the rounding of the data, and to the uncertainty about the date of a first rising of a star, Kugler concludes that the stated interval between the solstice and the first visibility of Sirius could only have happened between the years −800 and −400.

E3 By comparing the dates of the autumnal equinox, the lunar eclipse, and the solar eclipse, which all fall within the Babylonian month called Tisri, and aided by the fact that the lunar eclipse started 40 minutes after sunset, Kugler finds that the only Babylonian year between −800 and −400 which fits is the one which began in the spring of the year that we call −424. From this, it follows that the lunar eclipse is the total eclipse of −424 October 9 and the solar eclipse is the penumbral eclipse of −424 October 23; both dates are in the Julian calendar, with the day beginning at midnight.

E4 However, there is a difficulty. Because we do not know the accelerations of the sun and moon well enough, we cannot calculate accurately the times of the individual eclipses. Luckily, as Kugler himself pointed out, we do not need the individual times here; we need only the time interval between two eclipses that were only two weeks apart. If we use any plausible set of accelerations that makes the beginning of the lunar eclipse visible in Babylon, we find that the solar eclipse was not visible there. In a test of the situation (page 129 of the book), I chose acceleratons which made the lunar eclipse begin at sunset

[3] There is a difficulty here that is almost surely of typographical origin. In his translation of the text, Kugler puts the rising on day 21 of the month. In his main writing, and in his calculations, Kugler puts the rising on day 20. I think the day 21 that occurs in the translation is probably a typographical error, but someone with access to the text should check the matter.

rather than 40 minutes later; this is the extreme assumption we can make if the beginning of the eclipse was visible at all. Under this assumption, the solar eclipse was not visible at Athens or any point east of there.

E5 Thus, as Kugler concludes, it is not possible for both eclipses to have been visible in Babylon, and thus the text is not a record of observations. It is instead a record of calculations or predictions. It is wishful thinking to claim that the lunar eclipse was observed while the solar eclipse was merely calculated.

E6 There are other reasons for concluding that the text CBS 11901 is a record of calculations and not of observations. One reason is given by Neugebauer [1948], who makes a special study of equinox and solstice times in Babylonian astronomical texts. His conclusion is: ". . . *no* solstitial or equinoctial date which is found in (Babylonian astronomical) texts can be evaluated as an observation . . ." [Note by DR: CBS 11901 contains such data: §E1.]

E7 Another reason is given by Sachs [1948], who classified all the Babylonian astronomical texts known when he wrote. He finds only two classes of text that contain observations; all others contain only calculations. One class that contains observations is called a "goal-year" text; it concerns mainly the planets, and CBS 11901 is clearly not of this class. The other is called a "diary". A diary gives a variety of astronomical information, usually for a period of six months, and it devotes a separate section to each month. So far, CBS 11901 sounds like a diary, but it is not. In addition to the kind of information found in CBS 11901, the diaries typically give conjunctions of the moon and planets with major stars near the ecliptic, and matters which we would consider non-astronomical such as the weather, the height of the river, the prices of various agricultural products, and occasionally some important political events.

E8 We should note two other kinds of information that are not present in CBS 11901. There is no remark that the solar eclipse did not occur, and we are not told on what part of the moon the darkness first occurred. When we have to deal with observations of eclipses that were planned with the aid of prediction, we frequently find one or both of these remarks, depending upon the circumstances.

E9 Thus, CBS 11901 does not read like a diary that contains observations. Instead, it reads like the class of text that Sachs calls an almanac, which contains only calculation or prediction.

E10 However, this conclusion has been a matter for controversy. The earliest dissent from Kugler's conclusion that I have read personally is by de Sitter [1927], although he cites an earlier dissent by Carl Schoch that I have not read.[4] van der Waerden [1974, p.102] says that "the Mars and Mercury dates coincide much better with modern tables than is otherwise normal in the case of Babylonian calculations". He also says that "The lunar eclipse too coincides with modern calculation to within a few minutes". Thus he also dissents from Kugler's conclusion.

E11 A few sentences before, van der Waerden writes that Kugler "believed he could conclude that all the dates were calculated, because there is a complete absence of meteorological observations and because the text shows an eclipse not visible in Babylon without the comment, customary in the observation texts, 'It was missing'." I cannot find any place in the cited text where Kugler mentions either of these matters, but perhaps I overlooked it.

E12 Instead, as I read him, Kugler based his conclusion entirely upon astronomical calculations and upon such paleographic matters as vocabulary. I am not competent to judge the paleographic matters, but I do feel competent to judge the astronomical calculations and the points that I quoted from van der Waerden in the preceding paragraph.

E13 Actually, I do not see any way to settle the controversy. We can prove that only one of the two eclipses in CBS 11901 could have been visible in Babylon; let us say for the sake of argument that it was the lunar eclipse that was visible. Even so, we cannot say that the Babylonians observed it; perhaps it was cloudy that night. Nonetheless, we must always admit the possibility that it was observed. This is a different matter from admitting the alleged observation to our body of accepted and accurate astronomical observations. We accept observations with high weight only if we have strong reason to believe that they are genuine, but here we have strong reason to believe that the "observations" in CBS 11901 are actually calculations.

E14 On page 128 of my book, I wrote that ". . . I do not see any reason to assume that the text contains an observation of a lunar eclipse at all." On page 129, I wrote: "The reader may assume, if he wishes, that the lunar eclipse was observed. If he does so, he must recognize that this is a matter of opinion with no supporting evidence, and hence he is not entitled to give high weight to his assumption."

E15 I do not see any reason to change these statements. I do not believe that CBS 11901 contains any observations at all, and I certainly feel that one cannot use the data from it in astronomical calculations.

E16 I cannot attach much weight to the arguments by van der Waerden that were quoted a moment ago. He says that the Mars and Mercury dates "coincide much better with modern tables than is otherwise normal in the case of Babylonian calculations," implying that the dates are observed ones. He does not say anything about the other planets. Furthermore, the planetary dates in question are those of first or last visibility of a planet before or after its conjunction with the sun. These dates are uncertain matters to observe.

E17 Finally, he says: "The lunar eclipse too coincides with modern calculation to within a few minutes." He does not say how the modern calculations were made, and in particular he does not give the accelerations of the sun and moon that were used. (These accelerations are not important in comparing the two eclipse times with each other, but they are important in calculating the individual times.) The uncertainty in calculating the elongation of the moon at any time after the year -700 is, on a standard deviation basis, about $1''T^2$, in which T is time measured in centuries from 1800 [Newton, 1979, p.464]. For the eclipse of -424 October 9, T is about -22.2 [centuries], and the uncertainty in the elongation for a given instant is about $493''$, or about $0^\circ.137$. It takes about 16 [time]minutes for the elongation to change this much, so this is the uncertainty in a modern calculation of the eclipse time. I do not know if this comes within van der Waerden's meaning of a few minutes or not. Even if it does, this does not prove that the eclipse was observed; it merely means that the calculation of it was accurate.

E18 After mentioning Kugler's conclusion (that the material in CBS 11901 was calculated rather than observed), *HS* go on to write: ". . . while Newton agrees in this judgement, his analysis shows little understanding of what Kugler wrote and contains a rather strange result. Kugler dated the text to -424 . . . . but Newton says he questions the dating, although why is not made clear."

E19 This is a shot with no supporting evidence. I certainly did not fail to understand what Kugler wrote, and *HS* present no evidence that I did. I can see only two possible bases for the claim that I did not understand Kugler's work. One is that, because I could use a modern large-scale digital computer, I could perform certain calculations more accurately than Kugler could take the time to do; this is a refinement, not a misunderstanding, of Kugler. The other is that I questioned Kugler's dating "although why is not made clear."

E20 My reason is made quite clear and explicit on page 130 of my book. There I write: "Now it is likely that the errors in the Babylonian ephemerides, the ones upon which the information (in CBS 11901) is based, are greater than Kugler thought. It is certain that the errors in the modern ephemerides that he used are greater than he thought.[5] I have not

[4] de Sitter cites this as Berlin, 1926. [It later appeared in *Astronomische Abhandlung 8.2*, Kiel, 1930 (*Die säkulare Acceleration des Mondes und der Sonne*).]

[5] Every attempt that I have seen to date a text by using modern theory has greatly exaggerated the accuracy of the modern theory. The most extreme example I have seen is by Schoch [1928], and his exaggeration was accepted by Langdon and Fotheringham [1928]. Schoch claims that his calculations of the times of the new moon are accurate to 3 [time]minutes for times back to -2000! I showed a minute ago [§E17] that the uncertainty in calculating a full moon (lunar eclipse) was already about 16 [time]minutes for the year -424, and I showed on page 38 of my book

attempted to discover whether the errors are enough to bring the year into question."

E21 In other words, if there were sizeable errors in the basic document (CBS 11901) or in the modern calculations, there may be years other than −424 which fit the stated conditions. Again, *HS* have failed to understand the situation.

E22 I will take up my calculations relating the eclipses of −424 in a moment [§E29], but first I want to take up another related point. *HS* disagree vigorously with my calculations about the eclipses, and they then go on to say: "And this example is not an isolated[6] aberration. In the course of spot checking we have noted instances of incorrectly computed sunrises, confusion of tropical and sidereal year, and other suspicious syzygies. We need hardly point out that in research of this kind, in which the goal is to isolate very small cumulative errors in modern theory, precision in computation is crucial if the work is to have any meaning at all."[7] Here again *HS* give no examples of my alleged deficiencies.

E23 It is probably unnecessary to say that I understand[8] the difference between the tropical and sidereal years, and I have done so at least since my freshman year in college. It is always possible, of course, that I inadvertently used one word somewhere when I meant the other.

E24 The remark "precision in computation is crucial" again shows the failure of *HS* to understand the situation. The precision needed in computation depends upon the use to which the result will be put. As it happens, in the book under discussion, I did not need high precision in either the times of sunrise or of syzygies.

E25 Take the matter of sunrise (and sunset). Many of the older observations, particularly the Babylonian ones, give the time by means of a time interval from either sunrise or sunset. Therefore we need the time of sunrise or sunset in order to convert the recorded time into some kind of astronomical time. Now the difference between ephemeris time and solar time

that the uncertainty for −2000 is at least 3 hours, not 3 minutes.

[6] Note by DR: The implication, that an occasional alleged error is typical of numerous other unstated ones, is standard for a Capt.Captious Swerdlow attack. See also his equally competent (fn 20) diatribe against R.Newton in Phi Beta Kappa's *American Scholar 48*:523, 1979. (Also discussed at ‡6 fn 6. One notes that O Gingerich was on the *Amer Schol* board at the time. The private details of this review's production are even more repellant than what was printed.) Who is naïve enough to believe that, had *HS* found even 5 instances, they would not have laid out every one in gleeful detail? (Co-reviewer N.Hamilton, U.Ill at Chicago mathematician, has told DR in so many words that he derived pleasure from attacking Newton in *HS*.) A lengthy itemized list of author-fluffs in a Muffia review is not without precedent, as witness Gerald J.Toomer's review of O.Pedersen's 1974 *Survey* (*Archiv Internat Hist Sci 27*:137-150; 1977/6), which features exactly 100 errors. (The review immediately preceding *HS*'s review in the 1981/2 *JHA* lists roughly 50 errors.) Curious contrast: Toomer nonetheless calls Pedersen's *Survey* "useful" and "warmly recommended" (opinions DR concurs in); so how can merely 2 questionably-relevant alleged errors *in a 749 page book* (by R.Newton) be held by *HS* to destroy the credibility not only of the book under review but of the entire historical corpus of the author? Two mistakes in 749pp? Heck, *HS* achieve more than that in 4pp.

[7] Note by DR: Attacking others' alleged slips is particularly ironic coming from Swerdlow, whose 1968 Yale U Hist.sci thesis is infected, at its vital part (p.82), by math which is bungled with Ptolemaic neatness & republished (unchecked) by *Centaurus 14*:287-305 (1969): in eq.1 (p.298), Swerdlow needs 67;20 sin($360°/1300 = 16'36''55'''$) to be 0;19,30, though it's really 0;19,32. No problem: [a] Capt.Captious miskeys the argument as $16'36''.55$ & so multiplies 0;0,17,23,34,50 times 67;20, yielding 0;19,31,7,45,26,40, which he then [b] mistypes as 0;19,30,7,45,26,40 = 0;19,30. Cute. The Hipparchos distance-ratios thus found by Swerdlow (UChicago) are highlighted in RiceU Hist.sci archon A.Van Helden's *Measuring the Universe* (UChicago! 1985, pp.11-13), whose p.168 calls Swerdlow's thesis "the definitive work" on ancient distance-schemes, though Swerdlow's main new result requires that Hipparchos believed: [a] half-Moons occur c.0.1 radians from quadrature, & [b] the Sun's diameter is merely twice the Earth's, seriously inconsistent with what we know (from Cleomedes & Theon of Smyrna) of Hipparchos' estimates of the Sun-Earth ratio. See, e.g., p.140 of G.Toomer's plausible attempts at a compromise solution: *Arch Hist Exact Sci 14*:127 (1974). This entire area of research is murky. Some of the confusion can perhaps be alleviated by speculating that Hipparchos' values of 62 & 67 1/3 Earth radii might have been his figures for the Moon's mean & greatest distances, not least & mean distances (as Pappos had it). Using 62 Earth radii in the basic equations of Toomer pp.130-131 produces a solar distance of about $1/(1/59 - 1/62)$ = 1200-to-1300 Earth radii, near the value of *Alm* 5.15.

[8] Note by DR: The only serious confusion of this sort known to me is O.Neugebauer's amazing and fateful mislabelling of the Babylonian tropical year as a sidereal year (*HAMA* p.528 eq.2). (This yearlength value has now been directly connected to the solstices of Meton and Hipparchos. See ‡6 in this *DIO*. It is thus unquestionably a tropical year. An elementary point, requiring no induction, which had obvious implications even before the mystery was solved: the Babylonian yearlength in question is roughly 3 times closer to the tropical yearlength than to the sidereal yearlength.) [Original printing wrongly had "5 times". *DIO* thanks John Britton for the correction.]

was several hours when the Babylonian observations were made, and we will be lucky if we find the difference with an accuracy of ten percent. Thus we can tolerate a precision of, say, 30 [time]minutes in calculating sunrise or sunset, particularly if our errors are periodic with the time of year, so that they tend to average out.

E26 In spite of the low precision required, I adopted a simple method of calculating sunrise or sunset, whose error is periodic and which has, I believe, a maximum error of perhaps 3 or 4 [time]minutes. This exceeds the precision needed by roughly an order of magnitude. The method is described on page 342 of my book.[9]

E27 With regard to the syzygies, I could find only five syzygies that I used in the book other than those in −424 October, all being connected with lunar eclipses. I used these eclipses only for dating purposes, that is, for finding the relation between a particular Babylonian month and our calendar. For this purpose, a precision of half an hour is surely adequate, except in critical cases. (As it happens, none of the cases was critical.) Because of the low precision required, I did not find these syzygies from my highly accurate lunar eclipse [computer] program. Instead, I listed the positions of the sun and moon for times around the needed times, and found the syzygies by a simple hand calculation. I did not record the precision I kept in the results, but it was certainly greater than that required. When *HS* claim errors in my times of syzygy, they should state the size of the errors found and compare them with the required precision.

E28 It is worth spending a word about how I calculated astronomical positions when they needed to be calculated precisely, and how I tested the precision of the [computer] programs. I will not take the space to describe the programs here, but they are described in Chapter IX of my book. I will give the results of one test, which is typical. The Naval Observatory calculated a number of positions of the sun with dates ranging from −1062 to +590. When I compared my results with theirs, as well as with present-day results from the *American Ephemeris and Nautical Almanac* (annual), I found no discrepancy as large as 1 second of arc [$1''$]. *HS* of course make no mention of my precise programs which are used when precise results are needed.

E29 Now let us turn to my calculations regarding the eclipses in −424 October, and start by reviewing what Kugler did. He first narrowed the year to the range −800 to −400 by using the first visibility of Sirius, and found the approximate days of the year for the eclipses by the interval between their dates and the autumnal equinox. He then searched through a canon of eclipses looking for a pair of eclipses that would meet the conditions just found, plus the condition that the lunar eclipse should start about 40 minutes after sunset. After finding a unique pair in −424 October, he calculated the local circumstances at Babylon, using an odd mixture of numbers [Kugler, 1914, p.237] taken from canons of eclipses by Oppolzer [1887] and by Ginzel [1899]. For example, he took the middle of the lunar eclipse from Ginzel, even though he had found the date of the eclipse by using the time from Oppolzer, and he then took the half-duration from Oppolzer. He also took the middle of the solar eclipse from Oppolzer.

E30 Under these circumstances, I wrote that Kugler took the time of both eclipses from Oppolzer, and that he made a mistake in copying the time of the lunar eclipse. I am sorry that I made this error, but it is certainly an easy error to make, and one that has no effect on my conclusions. In fact, it is so easy to make that *HS* make an exactly similar error[10] in

[9] It is always possible that there is a typographical error in the results of a computation listed in the book, but this does not imply an error in my final result. I first combined all the results from computer calculations on work sheets, triple-checked them, and then had to prepare the book. This required two copyings and proof-readings, and errors could have crept in here even though the results used were accurate. I will take this matter for granted in the rest of this paper.

[10] Note by DR: In fact, [Ginzel 1899] does not even provide a time for the −424/10/23 solar eclipse! (Ginzel's *Handbuch der Mathematischen & Technischen Chronologie* vol.1, Leipzig 1906, p.552, provides a rough value, 20:29, but this is not identical to the 20:31 Oppolzer figure adopted by Kugler. The same book's p.537 gives 20:11 for the −424/10/9 lunar eclipse, altering the [Ginzel, 1899] value by -3^{m}. Kugler, *HS*, & Newton all use the [Ginzel, 1899, p.137] lunar eclipse time. There is no question of Kugler's source for 20:31; he explicitly states [1914, p.237]

saying that Kugler took the times of both eclipses from Ginzel.

E31 I also wrote that there are errors in Oppolzer's times, because of the approximations that he had to make in the solar and lunar theories in order to carry out his enormous body of calculations in the days before large-scale computers. Before I take up this point, let me take up the opinion of *HS* about accepting the lunar eclipse as an observation. They do not state their opinion explicitly, but I think it is suggested by the following passage: "It is clear that the solar eclipse was not visible in Babylon, but the lunar eclipse, which was total, certainly was, and that the text gives no time for the solar eclipse but a specific time for the lunar eclipse suggests some difference in their reports. But Newton writes that the text offers no grounds for a distinction." From this, I conclude that *HS* think that the reference to the lunar eclipse constitutes an observation.

E32 Whether this is so or not, the remark of *HS* needs comment. The presence of the time in the lunar report but not in the solar report does not constitute a distinction between calculation and observation. The Babylonians at the approximate time could reliably predict the occurrence and the time [of day] of a lunar eclipse, but they could not reliably predict the time and place of a solar eclipse. Thus CBS 11901 contains only information that could be calculated, and it contains no information that indicates observation. It does not even remark that the eclipse was total.

E33 Now let us turn to the errors in Oppolzer's (and Ginzel's) times. As *HS* state, the time of the lunar eclipse's beginning, in Babylonian [mean] time, is 18:25 according to Ginzel and 18:45 according to Oppolzer. This is a discrepancy of 20 minutes, which illustrates the errors in Oppolzer's (or Ginzel's) times arising from the approximations they had to make. I also wrote (p.129 of the book): "According to Oppolzer, syzygy for the solar eclipse occurred 3 minutes earlier, mean time, than did syzygy for the lunar eclipse. According to my calculations, it should be 55 minutes earlier."

E34 *HS* write about this point that this difference dwarfs "the differences of the sources we have compared and of our own computation. This is an extraordinary result, and if it is true, Newton knows something about calculating syzygies that no one else knows." I no longer have the computing programs I used and cannot check the matter myself. However, Dennis Rawlins of Loyola College, Baltimore, [has written] the necessary programs, and he has kindly checked the matter. [For the book's lunar and Earth-spin accelerations], he finds[11] that the solar eclipse [invisible at Babylon] was about 56 minutes later rather than about 55 minutes earlier. Apparently I committed the equivalent of a sign blunder when I reported the time difference.

E35 *HS* refer to my figure of 55 minutes as an aberration. Even if it were, this would not have anything to do with the main point that *HS* claim to have made. The figure of 55 minutes was not used *in any way* in my decision [see §E6-E9] not to use the record of the lunar eclipse as an observation. It was used only to illustrate the need to repeat Kugler's calculations, using a highly precise program carried out on a modern digital computer.

E36 I suspect that the difference between *HS* and Rawlins-Newton comes from the difference between the middle of a solar eclipse as seen at Babylon and [as seen at] the center of the earth. Rawlins and I have used the difference as seen at Babylon, since that is the time that governs the visibility of a solar eclipse at Babylon. The times at the center of the earth are irrelevant.[12]

E37 In my calculations, which are described above, I found that the solar eclipse was not visible at any point east of Athens if the beginning of the lunar eclipse was visible at Babylon. This calculation, and not the figure of 55 minutes emphasized by *HS*, was the basis of my decision not to use the time of the lunar eclipse. This point is made quite clear in the book. As we may expect by this time, *HS* ignored this central result and focussed their attention on a side issue.

F The Parapegma of Geminus

F1 Now let us turn to the other example which *HS* give of my lack of understanding[13] "in making the crucial decisions about whether a report represents an observation or a computation." This example concerns the document called the parapegma of Geminus [ca. −100]. A parapegma is a document that gives the lengths of time the sun spends in each sign of the zodiac, the times of the heliacal risings and settings of various stars, and the weather conditions. All these are presumed to repeat at the same time each year. The

that it is Oppolzer.) In the midst of the same frenzy of accusations against another scholar's purported unreliability, *HS* also err in charging that Newton gives no absolute time. In fact, [Newton, 1976, p.130] says that Kugler's −424/10/23 solar eclipse time is 72^{m} nearer sunset than he thought. The Kugler and Newton sunset times should be virtually identical, so, since Kugler follows Oppolzer in using 20:31, this tells us that Newton's time for the 10/23 solar eclipse syzygy was 19:19. Then, subtracting −55^{m} fixes Newton's adopted time of the 10/9 lunar eclipse as 20:14, identical to Kugler's figure (from Ginzel). This precise agreement, as well as the correctness of the sign and the proximity of 19:19 to 19:14 (the DR-computed geocentric solar eclipse time: see following footnote), suggests an alternate explanation for the 55^{m} discrepancy of §E33, namely: in rough preliminary scratch-work (CBS 11901 was ejected from RRN's sample *very* early on), RRN accidentally compared his own 19:19 solar eclipse syzygy time to the Ginzel-Kugler time (20:14) for the lunar eclipse. Regardless, it is revealing that *HS* had to resort to such patently peripheral RRN figures (not even used in computing his book's results) as a basis for denigrating his hated conclusion on Ptolemy.

[11] Note by DR: Using RRN's value ET−UT = 5 hours for that epoch, and the standard *AENA* lunar acceleration ($-22''.44$) adopted by the book [Newton, 1976, p.315], I calculate Babylon mean solar time of conjunction: lunar eclipse 19:11; solar eclipse 20:07 topocentric (19:14 geocentric), a time difference of 56^{m}. By contrast, DR's adopted earth-spin acceleration (fractional $\dot{f} = -19\text{x}10\text{e}{-}09/\text{cy}$) yields ET−UT = 4 hours; and, using this with the $-25''.1$ lunar acceleration of Dickey & Williams (*EOS 63*:301; 1982), I calculate: lunar eclipse 20:32; solar eclipse 21:13 topocentric (20:35 geocentric), a time difference of 41^{m}.

[12] Note by DR: Newton is too merciful here. The times at the center of the earth are relevant to the question of the caution & expertise of the loftily sarcastic charges by *HS* (§E34) that the 55^{m} gap proves that "Newton knows something about calculating syzygies that no one else knows." What Newton knows is simply: one must of course include lunar parallax (accounting for the difference between the earth's center and the observer's location: 6400 km!) in a precise solar eclipse calculation for a specific place (such as Babylon) — a procedure which is familiar to every positional astronomer in history, even that bumbling old faker C.Ptolemy. Incidentally, remark here and elsewhere the difference in RRN's & *HS*'s attitudes regarding error-apprehension: RRN admires Kugler (and excuses his errors), merely hoping to improve his accuracy. By contrast, *HS* approach RRN as fundamentalists approach Darwin: the slightest perceived slip is leapt upon, with tyrannosaurian gentility, as happy proof that a hated general theory is entirely false and abhorrent.

[13] Note by DR: *HS*'s 2nd attack boomerangs. RRN's Geminos seasonal values are used to get a rough figure for very ancient Greek accuracy; so *HS* must denigrate these because it is *HS*'s job to portray ancient accuracy as terrible — *in order to make Ptolemy's errors seem not so ghastly as Newton has shown them to be*. Yet *HS* lack the minimal integrity to note the ironic fact that their own argument actually *increases* our estimate of early ancient Greek accuracy. For, when attacking Newton's acceptance of Geminos, they prefer the Eudoxos Papyrus — where the season-lengths of Kallippos (330 BC) are *remarkably correct*. Why do honest-scholarship-wardens *HS* never note the wonderful accuracy of Kallippos' work? (Neugebauer remarks it in his *HAMA* p.627: "These values agree so well with the facts that their origin from observations can hardly be in doubt." The Kallippos seasonlengths referred to are given below in fn 15. See the excellent analysis of Kallippos' data by van der Waerden in *Archive Hist Exact Sci 29.2*:115f.) Since Ptolemy worked nearly 500^{y} after Kallippos, I hardly see that *HS*'s carping undoes RRN's basic point (regarding prePtolemy Greek solar data accuracy) or supports the cutely sardonic remarks at their p.62 wherein *HS* try to present Euktemon's errors as a triumph for Ptolemism. (Keep in mind *HS*'s defense-lawyer-ploy: if Euktemon was inaccurate, then Ptolemy's huge errors look less inexcusable.) To the amusing contrary: all that *HS*'s attack on RRN has accomplished is to replace Euktemon with Kallippos and thus move (§F19) accurate early Greek observational astronomy merely 100^{y} later (less than 20% of the Euktemon-Ptolemy interval) *while making it far more accurate*. Brilliant *HS* strategy. (For solar data: Ptolemy's rms error is about 32 hrs, so RRN's estimate of Euktemon's rms error is only about 4 times better — but Kallippos' 5 hr rms error is 6 times better.) *HS*'s fantasy gets so out of control — and so oblivious to Kallippos' accurate work — that the review's concluding paragraph states (while snidely asserting the truth to be so obvious that the reviewers are almost too bored to comment): "There is no evidence for [Newton's alleged] 'vast body of accurate Hellenistic observations' . . . . except for Hipparchos *and Ptolemy himself*, there was little concern for observation and less for accuracy." (Emphasis added. With a smile.) Even Ptolemy-adulator O Gingerich asserts (*JHA 21.4*:364-365; 1990/11) that, since ". . . Ptolemy's parameters . . . seem generally more accurate than his data base", then "there is an invisible data base behind" his work. (So *HS*'s insults apply to OG.) Regarding the quality & integrity of genuine Hellenistic scientists' admirable accuracy (vs. that of Eratosthenes & Ptolemy), see D.Rawlins papers at: *Isis 73*:259 (1982), *Vistas in Astronomy 28*:255 (1985), and *American Journal of Physics 55*:235 (1987). Accurate observers ignored by *HS* include Timocharis (300 BC) & Aristyllos (260 BC).

parapegma is divided into twelve parts, which correspond to the times that the sun spends in each [zodicial] sign. Day 1 in the parapegma is the first day the sun spends in Cancer, which is also the day of the summer solstice.

F2 As a minor but illustrative point, *HS* date Geminus as "first century A.D." without qualification or justification. On the other hand, the article on Geminus in the *Dictionary of Scientific Biography* [Dicks, 1972] says that he flourished about 70 B.C., while *Pauly-Wissowa* [1894] gives his dates only as lying between −100 and +200. *HS* say that I date Geminus to ca.−100, although on one of the very pages they cite [Newton, 1976, p.162 n.2] I explicitly write that I only take his date to lie between −100 and +200, and that I use "ca.−100" only as a date to use in citation.

F3 The parapegma is obviously not based upon the personal observations of Geminus, since each entry is explicitly attributed to some earlier astronomer. For example, the entry for day 3 of Scorpio reads: "Stormy weather according to Dositheos." By far the greatest number of entries are taken from either Callippus, Euctemon, or Eudoxus.[14] The times of most phenomena are taken from only a single source. However, the entry for day 25 of Cancer says that it is the day of the morning rising of Sirius according to Meton, where the entry for day 27 says that it is the morning rising of Sirius according to Euctemon. In addition, the times of the equinoxes and solstices are given according to both Euctemon and Callippus. However, it is the lengths of time between the equinoxes and solstices, as attributed to Euctmon, that concern us here. That is, we are concerned with the lengths of the seasons implicitly attributed to Euctemon.

F4 Beginning with summer, the lengths of the seasons attributed to Euctemon are 92, 89, 89, and 95 days. On the other hand, there is a [papyrus] called *Ars Eudoxi* [Dinsmoor, 1931, p.317], written apparently about −200, which gives the seasons according to Euctemon, presumably as preserved by Eudoxus in a writing that is now lost in the original. Note that *Ars Eudoxi* is about a century and a half later than Eudoxus. *Ars Eudoxi* says that the seasons according to Euctemon are 90, 90, 92, and 93 days.

F5 *HS* write: "Newton considers the parapegma the work of Geminus, . . . and finds some very important information in it (pp.162-73, 291-97) that no one seems to have found before." Anyone can find the information which I used (which is limited to the lengths of the seasons attributed to Euctemon) who can read either the Greek text or the German translation in the edition published by Manitius, which is cited in the references as [Geminus, −100].

F6 *HS* also write flatly that the parapegma is a composition unrelated to the writing of Geminos. Other writers are not so dogmatic. Dicks [1972], for example, says more cautiously that the parapegma "probably" represents older material. Many other writers simply use the parapegma as if it were due to Geminus, without comment.

F7 I do not understand the point of the argument. The parapegma certainly represents earlier material, since it is composed entirely of quotations from earlier writers. I do not see any way to decide if such a compilation of quotations was made by Geminus or some other writer. Further, at least for our purposes, the point is unimportant. The important point is whether certain quotations about Euctemon are accurate.

F8 On this point *HS* write: "Now the durations . . . have *no* relation to any of the authorities named[15] . . . . But Newton, by reasoning he does not explain and we cannot

[14] Euctemon is usually credited, along with Meton, with having measured the time of the summer solstice of the year −431. Callippus and Eudoxus apparently belong to the following century.

[15] Note by DR: Personally, DR tends to agree with *HS* that the Geminos seasonlengths are not Euktemon's. However, I concur with RRN (§D14) that this point does not in the least undercut the RRN book's conclusions (quite the reverse: see fn 13). Moreover, there is a hilarious irony (which, again, RRN is too nice to mention) implicit in *HS*'s superior cocksureness that the Geminos durations are unrelated to authorities, and that this alleged error proves RRN to be *careless, unreliable, & intelligence-insulting*. For, *HS* have forgotten a little something written by their very own don-mentor O.Neugebauer. (S's decades of hitherto-flawless sycophancy & dutiful hatchetry in ON's service have earned S his rightful place as ON's recognized intellectual heir.) ON says, while contrasting the Geminos parapegma's data with those in the Eudoxos Papyrus: "From the *dates and intervals* given in the 'Geminus'

fathom, decides that they must be the work of Euctemon . . . and that they must be the result of observation. Never mind that the Eudoxus Papyrus [that is, what I have called the *Ars Eudoxi*] gives altogether different intervals for Euctemon; these are dismissed in a footnote."[16]

F9 To take up the last point first, it is true that I discuss the *Ars Eudoxi* only in a footnote, but this footnote is half a page long [pp.164-165]. In this footnote, I show that the seasons derived from the *Ars Eudoxi* are consistent with my main conclusions. This is hardly the same as "dismissing" *Ars Eudoxi* "in a footnote."

F10 The reasoning that I do not explain and which *HS* do not fathom is so simple that I saw no need to mention it. I simply took the seasons to be the work of Euctemon because they are derived from the dates of the equinoxes and solstices[17] explicitly attributed to him.

F11 We should also note that *HS* criticize my treatment of the durations (which means the lengths of time the sun spends in each zodiacal sign). This is another example of their carelessness; I barely mention the durations, and my discussion is limited to the seasons.

F12 As I have already mentioned, *HS* cite my discussion of the "durations" as an example of my lack of understanding "in making crucial decisions about whether a report represents an observation or a computation." Actually, my discussion about the durations, or rather the seasons, played no part[18] in deciding whether to admit or exclude any data, and the entire criticism of *HS* is irrelevant to their point.

F13 Since I did not use the lengths of the seasons in deciding whether to admit or reject data, I should mention why I did use them. I used them, in conjunction with other data, in order to estimate the standard deviation of a Hellenistic measurement of the time of an equinox or solstice. I used the standard deviation in turn to estimate the probability that certain errors in measurement could have happened by chance, but this probability did not enter into my decisions. I made the decisions before I calculated the probability in question.

F14 Most writers I have seen take it for granted that the seasons (and durations) given by Geminus are not those due to Euctemon while those given by Eudoxus are. I presume this is why *HS* write that the durations in Geminus have no relation to the authorities named. I also presume that the unstated reason for preferring Eudoxus is that *Ars Eudoxi* is older than Geminus.

F15 I do not see the reason for such assurance. Both *Ars Eudoxi* and Geminus are late writings presumably based, in the part that concerns us, on the writing of Euctemon. If there are errors in quotation, they are just as likely to be in the earlier quotations made by Eudoxus as in the later quotations made by Geminus.

parapegma (cf. above p.581 [in *HAMA*]) one finds, however, for Callippus s_1 = 92 [days], $s_2 = s_3$ = 89, s_4 = 95 (cf. below p.1352, Fig.4). The explicit statement in the papyrus seems to me the more reliable source." (*HAMA* p.627 n.9; emph added. See also reference to this useful ON discussion in fn 13.) Thus, no less a figure than O.Neugebauer himself entertained the idea that the Geminos seasonlengths related to at least one of the authorities cited. (Even *HS* admit at p.61 that the Geminos data for Kallippos are not far from his Eudoxos Papyrus values, but this resemblance does not cause *HS* to qualify their emphatic certainty that there is no relation.) Did the late ON ever know that his toppe syc has decreed that one of ON's own hypotheses is typical of the worst excesses of the intelligence-insulting Evil One's carelessness & unreliability?

[16] The emphasis in this passage is in the original. The "authorities" are Euctemon, Eudoxus, and Callippus.

[17] Note by DR: The Summer Solstice is attributed to Kallippos.

[18] Note by DR: At p.294 of [Newton, 1976], RRN mentions his Geminos-based 7 hr standard deviation for Euktemon in connection with the credibility of a supposed −28 hr error in his −431 Summer Solstice, but RRN adds that a smaller standard deviation could be induced independently. See also the discussions below at §F13, §F20-F21, & §G2. Thus, RRN is correct in stating that he did not depend upon Geminos in rejecting the reality of the grossly false Euktemon S.Solst time given by Hipparchos & Ptolemy (an innocent explanation of which is proposed here in ‡6 §E5). I must add that *HS* fail to note certain important points: [a] When RRN suggested that this S.Solst was fabricated, he knew that Ptolemy could not be responsible for the date and was explicitly cautious in leaving it an open question as to whether Ptolemy fabricated the hour [Newton, 1976, pp.296-297], [Newton, 1977, p.96], & here at §F20. [b] As regards Euktemon, his conveniently false (SS) datum is isolated and is from secondary sources (centuries later) — while Ptolemy's suspiciously agreeable data are by the dozen and are all found right in his own magnum opus.

F16 Now let us look at the durations given. Those given by Geminus, starting with the duration in Cancer, are 31, 31, 30, 30, 30, 29, 29, 30, 30, 31, 32, and 32 days. Those in the *Ars Eudoxi* are 30, 30, 30, 30, 30, 30, 30, 31, 31, 31, 31, and 31 days. *HS* write that the durations given by Geminus "may be partially based upon observation, but are still mostly schematic. . . ." I do not see how *HS* can possibly have the information needed to make this statement about the durations in Geminus. It is probably true of the durations given in *Ars Eudoxi*. They seem to be based upon observation to the extent that they yield a valid estimate of the length of the year (365 days), and that they put the sun's perigee reasonably close to the right place. Otherwise they are clearly schematic, and they may well come from computed Babylonian ephemerides.

F17 On the other hand, in spite of *HS*'s statement, the durations in Geminus are almost surely observed. Their variation is too great for them to be mostly schematic. In addition, they place the solar perigee more accurately than the durations from *Ars Eudoxi*. Now we have good reason to believe that Euctemon observed, or at least participated with Meton in observing, the summer solstice in the year -431. It is plausible that he observed other solstices and equinoxes if he observed this one, and thus it is plausible that his durations, or at least his seasons, are based upon observation.[19] If so, his seasons are not those derived from *Ars Eudoxi*.

F18 In sum, Euctemon's lengths of the seasons are more likely to be those in Geminos than those in *Ars Eudoxi*. In this connection, the work of Pritchett and van der Waerden [1961] is interesting. They take all the quotations from Euctemon in Geminos to be genuine except the durations. I suppose this is a possible situation, but I would not wish to uphold it as dogma.

F19 Even if the seasons given in *Ars Eudoxi* should prove to be those due to Euctemon, this would not affect any important conclusion or decision that I reached. The seasons given by Geminus are still an ancient Greek set of seasons which show the accuracy that I stated. The only change needed would be that I could not attribute this accuracy to the time of Euctemon but only to the time of Callippus about a century later.

F20 When I attributed this accuracy (a standard deviation of about 7 hours) to Euctemon, I used it for only one purpose. I had already concluded from an analysis of ancient Greek solstices that the exact time of the solstice attributed to Meton and Euctemon was fabricated by someone about the year -100 for an entirely different purpose, and that this fabricated time is the only one that has survived in the literature. [See fn 18.] I used the standard deviation only to calculate the statistical confidence level that we can attach to this analytical conclusion. However, the statistical confidence level is quite high no matter what we assume about the accuracy of Euctemon's (and Meton's) measurements. The reason for this seemingly paradoxical statement is given by me in another work [Newton, 1977, pp.343-344]. [Note by DR: the reader is urged to consult the important discussion here cited. See also §G5.]

F21 One final remark should be made. It is possible that both sets of seasons attributed to Euctemon were actually used by him. He might have used the schematic seasons given by *Ars Eudoxi* in his early work before he had done much observing. Then, after he had made measurements of the seasons (perhaps in conjunction with Meton), he adopted the set of seasons, based upon measurement, which we find in Geminus.

F22 In summary of this section, it is likely that the lengths of the seasons given by Geminus are due to Euctemon, in spite of the dogmatic statement by *HS* that they are not. Even if they are not due to Euctemon, this would not affect any important conclusion that I reached. In particular, contrary to the claim of *HS*, this would not illustrate my lack of understanding the sources "in making the crucial decisions about whether a report represents an observation or a computation." Attributing the seasons to Euctemon did not play any part in chosing whether any report represents an observation or a computation.

[19] It is possible that Euctemon measured only the lengths of the seasons but not the individual durations. In this case, his durations would be schematic ones made to fit the lengths of the seasons. They are still based upon much more observation than the durations in *Ars Eudoxi*.

G Ptolemy's Alleged Observations

G1 I shall take the space to mention one other *HS* error. This concerns the astronomical observations that Ptolemy claims to have made himself. *HS* say of these that Newton "does not think highly of Ptolemy's observations, in fact, he believes they are all fraudulent. His reasoning is based mostly upon demanding rather great precision [of the ancient observations] . . . and then saying that Ptolemy's observations, which do not meet such standards, must be fraudulent." After some remarks about the equinoxes and solstices[20] "observed" by Ptolemy, they go on to say: "Through similar reasoning, but not much evidence, the argument is extended to *all* [emphasis in the original] of Ptolemy's observations, and Newton 'proves' [quotation marks in the original] his point by computing probabilities like 10^{200} to 1 that the observations are fraudulent."

G2 This misrepresents my reasoning in almost every respect. To start with, my reasoning is not based in any way upon demanding great precision of ancient observations. It is based upon the exact agreement, to the level of rounding used, between Ptolemy's alleged observations and the theories he pretends to derive from them. For example, consider the observations of the [three] equinoxes and the summer solstice that Ptolemy claims to have made himself. On the basis of these [four] observations, Ptolemy claims then to prove that Hipparchus's theory of the sun, derived almost three centuries earlier, is still valid in Ptolemy's own time, even to the exact values of the parameters.

G3 However, if we calculate the times of the equinoxes and solstices in question from Hipparchus's theory, maintaining a precision of more [better] than an hour in the calculations, and then round to the nearest hour, we get exactly the times that Ptolemy states. This is so, even though Ptolemy gives his results with a precision of an hour while Hipparchus gave his results [DR note: on which the whole solar theory is based!] with a precision of only a quarter of a day. Our intuition tells us that such agreement is impossible, and this conclusion does not depend upon the precision "demanded" of ancient Greek observations.

G4 In spite of this, numerous writers have claimed that Ptolemy's measurements were the result of chance errors in observation. To counter these claims, I felt it necessary to estimate the probability that Ptolemy's results could have happened by chance. Now, compared with modern theory, Ptolemy's [four] solar observations are all in error *in the same direction* by the order of 30 hours, but they all agree with preassigned values (from the [solar] theory of Hipparchos) within half an hour (half the rounding level used). I have estimated the probability[21] that all this could have happened by chance at 10^{-92}.

[20] Note by DR: One of the more amusing moments in the *HS* review, which RRN is too polite to note, is *HS*'s sarcastic mock astonishment while commenting upon a key RRN discrimination: "most remarkable of all, that solstices could be observed with more accuracy than equinoxes." That RRN is correct (in the very judgement which *HS* attack as "remarkable" folly) is obvious to any unprejudiced scientist familiar with the instrumental problems involved. (See the lucid discussion at [Newton, 1977, pp.81-82].) One notes that *all* known ancient astronomical observers (excluding Ptolemy, who did not observe) depended primarily upon solstices for gauging the year's length: Meton, Euktemon, Kallippos, Aristarchos, Archimedes, Hipparchos. (Hipparchos observed numerous equinoxes, but even *his* yearlength was based upon solstices: see ‡6 eq. 8.) However, Swerdlow, an historian with the official rank of professor in the Dep't of Astronomy at the Univ Chicago, cannot understand this elementary point: during a gloriously delirious passage (p.527) in his prominent 1979 attack on Newton (*American Scholar 48*:523; and see fn 6 & ‡6 fn 6), Swerdlow argues: "At the time of the solstice, the meridian altitude of the sun changes by less than fourteen seconds of arc per day, and measuring this quantity, let alone any fraction of it, was obviously ridiculous." The only ridiculous aspect of this astounding piece of reasoning is that a member of *the University of Chicago's Dep't of Astronomy* should so conspicuously exhibit his touching innocence of the implications of first-year calculus and of the standard technique known as "equal altitudes". It is easy to see that Hist.sci archon Swerdlow's reasoning is essentially equivalent to insisting that the time a vertically oscillating body reaches maximum altitude cannot be determined since at that moment it lacks vertical motion!

[21] The probability of 10^{200} to 1 that *HS* quote (see p.149 of the book under review) is based upon a larger set of data.

G5 I have pointed out at [Newton, 1977, p.90] that I do not mean for probabilities like 10^{-92} to be taken literally. For one thing, I used the normal law of error in calculating the probability, but there is no reason to assume that the normal law applies in these extreme circumstances. For another, I assumed a specific standard deviation for a single measurement, and one may question the standard deviation used. However, as I pointed out on page 92 of [Newton, 1977], a work available to *HS* [and cited by them] when they wrote their review, it does not matter much what law of error we use or what standard deviation. [See §F20.] The probability of chance occurrence is vanishingly small, far beyond the level of ordinary experience. This conclusion does not come, as *HS* claim, from my "misuse of probability" that "insults the intelligence of the most naïve reader."

G6 *HS* write that I extend my argument (about the solar observations) to all of Ptolemy's observations. This is not correct. My conclusion (page 493 of the book under review) is: "All of his own 'observations' that Ptolemy actually uses, and that are subject to test, prove to be fraudulent." The two qualifications are important. First, there is not enough information to let us test some of the observations he claims to have made for each outer planet. While personally I have no doubt that he fabricated these observations, this feeling is based upon his usual method of doing business, and I exclude these observations from my general finding. Second, there are some stellar observations [Newton, 1974] which Ptolemy claims to have made but which he does not use [12 declinations]. These observations disagree with his theories, but he does not use them in any way, and they pass all the tests for genuineness. However, the fact that he included these discordant observations in his work, without pointing out that they are discordant, increases the evidence that Ptolemy's work is a deliberate fraud.[22] It also suggests that Ptolemy did actually make some observations but he does not use them.

G7 Finally, *HS* claim that I extend my argument to all of Ptolemy's observations without much evidence. They could not have written this if they had read my book with any attention. Altogether, I base my conclusion upon a detailed analysis of the following sets of observations:

[a] measurements of the times of equinoxes and solstices,
[b] a measurement of the lunar evection,
[c] several measurements of the obliquity of the ecliptic,
[d] a measurement of the latitude of the site where Ptolemy claims to have made his observations,
[e] several measurements of the inclination of the lunar orbit,
[f] a measurement of the maximum lunar parallax,
[g] several measurements of the apparent solar diameter, and
[h] all of the planetary observations.

This includes almost all of the observations that Ptolemy claims to have made, and I included all of the others in a work [Newton, 1977] that was available to [& cited by] *HS* when they wrote their review. This cannot be seriously described as "not much evidence".

H The Integrity of Book Reviews

H1 In summarizing the quality of the review by *HS*, I cannot do better than to paraphrase one of their statements (§D13) about my book: The review by *HS* "is careless and unreliable to the point" that it should be read only be someone "who is prepared to examine every source". In other words, no statement in the review, no matter how simple, can be taken as accurate, although a few minor statements are correct.

[22] Ptolemy pretends to choose [the stars he uses] at random from a table containing many stars. Yet "by accident", the ones he uses are the ones that agree with his theory while the others are ignored.

H2 Since the review appeared in a scholarly journal, many people who are not particularly acquainted with the [Ptolemy controversy] situation[23] will probably take the review as valid. That is, the appearance of this review does damage to the field of learning involved, rather than promoting it. It is what a friend of mine calls "a subtraction from the sum of human knowledge."

H3 The problem is how to inhibit the appearance of such incompetent reviews in the scholarly literature.[24] I see only one general way to do this. This is to require that book reviews, like research articles, be subject to refereeing. That is, a book review, before it is accepted for publication, should be refereed, and the editor's decision to accept or reject the review should be made in light of the referees' report. I do not say that the editor should necessarily follow the referees' recommendations, but he should at least know what they are.

H4 Further, the author of the book in question should receive a copy of the review, and be given an opportunity to comment. He should be particularly on the lookout for factual errors such as those committed by *HS* in their review. If he wishes, the author should have the opportunity to write a rebuttal to the review, to be published immediately after the review and in the *same* issue of the journal.

H5 It will probably take much discussion to decide upon the way in which this policy should be implemented, and I can only make some suggestions. I suggest that a review should be sent to at least two referees, just as a research paper is sent by the best journals. In addition, a copy of the review should be sent to the author under review. The editor should not make his decision until he has received and studied the comments of the author and referees. Of course, if the referees and-or the author fail to send in their comments in a reasonable time, the editor should proceed without them. When he sends out the copies to the referees and the author, the editor should make the time limit known to them; I suggest it as reasonable to require that comments should be sent to the editor within three months.

H6 In summary, book reviews should be subject to the same scholarly standards that research articles are, with the additional requirement that the author of the book should have an opportunity to comment on the review, and if he sees fit, to write a rebuttal.

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[23] Note by DR: the Book Review Editor at the *JHA* who commissioned the review is O Gingerich, Ptolemy's prime public hagiofish, 2nd only in *JHA* rank to the unique Editor-for-Life.

[24] Note by DR: There is another issue relevant to this matter. Numerous journals' correspondence columns will not print replies to book reviews. (Indeed, last time I looked, the egregious *JHA* did not even *have* a correspondence column — as befits a journal that is operated by an Editor-for-Life whose response to dissent has included such openminded behavior as that noted at ‡1 fn 25, ‡6 fn 15, & ‡8 fn 35.) That being the case, an organized assault (upon a dissenting view), carried out in centrist-journal book reviews, permits the dissenter little or no reply space. This technique is one of the neatest (among those which certain embarrassed cliques employ) for protecting cherished nonsense, even for decades on end — e.g., the sacred Muffia tenet that a clumsy faker like Ptolemy was "the greatest astronomer of antiquity", as O Gingerich has publicly decreed (echoing Neugebauer's *HAMA* p.931) in OG's 1976/8/6 *Science* book review of *HAMA*. This review's gratuitous cracks at R.Newton (mild compared to Swerdlow's) were, as usual, protected from the slightest printed reply.

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[25] Note by DR: This work is the famous *Syntaxis*, otherwise known as the *Almagest* or (DR) *Almajest*. In the Greek title, $\overline{\iota\gamma}$ (13) is inadvertently misprinted as $\overline{\tau\gamma}$ (303) at Pedersen *Survey* 1974 p.15.

[26] Note by DR: In a quite different arena, Denmark's Heiberg was the scholar who (even while he was working on Ptolemy) Latinized the text for Danish composer Carl Nielsen's glowing choral work, *Hymnus Amoris* (1896).