‡4 Book Reviews

Reviewer: Hugh Thurston[1]

A James Evans's *The History & Practice of Ancient Astronomy*

A1 This[2] is the first book on early astronomy to be designed as a text-book and an excellent[3] text-book it is. It lives up to the word "practice" in the title with very practical and relevant exercises, including: making and using plots of the shadow of a gnomon; using astronomical tables; making a sun-dial, an astrolabe, an aequatorium, and an ingenious device called "Ptolemaic slats"' for finding quickly and conveniently the longitude of a planet on Ptolemy's theory; and forecasting synodic phenomena using Babylonian systems A and B.

A2 The book is really a history of *western* (principally Greek) astronomy rather than a history of astronomy. It treats the relevant parts of Babylonian astronomy adequately and clearly, as well as treating Islamic and mediaeval European astronomy reasonably well. It makes only the briefest mention of Indian astronomy although the Indian theory of epicycles, clearly inspired by the Greek theory, has resemblances and differences that are both interesting and significant. And the fact that Aryabhata had a rotating Earth would be worth mentioning.

A3 There are a pleasing number of quotations from ancient authors, and when an account of early work is from a secondary source Evans conscientiously points this out.

A4 There is also a clear account of the general historical background. Now for some details that particularly struck me.

A5 On page 40 is an impressive juxtaposition of early Babylonian and late Greco-Egyptian zodiac figures, showing how alike they are even in details.

A6 On pages 63 to 65 Evans explains Eratosthenes's distance of 5000 stades from Syene to Alexandria as a rough estimate rather than a real measurement. He does not mention Laplace's suggestion that Eratosthenes was unwittingly using a previous estimate obtained astronomically, nor Dennis Rawlins's confirmation of this using evidence from an ancient map of the Nile [1].

[1] Professor Emeritus of Mathematics, University of British Columbia. Footnotes are by D. Rawlins.

[2] Oxford University Press, 1998, $65. Following the suggestion by Rob't Newton in *DIO 1.1* ‡5 §H, Evans was informed that *DIO*'s Thurston was planning to review his book and was asked if he wished to receive a pre-publication copy of the review so that he could respond in the same issue. Evans's long-delayed reply (received as we were going to press) indicated that he would like a pre-publication copy of the review but that he was hesitant about publishing any of his wisdom in *DIO*. Thurston responded by suggesting Evans quickly contact DR for a copy of the proofs of the final edition of the review, but Evans did not do so.

[3] One feature deserving of particular commendation: in the Notes section at the back of the book, a header tells the reader just which parts of the book that page's notes apply to.

A7 Page 72 contains a neat explanation[4] of how Aristarchus arrived at his value of 1/30 of a right angle (3°) for the amount by which the elongation of a half-Moon falls short of a right angle — an amount that could not possibly be measured directly.

A8 Pages 94 and 95 describe Greek ideas about the Milky Way, a topic that is all too often ignored in histories.

A9 On pages 121f, Evans describes how Hypsicles used computations similar to those of the Babylonians. This little-known fact is relevant to the rather difficult discussion of just what details of Greek astronomy were derived[5] from the Babylonians. We know that the signs of the zodiac were, and so was sexagesimal computation, but we are not sure how much else.

A10 Evans's treatment of the tropical year is a little confused. On page 166 he quotes its length as "about 365.2422 days",[6] but in spite of the word "about" immediately mentions "measuring to such precision". In fact, the tropical year deviates[7] from the average yearlength, varying about in the range 365.23 to 365.25 days, as you can check by looking up and differencing the times of recent consecutive solstices or equinoxes. So the figure quoted is an average, and every time Evans mentions "the" length of the tropical year, the word "average" or "mean" should be inserted. So the remark (page 208, line 8) that it is necessary to show that the year has constant length implies that it is necessary to show something that is not true. [And even the year's average is not constant over millennia: fn 6.] What *is* necessary is to show that the long-term average does not vary [enough to be perceptible to naked-eye astronomers in a few centuries]: for example, that the average between the solstices of 432 BC and 140 AD is the same as the average between 280 BC and 135 BC to within the accuracy possible then. Hipparchus realized this: he claimed only that there was not enough evidence to show that the length of the year varies.

A11 On page 209 Evans comments on Ptolemy's "observations" of a solstice, which were out by a day and a half, and his three equinoxes, which were each out by roughly a day. In spite of these substantial errors, the times are — to the one-hour precision given — just what would be obtained by, as Delambre suggested,[8] mere calculation from earlier astronomers' observations. (See ‡1: §A, Table 1, and eq. 1.) Evans suggests that a "less radical" explanation is that Ptolemy selected from "many discordant" observations those that best agreed with his calculations and adjusted the quoted times to get exact agreement. It is a matter of opinion whether this is less radical (in my opinion it is substantially more radical) but it is certainly much less plausible. How on Earth could Ptolemy make observations so wildly inaccurate that he can select equinox and solstice times roughly 1 1/2 days too late, nearly three centuries after astronomers were quoting such data to 1/4 day precision and getting most of them about that accurate? In any case, deliberately selecting an extreme value to agree with a calculated one, and then further fudging the time to get exact agreement[9] while claiming to have made the observations "with the greatest

[4] Evans's argument is that Aristarchus got his 3° figure from "assuming the *largest imperceptible inequality* between" the crescent and gibbous portions of the month (emphasis in original). Question: how were these two portions' sizes gauged? Inevitable answer: by visually judging when half-Moons occurred — which is the very same problem one was originally attempting to solve by the foregoing "explanation". That Aristarchus's 3° estimate was not a precise figure but rather an upper bound (based upon an accurate human visual-discernment limit of 1/10000 radians) was first proposed at uncited *DIO 1* ‡7 §C1 and ‡9 fn 272.

[5] Models of celestial motion seem not to have been part of Babylonian astronomy. See Evans p.214.

[6] At page 166, Evans uses this figure — and its remainder (vs the Julian year) of 0.0078 days — in calculations involving classical antiquity, an era when the actual mean yearlength was nearer 365.2425 days.

[7] This is mostly due to the combined effects of nutation and perturbations (of the Earth's orbital motion) by the Moon, Venus, & Jupiter.

[8] Evans deserves credit for (at page 458 note 8) fully citing where Delambre's arguments are to be found.

[9] See *DIO 7.1* ‡1 §G11.

accuracy", is dishonest; so here Evans is, perhaps unwittingly, on the side of those accusing Ptolemy of fraud.

A12 Near the bottom of page 205 Evans describes a snag in using a gnomon: the tip of the shadow cannot be located precisely because the shadow is fuzzy, and Evans says that this is because the Sun is not a point-source of light but has an angular diameter of 1/2°. You can check for yourself that this cannot be the explanation. Set up a pole a couple of metres high on a sunny day: you will find that the fuzz covers a couple of millimetres. Now calculate the length of the penumbra cast by a source 1/2° in diameter: it is several centimeters, depending on the altitude of the Sun. The facts are that: [a] most of the penumbra is invisible rather than fuzzy, and [b] the gnomon measures the altitude of the upper limb of the Sun. (Incidentally, this would explain Eratosthenes' error in his famous measurement of the angle between Syene [Aswan, near the Tropic of Cancer] and Alexandria [2].)

A13 On pages 251 to 255 Evans gives the clearest explanation that I have seen of how Ptolemy dealt with the change[10] in the parallax of the Moon between two observations[11] half an hour apart.

A14 Starting on page 259 Evans explains how Ptolemy "confirmed" his erroneous value for the precession (1° in 100 years; it was actually 1° in about 72 years) by using several occultations of stars by the Moon, and accounts for the confirmation of an erroneous value by defects in Ptolemy's theory of the motion of the Moon (defects which in all cases produce errors of the right magnitude and in the right direction to give Ptolemy the figure that he wanted!). Evans mentions Delambre's suggestion that the observations were not actually made but the details were calculated from the desired result (as were Ptolemy's solstice and equinox "observations"). He does not mention, however, that Robert Newton has shown clearly how they were fabricated [3].

A15 To find the parameters for Ptolemy's theory of motion of an outer planet Evans (page 362) uses a method quite different from Ptolemy's. He finds the apogee from the longitudes between successive oppositions. Ptolemy found the apogee and eccentricity simultaneously by a most ingenious method of successive approximation using three oppositions — a real geometric tour de force. It is a pity not to mention it, even if a detailed description is too complicated for a text-book at this level.

A16 Page 423 gives the first clear explanation that I have seen of why the fact that the orbit of Mars in Tycho's solar system crosses the Sun's orbit rules this system out for people who believed in real crystalline spheres; but the fact that the orbits of Marcury and Venus do so was never mentioned (as Evans rightly notes).

A17 Most of the section on the catalogue of stars is concerned with the controversy over whether Ptolemy measured their coordinates, as he claimed, or instead updated a catalogue made at the time of Hipparchus.

[10] Without explanation, Ptolemy almost perfectly estimates the change as 5′. (It was actually 6′, as noted by Britton p.144 of the work cited at ‡1 fn 30.) The fact that the rate of change in parallax is in general minimal when the Moon is near the horizon, may be one more reason why Hipparchus systematically used the young crescent Moon for post-sunset placement of his principal stars. See *DIO 1* ‡6 §G2 and ‡9 footnote 288.

[11] These are the 139/02/23 observations treated in ‡1 §D. Note that page 255 gives for Alexandria a latitude which is too low by 72 geographical miles (30° vs actually 31°.2). Curiously, the same error was made by Włodarczyk (in material appended to an Evans paper, ref [8], and cited in its n.64) when discussing this same observation. See p.187 of the work cited in ‡1 ⊙8. Włodarczyk's Alexandria latitude (30°.4) is really that (30°22′) for the Lower Egypt klima's parallax table (*Syntaxis* 2.13). If Ptolemy did not interpolate, he might have used this table. But that fact does not alter the actual latitude of Alexandria. (A mixing-up of city and klima also occurs at Neugebauer *History of Ancient Mathematical Astronomy* 1975 p.305, but at least it is conscious. On the outcome, see *DIO 4.2*:55-57.) Evans also suggests (page 255) Ptolemy might have used a "table of ascensions" to find the time of day. If so, it need not have involved the Lower Egypt table for 30°22′, since the *Syntaxis* 2.8 "Sphaera Recta" table would easily yield right ascensions of 58° for the end of Taurus and 335° for Pisces 3°, and the 83° difference is indeed about 5 1/2 hours.

A18 The first[12] evidence adduced in favor of the theory that Ptolemy merely updated is the systematic shortfall of about 1° in the longitudes. If Ptolemy updated a catalogue made some 2 2/3 centuries earlier using his erroneous value for the rate of precession, that would produce this shortfall. Dreyer suggested an alternate explanation: the longitudes were obtained using the calculated longitude of the Sun, and Ptolemy's theory had the Sun's longitude 1° too low in his own time. Evans does not mention that Rawlins [4] has disproved this by showing that it would imply a periodic error in the latitudes which does not in fact exist.[13]

A19 The strongest evidence for Ptolemy having updated Hipparchus's catalogue was presented by Robert Newton. In describing Newton's investigation, Evans denigrates him by writing "the longer he worked the angrier he got", a remark quite out of place in a text-book. (In my opinion, it would be out of place anywhere.)[14] The evidence is that latitudes (which Ptolemy would not have corrected for precession) are whole degrees more often[15] than would be expected. (The fraction endings for latitudes are 0, 1/6, 1/4, 1/3, 1/2, 2/3, 3/4, 5/6. The 0 is not written but left blank.) Newton explained this by suggesting that the measurements were made with an instrument probably[16] graduated in whole degrees, and the fractions were estimated by eye. For the longitudes the commonest fraction was not zero but 2/3. Adding a whole number of degrees plus 2/3° to the raw observations would account for this.

A20 Moreover no longitudes end in 3/4 and only five (out of over a thousand), all of stars near the ecliptic,[17] end in 1/4. Adding 2/3 to the fractions of §A19 would result in fractions 2/3, 5/6, 11/12, 0, 1/6, 1/3, 5/12, and 1/2.

A21 The 11/12 & 5/12 are anomalous. Rounding them up or down would leave precisely the endings that actually occur in the catalogue longitudes (leaving aside the five with 1/4). So Newton's suggestion explains the difference between the distribution of endings for longitudes and latitudes. No-one else has produced a reasonable explanation for this.

A22 Evans tries to refute Newton's argument by showing that it leads to a false conclusion if applied to Ulugh Beg's star catalogue, where the commonest ending for latitudes is zero and for longitudes is 55′. Evans claims that applying Newton's argument would lead to the conclusion that this catalogue was also plagiarized. Evans is simply wrong: the argument implies no such thing. Newton's argument is that the excess of 2/3° implies that the catalogue in the *Syntaxis* was made by adding precession to longitudes measured a whole number plus 2/3 centuries before its epoch. It is other evidence — the 1° shortfall in longitudes — that identifies the whole number as 2, bolstered by the existence of a prominent astronomer at the relevant date.[18] Applying Newton's argument to Ulugh Beg's catalogue merely shows that the date of the observations differs from the epoch (1437.5) chosen for the catalogue by just the right amount to decrease the longitudes by a whole number of degrees plus 5′ at Ulugh Beg's nearly accurate rate of precession (which was 1° in 70 years). Just six years would do it. The given epoch of the catalogue is year 841

[12] For compact catalogues of evidences against Ptolemy's observership of the catalogue of stars, see *Amer J Physics 55*:235 (1987) p.236 and especially *DIO 2.3* (1992) ‡8 §C22.

[13] Also the northern longitudes. DR's error-wave-refutation of this Gingerich-Evans theory is found in [4]. But Evans 1998 repeats the theory anyway at pp.270-271. See similar imperviosity in *News Notes*. (And see *DIO 2.3* ‡8 §§C10f, C25, C31, and footnote 31.) There is a scene 40 minutes into the 1997 film *Private Parts* in which H.Stern tries to play frisbee with an everyways-challenged person whose hand never even moves to catch the disk, so it keeps hitting him in the face. Again & again. . . .

[14] The remark also happens to be untrue. DR knew Newton well for many years. He expressed no anger at either Ptolemy or his defenders.

[15] See ‡1 §N3.

[16] See ‡1 fn 39.

[17] See *DIO 2.3* ‡8 fn 20 and *DIO 6* ‡1 fn 108.

[18] And Pliny 2.95 testifies that Hipparchus compiled a comprehensive star catalogue.

in the Islamic calendar,[19] so Newton's argument shows that it was based on measurements made in $847 \pm 70x$, where x is a whole number. On p.270, Evans comes to the correct conclusion that the measurements were made in about 847 (1443 AD) but does not realize that he is using Newton's argument in doing so.[20]

A23 The controversy has been put to rest beyond all doubt by Shevchenko and Rawlins. Shevchenko [5] discovered that the anomalous[21] distribution of fractions for the longitudes does not apply to the stars in the southern constellations, so they were not measured directly by an instrument graduated in whole degrees. However, Rawlins [6] has discovered[22] that if he precesses the longitudes in the catalogue back by $2^\circ 2/3$ and computes the declinations, the anomalous distribution shows up very clearly in them. So Hipparchus must have measured declinations[23] (which is in any case the easiest coordinate to measure) and one other coordinate. (Likely other coordinates are found on page 103 of the Evans book.)

A24 Evans also mentions Delambre's remark, expanded on by Rawlins [4] (with explicit credit to Delambre),[24] that the catalogue failed to contain some stars that were too far south to be seen from Rhodes, where Hipparchus worked, though they were visible from Alexandria, where Ptolemy worked. Evans suggests that perhaps Ptolemy did not intend to catalogue stars right down to the horizon and cites Tycho's early catalogue of 777 stars which had only one star (Fomalhaut) with an altitude less than $4^\circ.3$ at culmination. However, Evans accepts that Tycho's later catalogue, intended to contain at least 1000 stars, went down to 2°.[25] The suggestion that Ptolemy's 1000-star catalogue (which stops short at 6°) is more like Tycho's 777-star catalogue than his 1000 star catalogue is, to say the least, not very compelling.

A25 Evans lists the magnitudes of six stars in the *Syntaxis*'s catalogue and their magnitudes (calculated by him) as seen at Rhodes and at Alexandria, which (assuming that what Ptolemy or Hipparchus meant by magnitude m is what we mean by magnitude m)[26] are more consistent with Alexandria than Rhodes. Stars so near the horizon are vulnerable to atmospheric extinction. Rawlins [7, §H7 and footnote 18] says that Evans's formula for extinction "breaks down badly" at low altitudes, and calls it "over-opaque".[27] Evans's pre-

[19] That is, every catalogue star's observed longitude was reduced by $5'$ in order to bring it down to an epoch 840 lunar years (of 354.37 days each), or very nearly 815 solar years, after the Hegira (mid-622 AD). Note that the lunar-calendar epoch Ulugh Beg thus arranged for his catalogue is almost exactly 13 solar centuries after Ptolemy's epoch (137.5).

[20] Having deduced the observations' correct date, Evans does not then point out that (since Ulugh Beg was still active in 1443) this date undercuts the case for the very plagiarism which Evans had just said (p.269) Newton's argument would suggest for Ulugh Beg.

[21] The expected random distribution is discussed at ‡1 §N5 and displayed in ‡1 Table 3.

[22] At least as crucial (to the controversy over the star catalogue) is the groundwork discovery in [6] that the distribution of the southern latitudes had no above-random excess of zeros, the very feature underlying the northern stars' high excess of 2/3 degree fractions. This countered the objection (in [5]) that Newton's hypothesis was weakened by the southern longitudes' lack of an excess of zeros. On this issue, note also ‡1 fn 38.

[23] Declinations are directly found by subtracting culmination zenith distances from the observer's latitude. Rawlins found that such data show a significant excess of zeros (see [6] §E) in the southern part of the star catalogue if the cataloguer was observing at the south tip of Rhodes.

[24] A point not clear from Evans's discussions: p.268 of the book, or [8] p.165.

[25] If Tycho's g Centauri (1597 Hven culmination altitude $2^\circ 00'$) is set aside (fn 28), the next lowest star is Fomalhaut at $2^\circ.6$. One notes that, if Hipparchus is understood to be the observer of the ancient catalogue, his lowest star's transit altitude was at most $1^\circ.8$.

[26] Evans investigates this question in a useful study summed up in Fig.12 of [8].

[27] See also [7] footnote 65, and *DIO 3* §§L8-L9, which includes Ptolemy's plain contradiction of the Evans extinction formula's applicability to antiquity. Evans believes that a modern (industrial-era) extinction coefficient of 0.20 magnitudes per atmosphere is best for antiquity (see [8] p.260), though he also computes parallel figures generously assuming 0.17 magnitudes per atmosphere. (By either coefficient, g Centauri would always be far dimmer at late-16th century Hven than anything Tycho

ferred formula (fn 27) for finding post-extinction magnitude gives g Centauri (included[28] in Tycho's catalogue) a magnitude of 8. It also gives Fomalhaut a magnitude of 4 1/2, whereas Tycho lists Fomalhaut as first magnitude.[29]

A26 Evans, on the other hand, not in this book but in [8], says that Rawlins's estimates of the magnitudes are too low, though he admits [8, note 41] that this would not affect Rawlins's conclusions.[30] To follow this topic further you would, unless you are an expert on atmospheric extinction, have to plough through appendix I of [8] and footnotes 18 and 63 of [7].[31]

A27 Evans writes "if we have devoted more space to this controversy than it seems to deserve, it is for two reasons". If I have devoted more space in this review than deserved, it is for one reason: this is a topic that will surely interest readers of *DIO*.

A28 To sum up, this is an excellent text-book, and it is a pity that it is marred by an attempt to have the last word[32] in the controversy over the star catalogue. But all the reader needs to do is to ignore this section, forgive the systematic failure to cite relevant work by Newton and Rawlins, and get down to enjoying the book's practical and revealing exercises.

References

[1] D Rawlins, "The Eratosthenes-Strabo Nile map", *Archive for History of Exact Sciences 26.3*:211-219 (1982).

[2] D Rawlins, "Eratosthenes' geodesy unraveled", *Isis 73.2*:259-265 (1982), especially page 263.

[3] Robert R Newton, *The Crime of Claudius Ptolemy* (1977) pages 225-237.

[4] D Rawlins, "An investigation of the ancient star catalog" *Publications of the Astronomical Society of the Pacific 94*:359-373 (1982).

[5] M Shevchenko, "An analysis of errors in the star catalogues of Ptolemy and Ulugh Beg", *Journal for the History of Astronomy 21*:187-201 (1990).

[6] D Rawlins, "Hipparchos' Rhodos observatories located", *DIO 4.1*:33-47 (1994) pages 39 to 41.

[7] D Rawlins, "Tycho's 1004-star catalog's completion was faked", *DIO 2.1*:35-50 (1992).

[8] James Evans, "On the origin of the Ptolemaic star catalogue", *Journal for the History of Astronomy 18*:155-172 & 235-278 (1987).

ever recorded.) But Ptolemy's cited testimony is consistent with a figure less than even the value used in [4] (0.15 magnitudes per atmosphere).

[28] In [7], Rawlins contends that the Tycho Centaurus stars' grossly erroneous four longitudes were faked (by Ptolemy's suspected method), and that the only data observed for them were crude zenith distances taken (1598.0) by cross-staff at Wandsbek for finding declinations. See fn 27 and [7] Table 2, §§C1, G2, H8, footnote 56.

[29] But, according to Evans's prime argument in favor of Ptolemy, Tycho should have listed Fomalhaut as at least 4th magnitude. This is the kind of consequent one ends up with, by assuming (Evans 1998 p.272 and [8] pp.262-266) that Hipparchus would, in a star catalogue famous throughout the civilized world, list magnitudes that were appropriate only to his own latitude. . . .

[30] Conversely, Evans's argument at §A25 would be not fatally weakened by accepting Rawlins's extinction coefficient. (For the argument's main difficulty, see fn 29.)

[31] Footnotes 16-18 and 63 of [7] provide Rawlins's formulae for refraction and extinction, which are conveniently compact, while being accurate and inclusive enough for professional use.

[32] A last-word fantasy is the prime cause of this otherwise entertaining and informative book's (selectively) anachronistic bibliography — and is a 3rd reason (left unstated at §A27's source [Evans 1998 p.273]) why Evans has republished the attorney's brief he presented in [8].

B Noel Swerdlow's *The Babylonian Theory of the Planets*

B1 This[33] is the most substantial book on Babylonian astronomy to appear since van der Waerden's *Science Awakening* and Neugebauer's *History of Ancient Mathematical Astronomy*. Unlike these two works and the delightful chapter on Babylonian astronomy in Neugebauer's *The Exact Sciences in Antiquity*, this work, of just over 200 pages, concentrates, not just on the theory of motion of the planets, as the title suggests, but on one aspect of the theory, namely how the Babylonians found the parameters that they used.

B2 The book starts with an extensive selection of extracts from Babylonian works dealing with celestial omens and recording observations of the positions of the planets. Examples are

> If in Tammuz Mars becomes visible the cemetery of warriors will enlarge. If a planet stands in the north there will be deaths; attack of the king of Akkad against the enemy land.

(Quoted on page 9. Tammuz is the fourth month of the Babylonian calendar.)

> On the 19th Venus stood in the region of Aries, 10 fingers behind Mars: the moon was surrounded by a halo, and α Scorpii stood in it. The 20th, Mars was one finger to the left of the front of Aries; it came close.

(Quoted on page 39.)

B3 Here the Latin term Aries has replaced the Babylonian *hun*. The Babylonian words, like the Latin ones, could refer to constellations or, as in Babylonian tables of motion and in astrology as we know it, to a sign of the zodiac. Swerdlow makes the point that in these early reports (this one is from 652 B.C.) they probably referred to constellations and were therefore not being used precisely.

B4 Swerdlow quotes (page 64) two fundamental principles which underlie the Babylonian theory of motion of the planets. The first is that the sun is assumed to move at constant speed round the ecliptic. This means that the Babylonians were using the "mean sun" not the real sun (even though they knew that the speed of the real sun varies). The second principle (which was first formulated by van der Waerden) is that each synodic phenomenon takes place at a fixed elongation from the sun.

B5 The synodic phenomena noted by the Babylonians for the outer planets are the heliacal rising (the first occasion on which the planet is visible before sunrise), the beginning of the retrograde motion, the rising at sunset (which is to all intents and purposes opposition), the end of retrograde motion, and the heliacal setting (the last occasion on which the planet is visible after sunset).

B6 The phenomena for the inner planets are the first and last visibility in the morning and evening and the beginning and end of the retrograde motion.

B7 The time from one phenomenon to the next phenomenon of the same kind is a *synodic period* of the planet. The distance around the ecliptic between the two positions of the planet is a *synodic arc*. This is the excess over a whole number of revolutions of the distance covered by the planet itself in this interval.

[33] Princeton University Press 1998, \$39.50. Swerdlow was sent a copy of this review and asked if he wished to comment upon it. He declined, offering that the review was too generous to require his criticism. He was then asked if a scholarly portion of his amiable declining letter might be published in *DIO*. This suggestion he also declined.

B8 Babylonian records contain data of the form: Π occurrences of a synodic phenomenon take Y years, during which time the phenomenon makes Z revolutions round the zodiac. Swerdlow quotes the relation

$$i\Pi = Y - Z \tag{1}$$

where $i = 0$ for Mercury, 2 for Mars, 1 for the other planets. He says "the number of phenomena Π is therefore equal to the difference between the number of zodiacal rotations of the sun Y and of the phenomenon Z". This is a slip: it would imply $\Pi = Y - Z$ (for every planet). The explanation of (1) is as follows. i is the number of complete years in the synodic period Y/Π of the planet. In one synodic period the sun travels Y/Π times round the ecliptic, so the synodic arc is $Y/\Pi - i$, i.e. $Z/\Pi = Y/\Pi - i$, which implies (1).

B9 The Babylonian theory of the moon states that (on average) a year is 12;22,8 months, i.e. 371;04 lunar days (in the usual notation for sexagesimals). A lunar day is one-thirtieth of a month. (This term is a translation of the Sanskrit *candradina*: *candra* = moon, *dina* = day. Unlike the Babylonians, Indian astronomers used different words for lunar day and civil day.)

B10 In what follows, all arcs are in degrees, all times in lunar days. $e = 11;04$, so a year is $360 + e$ lunar days.

B11 The (mean) synodic period is

$$\frac{Y}{\Pi}(360 + e)$$

and its excess over a whole number, namely i, of twelve-month periods is less by $360i$. Swerdlow calls this excess the synodic time.

B12 The synodic arc is

$$360\frac{Z}{\Pi} \tag{2}$$

Then the synodic time minus the synodic arc is

$$\frac{Y}{\Pi}(360 + e) - 360i - 360\frac{Z}{\Pi}$$

$$= \frac{Y}{\Pi}(360 + e) - 360\frac{Y}{\Pi} \qquad \text{from (1)}$$

$$= \frac{Y}{\Pi}e \tag{3}$$

B13 This means that from the synodic times we can deduce the synodic arcs and vice versa. Neugebauer suggested (in the *History of Ancient Mathematical Astronomy*, pages 429 to 430) that the parameters can be deduced from the maximum and minimum values of the synodic arcs. Now we can deduce the synodic arcs from the synodic times and, as Swerdlow emphasizes, the times, which are given to the nearest day in Babylonian records, are at least thirty times as precise as the positions, which are at best to the nearest sign.

B14 The Babylonians had two systems of mathematical astronomy, called by us, somewhat unimaginatively, system A and system B. An example of system B, giving 32 successive synodic arcs for Saturn, is given by Swerdlow on page 212. The first twelve entries are

1.	12; 35, 20	5.	11; 47, 20	9.	11; 28, 45
2.	12; 23, 20	6.	11; 35, 20	10.	11; 40, 45
3.	12; 11, 20	7.	11; 23, 20	11.	11; 52, 45
4.	11; 59, 20	8.	11; 16, 45	12.	12; 04, 45

To begin with, each synodic arc is 0;12 less than the previous one. This holds up to entry number 7. From entry number 8 onwards, each synodic arc is 0;12 greater than the previous one. So to begin with the arc decreases by steps of 0;12 each time until it reaches a minimum value between entries 7 and 8, and then increases by 0;12 each time. It is easy to calculate the minimum value (a diagram helps) and it turns out to be 11;14,02,30. The table continues, increasing by the same steps to a maximum, then decreasing to the same minimum again, and so on. The maximum turns out to be 14;04,42,30.

System B:

B15 The principle of a stepwise increase and decrease between a maximum and a minimum is the underlying theory of system B. The size of the step and the values of the maximum and minimum are the parameters that have to be found. Swerdlow's explanation of how they were found is as follows.
B16 We have, from the records, 9 sidereal periods of Saturn in 265 years, which cover 256 synodic periods. Then $Z = 9$ and $\Pi = 256$, and, by (1), $Y = 265$.
B17 We also have the times of occurrences of the phenomena, and on pages 197 to 199 Swerdlow displays 72 pairs of synodic phenomena. For example, the first pair are risings at sunset on the 19th day of the fourth month of one year and on the 16th day of the fifth month of the next year, giving an interval of 27 days (which can be taken to be 27 lunar days) more than twelve months.
B18 From the 72 entries Swerdlow chooses a maximum of 26 and a minimum of 23 lunar days. This is an arbitrary choice, but well over half the entries fall in this range.
B19 The number of occurrences of a phenomenon in a complete cycle is obtained by dividing the total change (i.e. twice the difference between the maximum and the minimum) by the size d of the step from one entry to the next. With the provisional choice of maximum and minimum this is $6/d$. It is also the number of mean synodic arcs in a revolution, which, by (2) is $9/256$ for Saturn. So provisionally $6/d = 9/256$, giving $d = 0;12,39\cdots$. Swerdlow's suggestion is that the step of 0;12 in the table was found by rounding this value.
B20 The mean synodic arc is $360 \times 9/256$, from (2). From this and the value of d we can easily compute the maximum and the minimum. We can then construct the whole table if we can find one entry. We can also find the positions of Saturn in the zodiac at each occurrence of the phenomenon if we can find the position of one. Swerdlow calls this the problem of alignment to the zodiac and deals with it separately (in part 3 of the book).
B21 Jupiter and Mars are dealt with similarly. Venus and Mercury do not use system B.

System A:

B22 In this system the ecliptic is divided into a number of zones, each allotted a synodic arc. If the longitude at one occurrence of the phenomenon is in a zone allotted arc w, then the longitude at the next occurrence is greater by w provided that this is in the same zone. If, however, it lies in the next zone, a distance x past the end of the first zone, the longitude will increase not by w but by $w - x + \frac{v}{w}x$, where v is the synodic arc allotted to the second zone. (There are further complications if it lies beyond this zone.)
B23 For convenience of calculation in sexagesimals the Babylonians chose the allotted arcs so that the ratio between any two of them is the ratio between two small whole numbers containing only 2, 3 and 5 as factors.
B24 Let us look at Saturn as an example. According to the procedure texts ACT801.3-8 it has two zones, one of length 200° allotted an arc of 11;43,07,30 and the other of length 160° allotted an arc of 14;03,45. We can easily check that these two arcs are in the ratio 5:6.
B25 Swerdlow suggests that these figures were found as follows. We take, as before, 26 and 23 as a provisional choice for the maximum and minimum synodic times. By (3) the corresponding arcs are found by subtracting $\frac{265}{256}e$, which is a tad under 11 1/2. If we round this to 11 1/2 we have provisional maximum and minimum synodic arcs 14 1/2 and 11 1/2. So choose $w = 11\ 1/2$ and $v = 14\ 1/2$ and allot w to a zone of length α and v to a zone of length β. The average number of synodic arcs in a revolution is $\alpha/w + \beta/v$ and this, by (2) is Π/Z, which, for Saturn, is $256/9$. So we have two equations

$$\alpha + \beta = 360$$

$$\alpha/11\frac{1}{2} + \beta/14\frac{1}{2} = 256/9$$

The solution for α is 201 + 1/27. If we round this to 200 we get zones of 200° and 160° as in the procedure texts. However, to obtain w and v we have to change $14\frac{1}{2}/11\frac{1}{2}$ to 6/5, although 5/4 is nearer.
B26 Swerdlow treats Jupiter and Mars similarly, though as they have either four or six zones there is more opportunity for arbitrary choice and the details are considerably more complicated. Mercury is more complicated yet because its different synodic phenomena have different parameters. Venus has to be treated entirely differently because its phenomena are not assumed to appear at a constant elongation from the sun and equation (3) does not apply to it.
B27 Swerdlow ends his chapter on Venus by saying that the synodic arcs are found in ways that are, as yet, too subtle to yield up their secrets and commenting "Unfortunately, much as one would wish, one cannot say too much of Venus". (page 712)
B28 Swerdlow ends with some general comments (on pages 181 and 182) which many readers of (and writers for) *DIO* will find it hard to agree with.
B29 "That the most sophisticated natural science of antiquity, mathematical astronomy, arose from the systematic recording of portents and omens in the service of prognostication and magic is against all received wisdom but is nonetheless true".
B30 "The discussions of two scribes of Enūma Anu Enlil contained more rigorous science than the speculations of twenty philosophers speaking Greek, not even Aristotle excepted. I say this seriously and not as provocation. I believe that most historians and philosophers dote upon childish fables and fragments of pre-socratics, requiring no knowledge of mathematics and less taxing to the intellect".
B31 "The models of Eudoxus and Aristarchus were clever but useless. And the work of Hipparchus was in great part an assimilation of Babylonian methods and parameters, which formed the foundation of Greek mathematical astronomy. The origin of rigorous technical science was not Greek but Babylonian". (I have slightly shortened these quotations.)
B32 If Swerdlow's ingenious and fascinating ideas are valid, this book makes an invaluable contribution to our knowledge of Babylonian astronomy.