DIO

A Thurston Collection

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All of the technical articles in this *DIO* are by Hugh Thurston (PhD, mathematics, University of Cambridge, England), who by now requires no introduction to readers of *DIO* (see, e.g., inside back cover and-or *DIO 4.2* ‡6 fn 1). Among his numerous professional publications is the well-received 1994 Springer-Verlag book, *Early Astronomy* (pbk 1996).

News Notes on *DIO*'s 1998 [added at end of year]:

Lightning Strikes Thrice: For the 3rd time, *DIO* (and-or its publisher) has been cited in a *New York Times* p.1 story. The latest instance is John Tierney's nailing-the-coffin-shut 1998/11/26 *NYT* article on Frederick Cook's original uncropped fake 1906 "Mt. McKinley summit" photo (long believed lost forever), which *NYT* generously notes debuted in *DIO 7.2.* (Our first centerfold.) This treasure's recoverer, author Robert Bryce (*Cook & Peary* 1997; *DIO 7.2-3*) appeared 4 days later on ABC-TV's *Good Morning America*.

Oxford Univ Press & Fraud: Oxford U Press has just published James Evans' *History & Practice of Ancient Astronomy*. (At $52, my copy prints 16 text-pages blank!) Vetted & reviewed by fellow debate-fleeing (‡5 §J4) Ptolemy-p.r.men 0 Gingerich & Noel Swerdlow, this sham-neutral book determinedly aims at rescuing Ptolemy from **the** most indicting (*DIO 2.3* ‡8 §C2) of the many fraud charges on his historian-police blotter: stealing Hipparchos' 1025-star catalog. On this, Evans 1998 mostly repeats his 1987 mega-paper (in 0's *JHA*), which fed 0's decades-long chimeral obsession[1] to slay the R.Newton-DR dragon, by assaulting DR's hated 1982 *PASP* analysis of Ptolemy's star catalog.[2] Since that paper, scholarship questioning Ptolemy's honesty has been published by Univ Md (1982), Greenwich Meridian Centenary (1984), *Amer J Physics* (1987), van der Waerden (1988), Royal Astr Soc (1988), Amer Astr Soc (1990), Springer-Verlag (1994), & 3 scientists in 1991-1997 *DIO*s. But Evans 1998's re-hash & HUGE bibliography (dense with Ptolemy-apologists) *cite none of these works*, which include [a] quantitative extensions & verifications[3] of the very 1982 paper under special attack, and [b] lethal[4] Evans 1987 miscues (none ever acknowledged), the funniest ones now (p.272) blithely re-issued intact under Oxford U Press' imprint! Though Evans 1987's prime pro-Ptolemy argument (undone by Tycho's αPsA magnitude) was gutted at *QJRoyAstrSoc 29*:279 (not cited), Evans 1998 p.273 ineducably repeats it; pp.269&272 claim the Newton 1977 & Rawlins 1982 tests upon Ptolemy, applied to Ulugh Beg's & Tycho's star catalogs, would mis-indicate UB plagiarism & get Tycho's latitude way off. False. And false. (See Evans' own fine print.) Rawlins 1982's most unevadable test isn't mentioned. Evans' anti-DR use of Tycho's star catalog won't cite this catalog's sole critical edition: *DIO 3*. Evans' attacks upon DR's *astronomical-history* never mention that DR publishes the US' only *astronomical-history* journal. Evans' top promoters 0 & Swerdlow used to scorn[5] non-citation of dissenting papers as vile. But now, for 0, NS, and Evans, the last *16 years* of prominent skeptical works are Memory-Holed.

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[1]One of the world's most securely eminent Hist.sci scholars admits (1997/4/25) he can't oppose 0 Gingerich on Ptolemy, for fear dirt would be done him. (And systematic non-citation *is* dirt.)

[2]Rawlins 1982's least-squares analysis found the cataloger's latitude, latitude-error, rough epoch, both adopted obliquities, & celestial longitude zero-point-error. All 6 of these independent solutions fail to fit Ptolemy; 4 hugely. Evans, despite massive expert-assistance & labor, finds none of these solutions betrayed by math errors. So he squirms on a few of the 6, & ignores the rest. [See ‡1 ⊙4.]

[3]*DIO 1* ‡6 §§F3, G, H5; ‡9 §I1, fn 144; *DIO 2.3* ‡8 §§C14 & C20, fn 50; *DIO 4.1* ‡3.

[4]See, e.g., *DIO 2.1* ‡3 fn 8, ‡4 §§F2-F3, H7, fn 65; *DIO 3* §L8; *DIO 4.1* ‡4.

[5]*DIO 1.1* ‡6 fn 6 & *DIO 1.2* fnn 5, 123, 167.

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‡1   R. R. Newton versus Ptolemy

by Hugh Thurston[1]

Readers of *DIO* will be familiar with the controversy over Ptolemy's honesty and competence. R. R. Newton was his most prominent critic. Ptolemy's defenders were mostly a collection of established academics (called "the Muffia" by Dennis Rawlins).

It is in fact hard work to extract Newton's cogent arguments, and — most important — the calculations supporting them, from his 411-page book *The Crime of Claudius Ptolemy* (published by Johns Hopkins University Press in 1977) and his other books and papers. I have not seen elsewhere a succinct exposition of his book's reasoning, so I give one here.

References of the form Cx are to page x in Newton's book. References of the form Sx.y are to chapter y of book x of Ptolemy's *Syntaxis*.

A   The Length of the Year (C84 to 94)

A1 The obvious way to find the number of days in the year is to divide the time-interval between two summer solstices by the number of years between them. (Or two equinoxes.)[2] Hipparchus found the value 365 1/4 − 1/300 days. This was six minutes too long. The error in the result is the error in the interval divided by the number of years. The greater the number of years, the better the result. Ptolemy should have improved on Hipparchus. He didn't.

A2 Ptolemy calculated the average length of the year three times (S3.1), each time comparing an equinox or solstice (which he claimed to have observed) with an earlier one. These data are set forth in Table 1. In each of the three cases used in S3.1, Ptolemy obtained exactly the same year as Hipparchus. The times that he gives for his "observations" are badly in error [⊙1] — by over a day on average — and are in each case off by just the amount needed to yield Hipparchus's over-long result. Clearly Ptolemy did not observe the equinoxes and solstice; he calculated times for them from the earlier observations, using the length of the year that Hipparchus had found. And, if you do this calculation yourself, taking morning to be a quarter of a day after midnight, and (like Ptolemy) express your results to the nearest hour, you will get precisely[3] the times and dates that Ptolemy cited.

A3 This reasoning was first given by J. B. J. Delambre (*Histoire de l'astronomie du moyen âge*, 1819, page lxviij). Newton went one step further than Delambre by also bringing in an equinox that Ptolemy claimed [S3.7] to have observed on 132/09/25. and showing that it was actually calculated in just the same way [⊙2 & ⊙3].

B   The Obliquity of the Ecliptic (C96 to 102)

In Ptolemy's time, the correct value of the difference between the noon elevations of the Sun at the two solstices (which is twice the obliquity) was 47°21′. In S1.12, Ptolemy quoted repeated observations in which he found that this angle always lay between 47°40′ and 47°45′. The error is unreasonably large for the instruments that Ptolemy said he used. However, Ptolemy's result is extraordinarily close to a value which he imputed to Eratosthenes (and himself adopted as accurate), namely, 11/83 of a circle. Ptolemy said that

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[Hugh Thurston died on 2006/10/29. The lead article of *DIO 14* (2008) is dedicated to his memory.]
End-notes (section ⊙, pp.14-17) and footnotes are by D. Rawlins.

[2]Though, see *DIO 1.1* ‡6 §A3.

[3]See extra irony at *DIO 1.2* §E1 and fn 64.

Table 1: Equinoxes & Solstices

Event	Earlier observation	Ptolemy "obs"	Actual Time	Error
Autumn Equinox	−146/09/27 midnight	132/09/25 14^h	09/24 05^h	33^h
Autumn Equinox	−146/09/27 midnight	139/09/26 07^h	09/24 22^h	33^h
Vernal Equinox	−145/03/24 morning	140/03/22 13^h	03/21 16^h	21^h
Summer Solstice	−431/06/27 morning	140/06/25 02^h	06/23 14^h	36^h

Superscript h stands for hour. Times are from midnight. Dates are Julian [⊙3].

Hipparchus also used it, but in fact Hipparchus used first 47°50′ and later 47°20′.[4] (See Dennis Rawlins's statistical study [⊙4], "An Investigation of the Ancient Star Catalog", *Publications of the Astr. Soc. of the Pacific* volume 94 (1982), pages 367 and 368.)

C   The Epicycle of the Moon (C112 to 130)

C1   Hipparchus assumed that the Moon moved on a simple epicycle and Ptolemy's theory was, for full[5] Moons, equivalent to this. It is possible to use the time-intervals between three eclipses of the Moon and the corresponding differences in the longitudes of the Sun to calculate the radius of the epicycle.

C2   S4.6 and 4.11 quoted four trios of eclipses which yield values of 5;13 and 5;14 (S4.6), also [⊙5] 5;15 and 5;15 (S4.11) on a scale on which the radius of the circle on which the epicycle travels is 60 units.

C3   These values are incredibly[6] close to each other. Newton calculated the effect of altering the time of the middle eclipse of the first trio by one hundredth of a day (about 1/4 hour) and he found that it reduced[7] the value obtained to 5;06 (C122).

D   The Sun's Longitude (C145 and 147)

Ptolemy said in S5.3 that he observed the Sun on 139/02/09 (6:45 Alexandria Apparent Time) and found [⊙6] its longitude to be 318°5/6. The longitude at this time as calculated from his tables was also 318°5/6. In S7.2 he reported [⊙7] a similarly measured [⊙8] and calculated solar longitude for 139/2/23 (17:30 Alexandria Apparent Time) to be about 333° and 333°1/20, respectively. During Ptolemy's era, his tables for the longitude of the Sun were out (on average) by over a degree. Consequently, if the observations had been genuine the calculations would not have agreed with the tables.

E   The Half-Moon (C145 to 158)

E1   The simple epicycle theory gives reasonable values for the Moon's longitude at full Moon, and for the maximum difference between the longitudes of the mean Moon M and of the Moon itself, namely 5°. This is too small at half-Moon.

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[4]The actual value in Hipparchus's time was about 47°25′.

[5]Many of the references to full Moon throughout this discussion could as well refer to new Moon; though, of course, lunar eclipses occur only at full Moon.

[6]All results are remarkably near the round value 5 1/4, which suggests that all four results were forced to come out very close to Ptolemy's lunar epicycle radius, 5;15.

[7]This huge sensitivity to mere quarter-hour uncertainties is lethal to Ptolemy's pretended precision here, since much of the raw eclipse-time data are obviously good to only about 1^h precision. See David Dicks at *DIO 4.1* ‡1 §D1 & fn 46.

E2   From an observation (S5.3) allegedly performed on 139/02/09 (§D) Ptolemy made this difference 7°2/3 at half-Moon. However, in his calculations he said that the parallax was negligible. It was in fact −9′ on his own parallax theory. He should thus have found a difference of 7°31′, not 7°40′.

E3   Ptolemy also quoted (S5.3) an observation by Hipparchus on −127/08/05 from which he again calculated the difference to be 7°2/3, once more neglecting parallax. But parallax was again −9′, with an effect in the opposite direction from §E2, so Ptolemy should have found 7°49′.

E4   The two observations disagreed, but Ptolemy fudged his calculations to make them agree [⊙9].

F   The Final Theory for the Moon (C149 to 156)

F1   In Ptolemy's final theory of the motion of the Moon (see Fig.VII.2 at C150), the mean Moon M moves round the Earth E at a constant speed. M maintains a constant distance not from E but from a point C_1 which moves round E at a constant distance at the same angular rate (as M) — but in reverse. The rate (and starting point) is chosen so that C_1 lies directly between E and M at full[8] Moon, but lies on the opposite side of E at either half-Moon. So the distance from the Earth to the Moon is $MC_1 + C_1E$ at mean syzygy, but $MC_1 - C_1E$ at half-Moon. This gives the lunar epicycle a larger apparent size at half-Moon and so increases the maximum angle between the mean Moon (M) and the Moon. The value of C_1E is chosen to give the "right" value at half-Moon, namely, the 7°2/3 value of §E. It turns out [⊙10] that C_1E has to be 10;19 on a scale on which the maximum distance is 60.

F2   The Moon moves (circling retrograde around M) on its epicycle at a constant speed not relative to the diameter through E but relative to a point K on a slightly different diameter. Following C150's Fig.VII.2, we specify C_2 as the point where this diameter when prolonged reaches the straight line through C_1 and E.

F3   Ptolemy calculated the distance C_2E from an observation of the Sun and Moon on −126/05/02 (S5.5) as follows. From the time of the observation he first calculated (from his S4.4 tables) the longitude of M. From this and the observed longitude of the Moon he then found the position of the Moon on the epicycle. From the time of the observation he also computed the distance the Moon has moved round on the epicycle from its zero-point K, and so he found K by simple subtraction and could then compute C_2E. He found it to be 10;18, equal to C_1E to a high degree of precision.

F4   From modern calculation, Newton found that, at the time of the −126/05/02 observation, the angle between M and the Moon was [⊙11] about 1°23′, not the 46′ that Ptolemy found (C156) by comparing the observed Moon to a value for M obtained from his tables. If used (in place of 46′), 1°23′ would have made C_2E equal 14;40, not 10;18.

F5   From an observation by Hipparchus on −126/07/07, Ptolemy computed $C_2E = 10;20$ (also in S5.5). He achieved this almost perfect agreement (with $C_1E = 10;19$) again by having a wrong value for the angle between M and the Moon (C156): 1°26′, instead of the actual angle at this time, 2°19′.

F6   There can be no doubt that Ptolemy decided in advance to make C_1E and C_2E equal, and fudged the calculations or the observations to give this result. He did something very similar[9] in his treatment of Mercury and again in his treatment of Venus.

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[8]At full Moon, C_1 will lie directly between E and M on the line SEM where S is the mean Sun. Same at mean new Moon, except that the line of mean syzygy is then SME.

[9]See below at §§P2 and Q2.

G   The Inclination of the Moon's Orbit (C184)

G1   To find the angle between the Moon's orbit and the ecliptic (the inclination) Ptolemy measured the zenith distance[10] of the Moon when its longitude was near 90°, its ascending node was near the vernal equinox, and the Moon was crossing the meridian. He claimed (S5.12) that repeated measures of the zenith distance always found about 2°1/8. Taking this as 2°07′, the obliquity of the ecliptic as 23°51′ (it was actually 23°41′) and the latitude of Alexandria as 30°58′N (it is 31°.2 N; Newton takes it to be 31°13′N), he found the inclination to be:

$$30°58' - 23°51' - 2°07'$$

That is, 5° exactly.

G2   With correct values for the obliquity, latitude, and inclination (which actually varies between 5°.0 and 5°.3), Newton found that the zenith distance (for all the possible times when Ptolemy could have measured it) was never outside the range 2°1/4 to 2°1/2 instead of "always" near 2°1/8.

H   The Distance of the Moon at the Quarters (C186 to C190)

H1   Ptolemy's theory of the Moon's motion makes the mean Moon much closer to the Earth at the time of half-Moon (one quarter or three quarters of the way through the month) than at full or new Moon. If the mean Moon's distance (ME) is 60 at full Moon, then it is 39;22 (§F1) at the time of half-Moon. The radius of the epicycle is 5;15 (§C3). So the distance of the full Moon from the Earth can be as much as 65;15, and the distance of the half-Moon can be as little as 34;07. This is much too big a variation. Anyone who watches the Moon will know that its apparent size does not vary correspondingly.[11] (Even the notorious "Moon illusion" — which Newton does not mention — could only suggest that the distance is less when the Moon is near the horizon, not when it is half-full.)

H2   Ptolemy estimated the distance of the half-Moon by measuring the zenith distance at sunset on 135/10/01. He says (S5.13) that he observed[12] the zenith distance to be 50°55′. But Newton found the correct value to be 50°14′. For the instruments which Ptolemy said that he was using, the error is unacceptably large.[13]

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[10]The "zenith distance" is a celestial object's angular distance from the zenith; it is thus the complement of that object's altitude above the true horizon.

[11] I.e., the Ptolemaic lunar theory implied that an Earth observer will see the Moon's angular diameter vary by a factor of nearly 2. Contra some Higher Opinion (see irony at C182f and *DIO 1.3* fn 284), Ptolemy actually believed in this impossibly counter-eyeball distance-variation — so much so that he arranged an equally impossible "observation" (§§H2-H3) to prove it. The Moon's actual distance-extremes (including equation of centre, all perturbations, and observer-nongeocentricity) are in a ratio of less than 7 to 6.

[12]Celestial coordinates based upon the observer's location (called "topocentric" coordinates) naturally differ from those based on the Earth's center ("geocentric" coordinates). (Only the latter were tabulated, but only the former can be observed.) Parallax is the difference between these coordinates. For the Moon, parallax is frequently as much as about a degree.

[13]If one takes Alexandria's latitude as 31°12′N, the zenith distance is 50°13′, so the discrepancy is 42′. This error is more than 2 1/2 times the semi-diameter (under 16′) of the actual body Ptolemy was allegedly measuring. And Ptolemy's purported instrument was a framing device (see C182 Fig.VIII.4), which would naturally use so snug a viewing frame that (as pointed out at C183 and C188-191) if he had actually used it: [a] the lack of serious lunar distance-variation would have been immediately obvious; and [b] the Moon would be *entirely outside* the frame during the 135/10/01 observation if Ptolemy pointed his instrument at the zenith distance he reports. [See also ☉2.]

H3   From his tables[14] Ptolemy calculated that the zenith distance as seen from the centre of the Earth was 49°48′, giving up a parallax of 1°07′ (50°55′ − 49°48′) and a distance[15] of 39 3/4 times[16] the radius of the Earth. The correct distance at that date was over 60.[17] Clearly Ptolemy fabricated the observation to support his theory of the Moon's motion.

H4   Ptolemy also calculated the distance of the Moon at this time, from his theory, to be 40;25 on a scale on which the greatest distance of the mean Moon was 60. This enabled him to calculate that the greatest, mean, and least distances of the full Moon were respectively 64 1/6, 59, and 53 5/6 times the radius of the Earth.[18]

I   The Distance of the Sun (C174, 194 to 199, and 202 to 203)

I1   Ptolemy explained (S5.15) a geometrical method[19] for finding the distance of the Sun which is equivalent, in modern terms, to saying that the sum of the apparent radius of the Sun and the apparent radius of the shadow of the Earth on the Moon is equal to the sum of the parallax of the Sun and the parallax of the Moon.

I2   He found the apparent radius of the Moon when it is at its greatest distance and the apparent radius of the shadow then from the magnitudes of partial eclipses on −522/07/16 and −620/04/22 and the calculated latitude of the Moon in the middle of each eclipse. (To calculate the time of the middle of the second eclipse from the observed beginning he used a semi-duration of an hour, though in S6.5 he used a semi-duration of 1/2 hour for an eclipse of the same stated magnitude.)[20] He found the apparent radius of the Moon to be 15′40″ and took this to be also the apparent radius of the Sun. The apparent radius of the shadow was 40′40″.

I3   The parallax of the Moon (its greatest distance on Ptolemy's lunar theory being 64 1/6 times the radius of the Earth: §H4) was 53′35″, so the parallax of the Sun was 15′40″+ 40′40″ − 53′35″, which amounts to 2′45″, giving[21] a distance 1250 times the radius of the Earth. Ptolemy obtained 1210, using a complicated geometrical calculation. The difference is inconsequential.

I4   But in another chapter (S6.5) Ptolemy found the apparent radii when the Moon is at the least distance a full Moon can occur (which on his theory is 53 5/6 times the radius

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[14]Ptolemy already had figures for latitude, obliquity, and lunar *i* (all three in serious error); thus, simple arithmetic could give 30°58′+ 23°51′ − 5° = 49°49′. (Correct figures would have given instead: 31°12′+ 23°41′ − 5°18′ = 49°35′.) After Ptolemy's tiny corrections (1′ net, though 5′ would have been more nearly correct) for the slight imperfections of the nodal and solstitial idealities, he found 49°48′. (He should have gotten 49°30′.)

[15] Ptolemy makes it 39 3/4 Earth-radii, while Newton more precisely computes 39 5/6. (One may easily check the numbers by the law of sines: sin 50°55′/sin 1°07′ = 39 5/6 Earth-radii geocentric. So 1°07′ of non-horizontal parallax corresponded to a horizontal parallax of 1°26′ or 86′. (The math should have been: sin 50°14′/sin 0°44′ = 60 Earth-radii geocentric. Actual horizontal parallax at the time: 57′. Both figures happen to be extremely average values for the Moon.)

[16] This figure is geocentric, and it is in quite close accord with Ptolemy's final lunar theory, developed above. See §H1 and fn 11.

[17]Topocentric: a little under 60.

[18] Using 39;50 (fn 15) and 40;25 (§H4), one may easily confirm: 60·(39;50/40;25) = 59 Earth-radii. (See S5.13 and Gerald Toomer, *Ptolemy's Almagest* 1984 page 251 note 49.) Multiplying 59/60 times ± 5;15/60 yields ±5 1/6. Adding this to 59, one gets (in Earth-radii) 64 1/6 or 53 5/6.

[19]Easily understood from C174's Fig.VIII.2.

[20] The S6.5 eclipse Newton refers to is that of −140/01/27. See C194 note and Toomer *op cit* page 253 note 56 and page 284 note 23. Ptolemy gives the magnitude for both eclipses (−620 and −140) as 3 digits (or 1/4 of the lunar diameter), though the magnitude of the −620 eclipse was actually 2 digits. (It is 3 digits by Ptolemy's tables.) Its actual semi-duration was 47 timeminutes. The −140/01/27 eclipse was in fact 3 digits and about an hour's semi-duration, though Ptolemy uses a half-hour: §I4.

[21] The cosecant of 2′45″ is 1250.

of the Earth: §H4) from[22] eclipses in −173/04/30 and (fn 20) −140/01/27. (To calculate the time of the middle of the second eclipse he took the semi-duration to be a half-hour,[23] though his table correctly gives a semi-duration of an hour for an eclipse of this magnitude.) Ptolemy did not use these radii to find the distance of the Sun. Had he done so he would have found that the sum of the apparent radius of the Sun and the apparent radius of the Earth's shadow was less than the parallax[24] of the Moon at the given least distance.[25] This is, to second Newton's phrase, "physical nonsense" (C203).

I5   Such an outcome only dramatizes what was already obvious from the delicate mathematics of §I3: this method of finding the distance of the Sun is so sensitive[26] to the exact values of angles which cannot be measured or calculated precisely that it will not — except by coincidence or fabrication — give a correct result. To obtain a result so far from the truth (the mean distance to the Sun is about 23,500 times the radius of the Earth),[27] but so close to a result previously obtained by Aristarchus and suspiciously close to the value that Ptolemy needed for his later and totally erroneous theory of the way in which the orbits of the celestial bodies fit together (*Hypotheseis ton planomenon*) would require an unacceptable coincidence. It must have been obtained by fabrication.

J   Positions of Stars (C211 to 212)

In S7.1 Ptolemy quoted some configurations of stars described by Hipparchus in his commentary on Aratus's *Phaenomena*. Newton investigated one. Hipparchus stated that β Cancri (to give it the modern name) was 1 1/2 digits north-east of the centre of the line joining α Cancri and Procyon. Ptolemy said that he found the same result. He couldn't have done so. At this date β Cancri was very close to the midpoint and north-*west* of it.

K   The Longitudes of Regulus and Spica (C217 to 218)

K1   Ptolemy (S7.2) used an observation of the Moon on 139/02/23 (§§D & E2), which Newton showed to be fudged (C145 to 146 and 148 to 149), to find the longitude of Regulus. The result was exactly 2°2/3 greater than the longitude that Hipparchus had measured 2 2/3 centuries earlier, giving a rate of precession equal to 1° per century. This is consistent with Hipparchus's statement (S7.2) that precession is at least 1° per century. (It was actually 1°23′ per century.) So the longitude of Regulus in the catalogue was fudged.

K2   If we add 2°2/3 to Hipparchus's value for the longitude of Spica we get Ptolemy's value, namely 176°2/3. The error is −1°17′. So this longitude was also fudged.

L   Declinations (C220 to 225)

L1   In S7.3 Ptolemy listed the declinations of 18 stars measured by Timocharis or Aristyllus, by Hipparchus, and by Ptolemy himself [⊙14]. He chose six of the stars, found the change in declination between Hipparchus's time and his own for each of them, and showed that this was consistent with a precession of 2°2/3 in the intervening 2 2/3 centuries.

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[22] In fact, the −173 eclipse was not near perigee.

[23] See fn 20.

[24] At C203, Newton uses Ptolemy's own value (§H4) for the least distance of the Moon to compute lunar parallax = arccsc(53 5/6) = 63′52″.

[25] Newton does the calculation (using the equation of §I1) at C203. Using shadow radius 46′ (S6.5), lunar semi-diameter 15′40″ (§I2), and lunar parallax 63′52″ (fn 24), he has:
solar parallax = 46′ + 15′40″ − 63′52″ = −2′12′.

[26] See N. Swerdlow *Centaurus 14*:287 (1969).

[27] The precise mean distance to the Sun is 23454.8 times the equatorial radius of the Earth.

L2   Ptolemy could have used up to 14 of the 18 stars. (The other four are too close to a solstice.) Accurate calculation of the rate of precession that would give the changes in declination in the six stars that Ptolemy used varied from 35″.1 to 41″.1 per year. The other eight gave values ranging from 45″.1 to 64″.4 — much better. (The correct value was 49″.8 per year.) Pannekoek suggested that the observations were all genuine and Ptolemy had simply picked six which agreed with his (erroneous) 36″ per year[28] value for the precession (*Vistas in Astronomy* volume 1 [1955] pages 60 to 66). But if he had, the standard deviation of the results from the stars used would have been larger than that from the others; in fact it is smaller, as it would be if they were fudged.

M   Occultations (C225 to 237)

In S7.3 Ptolemy discussed seven cases of occultation of a star by the Moon. For each case, he calculated the position of the Moon at the time, and thereby deduced the latitude and longitude of the star.

M1   On −282/01/29 the Moon's northern half covered the eastern part of the Pleiades. On 92/11/29 the southern cusp of the Moon covered the southeastern part of the Pleiades. However (C223) on Ptolemy's figures for the Moon (longitude 33°15′, latitude 4°),[29] it could not in fact have covered any of the Pleiades as listed by Ptolemy in his catalogue of stars.

M2   On −293/03/09 the Moon's rim reached Spica, which was reported as 5′ north of the centre of the Moon. Ptolemy ignored[30] this 5′ when computing the latitude of Spica from the data. On −282/11/09 Spica exactly touched the northern rim of the rising[31] Moon. On 98/01/11 Spica was hidden by the Moon.

M3   On −294/12/21 the Moon's northern cusp touched [⊙15] β Scorpii. On 98/01/14 the southern cusp was in a straight line with δ Scorpii and π Scorpii and as far from δ as δ was from π, and it covered β Scorpii. However, if the Moon was either where Ptolemy placed it, or where it really was according to modern calculation, it could[32] not have covered β Scorpii. (See Fig.IX.7 at C234.)

M4   Ptolemy used the change in longitude between two (or three) observations of the same star to confirm his erroneous value for the precession (§L2). If we compute the longitude of each star at the relevant date from the longitude in the catalogue and Ptolemy's erroneous precession, and round off to the nearest 5′, we get exactly the longitude that Ptolemy "found" from the observation in all seven cases. If Ptolemy had computed the longitudes in this way they were bound to confirm his value for the precession. Moreover all the latitudes also agree to within 2′ (see Table IX.3 at C230) with the latitudes in the

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[28] Same as the 1°/century value we recall from §K1.

[29] S7.3 has true position longitude 33°15′, latitude 4°. But the calculation is riddled with problems, well analysed by Toomer *op cit* page 335 note 70.

[30] At page 112 of John Britton's 1967 Yale dissertation (fortunately published at last in 1992), Britton correctly notes that in fact the Moon's centre went right over Spica. So Ptolemy's ignoring the 5′ turned out to be lucky. But, ironically, had he included the 5′, he would have gotten Spica's latitude (−1°54′, vs. his value of −2°) correct to 1′ by accidental cancellation of errors.

[31] The waning crescent Moon's north cusp passed merely 2′ south of Spica; however, this occurred not at the rising of the Moon but about 0ʰ.8 later. Toomer *op cit* page 336 note 75 rightly points out that Ptolemy's conversion of 3ʰ1/2 seasonal hours into equinoctial hours confuses day and night and thereby found 3ʰ1/8 when he should have gotten 3ʰ7/8. C236 adds (correctly) that for the time Ptolemy states (2:30 Alexandria Apparent Time), the Moon and Spica were actually both below the horizon. (Also true by his own tables.)

[32] Anyone who simply looks at Scorpio can see that β Scorpii is too far from the δ-π line for the Moon to touch both. Ancient readers could have realized this, without modern checks or any other special knowledge. [See ⊙16.]

catalogue (if we agree with Newton's suggestion (C228) that the $3°1/3$ for the eastern star in the Pleiades was a scribal error for $3°2/3$).[33]

M5   The amazingly exact agreement of the observations[34] with the catalogue would be impossible if they were genuine: there are too many sources of error. When a star is hidden by the Moon, it can be anywhere behind the Moon. The times are given only to the nearest half-hour [☉16], so even accurately observed events could be up to a quarter of an hour out, during which time the Moon moves about $8'$. The errors in Ptolemy's tables for the longitude of the Moon have a standard deviation of $35'$ (C238).

N   The Catalogue of Stars (C237 to 256)

N1   S7.5 and 8.1 consist of a catalogue of slightly over a thousand stars, giving their longitudes and latitudes, which Ptolemy (S7.4) claimed to have based upon his own observations. Newton suggested that Ptolemy did not do this, but instead updated a catalogue compiled at the time of Hipparchus by adding the difference in longitude caused by precession. Ptolemy's erroneous value for precession (§K1) would make the typical updated longitude more than $1°$ too low.[35] This effect could account for the notorious systematic error that does in fact exist in the longitudes.

N2   There is evidence that the catalogue was compiled at the latitude of Rhodes, where Hipparchus worked.[36] Every star listed was visible at Rhodes. Newton remarked[37] that many stars always below the horizon at Rhodes were visible at Alexandria (which is about $5°$ further south) but were not included in the catalogue. He did not name any, but Dennis Rawlins has filled the gap (Table II at page 364 of reference in §B). They include ϵ Carinae, λ Centauri, α Gruis, α Indi, and α Phoenicis.

N3   The fractional parts of the latitudes and longitudes are not distributed at random. In particular far more latitudes are whole numbers of degrees and far more longitudes end in $2/3°$ than would be expected. Newton produced a powerful argument that this implied that Ptolemy compiled his catalogue by adding $2/3°$ to the longitudes of an earlier catalogue. However, Shevchenko discovered that the dominance of $2/3°$ endings did not[38] occur in the southern constellations (*Journal for the History of Astronomy*, volume 21, pages 187 to 201). Therefore the figures that I give in Table 2 are for the northern stars not the figures for the whole catalogue which Newton gave. Consequently, Newton's reasoning applies to the northern stars but not to the catalogue as a whole.

N4   Newton suggested that the instrument used was an armillary astrolabon graduated in whole degrees (perhaps in half-degrees),[39] and that the observer estimated the fractions for those observations that did not fall on a graduation. The large numbers of whole degrees in the latitudes would be accounted for if the eye were (C247) "attracted, so to speak, to the degree mark" and assigned more measurements to it than to invisible marks.

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[33] See Toomer *op cit* page 335 note 71 and page 363 note 188. The star in question is #411 in the catalogue, evidently 27f Tauri (Atlas).

[34] The observations were supposedly performed independently via armillary astrolabon. See S7.4 and S5.1. For Newton's estimate of the mean longitude error ($22'$) of this instrument, see C216. Ulugh Beg's 15th century use of the same sort of instrument achieved similar accuracy.

[35] An error in precession of $-23'$ per century (§K1) will, in 2 2/3 centuries, grow to more than $-1°$.

[36] See ‡4 fn 12 for a full array of proofs that Hipparchus was the observer of the star catalogue.

[37] Delambre first pointed this out in his *Histoire de l'astronomie ancienne*, 1817, volume 2 page 284.

[38] Meaning that, in the southern constellations, the stars with 2/3 endings are outnumbered by the zeros. However, southern stars with 1/6 endings vastly outnumber those with 1/2 endings (in accord with the Newton distribution). This seeming dissonance is resolved at *DIO 4.1* ‡3, starting with the point emphasized at the conclusion of its fn 5.

[39] See *DIO 2.3* ‡8 §C8.

Table 2: Distribution of the 359 Northern Stars' Fractional Endings

Coordinates	integral	1/6°	1/4°	1/3°	1/2°	2/3°	3/4°	5/6°
Latitudes	108	29	33	39	75	36	10	29
Longitudes	62	61	0	67	29	95	0	45
See §N5	72	75	0	46	29	108	0	29

First column of data ("integral") is number of endings for whole degrees.

Table 3: Distribution of 359 Random Fractional Endings

integral	1/6°	1/4°	1/3°	1/2°	2/3°	3/4°	5/6°
60	45	30	45	60	45	30	45

N5   In more detail: because the fractions that appear are not evenly spaced we would not expect[40] the fractions to appear equally often if the odd fractions were distributed at random and allotted correctly to the nearest fraction that does occur. In fact, we would expect the 359 fractions for the latitudes to be distributed as in Table 3 [where the sum is 360 only because of rounding]. If we add $2°2/3$ to each of these fractions (and drop the integral part of the result) we obtain: $2/3°$, $5/6°$, $11/12°$, zero, $1/6°$, $1/3°$, $5/12°$, $1/2°$. Newton said (C250) that the "rules of the catalogue"[41] did not allow $11/12°$ or $5/12°$. He did not say why, but (C252) if we add the $11/12°$ stars to the zeros and the $5/12°$ stars to the $1/3°$ stars, and apply this procedure to the distribution of latitudes in Table 2, we get the distribution in the bottom row of Table 2. It is similar[42] to the actual distribution for the longitudes.

N6   Newton's suggestion explains not only the distribution of the fractions and the systematic error of $-1°.1$, but also the otherwise-puzzling absence of $1/4°$ and $3/4°$ endings in the longitudes.

N7   N.B. Rawlins has used similar reasoning (*DIO 4.1* ‡3 §§D-E) to show that the coordinates of the stars in the southern sky were found, not with an armillary astrolabon, but with an instrument that measured zenith distances.[43]

O   Conjunctions (C262 to 265)

Ptolemy recorded a near-conjunction of each planet with the Moon and a star.

O1   On 139/05/17, 4 1/2 hours before midnight (S9.10) the longitude of Mercury (found by comparing it with Regulus) was $77°1/2$. Mercury was $1°1/6$ east of the centre of the Moon, giving the Moon a longitude of $76°1/3$. Its longitude calculated from Ptolemy's tables is $76°1/3$.

O2   On 138/12/16, 4 3/4 hours after midnight (S10.4) the longitude of Venus (found by comparing Venus with Spica) was $216°1/2$. Venus was on the straight line joining β Scorpii (whose longitude in the star catalogue is $216°1/3$) to the centre of the Moon

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[40] See C247 or *DIO 4.1* ‡3 §B4.

[41] See C250. Newton did not explain, but the implicit question is (*DIO 4.1* ‡3 §C1): who'd not look askance at a set of 359 stars containing a great many with 5/12 or 11/12 endings, but none with 1/12, 1/4, 7/12, or 3/4 endings? That 2/3 (or 1/6) had been added to all endings would be obvious and so would reveal the method of appropriation.

[42] A χ^2 test (*DIO 4.1* ‡3 §C4) upon the last two rows of Table 2 shows that the discrepancies are not statistically significant.

[43] See ‡4 §A23.

(whose calculated longitude was 216°3/4) and 1 1/2 times as far from the Moon as from β Scorpii. This gives Venus a longitude of 216°1/2, exactly as observed.

O3   On 139/05/30, 3 hours before midnight (S10.8) the longitude of Mars (found by comparing Mars with Spica) was 241°3/5. Mars was 1°3/5 east of the Moon, whose calculated longitude was 240°, giving Mars a longitude of 241°3/5, exactly as observed.

O4   On 139/07/11, 5 hours after midnight (S11.2) the longitude of Jupiter (found by comparing Jupiter with Aldebaran) was 75°3/4. Jupiter was directly north of the centre of the Moon and so had the same longitude. The calculated[44] longitude of the Moon was also 75°3/4, exactly as observed.

O5   On 138/12/22, 4 hours before midnight (S11.6) the longitude of Saturn (found by comparing Saturn with Aldebaran) was 309°04′. Saturn was 1/2° east of the northern tip of the crescent Moon. The calculated longitude of the Moon (neglecting the equation of time and using a parallax[45] mistaken by 8′) was 308°34′, giving Saturn a longitude exactly (to the nearest minute!) as observed.

O6   The agreements between the observed and calculated[46] longitudes could not have happened by chance.

P   Mercury (C287)

P1   Ptolemy calculated the apogee of Mercury in his own time twice and — also twice — the apogee 400 years earlier. He concluded [⊙17] that it had increased by 4°. If we calculate from Ptolemy's data to one decimal place in degrees and add 4° to the earlier values, we obtain 189°.9, 190°.3, 189°.8, 190°.0. The correct value during the 4 century period under consideration was never less than 220°. From genuine observations it is not possible to obtain values so close together and so far from the truth.

P2   In Ptolemy's theory of the motion of Mercury the centre of the epicycle revolves at a constant distance from a point F — which itself revolves (retrograde) at a constant distance round a fixed point D — and at a constant rate round a point Z situated between D and the Earth E. Ptolemy needed the values of the three small distances, EZ, ZD, and DF (see C260 Fig.X.3), which Newton called *eccentricities*. Ptolemy calculated them from his observations and made them all equal. Newton calculated the values they should have had, i.e., the values that made Ptolemy's mechanism fit reality most closely, and found them to be far from equal.[47]

Q   Venus (C313)

Q1   Ptolemy found the apogee of Venus twice, and the two values were only 2′ apart, although they were in error by about 4°. Newton commented "the probability that Ptolemy's agreement could have happened by chance is so tiny that we do not need to estimate it."

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[44]C263-264 suggests Ptolemy here made errors in the calculation of the equation of time and of parallax when computing the Moon's true and apparent longitude. But in fact Ptolemy's figures here agree well (±1′) with accurate calculations from his tables.

[45]Parallax used at S11.6 = −66′; actual parallax = −58′; computed from Ptolemy's S2.13 Lower Egypt parallax tables = −74′. Britton (*op cit* pp.140-141) was the first to reveal that, at the time of this observation, Saturn was actually behind the Moon, "thus, like the previous observation [§O2], the circumstances which Ptolemy describes could not have been observed at the time which he reports." [S11.6: "Saturn sighted with respect to" Aldebaran.] So, a skeptic might wonder whether the original (pre-manipulation) version of this record was just that of a plain occultation.

[46]From lunar tables with mean error 35′. See §M5. [And ⊙16.]

[47]See C278 Table X.2.

Q2   Ptolemy's theory of motion for Venus needs two small eccentricities: the distance from the Earth to the equant and the distance from the equant to the centre of the deferent. He calculated them from observations and made them out to be equal. This is important because Ptolemy assumed that this equality applies to all the remaining planets and for each of them took it to be part of the theory of motion. They should not be equal, as Newton found (see C311, Table XI.2) by calculating the values which fit reality most closely. (And, I might add, as Kepler had earlier found.)

R   Epicyclic Anomaly (C320 to 321)

R1   For each of the planets Ptolemy calculated the epicyclic anomaly from an old and a new observation; and from the change in anomaly and the interval between the two dates he calculated the change in anomaly per day. This is done by dividing the total change in anomaly by the number of days in the interval. But the degrees per day that Ptolemy quoted and tabulated (S9.4) do not agree with the values obtained by division. For example, for Jupiter, division gives[48] (in sexagesimals)

$$0;54,09,02,45,08,57$$

whereas Ptolemy quoted[49]

$$0;54,09,02,46,26,00$$

The difference is tiny, but we are not talking about accuracy; we are talking about whether a division is right or wrong. It is wrong.[50]

R2   Newton suggested (C325 to 327) that for each of the planets Ptolemy calculated[51] the anomaly at the later observation from the anomaly at the earlier observation and the rate that he quoted. Rounding would account for the small discrepancy. Ptolemy neglected to calculate from the "observation" he quoted to verify whether it did give the change per day that he quoted.

S   Final Remark

S1   To my mind, the most remarkable thing about the whole affair is not Newton's intemperate language, arising no doubt from frustration at not being able to use data from the *Syntaxis* in his own researches, coupled with disdainful treatment by the historical (not astronomical) establishment. Nor is opposition to Newton particularly surprising; the establishment often digs in its heels and puts on blinkers when a radical and ingenious proposal is set forth. No: the remarkable thing is that Delambre's devastating and irrefutable proof that Ptolemy lied about his "observations" of the equinoxes and solstice was ignored for so long.

S2   For that matter, Christian Severin's similar accusation, without the calculations, was ignored or disbelieved for much longer. In 1639, in *Introductio in Theatrum Astronomicum*, L i f 33, he wrote: *Non tantum erasse ilium dixit observando sed plane finxisse observatum quod ex Hipparcho computaverit.*

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[48]A result accurate to the last place is given by Toomer (*op cit* p.669): 0;54,09,02,45,08,48.

[49]The null sixth sexagesimal place is absent in Ptolemy's first rendition, the *Canobic Inscription*. See *DIO 2.1* ‡3 fn 23.

[50]The sources of Ptolemy's tabular planet speeds, out to full 6-sexagesimal-place precision were 1st revealed by Rawlins [⊙18] & A.Jones (*DIO 11.2* p.30 & ‡4 eqs.31&45).

[51]This would be the same procedure used to fabricate the four solar observations [⊙2]. Again, Ptolemy computed data from theory instead of theory from data. At the very least, one can protest that this is mathematics, not science. And see *DIO 1.2* fn 99 for what establishment opinion thought of such behavior — before it became undeniable that Ptolemy had engaged in it.

⊙ **Notes by DR**

⊙**1**   [Note to §A2.] The remarkably large latenesses of Ptolemy's alleged observations average more than a day. (See Table 1.) The errors for all 4 of Ptolemy's claimed solar observations: $+33^h$ (132 AE), $+33^h$ (139 AE), $+21^h$ (139 VE), $+36^h$ (140 SS). Notice that the observations' errors exhibit close (but not perfect: ⊙2) fidelity to both the mean and periodic errors of his adopted Hipparchan solar theory. (See similar stellar effect at ⊙8.) Ptolemy's solar tables are believed to be those of Hipparchus because the tables' mean errors are huge, $-1°.1$ for his own era, though nearly null for Hipparchus's time. (This is true not only for the Sun, but for the Moon, all five planets, and the stars.) Because of the accumulated effect of the incorrect length of the Hipparchus year (§A1) on which the solar tables are based, these tables' error increased by 6^m/year (*idem*), or 1^h/decade. So, in the 26 decades between Hipparchus and Ptolemy, the mean error grew to about 26^h (over a day late), which represents $-1°.1$ of mean solar motion — and that is just the mean error of Ptolemy's solar tables. Notice, too, that there is a periodic error (also from Hipparchus) which has the considerable amplitude of $0^h.4$. Its effect, too, is faithfully reflected in Ptolemy's "observations", which is why, though the different observations average over a day of lateness, predictably (akin to Gingerich's finding: *DIO 1.3* fn 223) theory-responsive errors occur on either side of that mean. For Ptolemy's epoch (137.547), the tables' error in time t or in Ptolemy's solar longitude λ can be expressed as:
$\Delta t = +26^h 1/4 + (10^h 3/4) \sin(\lambda - 38°)$ in hrs; $\Delta \lambda = -65' - 25' \sin(\lambda - 44°)$ in arcmin.

⊙**2**   [Note to §A3.] You can use the AD 139 equinox as an example of how Ptolemy fabricated all his solar observations by simple arithmetic: since there are 285 years from -146 to 139, just multiply 285 times Hipparchus's year (§A1), and add the product to the Hipparchus observed autumn equinox, $-146/09/27\ 00^h$. As an equation, this is (⊙3):

$$-146/09/27\ 00^h + 285 \cdot (365^d 1/4 - 1/300) = +139/09/26\ 07^h 12^m \qquad (1)$$

a result which is (if rounded to the nearest hour) just equal to Ptolemy's very erroneous "observed" autumn equinox. See Table 1. All the other "observed" equinoxes in this table may also be computed by the elementary arithmetical method of eq. 1. (But Ptolemy did not even use the tables of the *Syntaxis* for his fabrications. (See *DIO 1.2* fn 166.) If one goes to that slight trouble, the results are discrepant (vs. his "observations") by 1^h in some cases. So we know that Ptolemy used mere arithmetic for these fabrications. Note that the errors are so enormous that the "observed" and actual solar disks do not even touch, much less overlap. Likewise for the lunar "observation" discussed at §§H2-H3.

⊙**3**   [Note to §A2, Table 1, & ⊙2.] All dates here are in the Julian calendar (yearlength = $365^d 1/4$). For solving eq. 1, there is an easy method (not Ptolemy's, since he used the Egyptian yearlength, 365^d): adding 285 Julian years to the Hipparchus $-146/09/27$ equinox yields the original time of the year plus a quarter day (since 285 is 1 mod 4); so, the intermediate result is: $139/09/27\ 00^h$. Adjusting for Hipparchus's yearlength, we need only alter the foregoing calculation by $-285/300$ days or $-22^h 48^m$, which is $01^h 12^m$ minus 1^d; so the fabricated autumn equinox obviously must be $139/09/26\ 07^h 12^m$.

⊙**4**   [Note to §B.] On receiving an offprint of this paper, B. L. van der Waerden, author of a Springer mathematical statistics text, offered a prediction which has proven sadly prescient (1982/7/24 letter to DR): "I am afraid that most historians will not understand your mathematics and — even worse — not be willing to admit that you are right, but I assure you that your mathematics is OK and that your conclusions are sound and extremely interesting." This is the same analysis that Ptolemy's defenders are still seeking an escape from. See *News Notes*; also *DIO 2.3* ‡8 §C & J.Evans *History & Practice . . .* 1998 pp.268f.

⊙**5**   [Note to §C2.] The S4.11 pair's implicit epicycle-radii are effectively computed by Newton (C122 Table VI.3) from his own and Ptolemy's reductions of the data. (Table VI.3 gives the equation of center E, but the epicycle radius is easily computed, since it is equal to $60 \cdot \sin E$.) We here cite the latter calculations. (The notation here, common for scholars of ancient astronomy, says that the four measures of the epicycle's size were, in the specified units: 5+13/60, 5+14/60, 5+15/60, 5+15/60, respectively.)

⊙**6**   [Note to §D.] Toomer *op cit* page 224 note 11 remarks that Ptolemy's language may indicate use of the analog-computer ecliptic-ring-shadow method for determination of the solar longitude via armillary astrolabon (the following assumes an accurately-fashioned instrument): the observer spins the armillary astrolabon equatorially, turning fused ecliptic rings #3 (ecliptic ring) & #4 (colure ring) in Rawlins 1982 Fig.1 (or Toomer *op cit* page 218 Fig.F) until its sunward part of ring #3 casts its shadow symmetrically upon the inward part of the opposite side of itself. He then freezes the equatorial motion and, within the clamped rings #3-#4, turns ring #2 (the latitude ring) until its shadow is cast upon itself; then the intersection of rings #2&#3 will automatically be at the graduation on ring #3 corresponding to the Sun's actual longitude. (Which ensures that all the instrument's rings are correctly oriented with respect to the actual sky.) We will call this the Fundamental Method. But S5.1 suggests another method, for locating non-solar objects: first set the reference-object ring (#5) upon the *tabular* solar position on ring #3 and then spin the rings #3-#4-#5 unit until the shadow of ring #5 is upon itself. (If the tabular solar position is accurate, all the instrument's rings are now oriented to reality, so the unit is clamped.) Ring #2 within is now quickly spun & used to locate the quarry object. We will call this the Tabular Method. Ptolemy's defenders prefer assuming his use of the Tabular Method for good reason: if the Fundamental Method would produce correct longitudes, then his claim that he used it (while consistently acquiring data which always agreed with his tables instead of reality) convicts him of fabrication. But it turns out that this defense doesn't hold, because Ptolemy states (S5.1) that the Tabular Method caused ring #3 to cast its shadow "exactly on itself" — just as in the Fundamental Method. But *whichever* of the two methods Ptolemy chose, he would typically find a huge conflict with the other, because his solar tables' average error was a *degree*. The fact that he was unaware of this flagrant and persistent contradiction raises a question regarding whether he ever actually used an armillary astrolabon. (See also ⊙9, *DIO 2.3* ‡8 C26[b], & *DIO 4.1* ‡3 fn 7. And see a similar problem at Rawlins 1982 page 367: Ptolemy's erroneous geographical latitude is incompatible with the Ancient Star Catalogue's data.)

⊙**7**   [Note to §D.] J.Włodarczyk (*J. Hist. Astron. 18*:173; 1987) suggests that the 139/02/23 solar longitude 333°1/20 was calculated from Ptolemy's tables (which give $333°04'$ for this time) — i.e., that he used the Tabular Method instead of the Fundamental Method. (Both described above in ⊙6.) Assuming that the observation was made right at sunset (with a full $34'1/2$ of mean horizontal refraction), the effect upon the Fundamental Method's measure of solar longitude would have been $+0°.8$ — but the 333° longitude's error was actually $-0°.7$. This enormous discrepancy ($1°1/2$) proves that the Fundamental Method was not used. Włodarczyk believes that Ptolemy instead merely set the reference ring (ring #5) on tabular longitude 333°. The above-noted $-0°.7$ error of Ptolemy's solar tables, added to a 0°.6 astrolabon mis-aim in longitude caused by refraction of the Sun's light, could lead to the measured 139/02/23 longitudes of the Moon and Regulus being read as much as $1°.3$ low. (Ptolemy's Regulus longitude was off by $-1°.6$ in 139 AD.) But most of Ptolemy's defenders (Britton *op cit* p.144 provides a welcome exception) have overlooked Ptolemy's equally crucial *2nd* observation of 139/02/23 — the very one which actually locates Regulus, an observation *which can only be trivially affected* (c.1′) *by refraction*. Ptolemy reports (S7.2) that he observed Regulus $57°1/6$ east of the Moon, whereas the actual difference (including refraction and parallax) at this time (18^h Alexandria Apparent Time) was $57°.7$. This huge error (twice the lunar semi-diameter) was *required*, to get Regulus's longitude to come out "right", i.e., *exactly* $2°2/3$ more than Hipparchus's Regulus longitude, which (unfortunately for Ptolemy) happened to be enormously wrong in the negative direction. So Ptolemy had to make a gross negative error to match Hipparchus's. (Thus, Regulus became Ptolemy's most negatively misplaced principal star. As with the solar errors remarked at ⊙1, Ptolemy mimics not only Hipparchus's mean errors but his fine errors.) Again, the consideration to be stressed here (which Włodarczyk *op cit* page 182 quotes, under the misimpression that he has undercut it) is Newton's at C254 (see also C218, 147-148): "the value [of Regulus's longitude] that Ptolemy obtains cannot be explained by experimental sources, no matter what their size. The crucial point is the exact agreement with preassigned values; and exact agreement, occurring time after time, cannot be the consequence of errors in measurement." (See ‡4 fn 11.)

⊙**8**   [Note to §D.] The wording at S7.2 (like that at S5.3: ⊙6) implies use of the Fundamental Method (which cannot have happened: ⊙6). By the way, Newton is wrong in doubting (*QJRAS 20*:383 [1979] page 391 footnote) J.Dreyer's calculation of the effect of refraction upon the Fundamental Method. Newton accounts only for ω, the effect on astrolabon spin-orientation, but neglects ξ, the direct effect of refraction upon longitude. If we call ψ the angle (tabulated at S2.13) between the ecliptic and the vertical, and call η the angle between the ecliptic and the circle of constant declination (where $\sin \eta = \sin \epsilon \cos \alpha$, for ϵ = obliquity and α = right ascension), then for refraction r, we

have: $\omega = r \sin \psi / \tan \eta$ and $\xi = r \cos \psi$. Longitude error $\Delta\lambda = \omega + \xi$. But a much simpler equivalent expression can be formed by using θ, the angle between the vertical and the circle of constant declination. (With $\cos\theta = cos\phi \sin HA / \cosh$, where ϕ = geographical latitude, HA = hour angle, and h = altitude, we can find ψ [useful for ecliptical parallax problems] by simply remembering that $\psi = \theta - \eta$.) Then we have just: $\Delta\lambda = r \sin\theta / \sin\eta$. (Which obviously breaks down near the solstices, where it is well-known that the Fundamental Method is inapplicable.) This equation is most clearly understood by seeing that $\Delta\lambda$ must be simply due to refraction's effect upon declination (as Dreyer said), this because the instrument is an analog computer of solar longitude (strictly) as a function of declination. Note: this computer will of course misread if the ecliptic ring is not tilted (with respect to the equator) by the correct obliquity. Ptolemy's $+11'$ obliquity error is therefore one more reason why one questions (also at ⊙6) whether he ever used an astrolabon. If his putative instrument's ecliptic ring #3 was actually tilted at his ϵ instead of the correct $23°40'.7$, he would surely have found that his solar longitudes obtained by the Fundamental Method were in error by large amounts — several *degrees* near the solstices.

⊙**9**   [Note to §E4.] A remarkable feature of the perfect agreement Ptolemy finds is that the annual error in his (Hipparchus's) lunisolar theory is near its absolute maximum of $0°.2$ for both the S5.3 observations and thus for both of his seemingly precise calculations (from these data) that Moon-minus-M equalled exactly $7°2/3$. Such considerations emphasize how rigged these calculations had to be in order that (even while degraded by $0°.2$ errors in parallax and in lunar elongation, among other effects), they nonetheless gave precisely the same answer down to the arcminute. (It is worth remarking that both the Hipparchus and Ptolemy half-Moon observations were taken at just the right time: in both cases, Moon-minus-M was actually very near maximum.)

⊙**10**   [Note to §F1.] For ease and clarity of manipulation, we may set $MC_1 = R$ and $C_1E = \rho$. Ptolemy arbitrarily established a scale in which M-to-E's maximum (syzygial) distance $MC_1 + C_1E$ or $R + \rho$ is defined as 60 units. Then he simply solved for ρ in the equation $(5; 15)/(60 - 2\rho) = \sin 7°2/3$. The equation forces ρ to equal 10;19. This in turn implies that R must be $60 - \rho = 49;41$, which makes M-to-E's minimal (half-Moon) distance equal to $R - \rho = 60 - 2\rho = 39;22$.

⊙**11**   [Note to §F4.] C156 reconstructs the actual angle Moon-minus-M as $1°23'$, but Newton says in a footnote that this modern calculation is rough, subject to an uncertainty of probably less than $10'$. DR finds $1°17'$, which is indeed within $10'$ of Newton's result. (For his calculation in §F5, Newton gets $2°19'$. DR finds likewise.) Note that in Hipparchus's time, tabular M virtually agreed with actual M. So, the error in Moon-minus-M should be effectively the same as the error of observation of the lunar elongation. That this is not true is due partly to the periodic error of the Hipparchus lunisolar theory and partly to errors introduced by Ptolemy's manipulation of the reductions of both S5.5 calculations. (See the large $0°.2$ discrepancy correctly computed by Toomer, *op cit* page 230 n.23.) Also, Hipparchus's observations of the elongation (angular distance of topocentric Moon from Sun) are consistently underestimated by a large amount. Were these errors heavily due to a misunderstanding of what was actually being observed by Hipparchus? (The hypothesis that follows suggests poor coordination between observers and mathematicians in Hipparchus's school. But we saw the same in Tycho's group at *DIO 3* §N. For a different sort of Hipparchus-circle disjunction, see *DIO 1.3* eqs.23&24.) If we include the $-127/08/05$ observation of S5.3 (§E3) with the S5.5 pair under study here, we have a nearly contemporaneous trio (the same trio whose solar positions are analysed in *DIO 1* ‡6, ‡9 §G10, and *DIO 6* ‡3 §D). The observational errors in Hipparchus's elongation measurements were: $-23'$ ($-127/08/05$), $-23'$ ($-126/05/02$), and $-26'$ ($-126/07/07$). (These figures are based upon *DIO* calculations, controlled here [and elsewhere in this paper] by close comparison to more exact values generated for *DIO* by Myles Standish of JPL, Cal Tech.) The fact that all these errors make the elongation much too *small* is provocative, especially since the effect existed regardless of which side of the Sun the Moon was on. A simple theory can begin to illuminate this odd circumstance: those using Moon-shots for navigation know that it is in general better to observe the Moon's limb than its centre. (The Moon was crescent for all 3 of the observations considered here.) If we assume Hipparchus's three elongation observations measured the angle between the solar centre and the lunar limb, we are left with errors of but $-8'$, $-6'$, and $-11'$, respectively, in absolute elongation. Analysis of his star catalogue has shown that, after the armillary astrolabon was oriented (⊙6) and clamped in place, Hipparchus took an average of 19^s to complete a stellar observation, during which time the Earth turned enough to affect a longitude reading by about $-4'$. (See *DIO 1.1* ‡6 §G4.) The effect would decrease measurements of absolute elongation for eastern elongation and increase it for western. Correcting the three residuals for this effect, they become $-12'$, $-10'$, $-7'$. With or without such correction, this is a remarkably steady data-set for antiquity. And it is possible that the observer (unwisely) used the Sun's

limb instead of centre. If, on this speculative theory, we apply solar semi-diameter ($16'$), the residuals fall to merely $4'$, $6'$, $9'$ (mean error $6'$). Anyway, with or without solar-semi-diameter correction: residuals of ordmag $0°.1$ are more than satisfactory here. (The slight systematic bias could be due to a personal equation in nonsymmetric measurements. E.g., if the solar limb was the intended reference point, the observer may have aimed ring #5 in such a way as to just barely allow the Sun's limb to glimmer over the ring's plane.) The results' evident consistency here argues in favor of the probability that the ΔT (barely over 3^h) used for these calculations is correct to within a very few timeminutes, since any error in ΔT will have an effect (about $1'$ per 2^m error in ΔT) upon the 3rd residual (eastern elongation) virtually equal to that for the other two (both western), but opposite in sign.

⊙**12**   [Note to §I4.] Taken literally, a negative $2'.2$ solar parallax (fn 25) would mean, since $\csc(2'.2) = 1563$, that the Sun's distance is about 1600 Earth radii but — in the opposite direction from which the Sun's light is arriving! Evidently, Ptolemy's cosmology was even more original and advanced than even his most adoring modern historian-promoters have hitherto perceived.

⊙**13**   [Note to §K1.] Ptolemy's stated star catalogue epoch (S7.4) is 137/07/20 (137.547). (Though, see ⊙14.) The epoch of Hipparchus's star catalogue was about 2 2/3 centuries earlier. For indications that his exact chosen epoch was $-127/9/24\ 1/2$ (-126.278), see *DIO 1.1* ‡6 §§F and G4. The Hipparchus and Ptolemy longitudes of Regulus were $119°5/6$ and $122°1/2$, respectively. Errors for the given epochs: $-35'$ and $-1°33'$, respectively. Hipparchus's longitude of Spica: $174°$ (error $-19'$).

⊙**14**   [Note to §L1.] The likely epochs of the several astronomers' star observations: Timocharis roughly 300 BC; Aristyllus c.260 BC (see p.263 of *Isis 73:259* [1982], *DIO 4.1* ‡3 fn 40); Hipparchus c.130 BC (see ⊙13); for Ptolemy's declinations, the epoch indicated by the stellar data is AD $159±8$ yrs (*DIO 4.1* ‡3 Table 3). As to whether Ptolemy made these accurate observations (which are not fabricated), see page 236 item (2) of D.Rawlins *Amer J Physics 55:235* (1987) and n.17 of *Isis* ref (above in this note).

⊙**15**   [Note to §M3.] Conjunction occurred about two hours later than the time Ptolemy accepts from Timocharis. The corresponding longitudinal discrepancy is huge: about $0°.8$ (more than triple the $15'$ lunar semi-diameter). And, at conjunction, the northernmost part of the Moon slipped past β Scorpii at a distance of $11'$ to the south (more than 2/3 of the Moon's semi-diameter); so the reported contact never occurred at all.

⊙**16**   [Note to §M5.] An $8'$ lunar motion in a half-hour is seemingly trivial, but it is nonetheless quadruple the size of the minuscule ($2'$) discrepancies implicitly claimed by Ptolemy (§M4). In fact, for all but one of the seven cases, the precision is an hour; thus, a $\pm 1^h/2$ error-range ($16'$ of motion) is built into six of the calculations. Note that (unlike Newton's point regarding the lunar tables' $35'$ mean error), this was *obvious to any careful reader from antiquity to the present*. (For other problems with Ptolemy's work that were discernable all along, see fn 32, *DIO 1.1* ‡7 §§A3-B1, *DIO 4.2* ‡6.) [See also *DIO 2.1* ‡2 §H9 & resulting *DIO 1.1* ‡6 fn 37.] Even more serious in connection with his use of lunar occultations (see also fn 46) are the errors in his tables for the longitude of the Moon, which have a standard deviation of $35'$ (C238), more than 10 times the $2'$ agreements he managed to arrange on the basis of computations from these tables. [For similar outrageously-overneat data-agreements in a modern hoax: see *DIO 10* §G2.]

⊙**17**   [Note to §P1.] In brief, Ptolemy used observations (real or no) contemporary and ancient to him, to prove that Mercury's apogee precessed at the $1°$ per century rate of stellar precession. This becomes critical because he then assumed that the other four planets' apogees precessed at the same rate. By contrast (a contrast that is curious for a work attempting to synthesize astronomy), he kept the solar apogee fixed. This feature presumably represents an assent to the by-then near-sacred nature of Hipparchus's solar orbit.

⊙**18**   [Note to fn 50.] Correct DR solutions to Ptolemy's mean motions of Mercury, Venus, & Saturn were transmitted to Owen Gingerich & R.Newton in 1980. [DR missed on Mars & Jupiter; however, his general (then-heretical) thesis that period-returns explained all the planets' mean motions was luckily redeemed — and placed beyond all doubt — when Alex Jones deftly solved Mars' & Jupiter's motions at a stroke in 2003 Sept: see the Jones source cited at fn 50.]

‡2   Mediæval Indians and the Planets

by Hugh Thurston[1]

A   The Kalpa Periods

A1   The main parameter in the mediæval Indian theory of the motion of the planets is the sidereal period. (Yes — even for the inner planets.) They presented this by listing the number of revolutions in a given period. One particularly interesting list is found in the *Brahmasphuta* astronomical tables, compiled in the seventh century A.D. The numbers of revolutions in a period known as a *kalpa* are as follows.

Sun	4,320,000,000
Saturn	146,567,298
Jupiter	364,226,455
Mars	2,296,828,522
Venus	7,022,389,492
Mercury	17,936,998,984

How can we explain these extraordinarily precise figures?

A2   The first few digits in each of the figures cited can be deduced from any reasonably accurate observations. The last few cannot. Let us look at the last four digits of each of the figures.

Saturn	7298
Jupiter	6455
Mars	8522
Venus	9492
Mercury	8984

It would strike a cryptographer at once that these are not random; there is some underlying system. We see this most clearly if we subtract the figures from 10,000. We get:

2702	(a)
3545	(b)
1478	(c)
0508	(d)
1016	(e)

Clearly $e = 2d$. No other regularities are quite as obvious as this, but let us look at the differences; $c - d = 970$, which, subtracted from 10,000 is 9030. This is 10×903, and b is the last four digits of 15×903.

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A3   So the number 903 makes itself felt. How can we explain it? Perhaps we have multiples of 903 here. If 2702 is a multiple of 903, the multiplier must end in 4. If we try 4, 14, 24 etc. times 903 we soon find that $34 \times 903 = 30702$ with the same last three digits as a. We can do the same for the other four figures.

$$
\begin{aligned}
34 \times 903 &= 30702 &&(\text{compare } a)\\
15 \times 903 &= 13545 &&(\text{compare } b)\\
26 \times 903 &= 23478 &&(\text{compare } c)\\
36 \times 903 &= 32508 &&(\text{compare } d)\\
72 \times 903 &= 65016 &&(\text{compare } e)
\end{aligned}
$$

So let us add these multiples of 903 to the figures in the *Brahmasphuta*. We get the following results.

146,598,000	(multiplier 34)
364,240,000	(multiplier 15)
2,296,852,000	(multiplier 26)
7,022,422,000	(multiplier 36)
17,937,064,000	(multiplier 72)

A4   In each case the last digit before the final three zeros is twice the last digit of the multiplier. This is no coincidence: we have not yet found the complete system. If we subtract 2000 times the multipliers from the figures we reduce these digits to zero. Subtracting 2000 is equivalent to adding 8000, so clearly we should have added multiples of 8903, not 903. (But see §A8). Let us do this.

$$
\begin{aligned}
146,567,298 + 34 \times 8903 &= 146,870,000\\
364,226,455 + 15 \times 8903 &= 364,360,000\\
2,296,828,522 + 26 \times 8903 &= 2,297,060,000\\
7,022,389,492 + 36 \times 8903 &= 7,022,710,000\\
17,936,998,984 + 72 \times 8903 &= 17,937,640,000
\end{aligned}
$$

This suggests that the Indians found approximate figures for the numbers of revolutions in a *kalpa* and subtracted multiples of 8903 to give the final ultra-precise figures. To see why they did this we have to turn from cryptography to astronomy.

A5   One ten-thousandth of a *kalpa* is a *kaliyuga*. The current *kaliyuga* started 4567 *kaliyugas* after the start of the current *kalpa*. There is a remarkable connection between the number 8903 deduced by pure arithmetic and the attested number 4567:

$$4567 \times 8903 = 40660001$$

The approximate numbers of revolutions in a *kalpa* listed above end in four zeros, giving a whole number of revolutions in a *kaliyuga*. Subtracting $8903n$ revolutions in a *kalpa* subtracts $4066.0001n$ revolutions in 4567 *kaliyugas*, i.e. a whole number plus $0.0001n$. A *kalpa* is a period at whose beginning and end the planets were believed to have mean longitude zero. So the *Brahmasphuta's* figures imply that at the beginning of the current *kaliyuga* the mean longitudes of the planets fell short of zero by 0.0034, 0.0015, 0.0026, 0.0036 and 0.0072 revolutions respectively. (0.0015 revolutions is just over half a degree.)

A6   All this suggests that the Indians had figures for the mean longitudes of the planets at the start of the current *kaliyuga* (which was actually -3101/02/18 Julian) and modified the approximate numbers of revolutions to give these figures. To do this, having chosen the number 4567 (no-one seems to know why they chose this particular number), they had only to find a number which, when multiplied by 4567, yields a number ending in 0001. So they had to find whole numbers that satisfy the equation $4567x = 1000y + 1$. This was a standard problem in mediæval Indian mathematics. They used a technique, usually translated as "pulverizer". It is a development of Euclid's algorithm.

A7   There is, however, a much simpler way to find a number x such that $4567 \times x$ ends in 0001. Only the last four digits of x matter. Let us call them $abcd$. The last digit of $4567 \times abcd$ is the last digit of $7 \times d$. For this to be 1, d must be 3. (Proof: multiply 7 successively by 0, 1, 2 ... 9. Only 3 yields a product ending in 1.) The last two digits of $4567 \times abc3$ are the last two digits of $67 \times c3$. For these to be 01, c must be zero. (Proof: multiply 67 by 03, 13, 23 ... 93. Only 03 yields a product ending in 01.) The last three digits of $4567 \times ab03$ are the last three digits of $567 \times b03$. For these to be 001, b must be 9. (Multiply 567 by 003, 103 etc.) Finally, multiplying 4567 by 0903, 1903, etc. we find that only 8903 yields a product ending in 0001.

A8   Instead of adding multiples of 8903 we could subtract multiples of 1097. Because $8903 + 1097 = 10000$ this will have the same effect on the longitudes. If we do this we get, instead of the round numbers reconstructed above (§A4), the following round numbers:

146, 530, 000	Saturn
364, 210, 000	Jupiter
2, 296, 800, 000	Mars
7, 021, 350, 000	Venus
17, 936, 920, 000	Mercury

A9   The upshot of all this is that anyone trying to find the source of the figures in the Brahmasphuta is not faced with the gargantuan task of explaining the extraordinarily precise figures first quoted but with the much less daunting task of explaining the round numbers reconstructed here.

B   Reference

Brāhma-sphuta Siddhānta, Indian Institute of Astronomical and Sanskrit research, New Delhi, 1966. (This edition has a long and useful introduction in English.)

‡3   Code-breaking in the second world war

by Hugh Thurston[1]

A   The initial break into Enigma

A1   Some time before the outbreak of the second world war the Poles began intercepting German code messages with a very distinctive feature. Here are the beginnings of some messages received on the same day. (Not the actual messages, of course; I have made up some examples to illustrate the Polish cryptographers' methods.)

```
 1   d   h   z   e   c   n   w   k   ...
 2   a   l   w   r   w   p   f   u   ...
 3   s   v   g   w   s   r   j   q   ...
 4   d   e   b   e   k   h   p   q   ...
 5   h   q   x   h   a   z   m   c   ...
 6   h   e   c   h   k   l   w   o   ...
 7   u   q   b   g   a   h   e   l   ...
 8   a   q   c   r   a   l   w   g   ...
 9   z   w   j   v   i   c   k   d   ...
10   m   u   m   x   r   o   w   j   ...
11   r   s   e   o   p   u   m   n   ...
12   s   p   q   w   q   q   x   o   ...
13   e   f   d   p   h   f   r   j   ...
14   e   m   a   p   e   d   l   v   ...
15   f   n   a   m   f   d   p   q   ...
```

A2   The feature is that whenever two messages have the same first letter they have the same fourth letter. For example, messages 2 and 8 both have first letter **a** and fourth letter **r**; messages 1 and 4 both have first letter **d** and fourth letter **e**, and so on. Similarly, whenever two messages have the same fourth letter, they have the same first letter. The same relation holds between the second and fifth letters and between the third and sixth letters. This is something which any cryptographer would spot, but the Poles continued with something a good deal more subtle. They looked at the correspondences between the first and fourth letters:

First	d	a	s	h	u	z	m	r	e	f
Fourth	e	r	w	h	g	v	x	o	p	m

and began to build them up into chains. From **de** and **ep** they formed **dep** and so on. They had far more than the fifteen messages I have displayed (at least eighty each day) and managed to incorporate all the letters of the alphabet, the final result being

$$\textbf{(depzvlyq)(aronjfmx)(gktu)(icsw)(b)(h)} \tag{1}$$

[Note added 2008/11/5. At Midway (1942/6/4), the Allies' code-breaking edge was instrumental in the Japanese invading fleet's sudden simultaneous loss of 3 of its 4 aircraft-carriers and thus most of its c.300 airplanes & veteran ace pilots . (Its commanders had learned of the US' nearby-lurking 3 carriers only 2^h earlier and could not prepare a full attack in time, an attack that indeed was just being launched at the very minute when elated US dive-bombers struck.) Yet, before Japan's entire 300-plane force was lost (along with the 4th carrier), just 16 of the final few planes sank the US carrier *Yorktown*, in retaliation, suggesting that had Japan's original full 300-airplanes been available to strike at the Allied fleet, the Battle of Midway would've ended quite differently.]

These chains are cyclic; not only does **d** as first letter correspond to **e** as fourth, but **q** (the last letter in the first chain) corresponds to **d**, the first in the chain. So I shall refer to them as cycles rather than chains. Note that **h** corresponds to itself; so does **b**.

A3   The corresponding results for second-to-fifth and third-to-sixth are

$$(\textbf{axzlwiobyvspq})(\textbf{hcurtmekdgjnf}) \tag{2}$$

and

$$(\textbf{adfbhgrsywpk})(\textbf{znmoieutjclx})(\textbf{q})(\textbf{v}) \tag{3}$$

Now we notice another striking feature. Each of these results contains two cycles of each length: (1) has two of length 8, two of length 4, and two of length 1; (2) has two of length 13; (3) has two of length 12 and two of length 1. To anyone familiar with the mathematical theory of permutations — and the Polish cryptographers were mathematicians — this rings a bell.

A4   The connection between cryptography and the theory of permutations is that an encoding is a permutation. If we write out the alphabet and under each letter write its encode, for example:

  Original   **abcdefghijklmnopqrstuvwxyz**
  Encode   **hiewdfvxgybuctramnskzqjlop**

then the encoding permutes the top row into the second row.

A5   There is a standard way of writing permutations. Here **a** permutes to **h**, so we write **ah**. Also **h** permutes to **x**, giving us **ahx** and so on. Continuing in this way, we write the above permutation in cycles as

$$(\textbf{ahxluzp})(\textbf{bigvqmcedwjyorntk})(\textbf{f})(\textbf{s})$$

We represent permutations by capital roman letters. The letter into which A permutes **x** is denoted by **x**A. If we apply permutation A and then apply permutation B to the permuted letters, the resulting permutation is denoted by AB and called the composite of A with B. For example, if A is **(abc)(de)(f)** and B is **(ac)(be)(df)**, then A sends **a** to **b** and B sends **b** to **e**, so AB sends **a** to **e** and one of its cycles starts **ae**. You can easily check that AB is **(aefdb)**. The result of applying AB to **x** is the same as applying B to **x**A, so **x**(AB) = (**x**A)B, and we can write the result unambiguously as **x**AB. Similarly (AB)C = A(BC) = ABC, the result of applying first A, then B, then C.

A6   If X is an encoding, the corresponding decoding is denoted by X^{-1}, it is the reverse permutation and its cycles are the cycles of X reversed. A permutation in which every cycle has length 2 is called a pairing. If X is a pairing, X^{-1} = X. The result in the theory of permutations that rang a bell for the Polish cryptographers is the theorem that the composite of two pairings must have an even number of cycles of each length (as do (1), (2) and (3)).

A7   We can see why this happens as follows. Suppose that we want XY, where X = **(ab)(cd)(ef)(gh)** and Y =**(ad)(bh)(cg)(ef)**, to take a reasonably compact example. In X, $\textbf{a} \rightarrow b$; in Y, $\textbf{b} \rightarrow h$; so in XY, $\textbf{a} \rightarrow h$. Write this as

  a h
   b

Similarly

  h c   and   c a
   g   d

  bringing us back to where we started. Putting these together we have the set-up

$$\begin{array}{l} \textbf{a h c } [\text{a}] \\ \textbf{b g d} \end{array} \tag{4}$$

A8   The northwest-to-southeast pairs are pairs of X, the northeast-to-southwest pairs are pairs of Y, the top row **(ahc)** and the bottom row reversed **(dgb)** are cycles of XY, necessarily of the same length. We continue in the same way with any unused letters, in this case

  e [e]
  f

each time obtaining two cycles of the same length. We deduce that each of (1), (2) and (3) is the composite of two pairings. This is what would happen if each of the six encodings is a pairing and the first six letters of each message are the first three letters repeated. We can see this as follows.

A9   Suppose that the first and fourth encodings are X and Y and that they are pairings. If the first letter in an encoded message is **a**, the original first letter is **a**X. If the original fourth letter is the same, the fourth letter in the encoded message is **a**XY. The same applies to any letter, so the first-to-fourth correspondence is XY, a composite of two pairings. This deduction ties in with what Polish Intelligence had discovered. The Germans had a machine, the Enigma, which encoded by pairings. It used three rotors, each of which could be set in 26 different positions, which could be denoted by letters. The recipient of a message would need to know the positions of the rotors, so the message would need to contain a trio of letters giving this information. It is a good bet that the first six letters of the message are this trio repeated.

A10   We now have the following problem: given XY, where X and Y are pairings, find X and Y. To take a simple example, what pairings will give

  XY= **(ahc)(dgb)(e)(f)** ?

The set-up labelled (4) above shows us how to find out. Write the second cycle of XY reversed under the first cycle. There are three ways to do this:

  a h c [a]   **a h c** [a]   **a h c** [a]
  g b d   **g d b**   **d b g**

And there is one way to write e under f. We get three solutions:

  X= **(ab)(hg)(cd)(ef)**,   Y= **(ad)(cg)(hb)(ef)**
  X= **(ag)(hd)(cb)(ef)**,   Y= **(ab)(cd)(hg)(e**f**)**

and

  X= **(ad)(hb)(cg)(ef)**,   Y= **(ag)(c**b**)(hd)(ef)**

Notice that the two cycles **(e), (f)** of length 1 imply that in every solution e and f encode into each other.

A11   It is easy to see that for (1) there are 32 solutions. The next step is to pick out the right solution, and to do the same for (2) and (3). To do this, the cryptographers used a fact that we found over and over again in cryptography: ask anyone to choose something "at random" and the result, though arbitrary, is usually far from random. The German operators did not choose the settings for the rotors randomly, but showed a strong predilection for trios like **aaa, bbb**, etc. or **abc, bcd** etc.

A12   Let the first six encodings be A, B, C, D, E, F, so that AD is (1), BE is (2) and CF is (3). From the two cycles of length 1 in (1), we see that in A and D the letters **b** and **h** encode into each other, so messages 5 and 6 start with **b**. Could either of them be **bbb**? Let us start with message 5. If we have

  A   B   C   D   E   F
  h   q   x   h   a   z
  b   b   b   b   b   b

then **(qb)** must be a pair in B and **(ab)** in E. This is not possible; in (2) **b** is in the same cycle as **a** and **q**, so no way of writing the second cycle of (2) under the first will work. Now let us try message 6. If we have

  A   B   C   D   E   F
  h   e   c   h   k   l
  b   b   b   b   b   b

then **(eb)** must be a pair in B and **(kb)** in E. There is a possible arrangement of (2), namely

a x z l w i o b y v s p q [a]
　h f n j g d k e m t r u c

This gives

B　=　**(ah)(be)(cq)(di)(fx)(gw)(jl)(ko)(my)(nz)(pu)(rs)(tv)**
E　=　**(ac)(bk)(do)(ey)(fz)(gi)(hx)(jw)(ln)(mv)(pr)(qu)(st)**

Again **(cb)** must be a pair in C and **(lb)** in F, and again there is a solution.

A13　　We can arrange (3) as

a d f b h g r s y w p k [a]
　z x l c j t u e i o m n

giving

C　=　**(az)(bc)(dx)(es)(fl)(gt)(hj)(iy)(kn)(mp)(ow)(qv)(ru)**
F　=　**(an)(bl)(ch)(dz)(ey)(fx)(gj)(iw)(km)(op)(qv)(rt)(su)**

Now let us decipher the rotor-settings using B, C, D and F.

1 .**aa**	2 .**jo**	3 .**tt**	4 .**bc**	5 **bcd**
6 **bbb**	7 .**cc**	8 .**cb**	9 .**gh**	10 .**pp**
11 .**rs**	12 .**uv**	13 .**xx**	14 .**yz**	15 .**zz**

We have a classic case of non-random trios. Messages 1 and 4 start with the same letter; the trios are obviously **aaa** and **abc**. And 13 and 14 will be **xxx** and **xyz**. If so, **(ad)** and **(xe)** must be pairs in A and **(ae)** and **(xp)** in D. These work well. From (1) we obtain

a r o n j f m x [a]
d q y l b z p e

A14　　What about the other cycle? Can message 3 be **ttt**? It can: we need **(ts)** in A and **(tw)** in D. The set-up

g k t u [g]
　i w s c

gives these. We now have the first six encodings and all the rotor settings.

A15　　　This is not enough to enable us to read the coded messages, but it is a good start. In §B, we shall see how the Polish cryptographers used six successive encodings (actually they needed only four) to analyze the Enigma machine. Much has been written about the successful exploitation of the German Enigma codes by the Allies, starting with F.W. Winterbottom's *The Ultra Secret*. This breakthrough by the Polish cryptographers, on which the British exploitation was based, was the start of something really big.

B　Analyzing the Enigma machine

B1　　　We saw in §A how Polish cryptographers attacked German messages encoded by a machine called Enigma. By purely cryptographic analysis of a day's messages they found six successive encodings produced by the machine. To follow the next steps we need to know how the machine was constructed. The military Enigma was modified from a commercial Enigma that anyone could buy.

B2　　　The Enigma has a keyboard like the one on a typewriter, except that it has only 26 letters: no numbers, no symbols, and no shift key. The keys are connected by wires to a *plugboard*, a board with 26 terminals on each face. Normally when a key — let us suppose that it is **x** — is depressed, a current flows from the key to terminal **x** on the front face of the plugboard and straight across to a terminal on the other face which I shall call rear terminal **x**. However, the operator has six wires, each with a plug on each end. If the two plugs on a wire are plugged into front terminals **x** and **y** they disconnect the two **x** terminals and the two **y** terminals, and connect the front **x** to the rear **y** and the front **y** to the rear **x**. If the current goes in at **x** it comes out at **y** and vice versa.

B3　　　From the rear terminals on the plugboard wires go to 26 terminals, which I will call entry terminals, arranged in a circle. A rotor is a thick disc with 26 terminals, arranged in a circle, on each face. Each terminal on one face is connected by a wire buried in the rotor to a terminal on the other face. When one rotor is placed in the machine, each terminal on the

front face contacts an entry terminal. The rear face of the rotor contacts the front face of a second rotor, which contacts similarly a third rotor. The rear face of the third rotor contacts a circle of 26 terminals forming what I shall call a reflector; these terminals are connected in pairs by wires.

B4　　　When a key is depressed the current goes through the plugboard to the entry terminals, through the first, second and third rotors, through the reflector, and back through the third, second and first rotors to the plugboard. From the front terminals of the plugboard wires go to 26 small light-bulbs each of which is labeled with a letter. The letter that lights up is the encode of the letter whose key is depressed.

B5　　　The current from key **x** enters the plugboard at front terminal **x**; if it goes through rotors, reflector and plugboard and back to terminal **y**, then current entering at terminal **y** would go through the same wires in the opposite direction to front plugboard terminal **x**. So if **x** encodes into **y** then **y** would encode into **x**: each encoding is a pairing, as described in part 1.

B6　　　The rotors can be removed from the machine and replaced in any order. There is a mechanism which makes the first rotor rotate through one twenty-sixth of a revolution each time a key is depressed. This moves each terminal of the rotor one place round the circle. The wires buried in the rotor are now in different positions relative to the entry terminals and so the machine produces a different encoding. At some point the movement of the first rotor makes the second rotor move on one place. Twenty-six moves later the second rotor moves again, and so on. The second rotor moves the third rotor in the same way. Anyone who wants to know what the machine actually looks like will find photographs in Gordon Welchman's *The Hut Six Story*.

B7　　　The first stage in reading the German messages is to reconstruct the wiring of the rotors and the reflector. On any one day the order in which the rotors were arranged in the machine and the connections in the plugboard remained unchanged. Choose four successive encodings, found as described in part 1, and hope that the second rotor does not move in the course of them. This will happen twenty-three times out of twenty-six, so the chances are good. The Polish cryptographers did find suitable encodings.

B8　　　Let the permutation produced by the plugboard be S. The rotors do not have letters engraved on the terminals, so let us imagine that the first rotor, in the position it occupies in the first of the four encodings, has engraved on each front terminal the letter of the entry terminal that contacts it, and has the same letter on the opposite rear terminal. Then the rotor produces a permutation of the alphabet, let us call it N. We similarly imagine letters on the terminals of the second and third rotors and the reflector. The second and third terminals and the reflector between them produce a permutation which I call Q.

B9　　　The permutation produced by going back through the first rotor is N^{-1}, the reverse of N. The permutation produced by going back through the plugboard is S^{-1} (which actually equals S). So the encoding is

$$SNQN^{-1}S^{-1}$$

For the next encoding the first rotor has moved one place. Let us see how this affects the permutation that the rotor produces. The entry terminals are arranged in alphabetical order, as the Polish cryptographers eventually guessed. (In the commercial Enigma they were arranged in German typewriter order **qwertzu** . . .) Let P be the permutation **(abc . . . xyz)**. The entry terminal after a letter α is αP. The rotor permutes it into αPN. When the rotor rotates one place it brings the terminal αP back one place to position α and the rear terminal αPN back to αPNP^{-1}. (When the Poles guessed that the entry terminals were arranged in alphabetical order they guessed that this order was in the opposite direction to the direction in which the rotors rotated.) The wire that connected αP to αPN now connects α to αPNP^{-1}, and the permutation now produced is PNP^{-1}.

B10   The reverse of a composite of permutations is the composite of the reverses in op-
posite order. That is, the reverse of XY is $Y^{-1}X^{-1}$. To see this, notice that if we compose
XY with $Y^{-1}X^{-1}$ we obtain $XYY^{-1}X^{-1}$. But YY^{-1} is the permutation which leaves
everything unchanged, so $XYY^{-1}X^{-1} = XX^{-1}$. (In fact, whenever a permutation and its
inverse occur next to each other in a composite, they cancel each other.) So $XYY^{-1}X^{-1}$
leaves everything unchanged, which means that $Y^{-1}X^{-1}$ is the reverse of XY. If we ab-
breviate PP to P^2, PPP to P^3, and so on, and $P^{-1}P^{-1}$ to P^{-2} and so on, then the first three
of the successive encodings, which I call A, B and C, are given by

$$\left.\begin{array}{rcl} A &=& SNQN^{-1}S^{-1} \\ B &=& SPNP^{-1}QPN^{-1}P^{-1}S^{-1} \\ C &=& SP^2NP^{-2}QP^2N^{-1}P^{-2}S^{-1} \end{array}\right\} \tag{1}$$

We have to find the permutations S, N and Q which make these equations hold. At this
time the cryptographers had a stroke of luck. The French Intelligence Service got hold of
some operating instructions for the military Enigma which gave the plugboard connections
for two months, and passed them on to the Poles.

B11   The permutation S is now known. If we set $U = S^{-1}AS$, then U is known and, from
(1), $S^{-1}AS = NQN^{-1}$. If we set $V = P^{-1}S^{-1}BSP$ and $W = P^{-2}S^{-1}CSP^2$ then V and W
are also known and

$$\left.\begin{array}{rcl} U &=& NQN^{-1} \\ V &=& NP^{-1}QPN^{-1} \\ W &=& NP^{-2}QP^2N^{-1} \end{array}\right\} \tag{2}$$

Then

$$\begin{array}{rcl} UV &=& NQP^{-1}QPN^{-1} \\ VW &=& NP^{-1}QP^{-1}QP^2N^{-1} \end{array}$$

From the first of these, $QP^{-1}Q = N^{-1}UVNP^{-1}$; substituting this in the second gives

$$\begin{array}{rcl} VW &=& NP^{-1}N^{-1}UVNP^{-1}P^2N^{-1} \\ &=& NP^{-1}N^{-1}UVNPN^{-1} \end{array}$$

If we set $F = NPN^{-1}$ we have

$$VW = F^{-1}UVF$$

To find F, we use a well-known result in the theory of permutations: that to obtain $F^{-1}ZF$
from Z we write Z in cycles and replace each letter by the letter into which F permutes it.
This gives the cycles of $F^{-1}ZF$. The reason is that two successive letters in a cycle of Z
are α and αZ. We replace them by αF and αZF. The permutation that sends αF to αZF is
$F^{-1}ZF$, because $\alpha FF^{-1}ZF = \alpha ZF$.

B12   Conversely, given permutations Y and Z, what permutations F will make $Y = F^{-1}ZF$? Unless Y and Z have the same number of cycles of each length no F will do. If
they have, write the cycles of Y under the cycles of Z in all possible ways. Each way
gives a solution. For example, if Z = (**ad**)(**bec**) and Y = (**be**)(**adc**) there are six possible
arrangements. Two are

$$\begin{array}{cc} \textbf{(ad)(bec)} & \textbf{(ad)(bec)} \\ \text{and} & \\ \textbf{(be)(cad)} & \textbf{(be)(adc)} \end{array}$$

giving F = (**abcde**) and F = (**ab**)(**c**)(**de**). Each of the other arrangements gives a solution.

B13   If UV and VW do not have the same number of cycles of each length, something is
wrong — perhaps the second rotor moved — but if they do we find the possibilities for F as
above and select those that consist of a single cycle, because $F = NPN^{-1}$ and P consists of
a single cycle. If there is more than one possibility we use our fourth successive encoding.
Treating the second third and fourth encodings in the way that we treated the first three, we
find another set of possible F's. The one and only F in both sets of solutions is the F that
we want. (Only by incredibly bad luck could there be more than one.)

B14   Knowing NPN^{-1} and P we can apply the same method to find N. There will be
26 possible solutions. It does not matter which one we choose: they will all give the same
encodings. We can see this as follows.

B15   If N and O are two possible solutions, then $NPN^{-1} = OPO^{-1}$, which means that
$O^{-1}NPN^{-1}O = P$. So $N^{-1}O$ applied to the cycle of P must reproduce the cycle of P. The
only way that this can happen is for $N^{-1}O$ to push each letter forward through the same
number of places in the alphabet. Then $N^{-1}O = P^n$ for some number n, and $O = NP^n$. So
if the actual rotor gives permutation N, the twenty-six solutions are NP^n for n from 0 to 25.

B16   If we take a rotor which gives the permutation N and twist the rear face through
n twenty-sixths of a revolution while holding the front face still, the wire which led from
α to αN now leads to αNP^n, so the permutation becomes NP^n. The twenty-six solutions
are the actual rotor twisted in this way for n from 0 to 25. But if we twist the rear face of
the rotor through n places we have only to twist the rest of the machine through n places
to re-establish the connections and produce the same encoding. The same method applied
to the second month for which the plugboard connections were known, when another rotor
was in first place in the machine, gives the wiring of this rotor.

B17   For the next step there is no systematic method like the one I have been describing:
cryptography is sometimes more an art than a science. The captured operating instructions
included a sample message and its encoding at a stated setting of the rotors. Using these
instructions and attempts to decode intercepted messages, the cryptographers, by making
trials and adjustments, found the correct solutions for each of the first two rotors and the
wiring of the third rotor and the reflector. They could now build a copy of the machine. To
follow their next steps we need to know how the rotors were set in position.

B18   Each rotor has around its rim the twenty-six letters of the alphabet in order. These
are not fixed to the rotor, but are on a thin metal ring that can slide round the rotor. When
the rotor is put in the machine and the lid is closed one of the letters shows through a small
window. The lug on each rotor that makes the next rotor turn is fixed to the ring, not to the
rotor itself. The operating instructions tell the operator which rotor goes in which place in
the machine, which letter on the ring is set against a zero mark on the rotor (one for each
rotor) and which terminals on the plugboard are connected. It also specifies a trio of letters
which I shall call the basic trio.

B19   The operator sets up his machine. To send a message he chooses a trio of letters
which I shall call the message setting. He turns the rotors until the basic trio is showing,
encodes the message setting twice, turns the rotors until the message setting is showing, and
encodes the message. The recipient turns his rotors until the basic trio is showing, decodes
the first six letters of the message (because Enigma encodings are pairings, encoding and
decoding on the Enigma are the same) giving him a trio of letters repeated, turns his rotors
until these letters are showing, and decodes the rest of the message. The ring setting and
message setting between them determine the position of the rotor. Conversely, if we can
discover the position of the rotor we can deduce its ring setting from the message setting
found as described in §A.

B20   To find which rotor is in first place in the machine on any one day we look for a pair
of messages in which the last two letters of the message setting are the same, for example
epz and **kpz**. **k** is six places after **e** in the alphabet, so the second message setting is either
6 places after the first in the sequence of encodings or 20 places before it, according to
when the first-place rotor makes the second rotor turn. If it turns between **e** and **k**, say at **h**,

we have the sequence

$$\mathbf{kpz}\ldots\mathbf{zpz}\ \mathrm{a}pz\ldots\mathbf{epz}\ \mathbf{fpz}\ \mathbf{gpz}\ \mathbf{hpz}\ \mathbf{cqz}\ \mathbf{jqz}\ \mathbf{kqz}\ldots$$

and **kpz** comes before **epz**. If it turns before **e** or after **k** we have

$$\ldots\mathbf{epz}\ \mathbf{fpz}\ \mathbf{gpz}\ \mathbf{hpz}\ \mathbf{ipk}\ \mathbf{jpz}\ \mathbf{kpz}\ldots$$

and **kpz** comes after **epz**. We write the second message under the first six places later, for example

$$\mathbf{p\ x\ l\ o\ w\ n\ z\ a\ b\ u\ v\ l}\ldots$$
$$\mathbf{x\ a\ k\ u\ v\ h}\ldots$$

If **kpz** comes after **epz**, two letters in the same column will be encoded by the same permutation, so if they are the same they will both represent the same letter in the original message. It is a property of the German language that if we write one message under another, about one time in 12, on average, two letters in the same column will be the same. But if **kpz** comes before **epz** two letters in the same column will be encoded by different permutations and will be the same only one time in 26 on average. With long enough messages we can distinguish between the two, and we can confirm our result by writing the second message 20 places ahead of the first. We can now tell whether or not the turn takes place between **e** and **k**. If it doesn't, and for one of the rotors it does, we can rule that rotor out as first-place rotor. On any one day there will be several pairs of messages that we can use and we can usually rule out two of the three rotors.

B21   Except for the days on which the intelligence service found the plugboard connections the cryptographers did not know the permutation S. But they did know that there were only six wires for the plugboard, so that fourteen letters were left unchanged. We look at six successive encodings found as described in part 1 on a day on which we have found which rotor is in first place. We hope that the second rotor has not moved in the course of them. Let the permutation produced by the second and third rotors be Q. We know the permutation N produced by the first rotor in a certain position, but we do not know its position here. For the first of the six successive encodings it will produce a permutation $P^x N P^{-x}$ for some x. For the second encoding it will produce the permutation $P^{x+1} N P^{-x-1}$ and so on. If the encodings are A, B, C . . . F, then

$$A = SP^x N P^{-x} Q P^x N^{-1} P^{-x} S^{-1}$$

If S left all letters unchanged we would have

$$Q = P^x N^{-1} P^{-x} A P^x N P^{-x}$$

and similarly

$$Q = P^{x+1} N^{-1} P^{-x-1} B P^{x+1} N P^{-x-1}$$

and four other equations involving C, D, E and F. We compute the right-hand sides of each of these equations for each value of x from 0 to 25, and for one value of x all six of them would be the same: they would all be Q. Because S does not leave all letters unchanged this will not happen.

B22   Let us see what does happen. Call the permutation produced by the first-place rotor in the first of the six successive positions N_1. Then $A = SN_1 Q N_1^{-1} S$. (Remember that $S^{-1} = S$.) The first of our right-hand sides is $N_1^{-1} A N_1$, i.e.

$$N_1^{-1} S N_1 Q N_1^{-1} S N_1$$

The cycles of $N_1^{-1} S N_1$, like those of S, are six pairs and fourteen singletons. The cycles of Q are thirteen pairs. Therefore at least one pair of Q must be made up of two of the singletons of $N_1^{-1} S N_1$, because the twelve letters in the six pairs of $N_1^{-1} S N_1$ cannot occur in more than twelve of the pairs of Q. At the other extreme, if these twelve letters between them form six of the pairs of Q, there will be seven pairs of Q made up of singletons of $N_1^{-1} S N_1$. On average there will be three or four. Let $\alpha\beta$ be such a pair. Then

$$
\begin{aligned}
\alpha N_1^{-1} S N_1 Q N_1^{-1} S N_1 &= \alpha Q N_1^{-1} S N_1 && \text{because } \alpha \text{ is a singleton of } N_1^{-1} S N_1 \\
&= \beta N_1^{-1} S N_1 && \text{because } \alpha\beta \text{ is a pair of Q} \\
&= \beta && \text{because } \beta \text{ is a singleton of } N_1^{-1} S N_1
\end{aligned}
$$

B23   So the pair of Q is also a pair of our first right-hand side. The same applies to the other five right-hand sides. So when we have found the right x a number of pairs of Q will be repeated in the right-hand sides. Conversely, one of the twenty-six sextets of right-hand sides will show repeated cycles, and this will be the one with the right value of x. This value of x gives us N_1, because $N_1 = P^x N P^{-x}$. In an example that I made up for myself, twenty-five sextets showed no particular pattern, but one showed five occurrences of one pair, four of another, and three each of three others. (Three more pairs occurred twice, but these could be coincidences, so I ignored them.) The five multiply-repeated pairs are five of the pairs of Q.

B24   Three of the multiply-repeated pairs occurred in the first right-hand side, so each of the six letters involved is a singleton in $N_1^{-1} S N_1$. But if α is a singleton in $N_1^{-1} S N_1$, then $\alpha N_1^{-1} S N_1 = \alpha$, so $\alpha N_1^{-1} S = \alpha N_1^{-1}$, and αN_1^{-1} is a singleton in S. This gave me six of the singletons of S. Using the other five right-hand sides I found seven more of the singletons. Having found some of the pairs of Q and most of the singletons of S, we look for a pair of Q, call it $\alpha\beta$, for which αN^{-1} is a singleton in S. Because $A = S^{-1} N_1 Q N_1^{-1} S$; applying $N_1^{-1} S$ to the cycles of Q gives the cycles of A. $N_1^{-1} S$ transforms α into $\alpha N_1^{-1} S$, which is αN_1^{-1}, because it is a singleton in S. We find the cycle of A that contains αN_1^{-1}; this must be the cycle that $\alpha\beta$ transforms into. Let the other letter in this cycle be γ. Then $\beta N_1^{-1} S = \gamma$, so γ and βN_1^{-1} form a pair in S. In this way (using all the right-hand sides) we find some of the pairs of S.

B25   If we have not found all the pairs of S we may have to use some ingenuity, varying from case to case, to complete the solution. In my example, I found four pairs and thirteen singletons of S, using between them all letters except **e**, **f**, **h**, **i** and **k**. Then S must transform each of these five letters into one of the five. I also knew that **ab** is a pair of Q and $\mathbf{a}N^{-1} = \mathbf{e}$ and $\mathbf{b}N^{-1} = \mathbf{f}$. Then $N^{-1}S$ transforms **ab** into **eSfS**. The only pair of A that is made up of the five relevant letters is **fh**. So **eSfS** must be **fh**. We cannot have $\mathbf{eS} = \mathbf{f}$ and $\mathbf{fS} = \mathbf{h}$, because $S = S^{-1}$, so we must have $\mathbf{eS} = \mathbf{h}$ and $\mathbf{fS} = \mathbf{f}$. So **f** is the remaining singleton, and **eh** is one of the two remaining pairs. The other pair can only be **ik**. In this way (or similar ways) S is found.

B26   Just before the war the Germans changed the method of encoding the message settings, introduced more rotors, and increased the number of plugboard connections. The cryptographers had to find new methods. These methods were later used by British cryptographers and are so well described by Gordon Welchman in *The Hut Six Story* that I need not describe them here.

C   Italy and Japan

C1   The Enigma was not the only machine used for encoding by the axis powers. Another was the Hagelin, used by the Italian navy. Like the Enigma, it was based on a commercial machine. The Hagelin works by means of five wheels. Each time a letter is encoded, every wheel makes a fraction of a revolution; one wheel makes a complete revolution every 13 moves, one every 11 moves, the others every 9, 8 and 7 moves. I don't guarantee these particular figures — I am working from a forty-year old memory — but the general idea is that it will be a long time before the wheels repeat their alignment. If my figures are correct, this happens only after $13 \times 11 \times 9 \times 8 \times 7$ moves.

C2   The first wheel is fitted with 13 lugs, the second with 11, and so on. When the machine is set up, each lug could be set to be either active or inactive. When an active lug is in the operative position, the wheel pushes the letter being encoded a certain number of places forward in the alphabet. The number for each wheel is determined when the machine is set up. There is also a number, which we called the *slide,* which could be anywhere from 0 to 25, and was always operative. For example, the numbers allotted to the wheels might be 5, 9, 6, 7, 3 respectively and the slide might be 8. Suppose that when a letter was encoded only the first and fourth wheels had active lugs in the operative position. Then the letter would be pushed forward $5 + 7 + 8$ places in the alphabet, so that **a** would encode into the 21st letter, **u**. Then the wheels move and a different set of lugs are in operative position for the next encoding.

C3   I don't know how the initial break was made. When I started to work on this code we had identified the machine, we knew that the set-up changed every month, and we had decoded a good many messages. The Italian navy did something that any reasonably competent cryptographer would regard as completely moronic: every message from the admiralty to the submarine command started *Da supermarina ad maricosom* and every message in the reverse direction started *Da maricosom ad supermarina.* (They used the Latin *ad* instead of the Italian *a* for "to". They also used *et* instead of *e* for "and".)

C4   Each month we had to discover the positions of the lugs, the slide, and the allocation of the numbers 3, 5, 6, 7, 9 to the wheels. (Again, I don't guarantee these particular numbers.) At midnight on the last day of each month two young but expert cryptographers came on duty, and well before the end of their shift, at 8 a.m., they had always found the new settings. They compared the standard addresses with the beginnings of intercepted messages. If the first letter of an intercepted message is **s**, then the first letter of the message, namely **d**, has been pushed forward 15 places. If the slide is 2 the wheels must have pushed the letter forward 13 places, and this can be made up from separate pushes 3, 5, 6, 7, 9 in only one way, namely $6 + 7$. So whichever wheels had 6 and 7 allotted to them had active lugs in the operative position, and the others had inactive lugs there.

C5   The stereotyped beginnings were twenty-four letters long. The wheel that rotated once every 7 moves had the same lug in the operative position for the 1st, 8th, 15th and 22nd letters. If the pushes due to the wheels were respectively 13, 16, 6, 14, the possible combinations would be

$$6 + 7$$
$$3 + 6 + 7 \text{ or } 7 + 9$$
$$6$$
$$3 + 5 + 6 \text{ or } 5 + 9$$

Then the push for the wheel cannot be 5, because that would mean that the lug is inactive in the first three but active in the fourth. Nor can it be 7. So it must be 3 (inactive), 6 (active) or 9 (inactive). Now we apply the same reasoning to the 2nd, 9th, 16th and 23rd letters, to see if we can eliminate any of these three. And so on. If we eliminate them all, we have the wrong slide, and start again with another. In practice, a wrong slide was eliminated quite

quickly. In this way, we find the push allotted to this wheel and the positions of the active lugs on it, and we find the slide. The rest is easy.

C6   Besides machine codes, manual codes were extensively used in the second world war. In the course of history many ingenious codes have been devised, and the interested reader can find them described in David Kahn's *The Codebreakers.* But not only is ingenuity no guarantee of security — the codebreaker may be more ingenious than the codemaker — but the more ingenious a code is, the harder it is to use. For large-scale use a code has to be straightforward enough for an ordinary signals clerk (who may be, especially in war-time, someone who is not particularly intelligent or well-educated) to use with the least possible risk of making a mistake. Mistakes in encoding are a godsend to the cryptographer. And the code has to be such that a mistake in transmission, or a word unread because of static or a weak signal, does not make the whole message unintelligible. As a result, by the time I started code-breaking almost, if not quite, all large-scale manual codes were of one type: the reciphered code-book.

C7   Early code-books were compiled by making a dictionary containing all the words likely to be used and numbering them, say from 00000 to 99999 (leaving gaps if there were fewer than 100,000 words). In order to deal with proper names and words that are not included, the book would contain code-groups for letters and syllables that can be used to spell them out. Messages sent in such a code are quite easy to decode, given enough material. Things are more difficult with what we called a "hat book". This is one in which the words are numbered in random order, not alphabetical order, as though they were thrown into a hat and withdrawn at random. Even this, surprisingly perhaps, is not secure. So the code-groups are reciphered. The simplest method of reciphering is by non-carrying addition. The encoder is provided with the code-book and with a key, consisting of groups of five digits, compiled as randomly as possible. If a code-group is, say, 92482 and the key-group is 79043 we add the digits of the key-group to the digits of the code-group separately, ignoring any tens digits that occur:

key-group	79043
code-group	<u>92482</u>
encoded group	61425

$$(7 + 9 = [1]6, \ 9 + 2 = [1]1 \text{ etc.})$$

C8   To avoid confusion with ordinary addition, non-carrying addition is performed from left to right. Eventually it was found better to subtract the code-group from the key-group; this makes decoding the same as encoding. This type of code is called a subtractor.

C9   Early on the key might consist of perhaps 20 groups, which would be repeated as often as required if a message were more than 20 groups long, but by the time I started a typical key would have several hundred or even several thousand groups. Each message would have to contain a group, which we called an *indicator*, telling the recipient whereabouts on the key encoding started, The longer the key the more secure the code. The extreme case is where it is possible to distribute so much key that no portion of it need be used more than once. One way to ensure this is to print the key on a perforated note-pad and instruct the operators to tear off each page once it has been used. We called this a *pad subtractor.* It is guaranteed secure. It is practical only for very limited amounts of traffic. We used it for messages containing information obtained by breaking enemy codes.

C10   Breaking subtractor codes depends on the fact that some words occur much more frequently than others. For example, on page 20 of *DIO* 3, which I happen to have open, there are about 350 words. One word (refraction) occurs 5 times, four words occur 3 times each, and seven words occur twice each. This is a far cry from the number of repeats that there would be if each of the several thousand words in one's working vocabulary occurred equally often. And military or naval prose is at least as stereotyped as astronomical prose.

C11   So we look for repeats. If, in a fair amount of traffic, two groups repeat, the chance that this has happened by accident is remote, there being one hundred thousand possible groups. The chance that it is the same code-group recovered by the same key-group is much higher. So we write one message under the other. We then look for other repeats and eventually, with a reasonable amount of traffic, we will have several messages overlapping. The number of messages that overlap is called the *depth*. We found that with a depth of seven or more we could usually start key-breaking.

C12   The users of a code aimed to distribute so much key that the enemy cryptographers wouldn't find much depth. This is often difficult and sometimes impossible. We knew, for instance, from the amount of traffic on our BAMS (British and allied merchant shipping) code, used for sending messages to convoys, that the Germans must surely be reading much of it most of the time.

C13   Suppose that six successive columns of four messages are as follows:

	1	2	3	4	5	6	
message A . . .	01384	92017	85318	79262	41047	33881	. . .
message B . . .	10789	92017	89064	30417	41047	19872	. . .
message C . . .	10789	13394	41037	30635	09910	30564	. . .
message D . . .	31480	10077	81032	12491	62324	07988	. . .

Messages A and B have been aligned by the repeats in columns 2 and 5. (The double repeat makes it practically certain that these are no coincidence.) Message C has been aligned by the repeat in column 1. Message D has been aligned by a repeat in some earlier or later column.

C14   The three repeated groups might derive from three different high-frequency code-groups. But two of them (or even all three) might derive from the same code-group. This happens often enough to be worth trying. So let's try it.

C15   Assume that the same code-group occurs in columns 2 and 5, messages A and B, and assume, quite arbitrarily, that it is 00000. (If it is actually xyzuv then every code-group and every key-group that we find will be xyzuv less than its actual value.) Then the key-groups for columns 2 and 5 are 92017 and 41047, so the code-group enciphered at C2, for instance is 89723:

key-group	92017
enciphered group	13394
code-group	89723

In fact, we have for the code-groups in the two columns

	2	5
message A	00000	00000
message B	00000	00000
message C	89723	42137
message D	82040	89723

The code-group 89723 has repeated. This confirms our guess. So we test all pairs of repeats in this way.

C16   The next step used the fact that the difference between the results of enciphering two code-groups by the same key-group is the same as the difference between the code-groups themselves. So we computed (using punched-card machinery) the differences between every pair of groups in the same column. We looked for repeats. For example, in column 2 the difference between groups C and D is 03327. In column 6 the difference between groups A and C is also 03327. So we see what happens if A6 = C2 = 89723 and C6 = D2

= 82040. For this to happen the key-group for column 6 must be 12504, giving

key	12504
A	89723
B	03732
C	82040
D	15626

C17   If we started with a depth of 7 there are three more groups in this column and in each of columns 2 and 5, which I have not displayed here. If any of these repeat we are on the right track. If not, this might be a dead end. We try all the repeats in this way. As I said, with a depth of 7 or more (and twenty or thirty columns) we could probably find all the key-groups, ending up with the equivalent of an unreciphered code-book. For this type of code the code-breakers were divided into two groups: the key-breakers — I was one — who did what I have just been describing, and the book-builders, who identified the code-groups once they had been stripped of their recipherment. The key-breakers were mostly mathematicians or chess-players, the book-builders were mostly linguists. They were not necessarily students of Italian, German and Japanese; the organizers took the viewpoint that if you can learn one language you can learn another, and in fact quite a lot of our book-builders were classicists.

C18   I don't know all the details of how code-books were built up, but I do know that the first code-group to be identified was usually the one for full-stop. It occurred frequently, with consecutive occurrences never very close together and never very far apart. Groups for "from" and "to" and addresses were also found fairly easily.

C19   When some code-groups had been identified we could use them to help in key-breaking. For example, if we suspected in a partly-decoded message that a certain group represented a number, we could try all the groups identified as numbers in that place, working out the key-group in each case and seeing what it brought up in the rest of the column.

C20   When the Italians became our co-belligerents, those of us who were working on Italian codes switched to Japanese. The code that I worked on was the naval attaché code. The code-book was ingenious. For each frequently-occurring word it had several alternatives; I seem to remember that it had six groups for full-stop. It also had groups which meant "message begins". The encoder placed one of these at the start of the message, divided the message in two, and sent the second part first. If the Italians had done this we would have found their machine code much more difficult, if not impossible, to break.

C21   The code-book did not have separate groups for spelling proper names. Instead, many groups had two meanings. For example, 41803 might normally mean "battleship", but after a group that meant "spelling starts" it might represent the syllable ENT. (The Japanese used roman letters for spelling.) Each succeeding group would have its alternative meaning until there came a group that meant "spelling ends". There were also code-groups that meant "next two groups are spellers" and "next three groups are spellers". By contrast with these clever devices the compiler of the code-book made a stupid mistake. He or she needed only 50,000 groups and chose them, not at random, but by having one of the first two digits of each code-group even and the other odd. This means that if we add the first two digits of each enciphered code-group together, code-groups enciphered by the same key-group are either all odd or all even. When two messages are arranged in depth they show the same sequence of odds and evens. Conversely, if two messages show the same sequences of twelve or more odds and evens, the chances are that they are using the same stretch of key. This gave us a useful method of getting more depth. It also enabled us to throw out some messages that we had set by repeats that were actually coincidences. Finally we discovered the indicator system. The last group but one in a message gave a position in the key (by page, row and column). The key-group there was added to the second group of the message to give the position in the key where the encoding started.

C22   The most interesting messages in this code were from the naval attaché in Madrid. Nearly all of them came from a spy in Algeciras giving the number of warships in harbour in Gibraltar. They presumably passed the information on to the Germans. (Just as our submarine tracking room kept a tag on their submarines, so their intelligence presumably kept a tag on our warships.) We could tell from these decodes how accurate their information was and how well our deceptive measures were working.

C23   These messages were very stereotyped. They enabled us to achieve a real *tour de force:* solving on a depth of 2. This is the minimum possible depth; a depth of 1 is both practically and theoretically impossible, being equivalent to a pad subtractor. A typical Madrid message might be:

message begins / Madrid naval attaché / from / X / to / Gibraltar harbour / in / battleship(s) / 1 / aircraft carrier(s) / 0 / cruisers / 2 / destroyers / 4 / submarines / 3 / small / ships / l0.

C24   In Japanese, words corresponding to prepositions like "from", "to" and "in" come after nouns; in fact, we called them postpositions. X is the addressee; I forget who it was. Two Madrid messages overlapped. Running "message begins" groups through one of them threw up "aircraft carrier" in the other. Trying "Madrid naval attaché" in the next column of the first message gave a small whole number in the second. And so on. After a while we were running types of warship through one message hoping to pick up numbers or types of warship in the other, and we finished by decoding both messages.

C25   Although the Japanese have a perfectly good syllabary (called hiragana) in which they can write their language, they prefer to use Chinese characters, reserving hiragana for postpositions, verb-endings, and similar items. (If you have travelled in Japan you have probably noticed that the names of railway stations and tram stops are written three times: once in characters, once in hiragana, and once in roman letters.) This posed a problem for us. The code-groups mostly represented Chinese characters, and it was essential to identify the character, not just its translation. This is standard practice in cryptography. For example, if the Germans were building up an English code-book and had identified the group for "order" they could not replace it by a German translation, because it might mean *Ordnung* ("in good order"), *Befehl* ("that's an order!"), or *Reihenfolge* ("alphabetical order"), and there are probably other meanings. We did not want to draw the Chinese characters. Most of us probably couldn't have made a decent job of it, and we would have had no practical way of indexing them. Anyone who has used a Japanese (or Chinese) character dictionary will know that it is slow and awkward.

C26   Every Chinese character used in Japanese (even those with only one meaning) has at least two entirely different pronunciations: the Japanese word that it represents and the Chinese word from which it is taken (or a rough approximation thereto). For example, the Japanese for "three" is *mitsu* and the Chinese is *san*. So in Japanese the character for "three" (three horizontal lines) is pronounced either *san* or *mitsu*. (This character can occur in proper names (is the Irish surname Twomey a fair analogy?) and is the first character of the name of the engineering firm Mitsubishi. It is also the first character of the name of the electronics firm Sanyo.)

C27   Again, the Japanese for "mountain" is *yama* and the Chinese is *shan*. So the character for mountain (a stylised diagram of three peaks) is pronounced *yama* or *san* in Japanese. That is why the mountain usually called Fujiyama is sometimes called Fuji san: it is just a different way of pronouncing the same character. Our solution was to identify each character by using both pronunciations. Some characters have more than two pronunciations; for them we chose two. So to us "three" was *san, mitsu;* "mountain" was *san, yama*; "north" was *hoku, kita,* and so on. I am using here the more-or-less phonetic (to an English speaker) spelling introduced by missionaries and used by Westerners. We actually used the spelling used by the Japanese themselves: *mitubisi* instead of *mitsubishi,* for example. Amusingly enough, when the Japanese introduced their system just before the war, *Jane's Fighting Ships* reported "the Japanese have renamed many of their warships".

C28   Even when we had pinned down a character our troubles were not over. Any one character can have several meanings, often quite different. We used to joke that every character had five meanings in Japanese: the one you want, the one you don't want, a religious one, an obscene one, and an obscure one. The dictionary that we used was made in Japan and its English was not always perfect. In particular, it translated one Japanese word as "a revolting lantern".

C29   I am not going to take up the question of how valuable our code-breaking was. A great deal has been written on this topic and opinions vary from rating it as a small but useful contribution, to the assessment of a Polish writer who described the theorem that the composite of two pairings has an even number of cycles of each length (which made possible the initial break into Enigma) as "the theorem that won the war".

‡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].

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

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[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.

[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.

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[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

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[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 $2°2/3$ 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 $4°.3$ 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-

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[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 $2°00'$) is set aside (fn 28), the next lowest star is Fomalhaut at $2°.6$. One notes that, if Hipparchus is understood to be the observer of the ancient catalogue, his lowest star's transit altitude was at most $1°.8$.

[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 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

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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).

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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.

‡5   Scrawlins

A   How You Too Can Write a Scrawlins

A1   You know, I can hardly walk down the street anymore without being pestered by fans who come up, deliberately fall in my path, grasp piteously at the hem of my garment, and beg me to explain: how-do-you-do-it? — tell us, how do you put together a *Scrawlins*?
A2   Waaaal, m'boy, it's all in the wrists — oh, sorry, wrong sport. Awright, soooo — you want to know how to do a *Scrawlins*? OK, I have this big computer wastebasket, see, which I toss junk-whims into. And it's so crammed with what any sane observer'd agree is oaffal offal, that only a whiff of a wisp of it could have any worth. Now we come to the hard part: as a (nonexistent) *DIO* deadline approaches, I slosh, sift, swim, and finally snorkel through this slagheap, with gnashing eye and eagle tooth. The process crescendos in a blurry tornado of workworkworkworkworkworkworkwork, as I carry all before me, with the **hefty bombmentum of a *RUNAWAY RHINO*** — yet with butterfly-delicate discrimination — braving the turbid ASCII-cesspool boiling all about me. Yea, at the last, I have deftly gleaned in triumph *every single one* of this infernal stew's rare coherent morsels.
A3   And then the remainder goes right to press.

B   Germs

B1   Politics' & religion's success-secret: masking colossal implicit conceit as humility.
B2   Whatever our pride sees as wise planning's achievement is chance at its root.[1]
B3   Labor's ultimate strike: what if they gave an election and nobody came?
B4   Humility is essential to discovery: let evidence instruct you, not the reverse.[2]
B5   If there were an eternal life after this one, we'd certainly[3] be in it already.

C   If Only There Survived a Gospel-According-to-Judas

C1   Suppose a member of a group or a cult sees misbehavior within: hypocrisy, fraud, and-or waste of funds on personal highlife. If he protests the dishonesty, what will happen to him? (I have some experience in this regard. See, e.g., *Fate* 1981 Oct. Also *DIO 4.3* ‡15 & *DIO 6* ‡3.) Answer: the jilted cult will defensively and vindictively slander him.
C2   So I'm skeptical of the hitherto universally accepted historical portrayal of turntoga-disciple Judas Iscariot as a wicked man. A look at John's gospel[4] makes it obvious that Judas was upset at Jesus' persistent (if sometimes tempestuous) relations with tax-collectors, rich pals, money-changers, & female consorts — including such a luxurious private life that Judas asked whether the funds squandered to support it could not perhaps be better spent on alleviating poverty. Jesus' sensitive reply regarding the poor (those trusting folk looking to Jesus for salvation): poverty is incurable; but, meanwhile, since life is short, I'm looking after Number One.[5] So, was Judas actually not the worst of Jesus' disciples but instead: the sole incorruptible idealist among them?

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[1] Both the ability and the concern to plan, grow from one's (lucky) cultural & (unearned) genetic heritages, atop eons of natural Darwinian teleologically-random processes. And see *DIO 4.3* ‡13 §B8.
[2] *DIO 7.2* ‡9 fn 42.
[3] See below at §L3.
[4] John 11.1-12.9 (Lazarus Massage Parlor). Variants: Matt.26:7-16, Mark 14.3-11, Luke 7.36-50.
[5] Jesus' "Parable of the Talents" (Matthew 25.14-30, Luke 19.12-27) might not be a parable at all. (Possibly misfiled by early admirers.) By one speculative interpretation, it can be seen as the advice of the CEO of a canny corporation or racket, telling his various franchise-operators that if there's no profit coming into the hub, the weak limbs get cut off "into outer darkness". But Jesus' knowledgeable mention of usury suggests that the "parable" is also a canny reflection of investment realities: the

D   Bumper Stickers We Could Have Seen

D1   Reagan Out in '84. NO MORE RON.
D2   Bush '88 to Bush '92: from BigMo to Rigmo.

E   Bumper Stickers We May Yet See

E1   A Congress Not Half Female Is Sham Democracy.[6]
E2   Rape Is Pro-Life.[7] (Is Celibacy?)
E3   Cuba: Sugar to Rot Your Teeth, Cigars to Rot Your Lungs, Marx to Rot Your Mind.
E4   Female Programmers Are Boole Dykes.
E5   MCPs Are Homos.
E6   Put Another Public-Defender Lawyer on Fat Gov't Welfare: Shoot Somebody.
E7   State-Funded Abortions: Moms Who Can't Afford Abortions, Can't Afford Kids.
E8   The More Capitalist a Nation, the Higher Its Murder Rate.[8]
E9   Female Muslims, Catholics, & Mormons! What's Next? Black Confederates?
E10   Stop Paroling Killers to Lower Jail-Crowding. DO THE CRIME, DO THE CRAM.
E11   Nobody For Mayor. This City's Been Mayored Long Enough.
E12   Two Wrongs Don't Make a Right. But Two Wrights Make an Airplane.

F   War's Benefit, The Eternal, & How to Detect Causation

F1   If we wish to ensure that our innocent current potential cannon-fodder will not be subverted by the outrageous (and probably un'mer'c'n) notion that all wars are wasteful folly, we must be prepared to preserve the hardwon fruits of war. I must therefore sound an alarum: the sole solid beneficial upshot of World War One (WW1) is now in severe danger.
F2   We remember that WW1 was "The War to End War".[9] Was it? Welllll, not quite. But, hey, how 'bout: it was the war to save democracy? Hmmmmm — not unless the postwar creation of Mussolini, Stalin, and Hitler was pro-democrat.[10]
F3   However, WW1 did notch a single firm achievement, **one that has now lasted to virtually the end of our century**. To put that astonishing feat into context, recall that the other creations of WW1 have disappeared or are unrecognizably altered: the Versailles Treaty, Yugoslavia, the Polish Corridor, East Prussia, Czecho-slovakia. So, thank-god one WW1-outcome still stands solid & unblemished, though it has heretofore — until *DIO*'s announcement at this moment — gone outrageously unrecognized. What is this grand accomplishment?

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well-off have enough of a cushion of wealth to invest the risk-capital that can lead on to far greater riches; while a poor guy is understandably afraid to gamble his last penny. (Which helps explain why it's mostly the richer who get richest.) Jesus obviously knew his way around the intense world of Jewish money changers: John 2.14-16. And he is known to have visited Phoenicia in the region of Tyre (an ancient Zürich) and tried to keep the visit secret (Mark 7.24). Did he have the equivalent of a numbered account?
[6] See *DIO 2.3* ‡6 §D.
[7] This is a nutshell version of the implicit defense of tilted, jilted, and wilted jailhouse-mom Mary Kaye LeTourneau. (See *DIO 4.3* ‡13 fn 16.)
[8] See *DIO 4.2* ‡9 §L2.
[9] Which is why WW1 was once optimistically called just "The World War". Until 1939.
[10] But, in a serious vein: it is notable that the top three European absolute monarchies (Germany, Austro-Hungary, Russia) all were kingless by the end of WW1, while the main three victorious nations (England, France, & US) were all constitutional gov'ts both before&after WW1. This supports the conclusion that: "democracy" is a more powerful illusion than monarchy, since people will fight harder for something they (think they) share in. (See §K2 [b].) But, if wars become a thing of the past, will now-fashionable wealth-sharing-with-the-public shrink from even the modest generosity of current advanced gov'ts? — since its usefulness to rulers will be less immediately compelling.

F4   Yes, Virginia, the First World War did one[11] thing right: it totally ended[12] baseball World-Championships in Boston and Chicago.

F5   These 2 cities' 4 teams had won 2/3 of all the 15 World Series [& the last 5 in a row] preceding 1918/11/11 (Armistice Day): Chicago Cubs — aka Cubbies — 1907, 1908; Boston Braves 1914; Chicago White Sox — aka Chisox or Pale Hose or Chisose — 1906, 1917; Boston Red Sox — aka Bosox or Crimson Hose or Bosose — 1903, 1912, 1915, 1916, 1918. (Moreover, none of these 4 teams had ever lost a Series except the Cubbies, and even then almost always to other Boston & Chicago teams: the Chisose [1906] or Bosose [1918], the sole deviation here being the Cubs' 1910 loss to the Phila As.)

F6   Yet, *not a single one of the almost* **eighty** *World Series since the First World War has been won by any of these teams.* (The Braves' post-WW2 Series wins occurred only after they departed accurséd Boston.) [Boston-Chicago had just won 5 straight Series: 1914-1918.]

F7   That's an impressive record. Had the Germans announced this plan from the start (the sneaky devils didn't of course — that's war for you), the odds on the plan succeeding (until 1998) by chance would be ordmag a billion-to-one.[13]

F8   **Top-Hun Slaughtered?**   Now that we have lodged this hypothesis, we will adhere to good philosophy of science, by searching for confirmation through further evidence. And an astounding temporal confluence appears, which puts the causal connection beyond doubt. Hermann Göring was Germany's most decorated surviving WW1 air ace: Top Hun. (The Red Baron ranked higher but died in battle.) Now, when he saw that baseball seasons following WW1 kept ending with no Boston teams even getting *into* the World Series, Göring realized that another dose of the original efficacious medicine was required (i.e., another world-war disaster for Germany), in order to bring Beantown's humiliation to full&final flower. So, he devoted himself in the 1920s to getting industrial funding for Hitler, so we could have WW2. After it was over, at the Nürnberg trials (1945-1946): while most defendants were trying to save their skins, Göring sneered at death. For, he was now imbued with the exaltation, known only to few men, of dying for a higher and lasting purpose, verily of knowing that, while departing this transient life, he was — as other patriots & saints before him — partaking of The Eternal.

F9   Through his contacts with US soldiers, Göring could from his jail cell slyly keep track of the 1946 World Series: Bosose-vs-St.Louis Cardinals. In the 7th & final game, Cardinal Enos Slaughter's legendary 8th inning dash (all the way home from 1st base, on a short Harry Walker double) with the winning run, brought a last smile to HG's flabby face, as: success loomed. The game shockingly ended in heavily-favored Boston's first-ever Series loss (after 5 previous victories in 5 appearances).[14]

F10   The Series ended at 3:47 CST or 10:47 CET. At this very moment (roughed to 10:45 CET in the *NYTimes*), Göring — knowing that his life's work was finally accomplished — took poison and expired in ordmag a minute. The precision of the coincidence (exact, within reportorial rounding error) renders it undeniably significant.

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[11] Semi-pacifist DR (*DIO 1.2* fn 42 & fn 45) is reminded of a slightly analogous immortal 1980s moment when Johnny Carson spoke reverently of the person who insisted on the 1861-1865 War Between the States, President Abraham Lincoln, "without whom, we would not have the dunk-stuff."

[12] The common explanation of the decline in the Bosose' fortunes is that (soon after WW1) the team sold Babe Ruth to New York. But Ruth returned in 1935 to Boston, with the Braves — who responded by diving from the 1st division (1934) straight into last place with one of the worst records ever (losing over 3/4 of their games).

[13] That's for 4 pre-specified teams losing for a third of a century in a 16-team situation, followed (the Braves moved out of Boston in the early 1950s) by 3 specified teams (Cubbies, Chisose, Bosose) losing for nearly a half-century in an environment of roughly two dozen teams. (Note to mathematicians: of course, there was no such pre-specification. Accounting for this factor [as, e.g., at *DIO 4.1* ‡3 §E7] will seriously lower the odds — though not to anything like chance-level.)

[14] The Chisose at least lost their first post-WW1 World Series deliberately (Black Sox, 1919). The Bosose haven't even that consolation.

F11   However, since National League TV-revenue obsession[15] has (a few years ago) successfully extorted out of the Cubs that team's assent to night baseball at Wrigley Field (Cubbies' home park, the last major-league stadium to resist the installation of night-lights) the Cubs will no longer be prevented[16] from participating in the World Series. (Stuck in a deep Cubbie-hole, the team has not even been *in* a Series for over a half-century — since 1945.) They might win one. (I trust we can count on the Chisose & the Bosose not to do so until the Third Millennium.) Don't let it happen. Do we want our kids to think that the 10 million WW1 dead died in vain?[17]

G   Call of the Wild Coincidence

G1   If the foregoing romp doesn't leave the reader wondering how DR has remained unsuperstitious, the following may.

G2   In the 1960s, I was lucky enough to have a chance encounter with J.C.Adams' 19th century paper establishing as in slight *excess* of 33^y, the period of the Leonid meteor shower, which had flared spectacularly in 1833 and 1866. This led me to hope for a big 1966 Leonid shower despite the poor 1932 & 1965 showings. So, on 1966/11/16-17 at 4 AM (having seen nothing in the city), I drove out into the country to look for the mutedly-pre-publicized 1966 display. And stayed for an unforgettable 1^{h}1/2 of brilliant, trail-leaving meteors — more than I've seen in all the rest of my life put together.

G3   But the improbabilities of this date do not stop there. For, just a few hours earlier, at 7 PM of 11/16, my wife and I had happened upon a shower of abandoned grey kittens on the street. We took all three of them home. These Leonids (as we always called them) were soon named Reggie, Chortle, and Restless, after stars in the very celestial cat which is the radiant of the meteor shower: Leo's *alpha*, *delta*. And *mew*.

G4   The last of our Leonids died on 1982/3/5 — on the same day my mother and step-father moved out of the home our family had lived in since mid-1947.

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[15] Baseball is clearly hoping that its well-publicized 1998 chemistry-enhanced 1-year homer-record — 70 by Mark McGwire — will help save the sport from oblivion. Comments: McGwire is probably the 1st one-year Homerun King who will (if steroids don't kill him quicker than the usual) live to see someone else break his record. (Baseball seems doomed to addiction to a perpetual homerun-craze, to survive its ongoing depression, and make good the debt incurred by its recent demented strike.) Previous records: N.Williamson's 27 in 1884, B.Ruth's several records, peaking at 60 in 1927, R.Maris' 61 in 1961. Leagues' one-year RBI records: H.Wilson's 190 in 1930, L.Gehrig's 184 in 1931. Except for Williamson (info not known to writer), all these men took drugs — tobacco (Ruth, Gehrig, Maris), alcohol (Ruth, Wilson), or better. Ages at death: Williamson 36, Gehrig 37, Ruth 53, Wilson 48, Maris 51. Not a healthy line of work. (But, as bulked-up track-star FloJo's fate shows: when glory beckons, any abuse of the body seems worthwhile to some.) Incidentally, an argument for oldtime heroes' superiority is afforded by these men's World Series batting averages. (Williamson died before there was a Series.) Oldies: Ruth .326, Wilson .319, Gehrig .361. (Each above regular-season average, if we eliminate Ruth's years purely as pitcher.) Moderns: Maris .217, McGwire .188. (Each well below an already unimpressive regular-season average). Possible explanation: increasingly fast & tricky ace pitchers have made hitting such a delicate art that celebrity hitters' reliability suffers (which inhibits legend-building). Also, year-round run-production increasingly depends on violent macho-power home runs, which lowers the import of bunched singles, doubles, & triples and the artful on-field scrambling action that originally made the game so popular. (One hopes baseball will not attempt to match the violence underlying other sports' appeal, as typified by the joke about the disappointed guy who went down to the Civic Center to see a fight, but a hockey game broke out.)

[16] To drive the point home, the baseball commissioner's office haughtily informed the Cubbies that, in case the team ever did qualify for a Series, their "home" games would have to be played in another park! — which swiftly produced lights-on compliance at Wrigley.

[17] One must not permit a "World Series Syndrome" (more relevant to youth than the Vietnam Syndrome, given the jock angle) that would have to be depressurized before we could convince idealistic youngsters to be generals' willing boytoys once again.

G5   Only recently did I learn of an even more far-fetched family coincidence. My step-father's only living sibling, James Avirett, married on 1942/10/16. This was the same day my father died, widowing my mother, who would marry my stepfather John Avirett (see *DIO 3* inside front cover) five years later (1947/7/22).

G6   So why do I nonetheless remain utterly unsuperstitious? Because: many thousands of events are available to be compared; thus, it figures that a few one-in-10000 shots are bound to come up.

G7   We have already, at *DIO 1.1* ‡8 §F4 (1991), pointed out a spectacular astrological coincidence. Still, I've experienced only one evidential datum that could've favored the truth of some kind of astrology: Daffy Duck and I were born in the same year.
But, then it turned out that Chinese astrology doesn't even *have* a Year of the Duck . . . .

H   BJ, SJ, & OJ — Oval Sex in the Oral Office

H1   President Bill Jefferson Clinton is publicly, earnestly repentant for trying to hide all that fun he had with the ellipsoidal phallivore, Monica L. But: BJ won't say what he *did* with ML. (Just that it was "inappropriate". But never sexual. Even though his emission's DNA was found on her clothes.) So, get this, he's attempting to hide what he did — even *while* claiming deep-sincere-sorrow for: hiding what he did. (And: he's still hiring expensive legal talent to deceive the public into thinking he didn't deceive . . . .)

H2   Only the most refined conmen of history can pull off stuff like that. And, to pair such talent together with a lawyer's brain! — well, it lets you get to the core of the matter: i.e., you begin to understand why he's President. . . .

H3   I'm reminded of a legendary Old-West traveling fleece-artist who used to circulate from town to town in his sales wagon, peddling "Swiss Gold Watches" for 10 cents apiece. He was of course happy to find himself showered with dimes when his victims were so gullible as to believe this ridiculous-on-the-face-of-it pitch. But what took him from delighted glee to ecstatic shock was his followup discovery: when he returned to the same town a year later, he not only sold his Swiss-Gold-Watches just as briskly as ever, he found that he was selling them to the same people.[18]

H4   FDR, JFK, & BJC.   There has been alot of yuppy-alibiing of the scandalous and randilouse careers of JFK & BJC by reminding the public of FDR's mistress. But, isn't there a difference between longterm-affection and feline-tommygun promiscuity? Given what we know of JFK & BJC, should we either: [a] Henceforth nominate no one under 70? (Is JP2 getting bored over there in VatCity?) or [b] Draw some wisdom from the caliphs and openly establish a permanent official White House harem? — [i] to ease our ruler's idle moments, & [ii] to enlighten the public openly regarding whether its elected leader is or isn't using people like throwaway paper honkerchiefs, without any regard for the effect on respect for the Presidency.

H5   In defense of political reason's power, Lincoln said that you can fool all of the people some of the time and some of the people all of the time but not all of the people all of the time. But that was before TV brought the Elvis factor into politics. If Lincoln were alive today, watching BJC's poll-ratings, he'd say: you can fool all of the people some of the time and you can fool 2/3 of the people all of the time — and folks, we can stop right there, 'cause that's a majority, so — BJ's stayin' on at the White House.

H6   Who says Monica has no ethics? She felt just *awful* that BJ was cheating. On her.

H7   Monica has said of herself that she's lied all her life. Yet the whole US knows (esp. after the FBI's DNA test) that her account of liaisons with the First Willy are more credible than his. And the whole US now knows its own ethical state when it finds that: about half the US public wants BJC to stay on in the Oral Office. I.e., they want as President a person *they know* is less trustworthy than a self-described lifetime-habitual liar.

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[18]See *DIO 2.1* ‡1 §A1, *DIO 2.3* ‡6 §G, *DIO 4.2* ‡9 §S3.

H8   BJC's defenders criticize the Starr report (not primarily for inaccuracy but) for containing too much sex (implying Starr is a stalker, since that lie didn't stick on Monica). And the Dems' chorus is that it's OK for BJC to lie about sex.
Slight problem here: BJC earnestly insists he "*did not have sex with*" Monica. (She didn't ingest.) Merely an "inappropriate" relationship. So we need a re-write: the Starr Report is too obsessed with Inappropriateness; and it's OK to lie about Inappropriateness, 'cause everyone does. (Pontius Pilate: "What is truth?" — John 18:38.)

H9   This is how the Presidential brain works: sex isn't sex if it's not straight. And lying isn't lying if the truth is an even more embarrassing brand of what you're lying about.

H10   Question which — outside of Hollywood (increasingly the US' Fifth Estate) — is not so easily answered: what about President BJ is so positive and so uniquely irreplaceable that it is worth throwing out national standards of truth and trustworthiness in order to retain him? Is an Al Gore Presidency so to be feared? — if so, why'd BJ pick him for Veep?

H11   Suddenly, these days, we hear (can't imagine why . . .) alot of propaganda defending the idea that lying[19] is no big deal. (Gosh, an awful *lot* of these moralists are lawyers.)[20] Keeping a proven deceiver as head of gov't makes about as much sense as founding a corporation's economics upon counterfeit money.

H12   Fresh off defending wife-killer[21] OJ's lies, Alan Douchewits has now transformed into a ubiquitously insistent defender[22] of BJ's lies. This is a **law**-prof at Harvard, the *alma mater* of *DIO*'s publisher — whose most indelible impression of his college years is Harvard's insuperable motto (which our class took seriously, believe it or not): *Truth* (*Veritas*). Note well, there are no footnotes on the Harvard escutcheon — just: *Veritas*, a single word.

H13   A shame some of that prominent University's most media-hungry professors (see also, e.g, *DIO 4.3* ‡15) can't comprehend that many words. (If their ox weren't being scored, feminist spokemen would be persistently boring in with boring packaged-snobbity humor like: what part of the word *Truth* don't you understand?)

H14   Why are feminists and unions getting behind BJC on the theory that he'll remember his promises to reward their help? (See above under Swiss-Gold-Watches: §H3. In a classic horse-may-die routine [see §N], BJC hasn't yet gotten RU 486 to his loyal feminists. See *DIO 4.3* ‡13 §E2. And he went big for union-hated NAFTA & GATT.) Bottom line: of what value is the promise of a proven deceiver?

H15   Flash-Flash! In a quickly convened special 1998/1/22 meeting, the Jokewriters Association of America unanimously recommended revocation of the Presidential-Term-Limits Amendment to the US Constitution.

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[19]Lying is the crime here. If BJC needs hot&cold-running girlpersons in his off-hours, most of us wouldn't begrudge him his fun. But deception is another matter. And the public is being led (by the Fourth & Fifth estates) to suppose that BJC is lying just about Inappropriateness. And nothing else. . . .

[20]What's the difference between a crooked lawyer and a crooked politician?
One you pay to lie to others, and the other you pay to lie to you.
What do you get when you cross a crooked lawyer with a crooked politician?
Chelsea. (Latter not *DIO* original.)

[21]Yes, *DIO* can even stoop to creating limericks:
> There was a big jock named the Juice
> Who tortured his wife with abuse.
> When he got on his knees,
> She gave him the freeze;
> So he cut her head nearly loose.

DIO got good reviews for our ironclad proof (*DIO 6* ‡5) of the identity of the Real Killer of Ron & Nicole. (Kato the were-dog did it.) But by far the best sendup of the OJ case was Gary (*Doonesbury*) Trudeau's "The Framing of O. J. Simpson . . . a conspiracy unravels" (*Time* 1996/11/25 p.122). If you missed it, head for the library.

[22]Another is John Dean. (Question: why do most observers seem implicitly to have assumed that Watergate's Deep Throat was an older person?)

H16   And, in case this revocation

> Doesn't work out for now,
> There is a way
> To Keep BJ,
> Here's how:

BJC just keeps running as Veep (every 4 years) on successive Dem Presidential tickets headed by great comedians like Jay Leno, Jonathan Winters, or Maxine Waters, where it's pre-understood that the President-elect steps aside right after inauguration to let the public keep enjoying the lovable Elvis-clone it craves.

H17   The press has joined the Dems in attacking Starr for investigating BJC. Question: what is wrong with scrupulously checking one's top leader? The Executive Branch's power is too disproportionately high anyway.

H18   Question to the top gov't opponents of Clinton's coverup-conspiracy, lovable ex-homewrecker Henry Hyde & persistently anti-female-rights Edward Jekyll: How can the US gov't expunge a criminal conspiracy, when the gov't is itself a criminal conspiracy?

H19   The Most Surprised. There's been alot of Dem chortling (albeit nervous) about GOP surprise at the public's mass-shrug at being straight-in-the-eye lied to. Comments:

[a] One wonders what kind of friends the average USer has, that this sort of treatment would seem so lightly taken & be so instantly forgivable.

[b] Despite the taking&airing of dozens of polls, the US Medium has not reported a single IQ-correlation poll on whether the First Ego ought to depart: how difficult would it be to check the intelligentsia's opinion vs. the semi-literates'? The former seem repulsed, while the latter feel: so-what? Among principled people, out&out lying is a crime that transcends politics. (DR is pro-womens' rights & pro-abortion-access. So he cannot stand the politics or the pseudo-piety of abortion-hater H.Hyde, who is the top GOPer against BJC. But how does all that recommend clinging to an untrustworthy leader? The dreariest aspect of the saga is the extent to which citizens judge not by the criminal's crimes but by his promised principles [see Dave Barry at fn 26] and appearance.)

[c] There's a sharp irony here that no one has yet noted in the Medium (even though it's right in front of everyone), namely, *the* person most surprised at the public's shrug is (political-genius-Rhodes-Scholar) BJC himself. Why else would he have committed so many elaborate crimes and delays, to cover up his nocturnal submission (to ML's services), if he initially anticipated that the public wouldn't care?

[d] While the adoring public begs nasty old Congress to lay off lovable BJC, plenty of intelligent people are appalled at the idea of keeping a thoroughly-proven liar in office. Privately, this includes even congressmen[23] (especially Democrats, quietly). So: is this the first time in US history when Congress has been less corrupt than its public?

H20   Of course, the real idea behind alot of BJ-protecting Libs is hysterically apocalyptic (neglecting the obvious reality that the corporate rulers of the US aren't going to let rightist loonies take over the gov't):[24] if BJC goes down, the Dem Party is finished! A religious Dark Ages looms! But, hold on. After all, the gradual ascent of often-dumb rightist rage (against, e.g., women's progress & abortion rights) is not going to be stopped for long by BJC posing as Horatius-at-the-Bridge. To the contrary, Dem[25] policies have encouraged the growth of drug&crime-ridden slums in city after city in the US, and thus helped create the very rightist reaction from which Dems now claim to be the US' best hope for salvation.

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[23] Washington pols & insiders are aghast at Clinton's lying. To them. (See, e.g., Sally Quinn in *WashPost Weekly Ed* 1998/11/9 pp.6-7.) And their head-shaking chorus of disbelief is:

Hey, this is a matter of HONOR, here — he's supposed to lie to The People, not his fellow liars.

[24] Though the historical example of 1933 Germany shows anything is possible, this mistake itself warns rulers against repeating it.

[25] Have any among the Punditz ever wondered aloud at the anomaly that the favorite political party of university professors is also the favorite party of the mafia and its chief slum-victims? Does this suggest a slight downside to the sanctity of the US' two-party system?

H21   On TV talkshows, Clinton's lies have for months made every rightwing commentator (even the most rabidly nutty) look honest compared to Liberals who've persistently defended a coreless-careerist,[26] pathological-lawyer, sex-addict President. [In extenuation: see §P11.] So Clinton's legacy and breastacy will be to give a gratis boost to the credibility of the very rightists he allegedly believes he's defending the republic from.

H22   It is not surprising that BJ made a big hit with his Jesuit profs at Georgetown. So, it should be equally unsurprising that his 1998/8/17 mental-gymnastics exercise (the famous 4 hour videotaped performance) has lodged a spectacular intellectual achievement. After 2000 years of less able theologians' frustration, Father Clinton, SJ, has finally solved one of the most central of all Christian mysteries, namely:

> how a mom could give birth without having sex.

H23   Linda Tripp[27] and Kenneth Starr may indeed have the ulterior motives some impute to them. (The Starr & GOP connections to tobacco are suspicious.) But [a] their charges appear to be accurate, and [b] they have shown the courage to oppose corruption in the most powerful office on Earth — in the face of hyena-pack attacks that have made them lepers. (Isn't it a rule of the military academies that it is wrong not to report others' wrongdoing if it is known to you?) Neither has gotten a word of press-recognition for [b] — perhaps because a US scandal always tends (nudged in the direction favored by the wealthiest interests) to evolve into a script of rigidly pre-defined rôles.

H24   Why is it that the Media pack-attack Linda Tripp (who was being pressured to follow a script and falsely testify in court — and who instead bugged to differ) for betraying-a-trust to one person — but are anxious to forgive Clinton for doing the same very thing to three hundred million persons? (And: why is a powerless lady who refused to commit perjury held in public scorn while the most powerful man in the world is kept in office after doing the crime?) The main difference is that BJ's betrayal was selfish (he's clinging to his job, & perhaps immunity from indictment), while Tripp nervily risked her job (and more) to establish the truth about serious national crimes (Presidential bribe-attempts, etc).

H25   Alot of white observers who understandably condemned as ignorant bigots the jury which sprung an incredibly minor actor named OJ, are now defending keeping on BJ, even though he's a liar, coverupper, conspirer, briber and intimidator of witnesses — and a slanderer prepared (until unexpectedly brought up short by DNA) even to falsely smear ML as a fantasizing stalker. The public craves to retain BJ not in some obscure acting job, but in The holy of holies, The apogee of all apogees: *THE UNITED STATES PRESIDENCY — the* top acting job in the universe.

H26   Why would the President of the US, who could've fooled around with alot of slim, attractive women, end up instead harpooning a sperm whale? (And then having his spineless spinvolk cater to the public's hope to blame it all on stalker-rapist ML — as if he were merely 50^{y}old putty in a 21^{y}old maw. Partial answers: [a] BJ had long been under "house arrest" (on Hilla the Hun's watch)[28] when the Monica affair occurred, so this limited his ranges in space and slime. [b] His approach is so dropflash-crude that most women (those

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[26] [Dave Barry (1999/1/16): each of the contenders (for the 2000 presidential race) will spend 1999 trying "to develop a Vision of the Future, defined as 'around $40 million in cash.' They will use some of this vision to pay for polls so they can find out what their [principles] are; they will use the rest for [TV ads] explaining these [principles] in terms that are understandable to the average American voter or cocker spaniel: 'Vote for John. You like John. John have same views as you. See John with family! See John wear dark suit! John very good. Other man very bad. Remember: John.' "]

[27] If Linda Tripp were part of a conspiracy, there'd be big money defending her; but nobody backed by major fiscal powers gets trashed by 100% of the media.

[28] This is said to be Clinton's own private nickname for his wife. (A partially-exonerating clue to BJ's wandering eye may be suggested by the F.Dunaway-W.Holden swiftie in the 1976 film *Network*.) On 1998/9/23 (NBC), Jay Leno asked Fred Willard: should our sex-addict President try going cold turkey? Willard's response: Now, Jay, there's no need to call the First Lady names. (It's odd that Hilla & the late Di are among the most popular women on Earth, without anyone asking an obvious question: if they're so admirable, how come their own husbands couldn't stand their company?)

of the slightest refinement or self-confidence) are repulsed. (His invincibly-enamoured female-majority supporters' minds seem staunchly able to avoid contemplating this point.)

H27 It's amusing to see BJ-defenders on the toob attempting to deny the evidence that the White House was attempting to bribe Monica (*and* Tripp)[29] into silence, vigorously promoting a party-line denial — even while many of these defenders are themselves "influenced" by supping at the White House trough.

H28 Feminist Erica Jong (1998/9/29): BJ merits a free pass for doing the very things feminism has been fighting because — his agenda[30] has helped feminism fight those very things. Or something. And she further thinks BJ should get this pass because: [a] He's an addict. [b] He's behaving better. (So far. [As we know.]) If there's an Annual Feminist-Consistency-Farce Award, Ms. Jong looks like a good bet, though in a strong 1998 field.

H29 Others agree that poor BJ merely needs-help,[31] because he's addicted to Inappropriateness. Comments:

[a] BJ has a far more serious and incurable addiction. To deception.

[b] The public also has an addiction. To Clinton. (TV 'snews has turned US elections into glamour[32] contests — and then scratches its head about why we get sex scandals . . . .) Which is the real reason (as against all the fancy Wisdom-of-the-American-Sheeple hymns straightfacedly peddled by the Medium)[33] that explains why Clinton's ratings stay solid, no matter how gross his behavior. For way too many women, this vicarious sexual addiction could prove alot more dangerous to democracy (than his own addiction) if it continues uncorrected — i.e., if women's affection for this Elvis-reincarnation[34] keeps leading them to set aside the trifling question of whether the love-object has a shred of ethics. (None of the original Elvis' band of shrieking fans ever gave a hoot about this little detail, either.)[35]

H30 Womantrouble-RetroTransformation:

After indictment, OJ suddenly became black again.

And, after the public learned of BJ's subpoenas (Emily Litella, where are you when we need you?), and he got into deep bleep, formerly-union-snubbing & NAFTA-hustling BJ suddenly became a Democrat[36] again. (Why haven't there been more BJ-OJ comparisons[37] in the Medium? Simple: Dems never forgave OJ for Cochran's use of Louis F's anti-Jewish tan-klan, so few of the nimblest hired-tongues-for-BJ have a kind word for OJ.)

H31 GOP critics of BJ keep spouting their favorite mantra-indictment: "obstruction of justice". Most of them are lawyers, so they haven't the instinct to translate that blahblah

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[29]What kind of youngster can afford *on her own* to bribe a friend by offering her a condo?

[30]In the BJ context, Shelby Steele emphasizes the modern-agentry ploy (so beloved of film-celebs — and even ex-royals) of ostentatiously supporting a few charities (breast-cancer & AIDS being particular favorites), even while leading a worthless and-or amoral private life. In an age in which logic is lost in a swamp of visual stimuli & propaganda, this tactic has been unfailingly effective.

[31]One recalls the joke about the two sociologists who happen upon a mugging, and one says to the other: "oh, the poor man, he needs help" — as they race to hold hands with the mugger.

[32]See similarly *DIO 4.2* ‡9 §F. Preachers like Jimmy Swaggert & the early Marjoe have long been dressing and acting more like filmstars than holy men. It seems odd to an educated outsider that these men's untutored faithful could so easily confuse sexual attraction with religion. JFK & BJ have now powerfully brought a similar (almost laughably pathetic) confusion to presidential politics.

[33][E.g., *Time* 1999/1/4 p.108 (boxed-highlighted Wisdom): "Most people decided that Clinton at his most slippery is still less a threat to American values than Starr."]

[34] How many women's dominant disappointment at BJ is simply that — Monica got to him first? Note our 1991/1/14 prediction (*DIO 1.1* ‡2 §G4, well before Clinton entered the 1992 race), that the public would send a resurrected Elvis to the White House.

[35]In the people-in-love-will-say-anything genre, I'm reminded of the sad tale of a famous homosexual tennis champion whose chickenhawkery so dominated his later years that a friend recalled him chuckling (one morning at breakfast): Guess-what? Fritzie stole my watch last night. Isn't that cute?

[36]See *Wall Street Journal* 1998/9/2 p.A18.

[37]The best exception is Leno (who joked about Clinton's denying sex with ML, even as 36 boxes of StarrStuff were being stacked): hey, when the evidence is taller than you are, even OJ says give-it-up.

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into plain English: **witness-tampering**. If the GOP is serious about getting BJ out of office (a big IF)[38] then it should start speaking English instead of legalese to the public.

H32 A Blow for Freedom? If our Com'r-in-Chief is really pro-Affirmative-Action, why don't black women ever rate his otherwise record-setting-indiscriminate amorous attentions? (No wonder they call it the White House.) Yet another OJ-parallel[39] —

Plutarch's Lives lives.

H33 The Ron "teflon" (nonexistent when he was a sub-average film actor) & Clinton's "escape-artistry" are self-protective press myths, invented to cover the central reality of politics in the US since the New Deal died away; the candidate backed by the most corporate power will win, regardless of his character or schemes: Iran-Contra, the Monica coverup, whatever. And that's because the nation's 4th-Estate thought-controller&kingmaker — The Medium — plays ball. (Not to use a more professional term.) Also: Reagan & Clinton both represent an unexpected coalition between [a] Hollywood[40] (whose power over US minds has never been greater) and [b] the US business community (ditto).

H34 And this is the most ominous aspect of the Clinton spectacle: the impunity.[41] (There's no comment from the equally-immune Medium on this matter.) If you serve the powers-that-are (and BJ politically flowered in mobtown Hot Springs, Arkansas), there is almost nothing you can do that will cause your exit. It's a clear message to any remaining severely retarded child who still imagines the US is a just, accountable, rational democracy.

I   Media-Protected PC Mythology

I1 A popular feminist myth holds that men dominate women simply because men are physically stronger. But, if so, then: why is the world run by older people, not younger?

I2 Incessant daily gov't-business-Medium propaganda insists that the US is a racist nation and that this explains blacks' continued mass problems of coping. Comments:

[a] Outside the Klan's dimbos, there is relatively little hatred of blacks in the US. (Though, there may indeed be some irritation at the insulated lilywhite country-club-set's institutions — the increasingly seamless gov't-press combine — jamming ghetto culture into everybody else's schools and neighborhoods.) As stevedore Eric Hoffer, LBJ's favorite popular philosopher, perceptively noted long ago: hatred festers primarily in those who feel inferior in power or whatever. (Also: historically, blacks have gotten the raw end of every mass involvement with other groups.) Which may explain why most race-hatred in the US is coming from the social bottom — and not just at WASPs, but at Koreans, Jews, etc.

[b] If (as the Medium implies at every opportunity) hate-prejudice were the cause of blacks' depressing mean social status, then Jews would be at the US' lowest rung.[42]

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[38]Is the Impeachment Show actually just standard Dembo-vs-Dumbo theatre? — a well-mounted Punch&Judy Act? Given the whipping-boy purpose he serves (*DIO 4.3* ‡13 §B5): "Does the GOP *really* want Clinton out of the White House?"

[39]See §H30 and *DIO 6* ‡5 §A2.

[40]See §H10. Anyone seeking an explanation of the homosexual lobby's rise from closet to media-sacred-cow might consider the phenomenon in the light of this superficially-incongruous coalition.

[41]The same has long been true of other wealthy showbiz celebrities. And: will (*can*) any President ever be ejected in the obsessively two-party US? Same question: when has either party held 2/3 of the Senate?

[42]§§I1&I2 are the sorts of ultra-simple analogies that TV 'snews moguls exist to keep off the air for decades in a row. (Incidentally, §I2 is obviously not a justification of race-hate. An arrogant poison that self-excused the aggression and sadism of the Nazis & Tojo's Empire of Japan — and which brought both gov'ts to ultimate grief. Everybody's.) §I2 merely points out that racial hatred is not necessarily the prime cause of the intractable woes of an allegedly-hated group.

J   Low Noon at Notre Dame

J1   Hugh Thurston's paper (‡1) summarizes Robert Newton's large 1977 body of proofs of Ptolemy's consistently forged calculations (many of which can be checked by anyone, e.g., ‡1 eq. 1). These demonstrations have been prominently on the record for over 20 years. During this time, Ptolemy's enraged defenders haven't overturned a jot of it — responding instead with a grab-bag of incoherent[43] alibis along with coherently one-sided personal[44] attacks upon Newton. For the intellectual company such behavior places these cultists in, see *DIO 7.3* ‡9 fn 47 & §§C9-C10.

J2   Despite Newton's massive, variously airtight demonstrations that Ptolemy's work is rife with (one might better say: *characterized by*) deliberate mathematical fraud, history-of-astronomy-political-center books (see, e.g., that cited in fn 44) keep imperviously puffing him as one of the greatest *astronomers* of all time — a spectacle which has long given real astronomers an eye-rolling upchuckle. After all, Ptolemy's fraudulence has been an open secret in the astronomical community for at least 4 centuries. (Tycho revealed Ptolemy's star-catalog theft in the preface to his own 1598 star catalog.) We note that astronomers' two great recent star catalogs are named successively for Hipparchos and for Tycho.

J3   Hugh Thurston and I appeared at the 1997 University of Notre Dame Astronomy Workshop, and at the end of the first session (1997/6/20), moderator Steve Dick (US Naval Observatory), in response to an earlier DR proposal from the floor, stepped aside and left a little unscheduled time for a brief friendly discussion of the Ptolemy Controversy.

J4   The guys were willing, but the dolls predictably refused to engage. Bernie Goldstein arm-waved us off as he turned his back on the scene. Noel Swerdlow bounded up the auditorium stairs and disappeared out into the hall. 0 Gingerich lowered his eyes and said nothing. James Evans followed suit.

J5   DR, trashionably attired in a Damon-Runyon-mobster-style dark shirt and holster[45] finally said forcefully but not impolitely:

> Gingerich, you've got us outnumbered 2-to-1 — what do you need?

J6   Workshop gooroo & brave pro-Ptolemy warrior 0 Gingerich kept on staring silently at a point roughly $30°$ beneath where Thurston and I were. Later, when leaving the hall, Gingerich was heard to say (with creditable candor): when this conference is over, this is all anyone is going to remember.

J7   [Addenda 1999:] Keith Pickering, Hugh Thurston, and DR repeatedly (in communications both to the Workshop and to the other side) offered to meet any number of Ptolemy's defenders at the next (1999 July) UND Workshop. No takers. (Similar earlier offers have likewise been refused for years.) The same clique continues not only to flee public discussion but insists on non-citing **all** of *DIO*'s years of novel contributions.

J8   When DR proposed (1998/11/4 & 12/3) scheduling an encounter for the 1999 Workshop, its chiefs Dick & Michael Crowe required a written proposal. When that was sent (12/11), no reply was received. So weeks later (99/1/16) DR, Pickering, & Thurston wrote directly to Gingerich, Swerdlow, & Evans, requesting that *they* support the holding of such a public discussion. Instantly, the Workshop was galvanized — and (1999/1/21) cut off the idea (without even acknowledging receipt of the 1/16 *DIO* letter that caused belated reply only to DR's earlier letter). Swift action was obviously seen as assisting the cowering threesome, obviating their need to reply — as if that would save them from yet another revelation of the level of their courage. (Question: since the Workshop knew that Ptolemy's brave defenders would not reply, where was the need to interfere? Other than as the sort

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of archon-brainkissing favor that no neutral body would debase itself by.) Since the Workshop's letter adhered to the cringers' party line that DR is too "strident" for them (an amusing alibi, given the defenders' slanders[46] — freely delivered, so long as the slandee's back is turned), DR pointed out that neither Pickering nor Thurston has ever said anything strong about Ptolemy's defenders. (And all of DR's public appearances have been polite.)[47] So, just the two of them have offered to take on the defenders. To this proposal: no reply from the Workshop. (There will probably be none until the program is frozen to specifications.) On 1999/2/9, 0 e-mailed Pickering that Evans & Swerdlow had decided not to attend the Workshop. Pickering e-mailed right back that he & Thurston (with or without DR) would be glad to meet 0 (& any other Ptolemy-backers) for a temperate public discussion of the Ptolemy controversy at the Workshop. As of press-time, the defenders are still hiding.

J9   This information is here entered into the record because the academic community should be aware of it when evaluating: [a] Certain forums' legitimacy, integrity, and independence. [b] Hiding&shunning cliques' science, logic, & openmindedness. And believability — even to themselves. In a 30 year context of suppression, omertà, and unfaced central technical blunders (some[48] of the most astonishingly elementary nature), the fact that such a clique continues *unconditionally* to receive every diplomatic and fiscal mark of high respect and honor in the history-of-astronomy community, should serve as a measure and a warning as to just how seriously this terrified community should be taken.

K   Control-Delusions and Defusions

Speaking slightly less facetiously than in §O, we may point out certain revealing parallels between US trials, elections, and religion. (See also §B1.)

K1 Diversion   All three serve as delay-depressurizers for human rage at injustice, by creating an illusion of deferred-but-eventual equity. They are thus powerfully effective buffers against armed revolution, which the public might well resort to if it knew how little reward is actually going to come true down the road.

K2 Inversion   All three create, for a powerless individual, a convincing illusion of control. [a] The jury believes that it decides guilt or innocence, though it usually cannot even ask questions, and is quite easily manipulated into simply going along with the most expensive lawyer. Or judge. Moreover, most juries are innocently unaware that if the judge wants to throw out any guilty verdict, he can. (The recent Louise Woodward case shocked alot of folks who never knew this.) A rich person can afford better lawyers, judges, appeals — and, if all else fails, he can purchase a governor's pardon. (Thus, no wealthy man has been executed in the US for over a half-century.) [b] US elections are merely a choice between two wholly-owned subsidiaries (fn 57) of the very corporations that are underpaying the voters who think their vote might get them a better deal; the prosperous and warring history of the spread of democracy is largely due to the convincing nature of this delusion: the voter believes he is controlling the gov't when the precise reverse is true. (See fn 10.) And, analogously to the courtroom escape-hatches just enumerated: on the outside chance that the public happened to elect someone unapproved by the nation's corporate rulers, there's always the Electoral College. Why else do you think it's there? [c] As for religion: praying is akin to going over the heads of earthly leaders to ask for a someday-fair-shake, from an imaginary ombudsman. (No wonder all exploitive gov'ts promote religion.) Well, at least god isn't deceiving. You have to exist before you can do that. Which brings us to ....

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[43] See, e.g., *DIO 1.1* ‡7 fn 17, *DIO 2.3* ‡8 §§C20&C22, *DIO 4.3* ‡15 §F5.

[44] Some examples at *DIO 1.1* ‡1 §C7, and even (as noted at ‡4 §A19) in the Gingerich-&-Swerdlow-sponsored 1998 James Evans *text*-book (see *News Notes*) — only adding to the disgrace to Oxford University Press represented by this slavishly party-line volume.

[45] Caaaalm down — it's just for a palmtop computer.

[46] *DIO 1.1* ‡C7 & fn 20, *DIO 4.3* ‡15 §§H4-H6.

[47] Dick has acknowledged this (98/12/3, regarding §J5) and on 99/1/6 said he'd received the 12/11 letter, about which he made no complaint throughout a friendly chat — though, by 1/21, the same 12/11 letter had become banworthy proof of DR stridency. [Note: Limited debate finally came off. See *DIO 9.1* p.2.]

[48] See, e.g., *DIO 1.3* ‡10, *DIO 4.1* ‡4 §A, or *DIO 4.2* p.56 Table 1.

L   Inverting Pascal's Wager

Blaise Pascal's argument for good behavior purports to be based upon what is in truth a generally valid principle: when making decisions, the wisest path is that with the highest associated product (S) of probability P and value V. That is, if there are n behavior-path options then we simply compare the various options' smartness S_i (i running from 1 to n), where

$$S_1 = P_1 \cdot V_1, \qquad S_2 = P_2 \cdot V_2, \qquad S_3 = P_3 \cdot V_3, \qquad \ldots \qquad S_n = P_n \cdot V_n \qquad (1)$$

and one then follows the path with the highest S. Pascal's Wager: no matter how small is the probability P_g of god's existence, the stakes V_g are infinite, so — since

$$S_g = P_g \cdot V_g = P_g \cdot \infty = \infty \qquad (2)$$

— it is best to obey god (take-the-god-option sounds more business-like), S_g being the highest available smartness S. Many rationalists have an inherent revulsion to this (a math-gambling approach to ethics) — based upon their wish to face the world with bravery, as against craven submission to power. But, even aside from its calculating selfishness,[49] the Pascal argument has (unadvertised) inherent logical difficulties, as well.

L1   Inversion. From Pascal's logic (above), we can reason as follows:[50] satan, who clearly governs the real world (where rulers mouth religion merely as a hypocritical ploy to exploit subjects) will take all-future-time out (from his busy schedule of corrupting the rest of humanity), just to eternal-roast-torment you if you worship the anti-satan called god; and satan will provide satanic pleasure eternally for all who follow himself. However low the probability P_d that this scheme is true, the associated value $V_d = \infty$ establishes (see eq. 3) that smartness $S_d = \infty$, which cannot be topped. Thus, the wise man must steel and psychically immure himself, in order to choose the strict satanic way of life — that is, to be naughty and have fun, just like he wanted to, before rulerships' lawyers-for-god addled his mind . . . .

To sum up pseudo-soberly: no matter how small are the odds on the existence of the devil[51] running the universe (rewarding sin forever in His Kingdom), the stakes are infinite, etc — that is, we consider an evil mirror-image of Pascal's god-argument (eq. 2). Again, we find that the smartness S_d of serving the devil is infinite:

$$S_d = P_d \cdot V_d = P_d \cdot \infty = \infty \qquad (3)$$

However: since the two behavioral options (S_g & S_d) are infinitely contradictory, the situation is impossible, and so neither these S nor any other claimants' bootstrap-self-proclaimed S can meaningfully equal ∞. (If a multiplicity of infinitely attractive options simultaneously pull at us, then life becomes an insane asylum. I.e., we have here a *reductio ad ∞ absurdum*.) But, by Pascal's eq. 1 logic, the only way these S values can be less than ∞ is if the probability P of the truth of *each* theory (god-rule or devil-rule) is infinitely small — i.e., zero. Putting this mathematically: if S is finite, then it follows from eq. 1 that (for god's or satan's infinite V):

$$P = S/V = S/\infty = 0 \qquad (4)$$

I.e., the probability of the existence of either an omnipotent god or devil is zero. So, there's Good News & Bad News. (Since we already know how conventional forces will see this, I'll instead report it as the *Libertine Gazette* would.)

<div align="center">

The Good News: Wet-Blanket God Doesn't Exist!

(The Bad News: Fun-Guy Satan Doesn't, Either. . . .)

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[49] See *DIO 1.1* ‡2 §H2.

[50] Our 1st argument (the Blaze-Pascal Wager: §L1) seems closely related to our 2nd argument (§L2). Similarly, Newton's 1st Law is merely a special case of his 2nd Law, though I've yet to encounter a physics textbook that mentions this.

[51] See *DIO 2.1* ‡1 §A9 on the awesome Problem-of-Good.

L2   Responsibility. This discussion will just be a rough quantitative formalization of the unanswerable Problem of Evil.[52] In order to pseudo-refute-evade it, theologians' standard pathetic "free-will" shellgame argument (which ought to be transparent to a 10 yr-old) is to blame all evil on man's free-will choice, without pointing out that: since — as the same folks claim — god created man (& his purported free-will) and thereby created the evil in man which man "freely" chooses to emit, then an (omnipotent) god is still the ultimate creator of all evil. If you estimate rationally the odds that the observable human universe is the creative responsibility of a very(-but-finitely) powerful deity, it is obvious (given the patent imperfection of the production) that the odds are quite small. Indeed, the deduced odds for god's existence may be crudely estimated by using an obvious (normalized) equation expressing the lowness of misery M (in a world which this deity allegedly created), namely, the inverse of M:

$$1/M = p \cdot D \qquad (5)$$

that is, misery-lowness ($1/M$) equals the product of the hypothesized deity-power D (for effecting goodness) and the probability p that god exists. We may re-write eq. 5 as:

$$p = 1/(M \cdot D) \qquad (6)$$

where, again, p is the rough probability of the god hypothesis. Now, with existing-misery M obviously substantial, probability p will not be low. But at least p won't be null — *so long as* the proposed deity-power D is finite. However, once this celestial power D is alleged [see below] to be infinite (as in the standard Christian theological contraption), the odds against its truth also become infinite:

$$p = 1/(M \cdot \infty) = 0 \qquad (7)$$

i.e., the probability that God exists becomes null.

L3   Time-Spans. A prime reason the Pascal-wager stakes are infinite is the proposal-lure of eternal life. But, we can examine this spectacular bribe quantitatively by analogy to a simple problem: take two spans in time and ask what are the chance-odds of being in either; obviously, it's the ratio of their spans. (E.g., it's 1439 times more likely to be between 00:01&24:00 than between 00:00&00:01.) Therefore (§B5): *if there were an eternal life, the odds are infinitely high that we would be in it right now.* (I.e., if the two time-span options are 100^y vs infinity, the probability of being in the former span is 100 divided by the sum of 100 and infinity — a ratio which equals zero.) Since we obviously are not in the hypothesized infinitely long life, one concludes that eternal life[53] is not our fate. We can re-phrase this as follows: if our finite terrestrial life were followed by an eternal afterlife, then 0% of all time resides in the former. If there were an eternal life after Earthly death, the probability is exactly 0% that Now would lie in our finite life here; but Now *is* in it.[54] A false consequent for a valid conditional proves the falsity of the antecedent; i.e. —

<div align="center">there is no eternal afterlife.</div>

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[52] See *DIO 4.3* ‡15 fn 42. The Problem of Evil predates Jesus; see, e.g., Lucretius, Books 2&5.

[53] Nothing in this argument precludes an infinitude of serial finite-reincarnations. When considering this option, we find two opposing forces attracting our sympathies: [a] Our inability to comprehend our own nonexistence (*DIO 4.2* ‡9 §K13). [b] The (slightly nontrivial!) issue of how one's self escapes one's own brain, one's own skull. (How does even theologian-level illusionism convert personality-degeneration [with age — and especially with death] into personality-survival? And: is [a] an argument or merely a viewpoint-limitation?) Though others disagree (see *DIO 4.3* ‡13 §§G4-G5), I personally have little desire for a life other than this one, beyond its inevitable end. The probability that another life could be even nearly as exhilarating as being Dennis Rawlins is vanishingly low. [Thoughts. (i) All I ask is to be with my wife forever; and the rest of our lives on Earth is a kind of forever, since that span is all there is. (ii) One's own death is unreal, since it cannot be experienced. So, if infinity is redefined as meaning Everything, then our finite life is (in a subtle sense: see fn 54's citation) infinite to us.]

[54] See *DIO 4.2* ‡9 §K13 for the *DIO* paradox that is a sibling to §L3's here.

M   Afterthoughts

M1   But, continuing §L into speculative regions: at death, does the experienced passage of time slow down? — to create the personal feeling that time has not stopped. This is not eternal life, but the lack of consciousness of one's own extinction-moment may be the best substitute we're going to get, in the real world. In the realm of ideals: the survival of one's work and play is another type of immortality, which I am so vain and sentimental as to savour. As for actual survival: how can we place credence in this, when we consider that, at death, the brain's nerve and blood cells (required for our consciousness's remembering & reasoning) cease activity? In this connection, we may consider our own very early youth — when such cells were in as small supply as at any point we choose to imagine back to. Almost no adult, thinking person believes he was so (able to reason — or even see) before he was born. So we can temporally mirror-image, to examine the afterlife question: if we go backwards in time, we can consider the early-embryo or even zygote stage — when human thought is as impossible as for a protozoan. So, a human without his material equipment is as unconscious as a corpse. To be permanently without that equipment is not heavenly exaltation but eternal unconsciousness. Death.

M2   All the foregoing analyses §§L1-L3 invert the superficially infinite strength of Pascal's argument — and thus not only overthrow it, but result in supporting conclusions which are the very contrary of his, namely, mortality and atheism. But these findings are merely part of a wider realization that religion is obviously based not upon logic but upon: [a] the incomprehensibility of extinction; [b] the utility (for rulers) of manipulating the ruled by the promise of invisible deferred rewards and-or punishments; [c] believers' fear of death (or hell); [d] and-or their wish for laughably-exaggerated benefit-promises. Item [d] is the main psychological basis for the embarrassingly childish notions of organized religion's (conveniently invisible) mega-entities: infinite god and eternal paradise. These are nothing but world's-record sales-exaggerations. Such pseudo-products cannot be sold to the public by rational persuasion. They sell because: the bigger the promise, the better[55] the con, a theorem (as logical as religion isn't) well known to experienced promoters throughout human history — from Las Vegas to numbers-racketeers[56] to Washington to VatCity.

N   The Original Horse-May-Die Deal

One of Henry 8's favorite jokes: A king attended an execution for an afternoon's entertainment and got a pleasant surprise when a thief on the block yelled out to the king that, if his life were spared for just a year, he could teach the king's horse to talk! Well, the king figured he had nothing to lose by agreeing — since, no matter what, he would end up with either the world's only talking horse or the same execution merely delayed a bit. After he announced the event's postponement, he left on other business. As soon as the king was out of earshot, a friend of the thief scoffed that he'd gained nothing, since he could never make good on his promise. But the thief responded: well, at least I've talked myself a gain of a whole *year* of life. And, during that year:

> The horse may die.
> Or the king may die.
> Or I may die.
> Or — the horse may talk.

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[55]Thus, e.g., the posher the seller's abode. See *DIO 4.2* ‡9 §§H7-H8, & *DIO 4.3* ‡13 fn 22.

[56]Which is what we used to call Dutch Schultz and Charley Lucky. But now, The Numbers are run by the gov'ts of those many states of the US who push lotteries — which can only be (inadequately) defended as a tax on greedy stupidity. See DR's *Skeptical Inquirer* proposal (*Skinq 2.1*:62 [1977] p.79) to earmark state-lottery profits for teaching statistics to the young, aiming at wiping out the very innumeracy which lotteries live off.

O   Skew Stew

O1   US Democracy: rulership by puppets elected-beloved by those too naïve to realize that voting[57] is not very relevant to their lives.

O2   Demography of the several bunnyrabbit religions (all of 100% male rulership): numerical-political advantage to those too brainwashed and-or improvident to use birth control. [See *DIO 4.3* ‡13 fn 8 & §E.]

O3   US Jury System: governance by verdicts of 12 people too dumb to get off jury duty.[58]

P   Defynitions

P1   MIM = 1999 in Roman-numerals. (You instead prefer MDCCCCLXXXXVIIII?)

P2   2001/1/20 = DLSWHSBJ Day.[59]

P3   Democratic election = voting for a pol who seeks powers unequal to yours, by swearing that you and everyone else are his equals.[60]

P4   "Middle Ages" = PC for "Dark Ages".

P5   Lead into battle = general's expression for issuing orders from the rear.[61]

P6   Ultimate lawyer ployer = Let's rewrite WW2, since the Nazis at Nürnberg were't pre-Mirandized.

P7   Unsinkable Kate Winslet = changed our pronunciation of *Titanic*.

P8   Death-row daily periodical = noosepaper.

P9   French PETA = pourquoi pig?

P10   Hitler Youth = Nazi Totsies. Or, the Goy Scouts. (Or, a few lucky-survivor septagenarians.)

P11   Sex addict = male.

Q   Compacting & Clarifying Dates

Q1   As noted at §P1, "MIM" is the roman-numeral expression for 1999. The alternate rendition there cited (MDCCCCLXXXXVIIII) reminds us to make the obvious suggestion: why not just cut out using roman numerals? They're confusing & cumbersome. And they are almost never more efficient than the equivalent arabic expression. But, ironically, 1999 begins a rare stretch when the reverse is true: from 1999 through 2001. So, should we retain romans until the end of the year MMI?

Q2   With the 3rd millennium looming, we urged in 1994 (*DIO 4.2* ‡9 §K6) a new international convention for compact writing of dates. The example there given to illustrate potential confusion was 3/4/2: does it mean March 4, 2002 (US convention) or 3 April 2002 (UK convention) or 2003 April 2 (astronomers' convention)? We recommended the last as best, because all the digits are *in the same descending order* — the same order that they occupy in the writing of, e.g., the year 2002 itself: leftmost marks the biggest unit, and the further right one reads, the smaller the unit. Our challenge is to find a way of telegraphing to readers that we are definitely using the year-month-day system. Suggestion: separate the numbers with backslashes, so that 2002\04\03 will be universally understood to signify 2002 April 3. *DIO* has been using this convention (for the printing date) on the inside back covers of all issues and reprints since 1998.

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[57] See *DIO 2.3* ‡6 §C and fn 23. (Also here at §K2 item [b].)

[58]§O3 not *DIO* original.

[59]Divorce-Lawyer-Solicitation White-House-SwitchBoard-Jam Day.

[60]See §B1 and §K.

[61]See Redd Foxx at *DIO 1.2* fn 52 on war: "I backed up so far, I bumped into a general."

R   Oxymorons

R1   Political science.
R2   Harmless crank.[62]
R3   Roman Catholic.[63]
R4   Anti-discrimination homosexual.

S   That's why the state bird is the loon [by Keith Pickering]

S1   The recent election of pro wrassler Jesse Ventura as governor proves again that humor can be an important ingredient in winning elections in Minnesota. Ventura deployed a radio commercial featuring the candidate (in his own unmistakable basso voice) listing his political beliefs. In the middle of a litany praising lower taxes, public education, and the usual mom-and-apple-pie issues, Jesse "The Mind" inserted this item: "I believe that Led Zeppelin and the Rolling Stones are the two greatest rock bands of all time," and then rolled along to the next issue without missing a beat.

S2   In a similar vein, Ventura ran a 3/4 page ad in the major papers on the Sunday before the election, contrasting his positions and background with those of Republican candidate Norm Coleman, in a list of over 40 items. (E.g., Coleman: pro-life; Ventura: pro-choice.) Amid this mostly serious political apposition was a scattering of loony tunes, ranging from the obvious (Coleman: Lots of hair; Ventura: No hair) to the venomous (Coleman: Professional wrestler of the truth; Ventura: Professional wrestler of other wrestlers).[64]

S3   Ventura (of billionaire R.Perot's Reform Party) was apparently outspent 10-to-one by Coleman and 7-to-one by Democrat Skip Humphrey,[65] yet was able to cut through the clutter of election-eve ads with a campaign that was the talk of the state. The adman responsible for the campaign was Bill Hillsman, the same guy who masterminded Minnesotan Paul Wellstone's equally quirky campaign for the U.S. Senate in 1990. Wellstone too was a longshot to win, facing a popular incumbent, but squeaked out a narrow victory by not being afraid to come off as a real (and sometimes funny) human being in Hillsman's ads.

S4   The most important (& underreported) implication of Ventura's win: perhaps public campaign financing can work. The $300,000 Ventura received from the State of Minnesota financed his media blitz in the last two weeks before the election. Ventura's political philosophy leans leanly toward least-government libertarianism, but it's a safe bet that this is one government handout that won't be touched in the Ventura administration.

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[62] Deception is not a harmless activity. And cranks all eventually become deceivers — because they have to evade, deny, distort, suppress, and-or ignore the evidence that proves them wrong. (See analogy at *DIO 4.3* ‡13 §C2.) Moreover, many become vicious towards those persons whom they imagine are all that stand in the way of their pet view's acceptance. (See *DIO 1.1* ‡1 §C7, *DIO 7.3* ‡9 fnn 46-47.)

[63] "Catholic" means universal, i.e., non-local. And, oh yes, "catholic" also means: open-minded.

[64] Ventura has now achieved enviable success as actor in three different arenas: wrassling, film, and politics. In a recent Hollywood comedy, he was mock interviewed: Jesse, I've heard wrasslers don't use steroids anymore. Ventura replied: "Nor any less." (Delivered *sotte voce*. For him.)

[65] Ventura thus made a virtue out of a necessity, proudly proclaiming his refusal to take special interest PAC money. As if any had been offered.

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E. Myles Standish (prime creator of the solar, lunar, & planetary ephemerides for the pre-eminent annual *Astronomical Almanac* of the US Naval Observatory & Royal Greenwich Observatory; recent Chair of American Astronomical Society's Division on Dynamical Astronomy): "a truly intriguing forum, dealing with a variety of subjects, presented often with [its] unique brand of humor, but always with strict adherence to a rigid code of scientific ethics. . . . [and] without pre-conceived biases . . . . [an] ambitious and valuable journal."

B. L. van der Waerden (world-renowned University of Zürich mathematician), on *DIO*'s demonstration that Babylonian tablet BM 55555 (100 BC) used Greek data: "*marvellous*." (Explicitly due to this theory, BM 55555 has gone on permanent British Museum display.)

Rob't Headland (Scott Polar Research Institute, Cambridge University): Byrd's 1926 latitude-exaggeration has long been suspected, but *DIO*'s 1996 find "has clinched it."

Hugh Thurston (MA, PhD mathematics, Cambridge University; author of highly acclaimed *Early Astronomy*, Springer-Verlag 1994): "*DIO* is fascinating. With . . . mathematical competence, . . . judicious historical perspective, [&] inductive ingenuity, . . . [*DIO*] has solved . . . problems in early astronomy that have resisted attack for centuries . . . ."

Annals of Science (1996 July), reviewing *DIO* vol.3 (Tycho star catalog): "a thorough work . . . . extensive [least-squares] error analysis . . . demonstrates [Tycho star-position] accuracy . . . much better than is generally assumed . . . . excellent investigation".

British Society for the History of Mathematics (*Newsletter* 1993 Spring): "fearless . . . . [on] the operation of structures of [academic] power & influence . . . much recommended to [readers] bored with . . . the more prominent public journals, or open to the possibility of scholars being motivated by other considerations than the pursuit of objective truth."