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Ancient Declinations and Precession

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The Babylonian Theory of the Planets

Hugh Thurston

The Babylonians developed sophisticated theories for the motions of the planets. These are interesting not so much for the light that they throw on the planets as for the methods used by modern research workers in interpreting them. Often the results of these researches are given without mention of how they were derived, and sometimes data are described as attested when in fact they do not appear in the Babylonian tablets but were derived from them.

Two types of tablet are concerned with the theories. Some tablets list calculated data. Others, called procedure tablets, explain the calculations.

There are two main theories: a zonal system in which the ecliptic is divided into zones in each of which a relevant quantity is constant, and a zigzag theory, in which a quantity increases at a constant rate from a minimum to a maximum, decreases at the same rate to the minimum, increases at the same rate, and so on. These (unimaginatively in my opinion) are usually called system A and system B.

Most of the tablets concerned with the planets give calculated dates and longitudes for the synodic phenomena; very few give data for the planet between the phenomena.

The synodic phenomena for an inner planet are its first appearance in the morning, MF, its subsequent disappearance, ML, its appearance and disappearance in the evening, EF and EL, and its stationary points. (The names morning first, morning last, and evening first and last, are from van der Waerden).

The synodic phenomena for an outer planet are its appearance (in the morning) MF, its disappearance (in the evening) EL, opposition, and the beginning and end of retrogression, BR and ER.

The Babylonians recorded the longitude of the planet at each occurrence in signs (of the zodiac) and thirtieths of a sign, which I shall convert into degrees.

The arc of the ecliptic separating two successive occurrences of a synodic phenomenon is a synodic arc.

Many zonal systems are described in procedure tablets. These include a system for Mercury's MF using three zones, a different system for its EF also using three zones, three different systems, each using two zones, for Jupiter, one using four zones and one using six zones, and finally a system for Saturn using two zones.

There are also tablets for the ML and EL of Mercury clearly using zonal systems. We have not found a procedure tablet explaining either of these but the zones can be deduced from the longitudes. The results are often cited, and the deduction must have been made early on, probably by Kugler in a work that I cannot find (nor can anyone whom I have asked). The same applies to a system for Mars using six zones.

The first aim of this paper is to show how the deductions could be made. The second aim is to comment on the relations between the synodic and the sidereal periods.

Zonal system

To show how a typical system works I use the one for the MF of Mercury explained in ACT 801. Given the longitude λ of one MF to find the next:

First step. If λ is between 121° and 286° add 106°
If between 286 ° and 60° add 141° 20'
If between 60° and 121° add 94° 13'20".

Second step. If the arc added takes you into the next zone, multiply the portion in this second zone by the arc added in the second zone divided by the arc added in the first zone.

<u>Example.</u> If λ is 201°, the first step takes us to 307°, of which 21° is in the second zone. We multiply this by 4/3 (as stated in ACT 801: of course, 141° 20' is 4/3 of 106°), getting 28°. So the next longitude is 28° in the second zone, which is 314°. (If this had taken us into a third zone, a third step would have been needed.)

Mercury

ACT 300a gives the longitudes of successive occurrences of ML. By subtracting one longitude from the next we find the length of the synodic arc. These arcs, arranged in the order of the longitudes at which they start, are:

start	arc	start	arc	start	arc
12°	120°46'40"	132°46'40"	119°53'20"	272°20'	99°40'
31°'40'	117°30'	149°10'	123°10'	288°	103°40'
43°	115°36'40"	165°33'20	122°26'40"	296°30'	106°30'
51°20'	114°13'20"	175°	121°36'	302°45'	108°35'
62°40'	112°20'	182°20'	120°25'	311°15'	111°25'
71°	112°20'	193°40'	117°35'	317°30'	113°30'
82°20'	112°20'	213°20'	112°40'	326°	116°20'
100°	113°20'	233°	107°45'	340°45'	119°15'
116°23'20"	116°36'40"	252°40'	102°50'	355°30'	120°53'20"

From 12° to 62°40' there is a steady decrease of 1' in the length of the synodic arc for every 6' increase in the longitude at which it starts. This suggests that the data follow a system like the one just described with an added arc of length X° in a zone covering these longitudes and an added arc of length 5X°/6 in a zone covering the end-points of these synodic arcs, which range from 132°46'40" to 175°.

Similarly,

- an increase of 1 in 5 from 100° to 149° suggests an added arc of length Y° here and one of length 6Y°/5 from at least 213°20' to 272°20'.
- A decrease of 1 in 4 from 182°20' to 252"20' suggests an added arc Z° here and 3Z°/4 from at least 302°45' to 355°30'.
- An increase of 1 in 3 from 288° to 326° suggests W° here and 4W°/3 from at least 31°40' to 82°20'.

There is an arc of X° from at least 12° to 82°40' and an arc of 4W°/3 from at least 31°40' to 82°20': Therefore X = 4W/3. Similarly Y = 5X/6 and Z = 6X/5. Therefore X = Z = 4W/3 and Y = 10W/9.

So we have four zones:

4W°/3 from x° to y°, covering at least 12" to 82°20'

10W°/9 from y° to z°, covering at least 100° to 175°
4W°/3 from z° to w°, covering at least 182°20' to 272°20'
W° from w° to x°, covering, at least 288° to 355°30'.

The synodic arc from 12° to 132°46'40" will pass at y° from the 4W°/3 zone to the 10W°/9 zone, so 132°46'40" = y° + (12° + 4W°/3, - y°)x5/6. Then

$$y/6 + 10W/9 = 122\ 7/9 \qquad (1)$$

The synodic arc from 71° to 182°20' does not share the steady decrease, so it must pass both y° and z° into the second 4W°/3 zone:

$$y + (71 + 4W/3 - y)x5/6 = y/6 + 355/6 + 10W/9.$$

Then $$182° = z + (y/6 + 355/6 + 10W/9 - z)x6/5$$

so $$y/5 - z/5 + 4W/3 = 182\ 1/3 - 71 = 111\ 1/3 \qquad (2)$$

For the synodic arc starting at 100°,

$$213\ 1/3 = z + (100 + 10W/9 - z)x6/5,$$

so $$4W/3 - z/5 = 213\ 1/3 - 120 = 93\ 1/3 \qquad (3)$$

From (2) and (3) y/5- 18, so y = 90. Then, from (1), 10W/9 = 107 7/9, giving W = 97 and 4W/3 = 129 1/3. From (3), z/5 = 36, so z = 180.

For the synodic arc from 182°20' to 302°45',

$$302\ 3/4 = w + (182\ 1/3 + 129\ 1/3 -w/)x\ 3/4 = w/4 + 233\ 3/4.$$

Then w/4 =69, so w = 276.

For the arc from 296°30' to 43°,
$$43 = x + (296\ 1/2 + 97 - 360 - x)\ x\ 4/3 = 44\ 2/3 - x/3$$
So x/3 = 1 2/3, giving x = 5.

To sum up, ML has four zones:
From 5° to 90° add 129°20'
From 90° to 180° add 107°46'40"
From 180° to 276° add 129°20'
From 276° to 5° add 97°.

It is unfortunate that Neugebauer described these as zones of constant synodic arc: the synodic arc of Mercury is nowhere constant.

Mars

ACT 501, 502 and 504 give calculated longitudes for BR. If we calculate the synodic arcs and arrange them in the order of the longitudes at which they start we have the following results (starting longitude followed by arc).

start	**arc**	**Start**	**arc**	**start**	**arc**
15°	50°	115°	30°	210°	60°
18°45'	48°45'	116°40'	30°	228°40'	69°20'
40°	45°	133°20'	34°26'40"	230°	70°
64°	38°40'	135°	35°	233°20'	71°40'
65°	38°20'	145°	38°20'	256°40'	80°50'
67°30'	37°30'	146°20'	38°53'20"	270°	82°30'
85°	31°40'	170°	40°	298°	75°30'
102°40'	30°	183°20'	46°40'	305°	73°45'
103°40'	30°	185°33'20"	47°46'40"	337°30'	62°30'
105°	30°	207°46'40"	58°53'20"	353°30'	57°30'

These fit together so well that it is quite clear that the three tablets use the same system. From 102° 40 to 116° 40 the synodic arc is 30°. Therefore there is a zone covering the ecliptic at least from 102° 40 to 146° 40 for which the added arc is 30°. From 64° to 85° the synodic arc is reduced by 1° for every increase of 3° in the longitude at which it starts. So here we have a zone with an added arc of which 30° is two-thirds. The added arc here is therefore 45°. To fall from 31° 40 at 85° to 30° needs a reduction of 1° 40 in the arc and therefore an increase of 5° in the longitude. Therefore the 30° zone begins at 90°.

From 133°20 to 146°40 the arc increases by 1° for every increase of 3° in longitude, so in this zone the added arc is 4/3 of 30°, i.e. 40°. To rise to 40° from 38° 53 20 at 146°

40 needs an increase of 1° 06 40 in arc and therefore of 3° 20 in longitude. Therefore the 40° zone starts (and the 30° zone ends) at 150°. So far we have:

Up to 90° add 45°
From 90° to 150° add 30°
Past 150° add 40°.

This agree well with a fragment of ACT 821aa, which says: from 30° to 90° add 45°, from 90° to 150° add 30°. It also says; beyond 90° multiply by two-thirds.

From 170° to 233° 20 the arc increases by 1° for every increase of 2° in longitude, so the next added arc is 3/2 times 40°, i.e. 60°. To reach 60° from 58° 53 20 at 207° 46 40 needs an increase of 1° 60 40 in arc and therefore of 2° 13 20 in longitude. Therefore the 60° zone starts at 210°. After that the arc is still increasing at the same rate, so the next added arc is 90°.

We have so far at least five zones. Two of them, 90° to 150° and 150° to 210°, cover precisely two signs of the zodiac; ACT 811b groups the signs into pairs, including these two pairs. It is a reasonable guess that the pairs are the zones for Mars. If so, we have

From 30° to 90° add 45°
From 90° to 150° add 30°
From.150° to 210° add 40°
From 210° to 270° add 60°
From 270° to 330° add 90°
From 330° to 30° add x°.

We have only to calculate x and then check to see whether the resulting system agrees with the data in the tablets.

The arc starting at 270° ends at 352° 30. Adding 90° to 270° yields 330° plus 30°. Multiplying the 30° by x/90 gives us 330° plus x/3°, so x/3° = 22° 30, giving x = 67° 30. This agrees with the data, and the MF and EL in ACT 502 both use this system.

Mean periods

The relation between the mean synodic and sidereal periods can be found by noting when a synodic phenomenon repeats at the same point in the sky and using the fact (which is obvious from a heliocentric viewpoint and seems to have been known to the Babylonians) that if X synodic periods of an outer planet equal Y sidereal periods and take Z years, then Z = X + Y. At least, it is obvious for oppositions, which always take place at the same elongation, namely 180°, from the sun. The Babylonians seem to have assumed that this holds also for the other synodic phenomena. Strictly speaking, this gives the mean of the periods that occur between the observations, but for a reasonably long interval this mean will be fairly stable.

Two such relations are given in procedure tablets. ACT 811a says: Mars 284 years 133 appearances 151 rotations. That is: 133 synodic periods and 151 sidereal periods each take 284 years. ACT 819 says: Saturn 9 rotations 265 years. So 9 sidereal periods equal 256 synodic periods (from the relation X + Y = Z).

The relation underlying a table using the zigzag system, if not found in a procedure tablet, can easily be deduced. For example, in ACT 600 for the oppositions of Jupiter, each entry differs by 19°33 from the next, so a synodic period causes this much change. The total change from minimum back to minimum is easily found to be 19°30. This corresponds to one revolution round the ecliptic and so takes one sidereal period. Therefore 1173 synodic periods equal 108 sidereal periods.

Such relations determine the synodic arcs. The mean distance covered by the planet round the ecliptic in one synodic period is Y/X revolutions and the synodic arc is the fractional part of this. So for Mars, where Y/X = 1 + 18/133, the mean synodic arc is 18/133 revolutions, which to the nearest minute is 48°43', a figure that-is actually given in ACT 811a.

Now let us look at the zonal system. The various added arcs represent the different speeds at which the phenomena progress through the zones. If a particle covers distances a, b, c,... at speeds u, v, w,... the total time taken is a/u + b/v + c/w +... and the average speed is a + b + c + ... divided by this time. This is the weighted harmonic mean of the average speeds.

Therefore the mean synodic arc should be the weighted harmonic mean of the synodic arcs. This would be formidably difficult to achieve. Instead, the Babylonians arranged the added arcs to make their weighted harmonic mean equal to the mean synodic arc. For example, the weighted harmonic mean of the six added arcs for Mars in ACT 501 is 18/133. And for Jupiter both the two-zone system and two four-zone systems yield the same figure as the one deduced from the zigzag. However for Mercury the weighted harmonic mean would give different results: 848/2673 revolutions for MF, 480/1513 for EF, 388/1223 for ML, and 217/684 for EL.

I would like to thank John Britton and Dennis Duke for helpful comments on an early draft of this paper, and John Steel for very helpful comments on a later draft.

Reference

ACT: Astronomical cuneiform texts. Edited by Otto Neugebauer, 1955. (Tablets with numbers 800 or higher are procedure tablets).

Source of Hebrew Month Duration: Babylonian Science or Ancient Tradition?

Morris Engelson

Summary.

The best-known ancient value for the average length of the month is deduced (in sexagesimals) from Babylonian tablets of about 200 BCE. However, a statement in the Talmud, identified with the Hebrew Bible and allegedly older, says that the month is not less than a certain value, which, when converted to sexagesimals, is identical to the Babylonian one.

This paper will:

1. Demolish the argument that, because the modern month is less than this value, the Talmud is wrong.
2. Show that it is not likely (though not impossible) that the Hebrew month duration was borrowed from the Babylonians.
3. Conclude that the source of the Hebrew month is unresolved.

Introduction.

The value deduced from the Babylonian tablets, and discussed by Ptolemy, is 29; 31, 50, 08, 20 days, in sexagesimal notation. I shall call this value BM. The statement in the Talmud [1] names a value of 29 days plus half a day plus two-thirds of an hour plus 73 parts of an hour. Here, a part (heleq in Hebrew, plural halaqim) is 1/1080 of an hour. I shall call this value HM. HM is usually presented as $29^d, 12^h, 793^p$. It is easy to show, by converting to sexagesimal notation, that HM is identical to BM.

The currently accepted mean value for the synodic month, based on measurements by NASA, the Naval Observatory, and others, is almost half a second less than both HM and BM.

Not less than.

The Talmudic statement by Rabban Gamliel (RG) that the month duration is "not less than..." poses a number of difficulties. The statement was made to show why witnesses, who claimed to have seen the new moon, could not have seen it this early because the time since the last new moon should "not be less than..." But the interval between two successive first visibilities is not the same as the average value between conjunctions, which RG cites. The precision given by RG does not fit a phenomenon (successive detection of the new moon) which can vary by many hours. Hence, many investigators in this field suggest that an originally simpler statement by RG was later modified.

Noel Swerdlow argues in a 1980 paper [2] that the Hebrew month duration was likely a learned adaptation from the *Almagest.* His position at this time, 25 years later, is even stronger [3], "Now I would consider the possibility of a direct transmission from Hipparchus extremely unlikely, even all but impossible, and say that the parameters must come from the *Almagest*." Sacha Stern notes on page 201 of his book [4] that "this is almost certainly a later interpolation." The original should have simply given 29 days and no more. A later interpolation eliminates all problems, along with the need for this essay. But I seek to examine whether this number, and the use of the heleq at 1080 parts to the hour, is of ancient origin instead. Therefore, I argue against interpolation; I believe that a good case can be made against it.

Interpolation and the source of 1080.

We have four possibilities to consider for the source of 1080 parts to the hour.

1. There was a later interpolation.
2. Otto Neugebauer suggests that the Hebrew sages were using the Babylonian še, which is $1/72^o$, so that 1080 še represents the rotation in one hour [5, pg 117].
3. Savasorda suggests a solution based on reduced fractions [*ibid*, pg 89, xlvii], given that BM is equal to 29 days, 12 + 793/1080 hours.
4. It is an ancient tradition predating the Babylonian tablets.

Against 1: There is no direct evidence for an interpolation. Stern tells us on page 202 of his book [4] that "The absence of manuscript evidence does not undermine the argument; it only suggests that the interpolation must have been made relatively early…."

Secondly, RG cites the information as a tradition from his grandfather. One scarcely needs to cite a tradition for the obvious fact that the month is not less than 29 days. Moreover, in the Talmud one does not modify a statement from a previous generation without explanation. Thus, Solomon Gandz [5, pg 90], "it is extremely improbable that a younger Amora [later sage] would expand or correct the Patriarch's dictum without giving either his name or his reason."

Against 2: We would not expect the Hebrew sages, who were not astronomers, to use such sophisticated astronomical reasoning. Moreover, this suggestion depends on a finger ($1/12^o$) being 6 še, whereas Maimonides held that it is 7 še [6].

Against 3: We need a Torah sage, who is also an exceptional mathematician, to introduce reduced decimal fractions some 2000 years ago.

Clearly, all four suggestions raise questions, which is one reason why an ancient tradition cannot be dismissed out of hand.

We need more information.

Clearly the rabbis of the Talmud would not have used the astronomy-based procedure suggested by Neugebauer, even if they were familiar with the astronomical relationships, if the ratio of barleycorns to finger were7:1, as Maimonides states. But the situation is not that simple. I have two references, courtesy of Yaaqov Loewinger, that show a ratio of 6:1. The following will provide the basics, and the reader is referred to the companion paper, identified as section II, for details.

We have a reference that the Baal ha-Tanya (18th century Torah authority) used a ratio of 6:1 [7]. "To confuse you even more", Loewinger notes in his message to me, a 15th century work claims that Maimonides used a ratio of 6:1 in a response to a question, but the ratio of 7:1 is also used there later [8]. So which ratio did Maimonides really use, and were there two traditions of 6 and 7 ratios side-by-side? If 6:1 was used, then is it not reasonable that the origin of the heleq is in accordance with the Neugebauer suggestion? My response is no, because the ratio of se'orah (barleycorn) to the finger (etzba) was not a fundamental value, given that the units of linear measure in the Talmud are derived from volume relationships and not as multiples of the barleycorn. The relationship of barleycorn to the finger, whether 6 or 7, was established "experimentally"and not by way of a fundamental relationship or definition [9]. Hence, this ratio, no matter what it was, should not have played a role in setting the value of a critical time unit; the heleq. More information on whether the number is 6 or 7, or both (as is most likely) would be very interesting. But I maintain that this is not material to the source of the heleq. My reasoning is explained in more detail in section II.

When less is more.

We know from measurements by NASA that the average month (at 29.5305888531… days) is 0.456 seconds less than HM (at 29.5305941358… days). However, the length of the month, when stated in days, has decreased by about 1 second in the last 2000 years. (Data from Stephenson [10] makes the decrease about 0.8 seconds).

The month, measured in units of 24-hour days, rather than atomic seconds, meets the RG "not less than" criterion 2000 years ago. Indeed, even a 0.31 seconds shift per month per millenium, suggested by Nachum Dershowitz, co-author of *Calendrical Calculations* [11], is sufficient. In fact, the current deficiency of 0.456 seconds is accounted for at 0.285 seconds per millenium in the last 1600 years since the Hebrew calendar has been fixed, and there has been no need to interrogate witnesses.

Furthermore, we are told in commentary [12] that RG was referring to only half of the tradition in the "not less" statement; the full tradition is that the time is "not less and not more." This contradiction (not less and not more, but not precisely equal) is resolved by noting that the heleq is the smallest time unit for expressing the duration of the month. The final result is expressed by rounding to the nearest heleq. While the current value of the month is less than HM by about 0.5 seconds, it is technically not "less" because the deviation at less than half a heleq (1.66 seconds) disappears in the rounding process.

Importance of the heleq.

Some Hebrew Bible authorities hold that the calculation of the calendar, especially the duration of the month and division of the hour into 1080 portions, was revealed to Moses at Sinai.

Maimonides begins the first chapter of book 3 in his *Mishneh Torah*, the section on The New Moon, with an explanation that: (a) we determine the duration of the month in accordance with the Moon; (b) the start of the month is based on when the new Moon can be seen; (c) the duration of the year is based on the sun and (d) various matters connected with the month were revealed to Moses at Sinai. He notes in chapter 11, that "these methods are indeed remote and deep, and they constitute the Secret of the Calendar, which was known only to the great sages and which they were not permitted to reveal to anyone." He goes on to state that such matters were maintained by "the tribe of Issachar, who lived in the time of the prophets." The well-annotated Stern [4] notes on page 207 that R. Avraham b. Hiyya claimed that "Hipparchus... had taken this lunation from early Jewish sources." We are told on the same page that "Isaac Israeli argued that Ptolemy obtained his lunation independently from the rabbis..." We then learn that R. Tuvia b. Eliezer talked about "the secret of intercalation which had been calculated since the day of Adam.…"

Such statements support a tradition that certain aspects of the Hebrew calendar are of ancient origin and could not have been borrowed from the Babylonians or Greeks. This idea is particularly important to the kabbalists (Jewish mystics) who derive various conclusions on the basis of the 29, 12, 793 duration of the month and the division of the hour into 1080 portions. For example, the total HM duration of 29, 12, 793 equals 765433 halaqim, which can be written as the descending sequence: 765432+1. This sequence is only possible with a division of 1080 parts to the hour. We might note that the suggested resolution of the "not less than" question, by rounding to the nearest heleq, is in line with an emphasis on a fixed number of halaqim. The reader will find additional information in the literature [13].

Did the Hebrew calendar borrow from the Babylonians?

Yes. *Rosh Hashanah Yerushalmi* states that the month names are Babylonian names. Nachmanides elaborates at length on this in his commentary on *Parshas Bo* (Exodus 12:2), where he explains why the months were originally numbered and later acquired foreign names: The numbers are to be a reminder of bondage in Egypt, while the names are a reminder of exile in Babylon. And Maimonides notes that in his day, the Hebrew calendar calculations include many elements from Greek science. This is permitted "since these rules have been established by sound and clear proofs, free from any flaw and irrefutable, we need not be concerned about the identity of their authors, whether Hebrew prophets or Gentile sages."

Despite these science-based factors, traditionalists also argue that the duration of the month and the division of the hour into 1080 portions is based on information from the time of Moses. We will now examine whether this position is tenable on the basis of secular scholarship.

History of the heleq.

The origin of the heleq is quite mysterious. Stern tells us on page 204 [4] that "this division of the hour was specifically designated for the lunation... it is not known to have been used in any other context." The earliest explicit reference to the HM value, and 1080 parts to the hour, appears in the writings of Muslim astronomer al-Khwarizmi in 823/4. We also have a less-definite reference to 1080 parts to the hour in a poem by R. Pinhas in the early 9th or late 8th century. However, Loewinger [14] notes that

R. Pinhas refers to “a double hour (120 minutes), and so his halaqim are double halaqim.” We also have the *Pirqei de-R-Eliezer* where the 73 halaqim of the month are mentioned. But there is considerable opinion that this is a later interpolation [4]. Then the trail is lost. We have more than 600 years of silence between the original statement by RG in the 1st century and the 8th century. Where does this time designation come from? And why this mathematical mode of expression and not some other? We can conjecture four possibilities:

1. The 1080 to the hour was introduced not much before the time of R. Pinhas as part of an emendation of the RG statement respecting the duration of the month.

   That does not seem reasonable on two grounds. RG could be excused for using the peculiar formulation involved in the use of the mean lunation to designate a “not less than” statement since he would not presume to alter an ancient tradition. But no tradition is involved in an interpolation. The explanation here is that whoever changed the text did not fully understand the implications of the added information, which refers to a mean rather than actual month. In effect we are not dealing with a skilled scientist or mathematician. By what means then, did this person come up with a sophisticated formulation involving reduced fractions? Thus, 73/1080 has a prime numerator, while 793/(1080*24) cannot be reduced any further since numerator and denominator have no common factors. The other flaw here is simply the question – why invent a new mathematics even if the person had the skill to do it? Why not use the time measures then in use?

2. The RG statement was borrowed from the Babylonians before his time, and the 1080 time measure was introduced sometime before RG made his statement. 1080 is, after all, “naturally” embedded within the BM structure. Thus we find the expression: $29^d\ 31'\ 50''\ 08'''\ 20'''' = 765433^d/25920 = 765433^h/1080$ in a modern technical paper [15]. The 1080 is a natural result using modern mathematics. The 1080 also seemed reasonable to Savasorda nearly 1000 years ago. It would not be impossible for someone to do the same even 2000 years ago.

   But why, then, was this information kept secret? Neither the month duration previously identified as HM, nor the 1080 portions are made generally known for hundreds of years. And why was it necessary to invent a new time unit in preference to whatever was generally available?

3. The 1080 measure was introduced at an unknown time for an unknown reason, and not necessarily to express the time duration of the month. Here we wonder why there is no trace of the original reason. We also wonder how it is that this time measure is uniquely suited to the expression of the time duration of the month.

4. The last possibility is that 1080 to the hour is a tradition of ancient origin. It exists because the Hebrew Bible requires it to exist. Human skill in mathematics is not involved. The matter was kept secret along with the other secrets of the calendar which were closely guarded by a small group of people.

Babylonian and Hebrew calendars.

As remarked above, the Hebrew calendar has many features identical to the Babylonian. But they are not identical. Unlike the simpler Babylonian lunar-solar cycles, the Hebrew calendar repeats only after 36,288 cycles of 19 years each, or 689,472 years [16]. One possibility: The Hebrew calendar is an adaptation of the Babylonian calendar. Another: The Hebrew calendar came from the time of Moses, but Babylonian and Greek details were incorporated later.

Not all sexagesimal fractions of an hour (or day) convert into an integral number of halaqim. But an integral number of halaqim always converts into sexagesimal fractions of an hour. Hence, the Babylonian figure could easily have been converted from a Hebrew source. But if the Hebrew figure came from the Babylonians, it is a coincidence that it is an integral number of halaqim.

In addition, while the Hebrews had no professional astronomers in the several centuries BCE, the Babylonians did. They would surely have recognized important astronomical information when they came across it. Hence, it is not impossible that the information flow was towards, and not from, the Babylonians. The idea that the Babylonians borrowed from the Hebrews would seem a heretical notion not worth considering. Surely, the Babylonians discovered this number on their own. Well, maybe they did and maybe they didn't. And if they didn't, then it is not totally out of line to consider a Hebrew source.

Source of sexagesimal lunation.

It turns out, that we have very little direct information respecting the source of the famous sexagesimal month. We know of this number from Ptolemy, 2nd century CE. Ptolemy notes that Hipparchus (2nd century BCE) knew this information. That's it; the direct trail stops at that point.

The majority of investigators agree to an unnamed Babylonian (or group) who established this value prior to 150 BCE. The reader will find an easily accessible general discussion in a book on astronomy [17], and a detailed discussion in a paper by John Britton [18] who offers the tightest suggested time frame for this at 310 ± 40 years BCE. Finally, we arrive at a new idea, from Dennis Rawlins [15], who uses a mathematical model to support the hypothesis that the Babylonians borrowed this value from the Greek Aristarchus (3rd century BCE).

The suggestion that the Babylonians borrowed this information calls for two supporting points: One point is to provide some evidence of how the supposed originator of this number got to it. That is precisely what Rawlins does in his paper, which includes a detailed mathematical analysis of the suggested process. The other point is to show that the Babylonians might not have discovered this number. Rawlins notes in the summary of his paper that, "The enormous mass of extant Babylonian data has never explained the origin of M_A. [M_A = sexagesimal lunation]." Christopher Walker, editor of [17], notes in an exchange of e-mails on May 28, 2002, that [19] "In any case, we never had a clue about how a Babylonian might have arrived at the value in question."

Britton starts with raw data and ends with the famous result with details neatly wrapped up. The paper shows, for example, how rounding from the Babylonian measurement in degrees, prior to conversion to days, moves the last-place fraction to the desired 20. It is all beautifully done. But it is the work of John Britton deriving a number after the fact, not the work of a Babylonian astronomer. Furthermore, an equally complete and compelling case, mathematically speaking, is made by Dennis Rawlins that the result was developed by Aristarchus, and not the Babylonians. Indeed, given the more sophisticated mathematics available to Aristarchus, one might argue that he is a more likely candidate.

Ultimately, we must admit that we have only conjectures. Most investigators today back a Babylonian source. But it could have come from the Greeks. And, unlikely as it is, it could even have been influenced by the Hebrews.

Modes of interpolation.
My previous conclusion that the source of the Hebrew month remains unresolved includes the possibility of an interpolation with information from the Almagest. It is simply not possible to rule this possibility out. But I would argue that those who insist that this is what happened have an obligation to show how it could happen. Absent such an analysis from those who argue for an interpolation, I attempted to do it myself. The result appears alongside this paper (section II), under the title *Hebrew Month: Information from Almagest*. I believe that the result does not yield a credible scenario. But the reader should make an independent judgment on this matter. In addition, the historical material presented will help place the issue in better perspective.

Concluding remarks.
Many of those who believe in the Hebrew Bible insist that the "HM" value came from a special Source and is not dependent on the Babylonian or Greek value. The "coincidence" that the two values agree is not a coincidence. Rather, it was arranged that way by Divine decree. Either the two results evolved independently or BM was influenced by the (allegedly) older HM. Those who do not accept the Hebrew Bible cannot accept such reasoning absent irrefutable proof. But no such proof is possible. Rather, my intent has been to show that an ancient tradition for the Hebrew month duration, and especially the time measure of 1080 parts to the hour, is not unreasonable and certainly not impossible.

The fact is, we simply do not know enough to reach absolute conclusions. Hence, those who insist that the Hebrew Bible is a book written by human authors need not accept an ancient tradition. The essential part of this essay is to argue that those who do accept the validity of the Hebrew Bible have an equally logical basis to accept an ancient tradition respecting the timing of the Hebrew month.

There are suggestions, but there is no proof of any sort, that the Hebrew month duration was borrowed from the Babylonians. On the contrary, there is ample, though minority, scientific support for the idea that it was the Babylonians who borrowed this number. I have no scientific evidence to support a Hebrew source. But given all of the foregoing, it would not be unreasonable to move this hypothesis from the impossible to the unlikely. Furthermore, I would argue that the suggestion that the Hebrews borrowed this number from the Babylonians has been shown to be far from the certainty that most espouse. I assert that science and the Bible do not directly disagree, leaving ample room for an accommodation in this, as in many other areas of inquiry.

<ins>As we go to press.</ins>
Dr. Dennis Duke has called my attention to an excellent summary and analysis of suggestions respecting the source of the heleq time unit at 1080 parts to the hour [20]. I would have incorporated this reference within the body of my paper had I known about it sooner. But this would not have changed my position or thrust of my arguments because this reference takes it as an obvious fact that the source of the Hebrew month duration is Greek-Babylonian astronomy, whereas I aim to challenge this assumption. Nevertheless, this is an important reference for background information. Similarly, useful for background purposes, is the treatise on the molad (mean lunar conjunction) by Dr. Irv Bromberg of the University of Toronto [21].

<ins>Acknowledgements.</ins>
I am indebted to Drs. Mark Geller, Christopher Walker, John Britton, Dennis Rawlins, and Hugh Thurston, each of whom provided me with references, including copies of as-yet unpublished material, and also personal comments in response to my questions about Babylonian astronomy and the well-known sexagesimal month value. I am equally grateful to Yaaqov Loewinger and Drs. Edward Reingold, Nachum Dershowitz, Sacha Stern and Noel Swerdlow for their help respecting the Hebrew calendar. I thank mathematician, Dr. Wallace Goldberg, for his comments on various ideas respecting the mathematical connection between sexagesimal-and halaqim-based time measures. None of these people is in any way responsible for my conjectures. All errors in this article, whether factual or in analysis, are solely the responsibility of the author.

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Hebrew Month: Information from Almagest?

Morris Engelson

Introduction. There are strong arguments against the idea that the source of the Hebrew month duration is either an ancient tradition or a transmission from Babylonian or Greek sources to Rabban Gamliel or his predecessors. Hence historians suggest that Rabban Gamliel (RG) did not provide the total time duration of the mean synodic month, but only said 29 days. The remainder of RG's statement was allegedly added to the Talmud at a later time based on information from Ptolemy's Almagest. I argue that this is an unwarranted conclusion absent a credible scenario for such an interpolation. I tried my utmost to create such a scenario, as discussed below, but the result is not very credible. Possibly someone else can do a better job. In the absence of anything more credible, I believe that my conclusion in my companion paper, *Source of Hebrew Month Duration: Babylonian Science or Ancient Tradition,* "that the source of the Hebrew month is unresolved" is correct.

Interpolation of the Talmud. The statement by RG, in Tractate Rosh Hashanah, page 25a, is in a Baraita. Quoting from the Introduction to Steinsaltz *The Talmud: A Reference Guide*, we note that "The Talmud accepts the contents of the Mishnah as incontrovertible facts." Not only that, but Mishnah type material (that is material from rabbis who compiled the Mishnah, known as Tannaim) that did not make it into the compiled Mishnah, but was quoted later (known as a Baraita), is also in the same category. Thus, "This special authority and importance is not accorded solely to the Mishnah, but also to other collections of statements of the Tannaim – the Tosefta, Baraitot…" Hence it would be a crime and sin for a later authority (known as Amoraim) among those who discussed the Mishnah, and whose discussions form the primary part of the Talmud, known as Gemara, to deliberately tamper with or modify a Baraita. And this would especially apply to a Baraita attributed to the authority of the Patriarch, Rabban Gamliel. This explains the statement by Solomon Gandz, which I quote in my paper that it "is extremely improbable that a younger Amora would expand or correct the Patriarch's dictum without giving either his name or reason." I would say that a deliberate interpolation is virtually impossible. We could argue, however, that the Amora thought that he was correcting an "error" in the Baraita based on the assumption that RG provided full information which was subsequently lost. Here the Amora would be "restoring" the Baraita to its "original" status and no deliberate tampering would be involved. This requires that we explain why and how this Amora invented a new formulation involving 1080 parts to the hour and reduced fractions, which are not part of the allegedly simpler form of the original Baraita. I discuss this in some detail in my paper. We would also need to provide a plausible reason why some Amora (ideally with a particular suggestion by name) would think that the original Baraita was in error.

An alternate hypothesis is that this was an accidental interpolation. An accidental interpolation is not impossible, but the disappearance of all evidence to this effect is highly improbable, and all but impossible. The fact that there is no such direct evidence is agreed by all. Thus, Stern in *Calendar and Community*, "The absence of manuscript evidence does not undermine the argument; it only suggests that the interpolation must have been made relatively early…" True. But I argue below that it could not have been made early enough to erase all traces.

An accidental interpolation could come about as follows. Someone studying this passage in a written volume notes in the margin the full value of the month's duration based on information from the Almagest. Perhaps the notes are not even in the margin but written around the text itself. The text is written by hand and the notes are written by hand. A later scribe uses this volume as the template from which to copy this volume of the Talmud. The scribe does not know or remember the original Baraita, and he mistakes the marginal notes as part of the text. Thus, the note is incorporated into the text. This particular copy happens to be used by other scribes to make other copies and many years later this version becomes the standard text. But there are difficulties with this idea.
Ptolemy's thirteen-book treatise was known originally as Mathematike Syntaxis and later as Megiste Syntaxis (The Great Compilation), till it was translated to Arabic in 827 with the name al-Magiste. Translation into Latin from the Arabic in 1175 gives us the Almagest. We know that the current version of the Baraita was already established by the early 800s; al-Khwarizmi references this information in 823/24, for example. Clearly the Amora who allegedly changed the Baraita had to be familiar with the more obscure original Megiste Syntaxis and not the well-known Arabic translation. That is not impossible, but it is not something to be taken for granted.

The primary redaction of the Babylonian Talmud was completed about the year 500 under the direction of Rabbis Ashi and Ravina, and some work continued till 550 and beyond. This is over 300 years after the Mishnah was completed around the year 200. The Mishnah, matters connected to the Mishnah such as Baraitot, and much of the Gemara was well known by heart during this time. One could not be an Amora, a Talmudic Torah Sage, unless one had memorized the Mishnah, as memory is the only way the material could be known. Indeed, even today, an accomplished Talmudic scholar will know all of the Mishnah and much of the Gemara from memory. It is difficult to accept that the content of our Baraita would have been forgotten by most of the Amoraim during this formative period between years 200-550.

We do not know when or by whom our Baraita was introduced into the discussion. It is simply introduced with the general statement *tanu rabannan*, which is translated as "the Rabbis taught." This would be the Rabbis of the Mishnah, whose authority is absolute, as previously explained. The Baraita is not questioned by anybody, which would indicate that the people involved were likely familiar with this version and certainly not familiar with some other version. The structure, forms and mode of discourse of the Gemara is uniform throughout the more than 60 books of the Talmud. It would appear that the various books were completed at substantially the same time somewhere around year 500. In any event, Ravina (late 5th century) is one of the discussants in Rosh Hashana, along with other individuals from the third, fourth and fifth centuries. Thus we can expect that the Rosh Hashanah volume, that was allegedly modified, could not have existed in written form till the year 500 or as late as 600. And even if it could have been completed earlier, other volumes were not, and all volumes are interconnected and discussed as a whole. This gives us a scant 200 to 300 years for one accidentally modified version to become the norm while all other copies disappear. In January 2004, an 800-year-old Torah scroll from the island of Rhodes was exhibited in Portland, OR where I reside. Yet we are to accept that all original copies of the relatively small Rosh Hashanah volume disappeared in a mere 200-300 years. And at the same time, the Talmudic sages of that time all suffered a memory loss respecting this particular item; they simply did not recognize that the written text does not fit what they remembered learning and memorizing from their teachers. And this includes the scribes, who are full-fledged Talmudic scholars, who likewise have a memory loss as they continued to copy the modified material.

<u>Interpolation prior to finished Talmud.</u> This leaves us with the last possibility for an interpolation. Here we assume that the interpolation was not on the finished volume of Rosh Hashanah, but only on the Baraita. We know that Gemara items were written down in private notes as discussions progressed. These were eventually compiled into the finished Talmud between years 200-500. Suppose that our Baraita is written down relatively early, say in year 230; about 30 years after completion of the Mishnah. This Baraita is written down in private notes and ignored for a while, while other matters are discussed. Let's say that someone inherits these notes some 30 years later and someone else inherits these notes, or better yet, a copy of these notes, some 30 years later still. We are now at about year 300 when the next person who inherited these notes notices that the RG statement is at variance with the latest scientific information, which he knows from the Almagest. [How he knows this from the then obscure Greek volume is a matter that I will ignore here.] He assumes that this is an error of omission by the previous copyist of the notes (on the assumption that RG would have given the complete value) and this person makes a "correction" to restore the full information into these notes.

The previous is a general outline of how it might have happened. Next I will present a scenario involving real people to see if the idea can be made to work. I emphasize that there is not a shred of evidence that this happened, or that the individuals I name below had anything to do with this. I am simply constructing a hypothetical scenario to see if it can be made to work.

One problem that we have is explaining how the relatively minor and obscure information about the synodic month made its way from the Greek version of Almagest to the Amoraim. Who would have the interest to search through the 13-volume Megiste Syntaxis, assuming our Amora somehow heard of this work, for material of interest in matters of Torah? The first name that pops out is Shmuel bar Abba (180-257). He was a superb astronomer hence the appellation Yarchinai (yerach=month). Shmuel would certainly have spent the time and effort to study the Almagest, had he known of its existence. And he would be very interested in the time duration of the mean synodic month given in this work. However, we cannot conjecture that he would modify the Baraita because he, of all people, would clearly understand the incongruity of using the mean synodic time between conjunctions as an argument that RG might make for disallowing the witnesses. Hence someone else, with less astronomical knowledge, had to modify the Baraita. But Shmuel is a good candidate for someone to get the information out of the Almagest. We note here for future reference that Shmuel left Babylon to study in Palestine in the academy at Tiberias under Judah the Prince, the redactor of the Mishnah. Shmuel later returned to Babylon where he was elevated to the leadership position of the Torah academy at Nahardia, upon the death of the previous leader, Shila. Shmuel was a friend and intellectual rival of Abba Areka (Rav) who also studied under Judah the Prince and who founded a rival Torah Academy in Sura.

Abba Areka (Rav) (175-247) was renowned as the greatest scholar of his time. This title passed to Shmuel upon Rav's death in 247. Among his many projects, Rav spearheaded the effort to collect and preserve Mishnaic materials that Judah the Prince did not, for whatever reason, incorporate into the redacted Mishnah. This material came to be known as Baraitot. Thus, we have a likely source of our Baraita in Rav, who is in contact with a possible source of the month duration from the Almagest, namely Shmuel. But Rav could not have made the interpolation because he predeceased Shmuel, and Shmuel would know that this is not a correct adjustment to the words of RG. The interpolation had to happen after 257 when Shmuel was no longer alive.

We conjecture that Rav has our Baraita in writing, and it states that RG said 29 days, and nothing more. We also conjecture that Rav has learned from Shmuel the full number from the Almagest. We further conjecture that Rav has written down the full value somewhere near, or possibly even on the same page as the RG Baraita. Someone inherits these notes from Rav and someone else inherits these notes later. This person does not know that the full value for the month comes from a gentile science treatise written by someone called Ptolemy. All he knows is that Rav has not given the full value in the Baraita. So he combines the two items by incorporating the full value into the Baraita. Sometime later this, interpolated, version is incorporated into the Talmud and we get the current version.

While this theory relies on conjecture and coincidence, it is not impossible. But this still leaves one matter to which I see no resolution. Why, and by what means, did this last Amora, who made the interpolation, modify the Ptolemaic result, which is in sexagesimals, into reduced fractions with a time measure of 1080 parts to the hour? Who among the various Amoraim of this time had the mathematical skill to do it? Why is it that someone sufficiently skilled in mathematics and astronomy would not recognize that the Ptolemaic value does not fit the intent to which RG is using it; namely to determine the duration from first visibility to first visibility of the moon? What compelled this person to invent this new time measure of 1080 to the hour? If this person did not invent this time measure then who did, and when? It seems to me that there are no good answers to these questions.

I note in my companion paper a beautiful analysis by Neugebauer that yields 1080 to the hour based on the equivalence of $1/12^0$ to the width of one finger. This requires that a finger-width equal 6 barley kernels (Babylonian še). But we know that Maimonides used the measure of 7 barley kernels (se'orah) to one Talmudic finger width (etzba). See Hilchot Sefer Torah [Laws Respecting a Torah scroll] 9:10. Is this a later divergence, or could these two measures have existed side-by-side during Talmudic times? The answer is simple. The Talmudic era etzba is the thumb, while the Babylonian finger is the shu-si = ring finger. Apparently, the two measures did exist side by side, and if the Talmudic sages chose to use fingers and barleycorns in their calculations, they would have surely chosen the Hebrew version. But the situation is not that simple, because there is a reference where Maimonides is quoted as using 6 to the finger, and not 7. I will state from the outset that I lack necessary details and would be grateful if someone could supply them. Nevertheless, I believe that a coherent picture emerges from the information that we do have, and this picture argues against the Neugebauer hypothesis.

<u>Measures of a finger.</u> The barleycorn (še) is the Babylonian unit of length; other units are derived as multiples of about 1/360 meters, which is the current estimate of the še. The shu-si (finger), and more specifically the ring-finger is defined as 6 še. The uban (thumb) is 6/5 shu-si, and approximately 2 cm in length using modern units. If I understand the system correctly, the 6:1 ratio is fixed. Someone with a smaller or larger finger may not make any changes, just as we may not change the number of inches to the foot to better fit the size of our foot. One matter that puzzles me is why they would choose the ring-finger which is more difficult to use for astronomical measurements than the thumb. I would think that the thumb is more logical. But then we note that the ratio 6:1 is a desirable value in sexagesimal notation. Hence I conjecture that they made the choice on the basis of what best fits sexagesimals.

The Hebrew etzba means finger (generic usage) and index-finger in specific usage. The Talmudic etzba, however is, with rare exceptions, not the index-finger but the thumb (gudal or agudal). All references noted in this paper and the companion section I, are explicit that the etzba in question is the agudal (thumb).The etzba is an important unit of linear measurement from which other, larger, units are derived. But we do not have an accurate, highly precise value for the etzba. This is because the size of the etzba is established from Biblical verses based on volume relationships involving cubic etzba units within various volumes, such as the egg. We have two end-point values for the size of the etzba. The smaller etzba, known as the Naeh-based value, is 2 cm wide. The larger etzba, using calculations by Chason Ish, is 2.4 cm wide. I would take it as a pure coincidence that the Naeh etzba is identical in width to the Babylonian uban, at 2 cm.

Note that the barleycorn (se'orah) has nothing to do with establishing the width of the thumb. There is no defined or required ratio of one to the other. Apparently people chose different values at different times based on experimental results. The Talmudic Encyclopedia notes under the heading Agudal (thumb) three relationships between the barleycorn and thumb. We are told that "The measure of the thumb is the width of seven average barleycorn." Also, there are some "scholars who have experimentally shown that it is seven barleycorns when they lie on their sides, but when they lie on their widths, there will be only six." Also, the width of the thumb is approximately equal to two barleycorns along their length.

Usage by Maimonides suggests that the primary choice is based on 7, but 6 is possible, and even 2 is possible. But, in my opinion, none of this has any bearing on the source of the heleq. The critical point is that there is not a fixed definitional relationship and neither is there a Biblical tradition. The Talmudic sages were not interested in establishing aesthetically pleasing ratios, whether in sexagesimals or decimals. Their interest was to establish what the Torah required of them. It would be entirely surprising if they were to define a critical unit of time, used for sacred purposes, on the basis of a measurement system for which there is no Torah-based reason and using astronomical relationships which are not connected to their primary purpose. I assert that the Neugebauer hypothesis makes perfect sense for the Babylonians to have invented, not the Hebrews.

Conclusion. Given all the above, we ask whether an interpolation using information from the Almagest is possible. The answer is that it is not absolutely impossible hence the word "possible" might be used. But it is highly, indeed, very highly unlikely. This is one reason why I conclude in my paper that the source of the Hebrew month duration remains unresolved. We have three possibilities: a transmission from Babylonian or Greek science to RG or his predecessors; a later interpolation from the Almagest; or an ancient tradition.

The conversion from sexagesimals to RG-reduced fractions remains the crux of the matter. Nevertheless, there are some arguments to be made that it is not impossible. We note that the Greeks favored the use of fractions with unity numerator into the sixth century. But 2/3 had a special status in this system. Thus, from The Nature of Mathematics by Philip E. B. Jourdain, from volume 1 of *The World of Mathematics*, Simon and Schuster. A fraction was represented as"… the sum of several fractions, in each of which the numerator was unity…: the sole exception to this rule being the fraction 2/3. This remained the Greek practice until the sixth century of our era." We now note that the RG result is stated as 2/3 of an hour plus 73 parts of an hour (with 1080 parts to the hour). Suppose this is a later, summary version of something that read 2/3 of an hour plus 1/18+1/120+1/270 units. The sum of these unit numerator fractions is 73/1080. Thus, we can conjecture that the Ptolemy sexagesimal value was converted to fractions commonly used by the Greeks of the time. But we are still stuck with the question of who combined these unit numerator fractions into one value with 1080 denominator, when they did it and why? We also need to explain when and why they switched from the Ptolemaic expression to the fractional expression. I say that we cannot claim interpolation from Ptolemy till we have a plausible explanation about the switch in mathematical formulation.

Glossary of terms:

- Tanna (plural Tannaim): Talmudic scholar from Mishnaic period (20-200 CE).
- Amora (plural Amoraim): Talmudic scholar from Gemara period (200-500 CE).
- Tannaitic period consisting of six generations, 20-200 CE.
- Transition period, 200-220 CE.
- Amoraic period consisting of eight generations, 220-500 CE.
- Judah the Prince (135-219 CE). Last generation Tanna, redactor of the Mishna. A descendant of Gamliel II, he was succeeded as Patriarch by his son, Gamliel III.
- Shmuel bar Abba (180-257 CE) and Abba Areka, also known as Rav (175-247 CE), were first generation Amoraim and transitional figures from the Tannaim.
- Rabban Gamliel, Gamliel II (also known as Gamliel of Yavneh), was the Patriarch approximately 90-110 CE. The ruling about the duration of the month would have been made while he was Patriarch.
- Gamliel I, also known as the Elder, the first to use the title Rabban (Rabbi or Master), was the grandfather referenced by Gamliel II.
- Mishnah: from the root to *review,* is a compilation of oral laws to review and memorize, based on the Torah (Hebrew Bible), compiled by the Torah Sages of the Tannaitic period and completed (redacted) about 200 CE.
- Gemara: literally *study*, is a commentary on the Mishnah based on discussions of the Amoraim. The Gemara is sometimes referred to as the Talmud, in a limited sense.
- The Talmud, in a broad sense, includes both the Gemara and Mishnah.
- Baraita (plural Baraitot) consists of Mishnah period material developed by the Tannaim which for some reason was not included in the redacted Mishnah.

Ancient Declinations and Precession

Dennis W. Duke, Florida State University

In *Almagest* 7.3 Ptolemy lists the declinations of 18 stars from the time of Timocharis and Aristyllos, from the time of Hipparchus, and from his own time.[1] For six of the stars he says that the change in declination over the period of 265 years between his time and Hipparchus' time corresponds closely to the change in declination of the endpoints of various segments of the ecliptic that are 2 2/3° in length. Ptolemy uses these correspondences to claim that the sphere of the fixed stars is rotating eastward about the poles of the ecliptic 1° every 100 years, in agreement with several alternative determinations of the rate of precession that he offers nearby in the *Almagest* (and, of course, in disagreement with the correct value of 72 years per degree).

However, Ptolemy gave the positions of these segments only roughly, within signs of the zodiac, e.g. "near the middle of Taurus." If the positions are ecliptic longitudes, as most previous commentators have assumed,[2] then some are grossly inaccurate. For example, for η Ursae Majoris he puts the segment near the beginning of Libra, or 180°, while the longitude of the star at his time was actually close to the beginning of Virgo, or 150°. Manitius assumed that they were polar longitudes,[3] while Rawlins more recently speculated that they might refer to right ascensions.[4] Polar longitudes and right ascensions cannot be distinguished conclusively in this case, since Ptolemy tells us only roughly where the segments lie – at the end of Aries, near the middle of Taurus,

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[1] *Ptolemy's Almagest*, transl. by G. J. Toomer (London, 1984), p. 330 ff.

[2] J. B. J. Delambre, *Histoire de l'Astronomie Ancienne,* (1817, reprinted New York, 1965), v. 2, p. 252; H. Vogt, "Versuch einer Wiederstellung von Hipparchs Fixsternverzeichnis", *Astronomische Nachrichten*, 224 (1925), col 36; A. Pannekoek, "Ptolemy's Precession", *Vistas in Astronomy* 1 (1955) p. 73-96; R. R. Newton, "The Authenticity of Ptolemy's Eclipse and Star Data", *The Quarterly Journal of the Royal Astronomical Society,* 15 (1974) p. 107-121. A concise summary of all these is in G. Grasshoff, *The history of Ptolemy's star catalogue* (New York, 1990), p. 30-31, 59-61, 73-75, 81-83. It is somewhat curious that each of these commentators has ignored the position values Ptolemy provides in *Almagest* 7.3. For example, Pannekoek writes explicitly "He [Ptolemy]….states…that the same difference of declination is found for two points of the ecliptic situated *about the star's longitude* [italics added] at a mutual distance of 2°40′". Toomer, *op. cit.* (ref.1), p. 333, footnotes 62 and 63, at least expresses puzzlement over the discrepancies.

[3] Ptolemy, *Handbuch der Astronomie*, German tran. and annot. by K. Manitius. 2 vols., Leipzig (1912-13, reprinted 1963). See in particular vol. 2, p. 20-22 for a series of six footnotes in which Manitius computes the changes in declination of the ecliptic segments, assuming that the locations of the segments as given by Ptolemy are polar longitudes. When he can, Manitius further assumes the polar longitudes that Hipparchus gives in his *Commentary to Aratus*.

[4] D. Rawlins, "Hipparchan Precession-Math Spherical Trig Relics, Manitius' Discernment, Ancient Calculus?", *Proceedings of the XXth International Congress of History of Science*, ed. G. Simon and S. Debarat, Liege (20-26 July 1997), realized that the locations specified by Ptolemy might be right ascensions in the form customarily used by Hipparchus of sign and degree increment, and that the passage in *Almagest* 7.3 might be originally from Hipparchus.

etc. However, the fact that Ptolemy did not mention the ecliptical longitudes of the stars in question suggests that he probably did *not* analyze the changes in declination in the same way that modern commentators have, and raises the question of exactly *how* he did analyze them. The following discussion suggests one approach, admittedly speculative, to answering that question.

Let us begin by thinking about the situation as it might have appeared, not to Ptolemy, but to Hipparchus. Let us assume that Hipparchus had that list of 18 declinations from Timocharis and Aristyllos,[5] although he might have been unsure of their dates, and particularly the distinction between the dates of Timocharis (*ca.* –290) and Aristyllos (*ca.* –260)[6]. He was certainly able to measure the declinations of those same stars in his own time, since the declinations of several of them and many others appear in his *Commentary to Aratus*. Therefore let us suppose that Hipparchus knew that the declinations were changing with time, and that he wondered why. Following additional hints left us by Ptolemy, let us further suppose that he formulated the hypothesis that the sphere of the fixed stars was rotating about the pole of the ecliptic, and he needed to use the changes in declination to estimate how fast. How would he do that?

Hipparchus did not have the earlier right ascensions, so he could not simply calculate the earlier longitudes and thereby the change in longitude over the intervening years.[7] However, he might have settled upon the following alternative algorithm, which follows in style the calculations he tells us about directly in his *Commentary*:

(1) assume that if we are given a value α of right ascension, then we can calculate the point π on the ecliptic that has the same right ascension, and the declination μ of that point.
(2) use the known right ascension α_2 of the star in his time to compute the corresponding point π_2 on the ecliptic.
(3) compute the declination μ_2 of the point at π_2 on the ecliptic.
(4) *assume* that the change in declination of the interval on the ecliptic is the same as the known change in declination of the star, $\Delta\delta = \delta_2 - \delta_1$, and so compute the declination μ_1 of the earlier point π_1 using $\mu_1 = \mu_2 - \Delta\delta$.

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[5] This assumption is therefore distinguished from the alternate assumption that Ptolemy himself chose the list of 12 stars.

[6] I am here using the date suggested by least-squares analysis of Aristyllos' declinations: see D. Rawlins, *Isis* (1982), p.263; *DIO* 1.2, p. 124, fn. 126; *DIO 4.1,* ‡3, fn. 40. All *DIO* issues may be conveniently found at *www.dioi.org*. See also Y. Maeyama, "Ancient Stellar Observations Timocharis, Aristyllos, Hipparchus, Ptolemy: the Dates and Accuracies", *Centaurus* 27 (1984) p. 280-310.

[7] Thus we are ignoring the fact that for one of the stars, Spica (α Virginis), Ptolemy tells us explicitly in *Almagest* 7.2 that Hipparchus knew, from the analysis of lunar eclipses, the ecliptical longitude of Spica in both Timocharis' time and his own. The values Ptolemy quotes, $\lambda = 172^\circ$ and $\lambda = 174^\circ$, are fairly accurate if we assume that the lunar eclipses in question were those that occurred on –283 Mar 17 and –134 Mar 21 (the true longitudes of Spica on those dates were 172.15° and 174.21°, respectively). If Hipparchus knew those dates, he could have estimated the rate of precession as about 74 years per degree, but Ptolemy didn't report such an estimate in the *Almagest* (by either Hipparchus or himself).

(5) compute the point π_1 on the ecliptic that has declination μ_1.
(6) finally, *assume* that the change in polar longitude, $\pi_2 - \pi_1$, is a good approximation to the actual change in longitude of the star in question.

Of course, this algorithm is just one way to explain the rather terse discussion that Ptolemy gives for each pair of declinations. We do know from an explicit example in the *Commentary* that Hipparchus was familiar with similar sequences of calculations, and that he routinely computed the right ascension and declination of any point on the ecliptic, the only non-trivial steps in the algorithm, but we cannot be sure whether his computations used trigonometry (the relevant formulae are $\tan\pi = \tan\alpha / \cos\varepsilon$, where ε is the obliquity of the ecliptic, and $\sin\mu = \sin\varepsilon \sin\pi$) or an analog method such as a globe.[8] Either way, though, Hipparchus would likely know that the algorithm gives a good approximation to the change in longitude of the star, and that it was not exact. He would likely also know that the result of the algorithm is insensitive to moderate variations in the assumed input right ascensions.

When the algorithm is applied to the 18 stars in the list in *Almagest* 7.3, we get the results shown in Table 1 and Figure 1. The two stars which show no change in declination (because they are so near the solstitial points) yield, of course, no result and are omitted from Fig. 1. They might, though, lead Hipparchus to exclude from further consideration all the stars for which the change in declination is relatively small.

What might Hipparchus conclude from these results? As far as we know he had no concept of averaging, or even how to draw a chart like Fig. 1 to get a visual impression of the data. However, Hipparchus probably knew that his algorithm was yielding only an approximation to the precession constant, and he might also have been uncertain about

[8] Ptolemy tells us explicitly in *Almagest* VII.1 that Hipparchus had a globe. R. Nadal and J.-P. Brunet, "Le Commentaire d'Hipparque I. La sphère mobile", *Archive for history of exact sciences*, 29 (1984), 201-36 and "Le Commentaire d'Hipparque II. Position de 78 étoiles", *Archive for history of exact sciences*, 40 (1989), 305-54 concluded that Hipparchus plotted stars on his globe using right ascension and declination and used the globe to deduce the rising, setting, and transit times reported in the *Commentary*.

Name		Δδ	α_2	π_2	μ_2	μ_1	π_1	$\pi_2 - \pi_1$	Δt	p	
α Aql		0.00	271.6	271.5	-23.9	-23.9	271.5	0.00	165	NA	
η Tau	*	0.67	26.8	28.9	11.3	10.6	27.1	1.83	165	90.0	*
α Tau		1.00	39.5	42.0	15.7	14.7	38.9	3.14	165	52.6	
α Aur	*	0.40	42.1	44.7	16.5	16.1	43.3	1.32	165	125.1	*
γ Ori	*	0.60	53.3	55.7	19.5	18.9	53.3	2.41	165	68.3	*
α Ori		0.50	60.4	62.6	21.0	20.5	60.2	2.41	165	68.4	
α CMa		0.33	77.9	78.9	23.4	23.1	75.5	3.41	165	48.4	
α Gem		0.17	79.1	80.0	23.5	23.3	78.0	1.98	165	83.4	
β Gem		0.00	83.1	83.7	23.7	23.7	83.7	0.00	165	NA	
α Leo		-0.67	122.7	120.5	20.4	21.1	117.3	3.19	165	51.7	
α Vir	*	-0.80	174.0	173.4	2.7	3.5	171.5	1.99	165	82.8	*
η UMa	*	-0.75	184.6	185.1	-2.0	-1.3	183.2	1.86	165	88.8	*
ζ UMa		-0.75	177.3	177.0	1.2	2.0	175.1	1.86	165	88.8	
ε UMa		-0.90	166.4	165.2	5.9	6.8	162.9	2.30	165	71.7	
α Boo	*	-0.50	189.7	190.5	-4.2	-3.7	189.3	1.25	165	131.8	*
α Lib		-0.60	194.5	195.8	-6.3	-5.7	194.3	1.53	165	108.0	
β Lib		-0.80	201.6	203.4	-9.2	-8.4	201.3	2.11	165	78.1	
α Sco		-0.67	216.2	218.7	-14.7	-14.0	216.7	2.02	165	81.7	

Table 1. Results for the estimate of precession using the change in declination between the time of Timocharis and Aristyllos (assumed as –293) and Hipparchus (assumed as –128). The rows marked with * are the six stars Ptolemy analyzes in *Almagest* 7.3. All angles are in degrees. The estimated precession constant *p* is given in years per degree.

the length of time that separated him from Timocharis' and Aristyllos' declinations (as indeed we are today; for simplicity, I have used a uniform 165 years). So under the circumstances, the conclusion that Ptolemy *reports* that Hipparchus drew as a summary of all his investigations of precession, that the change in longitude is *at least* 1° per 100 years, appears to me eminently reasonable. And given the results shown, Hipparchus can certainly be excused for not finding the *correct* value of 1° per 72 years.

It is also possible that for reasons now lost Hipparchus decided to base his estimate on the same six stars that Ptolemy chooses in *Almagest* 7.3 (a roundabout way of saying that perhaps Ptolemy chose those six simply because Hipparchus had chosen them). Perhaps the distribution in declination of those six stars appealed to Hipparchus in his effort to resolve whether precession was a property only of stars near the ecliptic or included all stars. In any event, for those six stars it happens that the average estimate of the precession constant is indeed about 98 years/degree, so it would be all the more understandable how Hipparchus got his conclusion. To be fair, though, we must also

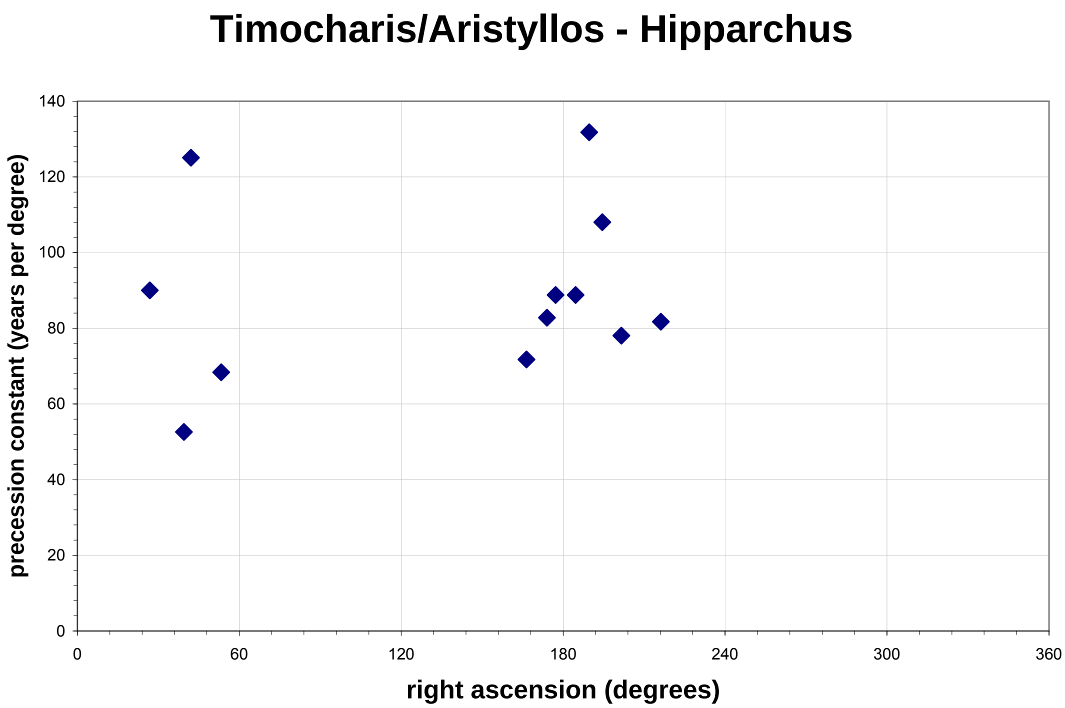

Figure 1. The precession constants for Hipparchus' stars. Four stars (α Ori, α Cma, α Gem, and α Leo) are so near a solstice that Hipparchus might have decided to exclude them from consideration. The average value for the 12 stars shown is 89 years/degree.

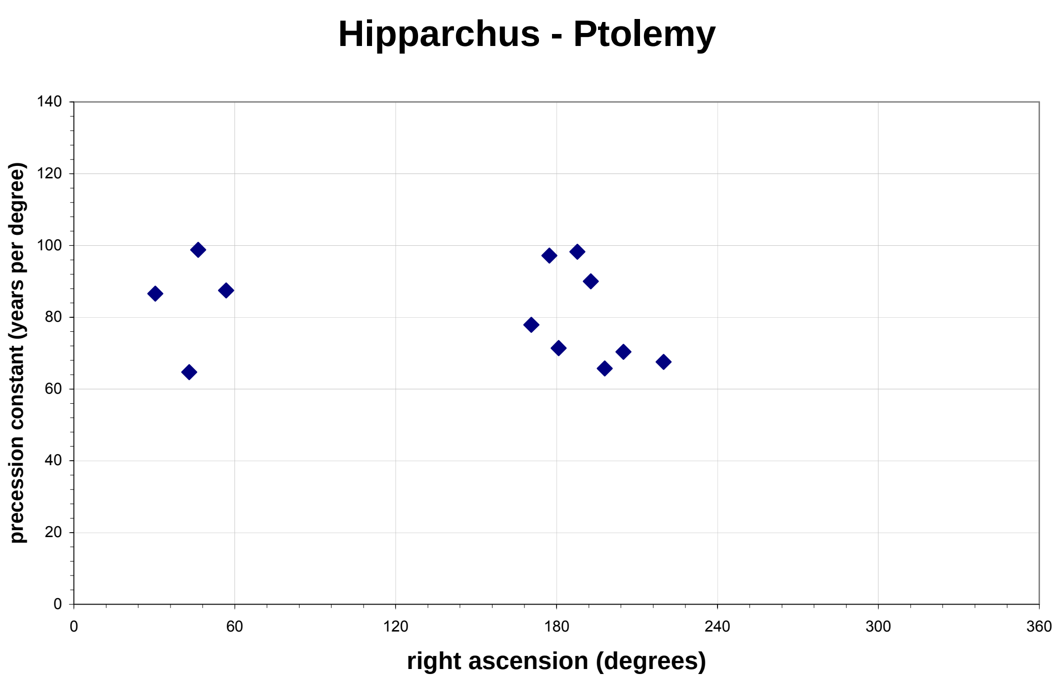

Figure 2. The precession constants for Ptolemy's stars. Four stars (α Ori, α Cma, α Gem, and α Leo) are so near a solstice that Ptolemy (or Hipparchus) might have decided to exclude them from consideration. The average value for the 12 stars shown is 81 years/degree.

Name		Δδ	α_2	π_2	μ_2	μ_1	π_1	$\pi_2 - \pi_1$	Δt	p	
α Aql		0.03	274.9	274.5	-23.8	-23.8	273.4	1.11	265	239.6	
η Tau	*	1.08	30.4	32.6	12.6	11.5	29.6	3.06	265	86.6	*
α Tau		1.25	43.0	45.6	16.8	15.5	41.5	4.10	265	64.7	
α Aur	*	0.77	46.4	48.9	17.7	17.0	46.2	2.68	265	98.8	*
γ Ori	*	0.70	56.7	59.0	20.3	19.6	56.0	3.03	265	87.5	*
α Ori		0.92	63.9	65.9	21.7	20.7	61.1	4.73	265	56.0	
α CMa		0.25	80.8	81.5	23.6	23.3	78.3	3.24	265	81.8	
α Gem		0.23	83.4	83.9	23.7	23.5	80.1	3.81	265	69.5	
β Gem		0.17	87.2	87.5	23.8	23.7	83.0	4.52	265	58.6	
α Leo		-0.83	126.5	124.1	19.6	20.4	120.5	3.62	265	73.1	
α Vir	*	-1.10	177.3	177.1	1.2	2.3	174.4	2.73	265	97.2	*
η UMa	*	-1.08	187.7	188.4	-3.4	-2.3	185.8	2.70	265	98.3	*
ζ UMa		-1.50	180.8	180.8	-0.3	1.2	177.1	3.71	265	71.4	
ε UMa		-1.35	170.6	169.7	4.1	5.5	166.3	3.40	265	77.9	
α Boo	*	-1.17	192.7	193.9	-5.6	-4.4	191.0	2.94	265	90.1	*
α Lib		-1.57	197.9	199.4	-7.7	-6.2	195.4	4.03	265	65.7	
β Lib		-1.40	204.9	206.9	-10.6	-9.2	203.2	3.77	265	70.4	
α Sco		-1.25	219.9	222.4	-15.8	-14.6	218.5	3.92	265	67.6	

Table 2. Results for the estimate of precession using the change in declination between the time of Hipparchus (assumed as –128) and Ptolemy (assumed as +137). The rows marked with * are the six stars Ptolemy analyzes in *Almagest* 7.3. All angles are in degrees. The estimated precession constant *p* is given in years per degree.

mention that if Hipparchus used a shorter time interval than what I have assumed, then his estimated value in years per degree would be correspondingly smaller, and hence closer to the correct value. But we have no information about what dates Hipparchus might have been using.

Now we move forward to Ptolemy. If the scenario sketched above is anywhere near what actually happened, then it is likely, as first suggested by Rawlins[9], that Ptolemy read about it in one of Hipparchus' now-lost books and is, in *Almagest* 7.3, simply echoing it, either in a form similar to what Hipparchus wrote, or in summary form. Indeed, applying the algorithm to the declination changes between Ptolemy's time and Hipparchus' time yields the results in Table 2 and Figure 2. For the six stars that Ptolemy singles out for analysis the algorithm yields 93 years/degree. Three of the stars yield values just under 100 years/degree, certainly close enough that Ptolemy is

[9] D. Rawlins, *op. cit.* (ref. 4).

justified in calling them 'the same' as his expected result,[10] while the other three yield somewhat smaller values, and for those three Ptolemy in fact does write that the agreement is only 'near' or 'approximate'. Thus it is quite possible that Ptolemy was simply rounding his six values, and also telling us about the three cases he was rounding the most.[11]

Neugebauer[12] and Toomer[13] have argued that the mere existence of the discussion of declinations in the *Almagest* shows that Ptolemy could not have inherited a table of stellar ecliptical coordinates from Hipparchus, otherwise why would Ptolemy have resorted to such a 'cumbersome process of comparing declinations'. One way to answer this argument is to agree with it, and assume that Ptolemy did *not* inherit a table of ecliptical coordinates, but rather a table of equatorial coordinates. Let us see what evidence we can present from Ptolemy himself to support this scenario.

First, the discussion above of the *Almagest* declination passages suggests that Ptolemy was simply duplicating and updating the same analysis that Hipparchus had published some 265 years earlier, and as we have seen there is no appearance whatsoever of ecliptical coordinates in that analysis, just as ecliptical coordinates play no role in Hipparchus' *Commentary*. Indeed, the evidence in Hipparchus' *Commentary to Aratus*[14] suggests that Hipparchus worked routinely in right ascension and declination,[15] and not, as often supposed, in some form of mixed coordinates.[16] Thus the above discussion is consistent with the idea that Hipparchus was working in equatorial coordinates.

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[10] Ptolemy is here comparing the changes in declination of the stars with the changes in declination of the corresponding ecliptic segments, but these are directly related to the estimated values of the precession constant, so I am mixing them intentionally in order to clarify the discussion.
[11] If, as Newton, *op. cit.* (ref. 2), alleged, Ptolemy fabricated the results for the six chosen stars, then it is unclear to me why he would have used the qualifications 'near' and 'approximate' to rather accurately characterize the level of agreement he found. In addition, D. Rawlins, "Ancient Geodesy: Achievement and Corruption", *Vistas in Astronomy*, 28 (1985) p. 257 shows that the accuracy of the remaining 12 declinations that Ptolemy also claims as his own exceeds the error in geographical latitude that Ptolemy claims he used for his observations in Alexandria. Perhaps the simplest consistent scenario is that Ptolemy used 18 values for his time measured by someone who knew the latitude of Alexandria.
[12] O. Neugebauer, *A history of ancient mathematical astronomy*, (3 vols., Berlin, 1975), p. 280.
[13] G. Toomer, *op. cit.* (ref. 1), p. 330, fn. 56.
[14] Hipparchus, *Commentary on the Phenomena of Aratus and Eudoxus*, trans. Roger T. Macfarlane (private communication). Until this is published, the interested reader must use Hipparchus, *In Arati et Eudoxi phaenomena commentariorium*, ed. and transl. by K. Manitius (Leipzig, 1894), which has an edited Greek text and an accompanying German translation.
[15] D. Duke, "Hipparchus' Coordinate System", *Archive for history of exact sciences,* 56 (2002) 427-433.
[16] See, for example, O. Neugebauer, *op. cit.* (ref. 12), p. 277-80; G. J. Toomer, *Hipparchus*, Dictionary of Scientific Biography 15 (1978), p. 217; J. Evans, *The History and Practice of Ancient Astronomy*, (New York, 1998), p. 103; G. Grasshoff, "Normal star observations in late Babylonian astronomical diaries", *Ancient astronomy and Celestial Divination* (1999), ed. N. Swerdlow, p 127 and footnote 23.

Second, Ptolemy writes in the final words of *Almagest* 7.2 "…their individual distances [in ecliptic longitude] from the solstitial or equinoctial points are in each case about 2 2/3° farther to the rear than those *derivable from what Hipparchus recorded* [italics added]."[17] Hence it seems that Ptolemy is well acquainted with the idea of *deriving* ecliptical results from Hipparchus' data, a required process under the proposed scenario.

Third, Ptolemy writes in *Almagest* 7.3 "…we find that it [ecliptical longitude] is practically the same as that *computed from the records of Hipparchus.*"[18] Although Ptolemy doesn't explicitly say that *he* is doing the computing, he is clearly saying that some computation has been done, presumably because it was necessary.

Fourth, in *Almagest* 7.4 Ptolemy goes to some pains to explain to us that his use of 'to the rear of' and 'in advance of' and 'to the north of' and 'to the south of' refer directly to ecliptical coordinates. However, there are several cases where his star descriptions use this terminology but are not in accord with the facts. Toomer points out several examples of this.[19] It is interesting, though, that in each case the wording *is accurate* in equatorial coordinates. So it is plausible that Ptolemy copied the star descriptions he used from some Hipparchan document that was accurate in equatorial coordinates, but occasionally forgot to change them to be uniformly accurate not for equatorial coordinates but for ecliptical coordinates. It is also possible that someone else, perhaps even Hipparchus himself, did the conversion and forgot to change some of the descriptions, but it seems most likely that the person farthest from the original data is the person most likely to make such oversights.

Finally, regarding potential star identification problems in his catalogue, Ptolemy writes in *Almagest* 7.4 "one has a ready means of identifying those stars which are described differently [by others]; this can be done immediately simply by comparing the recorded positions." This passage clearly implies that Ptolemy was not the first to use ecliptical coordinates in a star catalogue, and further, since he says the comparison may be done 'immediately', Ptolemy is probably also telling us that other star catalogues in ecliptical coordinates were readily available.[20] So by providing his new

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[17] Toomer, *op. cit.* (ref. 1), p. 329.

[18] But see D. Rawlins, *DIO* 1.2 (1991), p. 127, which vigorously disputes Toomer's choice of 'computed' as a translation of the Greek word συναγομέναισ. For example, Ptolemy, *The Almagest,* trans. R. C. Taliaferro (1952) translates the same passage as "…we find nearly the same distances contained *as were recorded and brought forward by Hipparchus*." It is also true that Manitius' German translation is more consistent with Taliaferro's version. However, a check of the on-line Liddell-Scott-Jones Greek Lexicon (which may be found at *http://perswww.kuleuven.ac.be/~p3481184/greekg/diction.htm*) clearly supports both translations, depending, of course, on the context. In the present context, Toomer's version appears to me favored.

[19] Toomer, *op. cit.* (ref. 1), in n. 110 on p. 344, n. 120 on p. 347, n. 31 and n. 34, 35 on p. 377. Toomer also includes another case in n. 117 on p. 346, but his discussion is in error in that footnote.

[20] Noel Swerdlow, private communication, 2001.

table in ecliptical coordinates, Ptolemy is presumably simply conforming to the standard presentation of his day. There is no corresponding evidence that Hipparchus ever felt such a motivation in his day, speculations about his discovery of precession notwithstanding.

Besides these passages from Ptolemy himself, it is worthwhile to recall also the historical summary of Dreyer, that "precession is never alluded to by Geminus, Kleomedes, Theon of Smyrna, Manilius, Pliny, Censorinus, Achilles, Chalcidius, Macrobius, Martianus Capella!"[21] Although 'absence of evidence is not evidence of absence', surely these omissions, coupled with Ptolemy's statement that at most Hipparchus attached only a lower bound on the rate of precession, suggest that Hipparchus himself never reached a firm and final conclusion on the phenomenon, and so might well have been content to remain in equatorial coordinates.

Together, then, these passages suggest that Ptolemy himself *might* have derived the longitudes from an Hipparchan table of equatorial coordinates. While far from conclusive, the argument at least has multiple instances of textual support. Therefore, it would be interesting to find additional evidence that either favors or disfavors this scenario, perhaps in the data of the *Almagest* star catalog itself.

Acknowledgment

I thank Dennis Rawlins for bringing his paper in ref. 4 to my attention, Alexander Jones for a discussion of the translation issue mentioned in footnote 18, and Hugh Thurston for a number of helpful suggestions and comments.

[21] J. L. E. Dreyer, *A History of Astronomy from Thales to Kepler*, (New York, 1953), p. 203.

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Paul Forman (History of Physics, Smithsonian Institution): "*DIO* is delightful!"

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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.*" (Due to this discovery, BM 55555 has repeatedly been on display at the British Museum.)

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, [and] 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."