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Ptolemy in Perspective

Use and Criticism of his Work from Antiquity to the Nineteenth Century

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Ptolemy in Perspective

Use and Criticism of his Work from Antiquity to the Nineteenth Century

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Introduction

Among the scientific authors of the ancient Greco-Roman world, none gives us such a strong impression of writing for posterity as Ptolemy. He lives in a time when learned and eloquent men seek and attain public adulation and private patronage, when the physician Galen performs dissections of pigs and sheep before the elite of Rome and when the sophist Alexander the "Clay Plato" dazzles the Athenian masses as much by his grooming and deportment as by his declamation. From this milieu Ptolemy is utterly remote. Outside of his books he is nothing; no contemporary mentions the man, and no later account of his life or person will preserve an authentic report. He addresses his books without flourish to a certain Syros, about whom we know nothing, and in them there is no personality, no reference to himself except as an observer, scholar, and theoretician, no allusion to his environment. His criticisms of other scientific practitioners are free of polemic. He habitually uses sesquipedalian verbs and writes vast, labyrinthine sentences; but his vocabulary and phrasing are repetitive, eschewing figurative language, and his tangled syntax results from the impulse to express the conditions and consequences of a thought all at once, and is worlds away from self-conscious rhetoric. Galen cannot refrain from bragging how profitable any book ascribed to himself is for the booksellers of Rome; one suspects that even in studious Alexandria Ptolemy's technical treatises are not exactly bestsellers. Nor is there anything meretricious about Ptolemy's efforts to give his science a public face: the inscription he erects at Canopus represents his cosmos as a bare list of highly precise numerical parameters, and his world map is a geometrical construction unembellished by crocodiles and pygmies.

And so Ptolemy's biography is practically complete when we have said that his full name was Klaudios Ptolemaios, and that he lived in or near Alexandria, made astronomical observations between the mid-120s and the early 140s of our era, and wrote books on scientific topics of which about a dozen have come down to us.[1] About half are astronomical, and of these, three are especially important. The *Mathematical Composition* (*Mathematikê Syntaxis*), better known since the Middle Ages as the *Almagest*, is a systematic treatise in thirteen books in which Ptolemy deduces the structure and quantitative parameters of geometrical models for the heavenly bodies from empirical evidence including specific dated observations. The *Almagest* also uses these models to derive tables for calculating the positions of the heavenly bodies on any given date, together with other phenomena such as eclipses and planetary first and last visibilities, but the tables also had a life of their own in slightly revised form as a separate publication that Ptolemy called the *Handy Tables* (*Prokheiroi Kanones*). Lastly, Ptolemy's *Planetary Hypotheses* (*Hypotheseis tôn Planômenôn*), a treatise in two books, gives a technical description, again slightly revised, of Ptolemy's celestial models, suggests how they are likely to be arranged relative to each other and what their absolute dimensions would be, and offers a physical interpretation of the geometrical models in terms of systems of revolving bodies composed of *aithêr*, the Aristotelian fifth element.

All Ptolemy's other writings have at least a glancing connection to his astronomy, but two are especially close. The *Tetrabiblos* (again a nickname—we do not know Ptolemy's own title, but a credible guess is *Apotelesmatika*, roughly "Astrological Influences") is a four-book treatise arguing for the viability of astrology as a physical science of the effects of celestial bodies on the terrestrial environment and on individuals, and on this basis Ptolemy undertakes a systematic reform of the general principles of astrological prediction. The *Geography* (*Geôgraphikê Hyphêgêsis*, "Guide to World-Cartography," also commonly known before modern times as the *Cosmography*) provides the principles and materials for the drawing of a map of the known parts of the world based on a critical analysis of astronomical measurements and other geographical reports.

Ptolemy crafted each of his major treatises to be self-standing, so that no reader would have to be familiar with other texts on the same topic to follow Ptolemy's argument on its own terms. Perhaps in part for that very reason, Ptolemy's tended to be the only works of their genres to survive into late antiquity and the medieval Byzantine and Arabic traditions. (The exception is astrology, which was handed down through many other texts in addition to the *Tetrabiblos*.) Each of Ptolemy's treatises has a distinct, sometimes complex path of subsequent reception and exploitation as a text of living scientific value, or of criticism that could lead to rejection. In the case of the *Almagest*, the presence of observation reports that, if trustworthy, might contribute to the measurement of the Earth's variable rate of rotation has kept that work from entirely subsiding into a condition of purely historical interest up to our own time.

From May 31 through June 2, 2007 the Division of the Humanities and Social Sciences at the California Institute of Technology, with generous support from the Francis Bacon Foundation, hosted a conference on uses and criticisms of Ptolemy's astronomical, geographical, and astrological works from antiquity through modern times, the focus being on the role of Ptolemy in current scientific practice and dispute. The present volume gathers most of the papers from that conference together with a new paper by the editor.

In the *Almagest* Ptolemy treats Hipparchus as his only legitimate predecessor in theoretical astronomy, making only brief and dismissively vague allusions to the astronomers of the intervening three centuries and his own time. Modern elucidation of traces of Greek astronomy in Indian sources (a field of evidence by no means exhausted, though tricky) have revealed that Ptolemy elided over a great deal of work in mathematical astronomy that had been done after Hipparchus, and the ongoing discovery of astronomical texts and tables among the papyri from Roman

Egypt is providing the historian with a still small but growing body of material relating to the immediate background against which Ptolemy's astronomy was first received. In the first paper in this volume, Anne Tihon gives us a first glimpse of a deeply interesting new papyrus manuscript containing passages from a work of unknown authorship written during the years when Ptolemy was still making the observations on which the *Almagest* was based. The topic is how to calculate the Sun's longitude in the zodiac using a set of tables that had surprising points of resemblance to Ptolemy's tables, and even more surprising differences. Among the lessons offered by this papyrus is that Ptolemy's was not the only version of solar theory descended from Hipparchus' researches, and that its correctness would not have been a straightforward matter to his better-informed contemporaries.

Alexander Jones (the editor) takes up a closely related topic, the problem of defining a frame of reference for celestial longitudes. The astrologers of Ptolemy's period did not distinguish between the tropical and the sidereal year, and used a frame of reference that was assumed to be tropical but in fact was approximately sidereal. Ptolemy's tables assume a tropical frame while attributing a precessional motion to the fixed stars. Papyri show that for two centuries after his time, Ptolemy's tables were commonly used only together with a correction that brought computed positions into a standardization of the prevalent frame of reference; this correction is identical to a formula associated by Theon of Alexandria with the doctrine of trepidation, or oscillating solstitial points. Jones attempts to account for the origin and motivation of this formula, and the cause of its later abandonment.

In the *Tetrabiblos* Ptolemy states his dissatisfaction with two methods of dividing the signs of the zodiac into Terms, that is, zones of a few degrees governed astrologically by one or another of the planets, and he recounts his discovery of a superior method in an old and damaged manuscript. Stephan Heilen offers a thorough critical treatment of the question whether Ptolemy's story is true or an audacious fabrication before exploring the reception of Ptolemy's system of Terms by astrologers from Ptolemy's day to the Seventeenth Century. This specimen of Ptolemy's reforming approach to astrology experienced a rather sad fate: transmitted in variously corrupted forms, it won little acceptance even from authors who gave lip service to Ptolemy's rationale for the system.

The *Geography* provides a set of resources for drawing maps of the world, including a catalogue of some eight thousand localities with their longitudes and latitudes. The earliest manuscript copies of the *Geography* that we have were produced more than a millennium after Ptolemy, and many of them have maps accompanying the text. The origin of these maps, and whether they descend through graphical copying from maps made by or for Ptolemy himself, have long been vigorously disputed. Relying on minute study of the manuscript maps and texts, Florian Mittenhuber makes a lucid and convincing case that the extant maps are the end of an unbroken chain of maps originating in antiquity, if not in Ptolemy's time, contrary to the belief of several scholars (including, hitherto, the editor) that they were recreated around A.D. 1300 purely on the basis of the transmitted text.

When astronomers or geographers attempted to repeat certain of Ptolemy's observations and measurements, different sorts of consequences could follow. In antiquity we know of no initiative to correct Ptolemy. Thus around A.D. 500 the Alexandrian Neoplatonist philosophers Heliodorus and Ammonius made a handful of observations of the positions of the Moon and planets, and on one occasion they noticed a discrepancy with Ptolemy's tables, but they apparently made nothing of it. On another occasion, Ammonius observed the position of Arcturus, finding it just where it was predicted from Ptolemy's star catalogue and precession rate; this observation did have the conservative effect of establishing the credibility of Ptolemy's precession theory in the eyes of Ammonius' pupil Simplicius. To help understand comparable but more complicated episodes from the Arabic astronomical tradition, Jamil Ragep draws a distinction between observations made to confirm a standing measurement, which are biased in favor of the *status quo*, and observations made to test. Ptolemy's estimate of the size of the Earth (from the *Geography*) was easy to revise once a test yielded a different figure, since there was no question of the Earth's changing size since Ptolemy's day. Tests conflicting with Ptolemy's values for the length of the tropical year and the obliquity of the ecliptic led to greater difficulties because of uncertainty about whether Ptolemy's measurements were inaccurate or the parameters had changed over time. Ragep argues that the sporadic occurrence of testing rather than confirming parameters in Arabic astronomy is a practice the historian should not take for granted but one demanding explanation.

The history of astrology in Europe during and after the Renaissance remains largely underexplored, and this is particularly true of the roles played by the *Tetrabiblos*. Darrel Rutkin presents two specimens of the uses to which Ptolemy could be put in polemic and education during the Fifteenth and Sixteenth Centuries. Pico della Mirandola professes a qualified respect for Ptolemy while twisting Ptolemy's discussion of the nature and validity of astrology into weapons in his attack on the discipline. A century later, Filippo Fantoni's lectures on the *Tetrabiblos* are the vehicle for a vigorous rebuttal of Pico, while Fantoni attempts to reconcile Ptolemy's theory that the heavenly bodies influence the sublunar world through the exertion of the elementary qualities hot, cold, moist, and dry with a more rigorous Aristotelianism.

N. M. Swerdlow takes up the threads of Ptolemy's solar and precessional theory, which have run through several of the preceding papers, with a precise account of how Tycho, Longomontanus, and Kepler tried to sort out the confusion of models and parameters that had resulted in part from the discrepancies between the *Almagest* and observations by Arabic and European astronomers. Tycho believed that the solar parameters had indeed changed since antiquity, but he deferred his solution of the long term behavior of the solar model to an ultimate comprehensive solar theory that he did not live to produce, meanwhile engaging in a dispute with Scaliger on the nature of precession that incidentally illuminates Scaliger's obstinate conviction that classical scholarship, not modern astronomy, held the key. Longomontanus' solar theory professed to be the fulfilment of Tycho's promise of a solution for all time; he proves to have been acutely critical of, though still dependent on, the observation reports in the *Almagest*, while also favoring certain parameters for their numerological perfection. Kepler, in turn, succeeded in isolating Ptolemy's reports of his own observations of equinoxes and solstices as the outliers (though the question of what caused Ptolemy's errors continued to trouble him), and thus he reintroduced a near-Ptolemaic simplicity into his solar theory by upholding the constancy of both the sidereal and tropical years, while including in his model a variable obliquity of the ecliptic.

Ptolemy's solar observations had not long been superannuated when his eclipse reports seemed to acquire a new utility from Halley's discovery of the secular acceleration of the Moon. John Steele narrates how Dunthorne, Mayer, and Lalande attempted to sift through the ancient eclipse records transmitted by Ptolemy and other sources. Dunthorne considered Ptolemy's reports to be insufficiently precise, while Mayer and Lalande suspected Ptolemy of tampering with them. But all three made some use of them, especially Dunthorne who singled out three reports that proved the phenomenon of secular acceleration and provided a basis for measuring it (though Mayer and Lalande preferred to use more recent but more precise observations for the latter purpose).

Notes

1. For detailed biography see Toomer (1975) and Jones (2008).

An Unpublished Astronomical Papyrus Contemporary with Ptolemy

Anne Tihon

Several years ago, Jean-Luc Fournet drew my attention to an unpublished astronomical papyrus which is preserved in the Library of the Institut Français d'Archéologie Orientale (IFAO) in Cairo (*P. Fouad* 267 A). This text appeared to me at once to be an interesting document, and we decided to make a joint publication. But I was far from guessing how difficult and important it would be. At first glance I believed that we had a fragment of an ordinary commentary on Ptolemy's *Handy Tables*, or a fragment of an astrological treatise explaining with an example the use of the tables. But quickly I realized that we had here something rather different, a sophisticated document which has, to our knowledge, no equivalent in the astronomical material coming from late Antiquity.

The papyrus is a folio from a codex, and we thus have two parts: Part *a* (recto) and Part *b* (verso). The papyrus is remarkable for a number of reasons: it is a long text (22 lines plus tables on the recto, 32 lines on the verso), and moreover it is an extract from an astronomical or astrological treatise. It mentions an observation of Hipparchus not known elsewhere, and uses tables different from those of Ptolemy. Finally it gives an example set in the year A.D. 130, making it contemporary with Ptolemy.

The analysis of the text proved to be especially difficult, and there remain some unsolved problems. However it seemed to us useful to make the document known and to present the results of our study, as well as the questions raised by the papyrus. This paper is a preliminary presentation of the document. I will give here a provisional translation of Part *a* with a short summary and a quick analysis of its content. The reading of Part *b* is much more difficult, and I am not able to give a provisional translation at this stage of my research; but I will summarize briefly the matters treated in this part. The text itself will be edited critically, with photos and full commentary, in a book by J. L. Fournet and myself to appear in the *Publications de l'Institut Orientaliste de Louvain* (Louvain-la-Neuve), with the collaboration of Raymond Mercier. The analysis of such a document is a difficult and perilous exercise: the text cannot be read properly without having a precise idea of its content while the content cannot be understood without a perfect reading of the text! What I present here is thus a first approach based on the securest possible readings of the text and confirmed by calculation. But many questions and details need improvement and correction.

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Part *a*: Provisional Translation[1]

1. …
2. …
3. of the Sun…
4. at 9 h of the night[2] we have established <first of all> starting with the point
5. calculating the nativity… by using 365 days $^{1}/_{4}$ less $^{1}/_{.9}$ (=$^{1}/_{309?}$) ; <secondly>, without fraction[3]
6. since only a quarter day elapsed beyond the year, and that is the average (value);
7. and thirdly starting with the solstices, based on 365 days $^{1}/_{4}$ $^{1}/_{102}$ which, conforming to the observations of Hipparchus
8. he realised; showing thus for the present years how many degrees…
9. the tropical points are displaced in the retrograde sense (εἰς τὰ προηγούμενα) since the time of Hipparchus
10. with the degrees from the beginning of the table of the Syntaxis up to
11. the observation of Hipparchus which occurred in the year 166 after the death of Alexander (= 26 June 158 B.C.)
12. 28 Pachon at …[4] hours of the day. (Thus) in the retrograde sense
13. the tropical points were displaced in longitude by …[5] degrees which one must subtract
14. from the sum of the (numbers) of the tropical longitude of the Sun as we will show in the example.
15. Therefore making (the sum) of the mean numbers of the three (motions) of the Sun
16. one obtains for each the mean motion of the concentricity of the Sun from the apogee and with the magnitude obtained
17. we enter into the second column (or table?) of the eccentricity,[6] to be subtracted up to 180°.
18. Take as example the <15th> year of Hadrian
19. Athyr 11, according to the Egyptians, Choiak 20, the night (of 20) to 21 at 9 h
20. There are for the Sun up to the death of Alexander $\overset{\Gamma}{\mathrm{M}}$ Ƶ 334 years
21. and from Alexander up to the <15th> year of Hadrian 454 years. In all one has $\overset{\Gamma}{\mathrm{M}}$ …
22. I have given the result (?) of the degrees for each year thus:

From a point		ΔΓ[7]	mean	tropical	
$\overset{\Gamma}{\mathrm{M}}$	240; 0, 0	$\overset{\Gamma}{\mathrm{M}}$	.; . 0	$\overset{\Gamma}{\mathrm{M}}$	264[8]
Ƶ	8; 0, 0	Ƶ	.4 . 0	Ƶ	97
775	161; 36, 0	775	<1>69; 2, 20	775	171
13	356; 40, 19	13	356; 47, 48	13	356
Choiak	88; 42, 14	Choiak	88;4<2>, 22	Choiak	88
20	18; 43; 34	20	18;<43, 37>	20	18
h 9th night	0; 51, 45	h 9th night	0;<51, 45>	h 9th night	0
= beyond the circles	<154; 33, 52>	= beyond the circles		= beyond the circles	278
…	…	…	…	…	…

Commentary on Part *a*

Summary

In broad outline one can understand the text as follows:

(ll. 1–7) It is a question of calculating a nativity, that is an astrological *thema*. Here only the position of the Sun is involved. The text gives the method, then an example. The position of the Sun is calculated in three ways. We may complete the data in the following way:

(1) απο σημιου	"starting with a point"	360° / 365 1/4 1/102 d	sidereal year
(2) ομαλος	"mean"	360° / 365 1/4 d	mean year
(3) απο τροπων	"starting with the solstices"	360° / 365 1/4 - 1/<30>9 d	tropical year

The expression απο σημιου (1) means "starting at a point." In some ancient astronomical texts the expression is used for the revolution from one point of the celestial sphere to the same point. This is in contrast to the rotation from one solstitial or equinoctial point to the same solstitial or equinoctial point. The meaning of απο σημιου is clearly the sidereal revolution. In the text however there is a transposition between (1) and (3) since the value given for (3) is that of the sidereal year. As a philologist I do not like to suppose that the text is corrupted or erroneous. But here one can see by the title of the third column that the scribe erred, and corrected the title απο σημιου to τροπικος. We will see later how the numbers have been calculated, but they are rather clear in the papyrus.

(ll. 7–14) The value given for the sidereal year, 365 1/4 + 1/102 days, is said to agree with the observations of Hipparchus. The author of the "Syntaxis" used here has shown that the tropical points were displaced retrograde since the beginning of this Syntaxis up to the observation made by Hipparchus on Pachon 28 of the year of Alexander 166 (158 B.C. June 26). The number of degrees cannot be read in the text as preserved. These values must be subtracted from the tropical longitude of the Sun (col. 3).

We therefore have here three important data: (1) the length of the sidereal year according to Hipparchus: 365 1/4 + 1/102 days; (2) the mention of a "Syntaxis" different from that of Ptolemy; (3) a new observation of Hipparchus of the summer solstice, 26 June 158 B.C.

(ll. 15–17) We have the εγκεντροτης of the Sun, i.e. the "concentricity" of the Sun. With this quantity one must enter into a second table, a table of eccentricity, but this is not entirely clear. One finds a correction that must be subtracted up to 180°.

The terminology differs from that of Ptolemy: the word εγκεντροτης, "concentric," does not appear in Ptolemy, who uses "homocentric," but is used by Theon of Smyrna who often refers to Hipparchus.[9] Thus, as it seems, the papyrus assumes a model with an eccentric, with a correction table like that of Ptolemy, but the text is not perfectly clear.[10]

(ll. 18–22) The text gives an example for 11 Athyr (Alexandrian) = 20 Choiak Egyptian at 9 h of the night, that is A.D. 130, November 8. The text of the papyrus is therefore contemporary with Ptolemy. There is a problem concerning the Alexandrian day which should be the 12th of Athyr.

The chronological sum is the following:

334	from <*x*> to the death of Alexander
+ 454	from the death of Alexander to the year <15> of Hadrian
= 788	Total

The starting point of the tables—after the sections $\overset{\Gamma}{\mathrm{M}}$ and Ƶ which will be discussed later—is 658 B.C. Thoth 1 (this date is without historical significance, but is 500 Egyptian years before the observation by Hipparchus mentioned in the text.) Then follow the tables with calculation which will be commented later. This the content of Part *a*.

Explanation

Let us consider the following figure, which I find useful for understanding and commenting on the text. We suppose that at the beginning of the era the Sun (*S*) is placed at an equinoctial or solstitial point.[11] For convenience, we take Γ_0 as the Vernal Equinox, while E_0 is the corresponding point of the sidereal sphere, and M_0 the corresponding point on the "mean motion" sphere.

The three ways of calculating the longitude of the Sun are shown in Fig. 1:

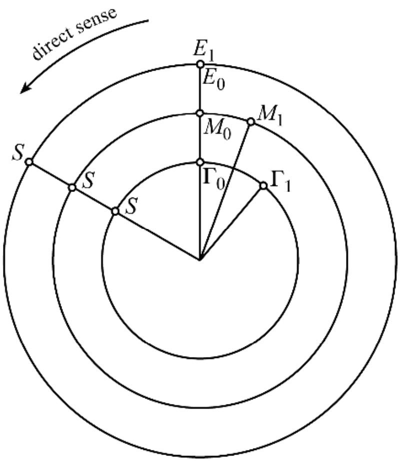

Fig. 1 The three mean motions in the papyrus

The points M_0 and Γ_0 are subject to a retrograde motion, that of Γ_0 being the precession. So at a given date the motion in longitude (over complete circles) during the periods of time given in the tables is:

$E_1 - S$ for the sidereal longitude of the Sun given in col. 1
$M_1 - S$ for the mean longitude given in col. 2
$\Gamma_1 - S$ for the tropical longitude given in col. 3

and $\Gamma_0 - \Gamma_1$ is the precession.

Dating and Chronological Divisions

As we have seen, the text gives an example, according to the Alexandrian and Egyptian calendar, using the regnal year of Hadrian; it also refers to the death of Alexander. The date is thus November 8, A.D. 130—exactly the time of Ptolemy. The chronological division is partly identical to that of the *Handy Tables*: groups of 25 years, single years, Egyptian month, day, and hour, as one can see in the tables: 775 years, 13 single years, Egyptian month (Choiak), day (20) and hours (9th of the night). The years here are elapsed years, contrary to the usage of the *Handy Tables* (where one would find for example 776 instead of 775).

The Length of the Years Used in the Tables

As we have already seen, the length of the different years used for those calculations is given in the text itself, but it can be reconstructed also from the values given in the tables. The concordance between text and calculation is a guarantee of the validity of our reconstruction. Since we have the motion of the sun during periods of 775 Egyptian years, 13 years and so on, it is possible to derive the following values:

Tab. 1	Tab. 2	Tab. 3
365;15,35d	365;15d	$365;14,46 > n < 365;14,50$d
$= 365\ {}^1/_4 + {}^1/_{102}$ d	$= 365\ {}^1/_4$ d	(probably $365\ {}^1/_4 - {}^1/_{309}$ d)
sidereal year	mean year	tropical year

The value of the tropical year remains somewhat uncertain, because the right edge of the papyrus is cut off, and the sexagesimal fractions are missing. But the value must be very close to the Ptolemaic value and the last figure: 9 (Θ) is clearly readible in the papyrus. I thus suggest reading ${}^1/_{309}$ (<T>Θ). With help of these numbers, we may reconstruct the daily motion of the Sun and the yearly motion (in 365 days) as follows:

	Tab. 1	Tab. 2	Tab. 3
daily motion	0.98560004867°	0.98562628...°	0.98563501645...°
	= 0;59,8,9,36,37°	= 0;59,8,15,16,...°	= 0;59,8,17,9,48,...°
yearly motion (365d)	359;44,38,27,45,5°	359;45,12,52,20°	359;45,24,24,37,0°
	sidereal year	mean year	tropical year

The Value of Precession

The text mentions explicitly the precession of the tropical points: between the start of this *Syntaxis* and the Summer Solstice of B.C. 158 June 26 observed by Hipparchus, the solstitial points are displaced in the retrograde direction; unfortunately the value of the displacement is not identifiable in the papyrus.

However the value of the movement of precession can be inferred with a good approximation from the data in the text. If one calculates the difference between the sidereal year of the papyrus and the tropical year one obtains an approximate value of

$$0;0,45,56,51,55^\circ \cong 0;0,46^\circ$$

which is $^{1^\circ}/_{78\mathrm{y}}$. At this stage of our research, other values (such as $^{1^\circ}/_{79\mathrm{y}}$) are still acceptable, but the next step of our analysis will decide in favour of $^{1^\circ}/_{78\mathrm{y}}$.

The Meaning of the Abbreviations $\overset{\Gamma}{\mathrm{M}}$ *and* Ƶ

In the two first lines of the tables, one finds two signs, the meaning of which is the most mysterious question posed by this document. The Greek letter M (*mu*) with a small Γ (*gamma*) written above is a common way of writing 3 *myriades* (30,000), but *prima facie* such a huge number made no sense in this kind of astronomical calculation, which seemed rather similar to any calculation performed with Ptolemy's tables. The second line marked by a Ƶ (*zêta* crossed with a bar) raised the same problem.

I had the idea that $\overset{\Gamma}{\mathrm{M}}$ could be an abbreviation for μέγας, and Ƶ for ζύγος (or ζεῦγος), both being well attested in the papyri. One would have had here an equivalent of the Sanskrit *mahayuga* (a great period), and *yuga* (a smaller period). In the end, I realized that these are indeed large periods, the magnitudes of which are given by the two symbols $\overset{\Gamma}{\mathrm{M}}$ = 30,000 and Ƶ = 7,000. So the comparison with the Sanskrit *yuga* had to be abandoned.

We know that the difference between col. 3 and col. 1 gives the precession during the given period. So, if we combine such periods with the value of the precession (${}^{1^\circ}/_{78\mathrm{y}}$), we get the following results which are quite convincing:

$$30{,}000\ (\overset{\Gamma}{\mathrm{M}})\ \text{years} / 78 = 384;36,55,\ldots^\circ - 360^\circ = 24;36,55,\ldots^\circ$$

This corresponds to the difference between col. 3 and col. 1:

$$264^\circ - 240^\circ = 24^\circ$$

which gives the value of the precession for this period. In the same way,

$$7{,}000\ (\text{Ƶ})\ \text{years}/78 = 89;44,36\ldots^\circ$$

corresponding to the difference between col. 3 and col. 1:

$$97^\circ - 8^\circ = 89^\circ$$

which is again the value of the precession for this period. If we apply the same procedure using another rate for the precession, for example, ${}^{1^\circ}/_{79\mathrm{y}}$, the results are very far from the papyrus.

These periods are consistent with the results found if we recalculate the longitude for each tables, for these periods:

	daily	$\overset{\Gamma}{\mathrm{M}}$ (30,000 × 365 days)	papyrus	Ƶ (7,000 × 365 days)	papyrus
Tab. 1	0.9856°	(29,978 × 360°)	240°	(6,995 × 360°)	8°
	= 0;59,8,9,36°	+ 240°		+ 8°	
Tab. 2	0.98562628…	(29,979 × 360°)	… ?	(6,995 × 360°)	<7>4;2(?),0°
	= 0;59,8,15,16,…°	+ 167;48,10,…°		+ 75;9,14,…°	
Tab. 3	0.98563501645	(29,979 × 360°)	264;<…>°	(6,995 × 360°)	97;<…>°
	= 0;59,8,17,9,48…°	+ 263;25,25,8,20,…°		+ 97;27,51,56,40,…°	

The Tropical Longitude of the Sun at the Date of the Example and at the Beginning of the Era

Now we have elucidated all the lines of the calculations written at the bottom of the Part *a*. We are able more or less to complete the numbers—but some problems are caused by the fact that we do not know how many sexagesimal terms must be considered. For large periods like 30,000 or 7,000 years, it makes a real difference if one neglects the fourth or the fifth sexagesimal place.

Another problem arises from the fact that one expects to find as a final result the position of the Sun at the given date, namely A.D. 130 November 8, expressed in either sidereal, mean, or tropical longitude. This is not the case. According to a modern calculation, the tropical longitude of the Sun must be around 15° of Scorpio; according to Ptolemy's tables, at 225;49,35° (mean position) and 224;58,35° (true position).[12] None of the tables directly gives such a position. But two positions of the Sun are given in Part *b*, both in Scorpio (approximately 14° and 18°) but the reading of them is not well established.

Between the end of the tables in Part *a* and the legible text of Part *b* there is quite a large a gap, and one may suppose that the last part of the calculation was given there. One may think that, as in the *Almagest*, the epoch position at the beginning of the era is not built into the tables. At the end of a calculation made with the tables of the *Almagest*, one has to add this epoch position which is written at the top of the tables. Here, we know the time of the origin: the 1st of Thoth 37,788 Egyptian years (30,000 + 7,000 + 788) before the date of the example (A.D. 130), but we do not know the epoch position of the Sun. However it may be reconstructed from the data of the text. We may take the beginning of the third row of the tables which correspond to the 1st of Thoth 658 B.C. (4th of February 658 B.C.). The tropical longitude of the Sun at that moment, calculated with the *Almagest*, is 309;6,42° (mean position) and 311;16,42° (true position). Since the sum of the two first rows for the tropical longitude is around 361°, the approximate mean tropical longitude of the Sun was around 308° at the beginning of the era—though one would like to have these figures recalculated with more precision. If one adds this position, the result is:

$$\sim 308° + \sim 278° = 226° \text{ or Scorpio } 16° \text{ (mean position)}$$

a result which is roughly approaching the expected tropical longitude of the Sun at the date of the example.[13]

Sidereal Longitude

A sidereal longitude implies a reference star, but no name of a star is given in the text. In our explanation, we have taken an arbitrary starting point, E_0, which was the corresponding point of Aries 0° Aries (Γ_0) on the sidereal sphere at the beginning of the era. But we do not know if that point corresponds to a special star, and we do not know which was the shift between the tropical zodiac and the "sidereal zodiac" at the date of the example.

Nevertheless we note that the distance between Γ_1 and Γ_0 (the precession), which is here about 124°, is not far from the longitude of Regulus as given in the *Handy Tables* for the year A.D. 130:

25 y = 451	122; 24
+ 3 y	+ 0; 1, 48
454 y	122; 25, 48

On the other hand, one may also remark that during the 37,500 Egyptian years counted from the beginning of the era to the observation of Hipparchus in 158 B.C., the precession at the rate of $^{1°}/_{78y}$ is

$$37{,}500/78 = 360° + 120;46,9\ldots°$$

This could explain the bizarre choice of these unusual periods of 30,000 and 7,000 years. At the starting point of the system, the unknown author of this *Syntaxis* would have assumed that Regulus and the Spring equinox are both placed at the point E_0. But this hypothesis is rather uncertain. What we can say is that in the *Handy Tables* Ptolemy uses two systems for counting the longitude of the planets: they are first calculated from Regulus (α Leonis) and then adjusted to the usual tropical longitude measured from Aries 0°.

There are other questions which need more investigation, for example, why calculate also a "mean" longitude with a year of 365 ¼ days?[14] What was the underlying construction with an "enkentros" and an "ekkentros"? There is nothing in the text which can be interpreted as a reference to an apogee, but a construction with an eccentric certainly implies fixing an apogee and a perigee.[15]

Part *b*

The second part of the text (Part *b*) is difficult to read, but the problems treated in the text are rather clear: it is a matter of correcting the time, meaning seasonal and equinoctial time, and the author uses a table of oblique ascensions for the *clima* of Alexandria. The data are close to the *Handy tables*, but not exactly the same. One must note that the ascensions are expressed in terms of zodiacal signs—in the papyrus, the sidereal circle, the tropical ecliptic, and the equator are all divided in "zodiacal signs."

After converting the seasonal time in equinoctial hours, the author corrects the position of the Sun. There are two different positions given here, Scorpio 14° <…> and 18° <…>, but the sexagesimal figures are not clearly established.[16] Moreover, it is hard to decide which one is the sidereal longitude, and which one is the tropical longitude. After adjustment of the Sun's position due to the correction of the given time, the author continues with the calculation of the declination. Obviously, his table is very close to the *Almagest* table, but not exactly the same. The text is coming to an end, as it seems, with an explanation concerning the "direction" of the Sun, how to know if the Sun is in the "ascending" or the "descending" part of the ecliptic, and in the northern or the southern part, a problem which has no great interest for a modern scientist, but which is commonly discussed in the ancient astronomical commentaries.

As a conclusion, I would like to underline the exceptional interest of this papyrus. We find here a striking evidence of the fact that many different tables and astronomical *Syntaxeis* did exist at the time of Ptolemy.[17]

Notes

1. Underlined words are suggestions for a better understanding of the text.
2. Uncertain.
3. Uncertain.
4. The number is missing.
5. Illegible.
6. In the text “concentricity”?
7 Uncertain.
8 The border of the papyrus is damaged and the sexagesimal rows have disappeared.
9. Hiller 1878, 181 lines 7–9.
10. Since this paper was written, I have succeeded in identifying in the papyrus the symbol for the apogee of the Sun; so it is clear that the text implies a model with an eccentric. But the terminology still has to be clarified.
11. This starting point has been taken arbitrarily, and the real position of the Sun at the beginning of the era will be estimated later.
12. Calculation made to 2 sexagesimal places.
13. The figures given here are rough approximations.
14. Nallino 1903, vol. 1, 40: Hipparchus autem longitudinem anni ex 365 diebus et ¼ diei tantum constare fecit.
15. As I remarked above, I am now able to identify in the papyrus a symbol which represents the apogee; doubt about an eccentric model can be eliminated (see note 10).
16. The two positions of the Sun have been identified in Cairo as Scorpio 18;30° (sidereal longitude) and Scorpio 14;20,18° (tropical longitude), but they still have to be confirmed by a new examination of the original document.
17. Postscript: Since the meeting at Caltech in June 2007, I had the opportunity to examine the papyrus in Cairo with Jean-Luc Fournet in February 2008. Thanks to his talent, almost magical, and his expertise in the reading of papyri, significant progress was made in the edition of the text, especially in Part b. As a result, changes will have to be introduced in the translation and in many points of my analysis even if the general interpretation presented here can be maintained. Points which need to be improved or modified are indicated in the footnotes. I would like to express my warm thanks to Mme L. Pantalacci, Directrice of the IFAO in Cairo, who allowed me to work on the papyrus in the Institut Français d’Archéologie Orientale.

Ancient Rejection and Adoption of Ptolemy's Frame of Reference for Longitudes

Alexander Jones

Theon and Ptolemy

Easily the most often cited passage in Theon of Alexandria's *Little Commentary on Ptolemy's Handy Tables* is the twelfth chapter, which is brief enough to quote in its entirety:[1]

> But since, following certain opinions [κατά τινας δόξας] the astrologers of old [οἱ παλαιοὶ τῶν ἀποτελεσματικῶν] want to have the solstitial points shift starting from some starting point of time in the direction of the trailing parts [i.e. eastwards in longitude] for 8 degrees, and to turn back again for the same [8 degrees]—which Ptolemy does not believe, because of the fact that without the addition arising from this kind of supplementary computation the aforesaid calculations by means of the tables agree with the empirical determinations [ταῖς καταλήψεσιν] by means of instruments, which is why we too advise not to adopt this kind of correction—nevertheless we will set out the method concerning this supplementary computation made by them.
>
> They take the 128 years before the reign of Augustus, on the hypothesis that the maximum shift of 8 degrees was in effect then, and that [the solstitial points] take this as the starting point of turning back [καὶ ἀρχὴν λαμβανόντων ὑποστρέφειν], and adding to these [128 years] the 313 years from the beginning of Augustus' reign to the beginning [of the reign] of Diocletian and the given number [of years] from Diocletian, and taking the eightieth part of the sum on the hypothesis that every 80 years they [i.e. the solstitial points] shift one degree, and subtracting the degrees resulting from the division from 8 degrees, they add the remainder, as being [the degrees] of the shift of the solstitial points [in effect] at that time, to the positions of Sun and Moon and the five planets that are obtained by means of the aforesaid calculations.

Theon's chapter is well known as the earliest testimony for "trepidation," the theory of a slow oscillating component in the positions of the equinoctial and solstitial points, and as, probably, the principal (if not the only) Greek authority behind medieval Arabic discussions of trepidation.[2] Less generally remarked on is the oddness of Theon's presentation taken in its own right and in the context of the *Little Commentary*.

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The first odd thing is that the chapter is there at all. Theon's work, though it is customarily given the title "Commentary" in modern languages, is really no more than a set of instructions, with worked examples, for the use of Ptolemy's *Handy Tables*. Outside this one chapter, Theon's instructions cover much the same ground as the much terser ones that Ptolemy provided in his own monograph, *Arrangement of and Computation by the Handy Tables*,[3] but neither in that work nor in his other surviving writings does Ptolemy so much as hint at a doctrine resembling trepidation. Theon's huge commentary on Ptolemy's *Almagest* and his *Great Commentary on the Handy Tables* say nothing on this topic; nor does Theon bring up comparable historical sidelights elsewhere in the *Little Commentary*.

Then there is the chapter's title, περὶ τροπῆς, or as some manuscripts have it, περὶ τροπῶν. The second version, using the plural of the noun τροπή, is the *lectio facilior*, seeming easy to translate: "concerning solstices." But as a description of a chapter that is not exactly about solstices, but about a certain doctrine about the solstitial *points* (τροπικὰ σημεῖα), this does not seem to be quite apt. The first version with the singular form of the noun has a somewhat better claim according to the manuscript tradition of the *Little Commentary*, but since "concerning solstice" would be unidiomatic as well as inaccurate, we are left wondering whether there can be another technical meaning of τροπή (which literally means "turning" or "reversal of direction") that fits the context. Could it mean what we call trepidation?

Our next puzzle is the identity, and more particularly the date, of the "astrologers of old." The Greek adjective, παλαιός, is often translated "ancient," but the past time to which it refers need not be very remote from the author's present (i.e., for Theon's *Little Commentary*, the 360s or 370s). Theon offers his reader conflicting signals. On the one hand a natural interpretation of his assertion that "Ptolemy does not believe" the opinions of the astrologers is that Ptolemy rejected a theory already current in his time. (Theon would have to mean a *tacit* rejection.) On the other hand, Theon presents the supplementary computation (ἐπιλογισμός) that the astrologers are said to have made as a kind of add-on to the normal calculation of planetary positions based on the *Handy Tables*, and the algorithm is framed in the expectation that the given date employs the Era Diocletian, which began with Diocletian's first regnal year in A.D. 284/285.

The theory of the astrologers is at first set out in rather vague terms. Starting at some epoch date, the solstitial (and equinoctial) points are supposed to begin shifting eastwards, that is, increasing in longitude—relative to what?—and after 8° of this motion, they are supposed to retrace the same interval in the opposite direction. (Theon does not state, though it is natural to infer, that similar cycles of back-and-forth motion of the solstitial points preceded and followed the cycle he describes *ad infinitum*.) Bridging this general statement of the theory to the algorithm that is supposed to yield a numerical correction based on the theory are the statements inserted in the algorithm as participial clauses expressing grounds of belief, here translated "on the hypothesis…." An epoch year 128 years (Egyptian civil calendar years, apparently) before the "zeroth" regnal year of Augustus, that is, 159/158 B.C., is said to have been when the shift was at its maximum of 8° and the solstitial points "turned back."[4] The rate of motion of the solstitial points is given as a constant 1° in 80 (again civil) years. The difference resulting from subtracting one-eightieth of the number of years elapsed since the epoch year from 8° is interpreted as the shift of the solstitial points in effect on the desired date. A reasonable reading of these statements as clarifications of the previously outlined theory is that in the more distant past, leading up to 159/158 B.C., the longitude of, say, the summer solstitial point increased from Cancer 0° to Cancer 8° at a rate of 1° in 80 years, whereas after this epoch year its longitude decreased at the same rate from Cancer 8° back to Cancer 0°; the whole cycle would have spanned the interval from 799/798 B.C. to A.D. 482/483.

We still have to consider the algorithm on its own terms, independent of the rationalizations that Theon has inserted. He makes it clear in the chapter's last sentence that the algorithm is supposed to yield a correction to be applied to the longitude of any heavenly body (Sun, Moon, planet, or fixed star) that has been computed by means of the *Handy Tables*, and this fact explains the placement of this chapter immediately following the last of the chapters describing how to use the longitude tables.[5] As expressed by Theon, the algorithm can only be applied to given dates between Diocletian 1 and Diocletian 199 (i.e. A.D. 284/285 through 482/483), but can trivially be adapted to work for years as far back as 129/128 B.C. merely by counting civil years directly from the epoch year. If y is the total number of civil years since the epoch, one calculates the correction

$$c = 8 - y/80 \tag{1}$$

which is to be reckoned as degrees to be added to any longitude derived from Ptolemy's tables for the year in question. It is easy to see that the algorithm is consistent with our interpretation of Theon's explanation of the trepidation model, that is, that by adding the correction c we shift the longitude of the heavenly body by the same amount in the same direction as the model says the equinoctial and solstitial points are shifted in the same year.[6] Theon does not say how the algorithm could be extended to dates before 129/128 B.C. or after A.D. 482/483—after all, he would not expect his readers to need to compute positions of heavenly bodies so far in the past or future—but one would imagine that the correction should reflect the change of direction of the equinoctial and solstitial points. Thus before 129/128 B.C. we can use the same formula, where y is now the number of years by which the given date *precedes* the epoch date, whereas for a date y years after 482/483 one just uses $c = y/80$.

But it is worth asking once more the question, relative to what? In other words, what meaning did the astrologers assign to a statement such as that in A.D. 323/324 the longitude of the summer solstitial point was (for the sake of argument) Cancer 2°? And again, if after applying the algorithm to "correct" a longitude of Saturn computed by the *Handy Tables* for a particular date, an astrologer obtained the result Gemini 23°, what was this result supposed to mean?

Ptolemy's treatment of the question of the appropriate frame of reference for celestial longitudes in the *Almagest* is straightforward and lucid.[7] In 1.8 he defines the ecliptic as the apparent path described by the motion of the Sun, and asserts that the ecliptic is a great circle, bisected by and bisecting the celestial equator.[8] The frame of reference for measuring longitudes along the ecliptic is established, almost casually, in 2.7, where Ptolemy writes,

> We shall employ the names of the zodiacal constellations [ταῖς τῶν ζῳδίων ὀνομασίαις] also for the twelfth-divisions of the inclined circle [i.e. the ecliptic] and on the hypothesis that their starting points are taken from the solstitial and equinoctial points, calling the first twelfth-division starting from the vernal equinox in the direction of the trailing parts in the motion of the totality [i.e. eastwards] "Aries," the second one "Taurus," and likewise for the ones that come next, according to the order of the twelve zodiacal constellations that has been handed down to us.

Ptolemy establishes two things here: that that for the purpose of expressing longitudes the ecliptic is considered as divided into twelve equal arcs of 30° named after the zodiacal constellations, with the degrees counted eastwards, and that these arcs (i.e. zodiacal signs, as distinct from constellations) are fixed such that Aries 0° is the vernal equinoctial point. Thus the frame of reference is strictly tropical.

The equality and nomenclature of the zodiacal signs and their alignment such that the vernal equinox is at Aries 0° rather than, say, Aries 8°, is purely conventional for Ptolemy, but the elementary principle that longitudes are to be counted relative to the equinoctial and solstitial points is a necessary part of Ptolemy's exposition in the *Almagest*. At this point in Book 2, Ptolemy has no celestial objects available for reference except the uniformly revolving celestial equator and the ecliptic, which he *assumes* is fixed relative to the equator so that the equinoctial and solstitial points revolve uniformly. Later, in 3.1, he will demonstrate that the tropical year, the interval between the Sun's successive passages of the same equinoctial or solstitial point, is constant (whether reckoned in true solar days or in uniform time units), a conclusion that effectively confirms the hypothesis of a fixed ecliptic and the appropriateness of the tropical frame of reference. In the same chapter, Ptolemy points out that it would be absurd [ἄτοπον] to define the starting point for a solar year as the Sun's passage of a planet or fixed star (which, looking forward to the precession theory of Book 7, he regards as no better than a planet for such purposes) rather than by the cardinal points of the Sun's own motion. Though he is here addressing the problem of defining a specific periodicity, the same considerations would rule out Ptolemy's accepting any of the visible heavenly bodies as a reference point for longitudes.

Hence the meaning of any longitude computed according to Ptolemy's tables, whether in the *Almagest* or in the *Handy Tables*, is unambiguous. The vernal equinoctial point is *by definition* Aries 0° for all time, and a computed longitude Sagittarius 23° means that the heavenly body is 263° east of the vernal equinoctial point.

Now Theon says nothing about frames of reference in his chapter on the trepidation theory. It goes without saying that if the vernal equinoctial point is supposed to be at Aries 8° in 159/158 B.C. but at Aries 0° in A.D. 482/483, the frame of reference for these longitudes is not tropical. But it is not at all clear whether we should think of the frame of reference (however it is to be defined) as being somehow fixed while the equinoctial points move back and forth, or whether it is really the equinoctial points that are fixed while whatever defines the new frame of reference moves back and forth. Theon's description of the theory of course implies the former. But since his correction algorithm is designed to preserve the elongations of all the heavenly bodies from the vernal equinoctial point exactly as they result from Ptolemy's tables, it follows that if the vernal equinoctial point performs a back-and-forth motion, all the heavenly bodies must be performing exactly the same slow oscillating motion in addition to their other motions, which scarcely seems plausible. To take the simplest example, according to the *Handy Tables*, reflecting Ptolemy's precession model, any fixed star's longitude increases uniformly in Ptolemy's tropical frame of reference at a rate of 1° in 100 years, or 0;0,36° per year. In the astrologers' frame of reference, however, the star's longitude would decrease uniformly, though by just 0;0,9° per year between 159/158 B.C. and A.D. 482/483, but before and after that interval the rate would be a constant increase of 0;1,21° per year.

Another way of considering the matter is in terms of time. According to Ptolemy's solar theory, based on his extensive discussion of the constancy and duration of the tropical year in *Almagest* 3.1, the Sun traverses 360° (in either mean or true motion) in Ptolemy's tropical frame of reference in 365 1/4 – 1/300 days (365;14,48 days). Hence in the frame of reference of Theon's astrologers during the interval between 159/158 B.C. and A.D. 482/483, in 365;14,48 days the Sun will traverse 360° – 1°/80 (359;59,15°). This interval is still a tropical year, because the solstitial and equinoctial points will have regressed by 1°/80.[9] The Sun's mean period of longitudinal revolution according to the astrologers' frame of reference, however, will be:

$$y_{\text{trep1}} = 365;14,48\text{ d} \times {}^{360}/_{359;59,15} \cong 365;15,33,39,\ldots\text{ d} \cong 365\ {}^{1}/_{4} + {}^{1}/_{107}\text{ d} \qquad (2)$$

On the other hand, during the intervals when the solstitial points are shifting in the opposite direction, the Sun's mean longitudinal period will be:

$$y_{\text{trep2}} = 365;14,48\text{ d} \times {}^{360}/_{360;0,45} \cong 365;14,2,20,\ldots\text{ d} \cong 365\ {}^{1}/_{4} - {}^{1}/_{62}\text{ d} \qquad (3)$$

y_{trep1} would be credible as a value for the *sidereal* year, though it is not Ptolemy's value.[10] Van der Waerden maintained that the frame of reference of the astrologers was meant to be sidereal, notwithstanding the difficulty of making sense of the changes of direction and the conflict with Ptolemy's precessional rate.[11] In the following we shall verify this interpretation and further attempt to trace the rise and fall of Theon's formula.

Longitudes and Years in the Earlier Greek Horoscopes

An interpretation of Theon's formula as a conversion from Ptolemy's tropical frame of reference to a sidereal frame of reference is strongly supported by consideration of the computed longitudes of heavenly bodies that we find in Greek documents and texts produced during the centuries immediately before and after Ptolemy. These sources include horoscopes and astronomical tables preserved on Greco-Egyptian papyri as well as astrological treatises transmitted through the medieval tradition. Systematic study of this material, including the frame of reference problem, was begun by O. Neugebauer in the mid twentieth century. The considerable expansion of the body of evidence since them justifies a new look at the problem.

In 1942 Neugebauer published an edition of two planetary almanacs written in Egyptian Demotic script, the papyrus *P. Berlin* 8279 and the Stobart Tablets, a set of wooden boards.[12] These are instances of a variety of table, now referred to as sign-entry almanacs, that was widely used in Roman Egypt and of which we now have numerous fragmentary examples ranging from the late second century B.C. to the fourth century of our era. They consist of lists of dates when each of the five planets was supposed to have crossed the boundary from one zodiacal sign to one of its neighbors. The two Demotic almanacs edited by Neugebauer remain the most extensive known sign-entry almanacs, as well as the only ones not written in Greek. Neugebauer recognized that the dates in the almanacs were generated by computation, not observation, and he found that on average the planets' crossings of sign boundaries tended to be assigned in the texts to dates on which modern theory yields a tropical longitude a small number of degrees less than that of the tropical sign boundary.[13] Hypothesizing that the cause of the differences was that the almanacs employed a sidereal frame of reference such that one of the boundaries was aligned with a particular star, he deduced that there should be a slow precessional decrease in the differences, a decrease that he believed was apparent in the data.

Nearly two decades later Neugebauer produced, in collaboration with H.B. van Hoesen, a collection and study of all the ancient Greek horoscopes then known from papyri and other documentary sources as well as from medieval astrological manuscripts.[14] Unlike the Demotic almanacs, Neugebauer's corpus of Greek horoscopes was far from homogeneous, and most of the horoscopes could not be used for investigation of the longitudes because they only specified the zodiacal sign occupied by each heavenly body without further precision. Neugebauer identified two sets that gave positions to at least degree precision and that were sufficiently numerous to permit meaningful analysis.

First, he found that in the horoscopes for fifth and early sixth century dates from medieval manuscript ("literary") sources, the longitudes tend to average about 2–3° less than modern theory tropical recomputations, with no noticeable shift taking place over an interval of several decades.[15] He suggested that part of the difference between the text and modern theory longitudes might be attributable to the "defect in the value of Ptolemy's constant of precession"; although he was unable to account in this way for about 1° worth of the difference, he was prepared to say that the frame of reference of the horoscopes was simply Ptolemy's.[16]

Secondly, he showed that the nearly thirty dated solar longitudes recorded in the astrological treatise of Vettius Valens, covering dates spanning just over a century from A.D. 54 to 157, are consistently greater than modern theory tropical longitudes, with a clear shift such that the average difference about A.D. 50 would be about 5°, and about A.D. 160 would be about 3 1/2°, evidently a sidereal frame of reference.[17] He further noted that the lunar and planetary longitudes in Vettius Valens, though more erratic and, in the case of the planets, less abundant, appear to confirm this frame of reference.

N. Kollerstrom has recently taken a different approach to examining the longitudes in the *Greek Horoscopes* material.[18] To reduce the "noise" in the data attributable to defects in the ancient planetary models, he considered only horoscopes (from all sources) in which degree-precise longitudes were preserved for at least four of the heavenly bodies (excluding Mercury), and calculated from them an average text-minus-modern value for the horoscope as a whole.[19] Kollerstrom found that, treated in this manner, the horoscopes—including the late ones—clustered around a trend line representing the difference between a sidereal and tropical frame of reference, and he further argued that this line was approximately the extrapolation of the trend line found by P. Huber for the sidereal frame of reference of Babylonian astronomical texts of the last three centuries B.C.[20]

The predominance of sidereal longitudes in the astronomy of the papyri and astrological texts, at least as late as the third century, is not in doubt. It is instructive, however, to make the comparison, not with modern theory calculations, but with the tropical longitudes generated by Ptolemy's tables.[21] Moreover, since we have very limited knowledge of the planetary theories used by the astrologers except that there could be considerable differences between their predictions and those of Ptolemy's models, we will limit consideration to solar and lunar longitudes. There undoubtedly existed many models for the Sun's motion during the Roman period, but in principle the differences in predicted longitudes based on different models ought seldom to exceed a fraction of a degree. For lunar longitudes one probably has to allow a slightly more generous tolerance, but very large discrepancies would not be expected among models calibrated with respect to eclipses.

Figure 1 shows the text-minus-Ptolemy differences for solar longitudes in horoscopes preserved in papyri and astrological texts; the corpus includes several papyri that came to light since 1959.[22] Most of the text longitudes are given as a whole number of degrees; these are marked with asymmetrical error bars to allow for the possibility that the values have been either truncated or rounded off from more precise values.[23] The solid line represents the correction to Ptolemy's tropical longitudes prescribed by Theon's formula. Most of the data points are near the line, even at the earliest period when there can be no question of use of Ptolemy's tables; this is a clear demonstration that Theon's formula effectively converts Ptolemy's longitudes to the prevailing sidereal frame of reference. The outliers exhibit no pattern, and many of them are probably to be explained as resulting from scribal or computational errors. From the first century to the first half of the fourth there are no more than one or two data points that could be interpreted as tropical longitudes, and these are just as likely to be errors.

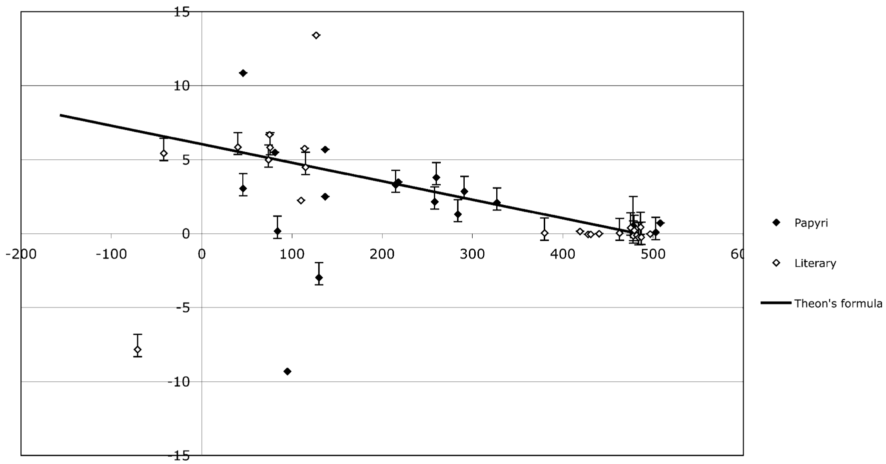

Fig. 1 Solar longitudes in Greek horoscopes: text minus Ptolemy

Text-minus-Ptolemy differences for the lunar longitudes in the horoscopes are graphed in Fig. 2. Since, however, it does not appear that the Moon's second anomaly was known before Ptolemy, we also graph in Fig. 3 the differences between the text longitudes and longitudes computed according to the simple epicyclic theory that Ptolemy presents in *Almagest* Book 4 before modifying it in Book 5 to accommodate the second anomaly. As it turns out, the scatter in the data up to the fourth century (partly due to uncertainty about the precise time of day or night of the nativities) makes it difficult to judge whether a single-anomaly or two-fold-anomaly model is the more appropriate for comparison. The later horoscopes

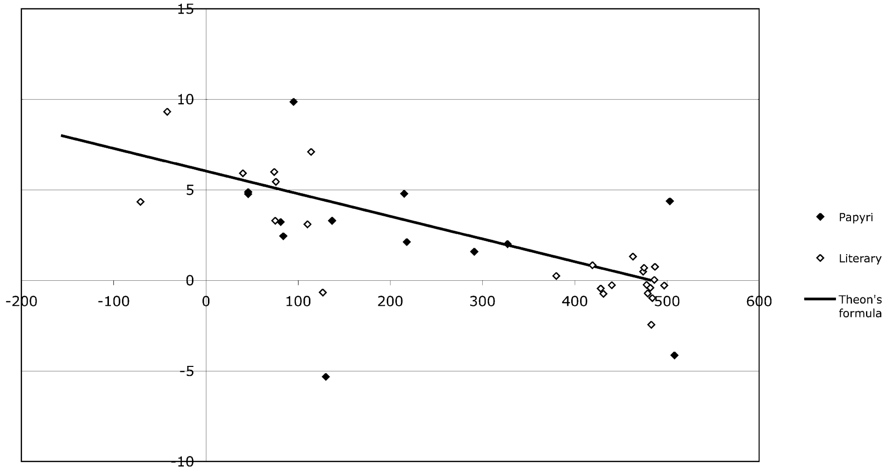

Fig. 2 Lunar longitudes in Greek horoscopes: text minus Ptolemy

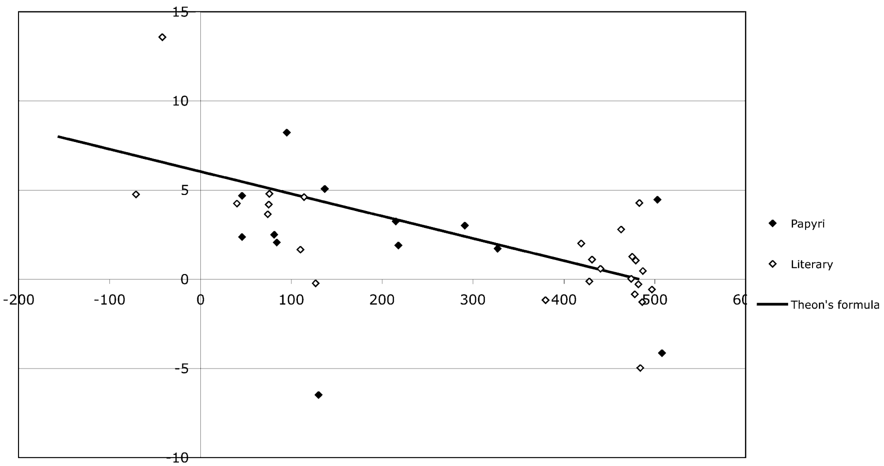

Fig. 3 Lunar longitudes in Greek horoscopes: text minus Ptolemy's simple epicyclic model

clearly conform more closely to Ptolemy's final model. Both graphs show essentially the same trend in the differences as we find for the solar longitudes.

The graphs show the ambiguity of the situation for the fifth century: since Theon's formula reaches zero towards the end of the century, the cluster of data points between A.D. 400 and 500 might appear consistent with either Ptolemy's tropical longitudes or sidereal longitudes. Since the horoscopes from before the middle of the fourth century are consistently sidereal, it would be reasonable to assume that they continued to be so afterwards. However, this is a question to which we will return in section "Ptolemy's Tables and Theon's Formula in Practice".

In Fig. 4, text-minus-Ptolemy differences are graphed for the solar longitudes in Vettius Valens Book 8 Chapters 7–8 and Book 9 Chapter 19, which are not

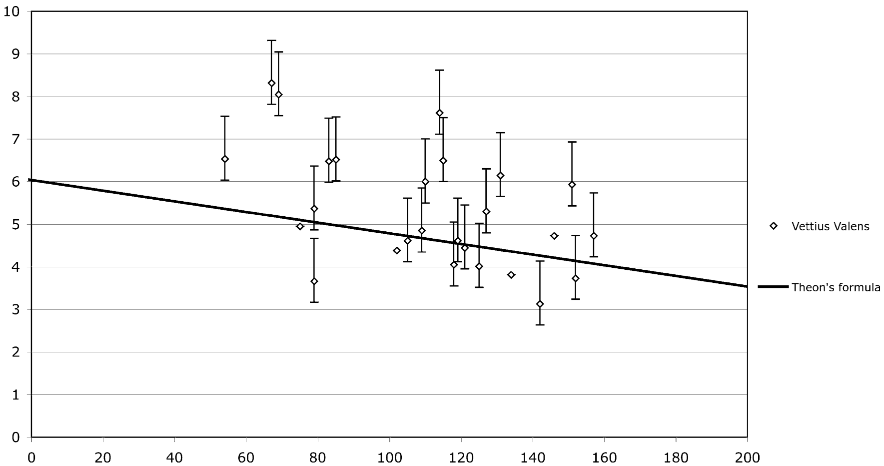

Fig. 4 Solar longitudes in Vettius Valens 8.7–8 and 9.19: text minus Ptolemy

presented in the context of complete horoscopes.[24] These are also evidently sidereal longitudes, with an obvious precessional shift, though the scatter is too great to be explained merely in terms of a solar theory different from Ptolemy's. Erratic computation, use of more than one set of tables, or astrologically motivated tampering with the data must be at work, as one can see from the case of Vettius Valens's solar longitude for July 19, A.D. 75, which in 8.7 is given as Cancer 27;43° but in 3.5 as Cancer 29;30°. Figure 5 graphs the differences between Valens' lunar longitudes and those predicted by Ptolemy's simple epicycle model.[25] Interestingly, these show less scatter than the solar longitudes, and their general proximity to Theon's formula is striking.

Two passages in Valens' treatise comment on the astronomical tables available in his time; though the passages are characteristically murky and textually corrupt, their gist is clear enough. In one (6.4), he draws a distinction between the tables on which the ignorant rely, which make no effort to avoid error (εὐχερεῖς), and the accurate tables that, though in agreement with nature, are shunned because the method of their use is contorted (διὰ τὸ σκολιὸν τῆς εἰσόδου). One should, of course, use the latter tables, though even they are only approximate; for Apollinarius himself, notwithstanding his thorough astronomical researches, concedes that his tables may err by as much as a degree or two. The other passage (9.12) makes a similar point with particular emphasis on solar tables. Various authorities have given different values for the length of the year, all slightly greater than 365 1/4 days—Valens gives several examples of year lengths according to specific individuals and nations—but certain authors of solar tables have constructed them according to a four-year cycle as if the year was exactly 365 1/4 days. After recounting

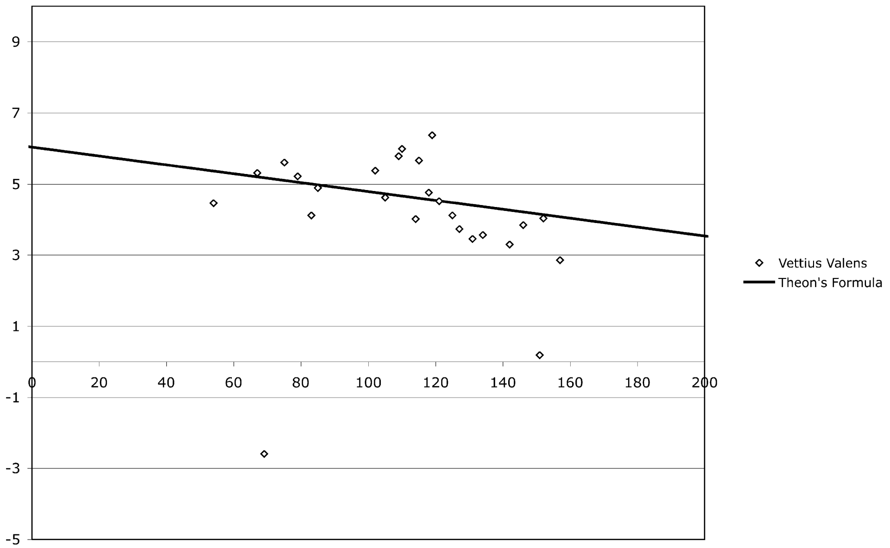

Fig. 5 Lunar longitudes in Vettius Valens 8.7–8 and 9.19: text minus Ptolemy's simple epicyclic model

his resolution to compile a better table for both the Sun and Moon, a project he gave up for lack of time, Valens tells us that he “thought it best to use Hipparchus for the Sun, Soudines and Kidynas and Apollonius [*read* Apollinarius?] for the Moon, and what is more, Apollonius [*read* Apollinarius] for both kinds, if one uses the addition of 8° [τῇ προσθέσει τῶν η μοιρῶν] as I thought best to do.”[26]

Valens’ list of year lengths is unfortunately marred by corruptions in the unique independent manuscript (*Oxford Selden* 22), as shown in the following tabulation where the first column gives the manuscript text, the second gives the readings (incorporating emendations by Kroll, Boll, and Pingree) suggested in Pingree’s text and apparatus, and the third gives a translation of the suggested readings:[27]

μεθ᾽ ὧν μὲν ὁ Ἀθηναῖος	Μέτων μὲν ὁ Ἀθηναῖος	Meton of Athens
καὶ Εὐκτήμων καὶ Φίλιππος	καὶ Εὐκτήμων καὶ Φίλιππος	and Euctemon and Philippus
τξε θ ιθ	τξε ε ιθ	365 $^{5}/_{19}$
Ἀρίσταρχος δὲ ὁ Σάμιος	Ἀρίσταρχος δὲ ὁ Σάμιος	Aristarchus of Samos
δ κξβ	τξε δ ρξβ or τξε δ σξβ	365 $^{1}/_{4}$ $^{1}/_{162}$ or 365 $^{1}/_{4}$ $^{1}/_{262}$
Χαλδαῖοι τξε δ εζ	Χαλδαῖοι τξε δ σζ	Chaldeans 365 $^{1}/_{4}$ $^{1}/_{207}$
Βαβυλώνιοι δὲ τξε δ ρμδ	Βαβυλώνιοι δὲ τξε δ ρμδ	Babylonians 365 $^{1}/_{4}$ $^{1}/_{144}$

A hopelessly garbled version of the list survives, without any of the context of Valens’ treatise, in another manuscript (*Vat. gr.* 381) under the heading κανονογράφοι (“authors of tables”). The source was certainly a lost copy of Valens, but the names and numbers are scrambled, with Apollinarius and Soudines inserted from further down in Valens’ text.[28] Here we find purporting to be year lengths:

τξε θι ε, apparently associated with (i.e. preceding the names of) Euctemon, Philippus, and Apollinarius

τξε δ ιδ or τξε δ ρδ (the reading is unclear), associated with Aristarchus of Samos (“σαβῖνος”)

τξε δ εζ, associated with “a Babylonian” (Βαβυλώνιος)

τξε δ γ ε, associated with Soudines (“Σωδίνων”)

τξε δ ρσ, with no associated name

Out of this mess, the only readings that inspire confidence are 365 5/19 days[29] for Meton, Euctemon, and Philippus, because this is the year length resulting from the Metonic period equation

$$19 \text{ years} = 235 \text{ synodic months} = 6940 \text{ days}, \tag{4}$$

and the Babylonians' 365 1/4 1/144 (i.e. 365;15,25 days), because this is, rounded to two sexagesimal places, Ptolemy's sidereal year (though we have no confirmation that it was a Babylonian parameter).[30] If we wished to play the emendation game further, it would be tempting to change the Chaldeans' certainly corrupt τξε δ εζ not to τξε δ σζ (365 1/4 1/207) but to τξε δ ρζ (365 1/4 1/107), which approximates the year length implied by Theon's formula (cf. equation 2 above) as well as a year length indirectly attested in a cuneiform text from (probably) Babylon.[31]

The textual fog does not obscure the fact that every year length cited by Valens was greater than 365 1/4 days, and this is confirmed by his speaking of an "added supplement" (προστεθεῖσαν ἐπουσίαν) to the 365 1/4 day year. It is also most interesting that when he comes to the choice of the best solar and lunar tables, Valens mentions "the addition of 8°" as an option. This obviously has some connection with the 8° of the Theon's trepidation model and conversion formula, though in Theon's formula, of course, the amount to be added to longitudes is 8° *minus* a variable quantity.

Valens fails to explain in this passage whether his supplement is to be added to the longitudes yielded by Apollinarius' tables, or whether it was already built into the tables. The meaning becomes clear if we look at certain abstruse astrological calculations of length of life illustrated in 8.8. The principle is that the length of a person's life is the oblique ascension of the ascendant point at the time of the person's birth multiplied by a fraction that is also functionally dependent on the ascendant according to a cyclical pattern. Valens assumes that one is informed of the time of birth to a precision of one seasonal hour (e.g. "third hour of night"), and he delineates an astrological technique for finding the time of birth to a precision of a fraction of an hour, but this need not concern us here. To obtain the ascendant point, Valens uses an astronomically correct algorithm, essentially the same one that one would use with Ptolemy's *Handy Tables* (for simplicity I give the form appropriate for a diurnal birth):

(1) In the table of oblique ascensions, look up the Sun's longitude and find (a) the corresponding number of degrees of the equator that rise in one seasonal hour (what in the *Handy Tables* are called ὡριαῖοι χρόνοι, "hourly time-degrees"), and (b) the Sun's oblique ascension.
(2) Multiply the hourly time-degrees by the number of seasonal hours since sunrise, and add the product to the Sun's oblique ascension.
(3) In the table of oblique ascensions, look up this sum in the column of oblique ascensions, and find the corresponding degree of the ecliptic, which is the ascendant.

However, before step (1) Valens subtracts 8° from the Sun's longitude, and after step (3) he adds 8° to the ascendant; and he refers to these operations as "the subtraction of the 8°" and "the addition of the 8°." Thus there must have been an eight degree difference between the frame of reference in which Valens expresses the horoscope—which was also the frame of reference of the solar tables that he used—and the frame of reference of the ascension table. Now Valens gives complete numerical specifications for his ascension tables in 1.7. They were structured on an arithmetical pattern extrapolated for a range of terrestrial latitudes from the schemes of ascensions built into the Babylonian lunar theories, but unlike the Babylonian schemes they were normed such that the equinoctial points are fixed at Aries 0° and Libra 0°, just as in Ptolemy's tables.[32] But since Valens subtracts 8° from any longitude before entering it in the table, and adds 8° to any longitude read off the table, his frame of reference is ostensibly a *tropical* frame of reference normed such that the equinoctial points are at 8° within their signs.

It must be stressed that though Valens knew of tables that employed a 0° norm as well as tables with the 8° norm, he nowhere hints at any awareness of precession. His treatment of the longitudinal frame of reference is essentially identical to that of the Babylonian lunar theories, in which longitudes and rates of longitudinal motion are *effectively* sidereal (e.g. the period of the Sun's longitude is slightly longer than 365 1/4 days) while the tropical and equinoctial points are assigned fixed longitudes, so that according to the internal logic of the system the frame of reference is tropical.[33] The 8° norm is in fact that of the Babylonian System B lunar theory, which was apparently the better known of the two Babylonian systems in the Greco-Roman world.[34] A certain disconnection from astronomical reality applies to Valens' frame of reference. Thus in one of his sample calculations in 8.7 he gives the Sun's longitude on A.D. 79 March 16, about 2 h before midnight, as Pisces 29°, so that the Sun should have reached the vernal equinoctial point, Aries 8° during the night of March 25/26; whereas in reality the equinox occurred during the night of March 22/23, about three days earlier. The divergence increases with time at the rate of precession, or, to be more exact, at the rate corresponding to the difference between the year length built into Valens' solar tables and the true tropical year, so that by the mid second century dates that are the latest cited in Valens' treatise his theoretical equinox would be about four days later than the true equinox.

Longitudes and Years in Astronomical Papyri

At the date of publication of *Greek Horoscopes* little direct evidence was yet available for the varieties of table that were used by the Greek astrologers to calculate longitudes of the heavenly bodies, except of course for Ptolemy's tables. In fact aside from the obscure remarks on tables in Vettius Valens, the only relevant documents known were two papyri, *P. Rylands* 1.27 and *P. Lund inv.* 35a, from which Neugebauer and van der Waerden deduced a scheme for generating series of epoch dates on which the Moon is near minimum apparent speed, with associated lunar longitudes and arguments of latitude.[35] The epoch dates follow a cycle of 3031 days (comprising 110 periods of lunar anomaly), and the longitudes over each cycle increase by a constant:

$$\Delta_{3031}\lambda = 337;31,19,7^\circ + 110 \times 360^\circ \tag{5}$$

implying a mean daily motion

$$\Delta_1\lambda = 13;10,34,51,57,\ldots^\circ \tag{6}$$

Since the Moon's mean motion in tropical longitude is 13;10,34,58,...°/d, van der Waerden deduced from the difference in the third sexagesimal place that the longitudes of the scheme in the papyri were sidereal.

It is now known that this epoch cycle formed part of a complete scheme for computing lunar longitudes and arguments of latitude on arbitrary dates, which now is designated the "Standard Lunar Scheme" since its use was remarkably widespread during the first four centuries of our era.[36] Between epoch dates, the Moon's progress was calculated as the running total of a linear zigzag function for daily motion; the ideal mean value of the function, i.e. the mean of the theoretical maximum M and minimum m, is

$$\mu = 13;10,34,52^\circ \tag{7}$$

However, because the function has an odd-numbered period (3031 days) and the tabulated values include the minimum m, there is a slight bias in the 3031 values of a complete period such that their total is not 3031μ but $3031\Delta_1\lambda$, which is in fact the minimum possible total for a cycle of values generated according to the parameters of the zigzag function.[37] The difference between μ and $\Delta_1\lambda$ is too small to be astronomically significant, and I suspect that μ was the parameter around which the Standard Scheme was originally designed.

The Standard Scheme shows every sign of having been constructed with great care, and it may be presumed that it was meant to combine with a suitable solar theory to generate accurate predictions of conjunctions and full Moons. Hence although the scheme does not directly incorporate a parameter representing the Moon's mean daily motion in elongation from the Sun (which is not dependent on the frame of reference), we may assume an accurate approximation,

$$\Delta_1\eta = 12;11,26,41^\circ \tag{8}$$

so that the Sun's implied mean daily motion is approximately

$$\Delta_1\lambda_{\text{Sun}} = 0;59,8,11^\circ \tag{9}$$

and the corresponding value for the sidereal year is

$$y_{SS} \approx 365;15,26 \text{ days} \tag{10}$$

This is close to Ptolemy's sidereal year, approximately 365;15,25 days. The three-sexagesimal-place precision of the Standard Scheme's parameters is just sufficient to make this a significant result, since if μ had been chosen as 13;10,34,51°, one would have obtained a sidereal year of approximately 365;15,33 days, close to the sidereal year from Theon's formula (equation 2 above).[38]

The epoch positions of the Standard Scheme were supposed to be valid for sunset of the evening preceding the nominal epoch date; since there was no correction for the varying length of daylight, we can consider the epoch time to be 6 h past noon, more or less for the meridian of Alexandria. Thus we can compare one of the Standard Scheme epochs from *P. Rylands* 1.27 with computation using Ptolemy's simple epicyclic lunar model as we did for Valens' lunar longitudes:

$$\begin{aligned} &\text{33 B.C., October 26, 6 h past noon} \\ &\lambda_{SS} \approx 42;19° \\ &\lambda_{Ptol} \approx 36;51° \\ &\Delta\lambda \approx 5;28° \end{aligned} \tag{11}$$

This is about a degree less than the quantity that Theon's formula yields for this date, 6;26°. Since the Standard Scheme's implied sidereal year is slightly shorter than that of Theon's formula, the Standard Scheme's frame of reference very gradually approaches that of Theon's formula as time progresses, the difference diminishing by about a tenth of a degree per century.

P. Oxy. astron. 4220 comprises four fragments from three distinct papyrus manuscripts of astronomical tables. One of two fragments belonging to a set of lunar tables structured rather like the mean motion tables of the *Handy Tables* tabulates the motion of the lunar apogee in multiples of an (unreformed) Egyptian calendar year of 365 days, from which we can estimate the motion in 365 days as approximately 40;38,35,30°±0;0,0,3°. According to Ptolemy's lunar parameters, the tropical motion of the apogee in 365 days is approximately 40;39,38,45°. If the difference was due entirely to a divergence between an assumed sidereal year and Ptolemy's tropical year, the precessional rate relative to Ptolemy's frame of reference would be about 1° in 57 years; however, we cannot rule out inaccuracy in determining the Moon's period of latitude as a component of the difference.

Nearly twenty fragments of papyrus tables of planetary epochs are known.[39] These tables are structurally similar to the Standard Scheme epoch tables for the Moon, but they were computed according to algorithms similar or (in most respects) identical to the algorithms of Babylonian planetary tables. In almost all these tables, the synodic time in days between successive occurrences of a specific phenomenon (say Mercury's first morning visibility) is related to the synodic arc in degrees traversed by the planet between the two dates according to the simple relation:

$$\Delta t = \Delta\lambda + c \tag{12}$$

where c is a constant derived from the assumed period relation for the planet

$$\Pi \text{ phenomena} = Z \text{ revolutions in longitude} = Y \text{ years} \tag{13}$$

by the equation

$$\Pi c = Yy - 360°\times Z \tag{14}$$

We can thus derive the assumed length of the year, y, from c so long as c is known to sufficient precision, which for our present purposes means to two sexagesimal fractional places.

In *P. Oxy. astron.* 4160a, a table of epochs for Jupiter, we have:

$$\begin{aligned} &c = 365;44,37 \\ &\Pi = 391 \\ &Z = 36 \\ &Y = 427, \text{ hence} \\ &y \approx 365;15,34\pm0;0,1 \text{ days} \end{aligned} \tag{15}$$

which is close to the sidereal year from Theon's formula (equation 2 above). However, in *P. Oxy. astron.* 4158, a table for Mars, we find a significantly longer sidereal year:

$$\begin{aligned} &c = 731;14,4 \\ &\Pi = 133 \\ &Z = 18 \\ &Y = 284, \text{ hence} \\ &y \approx 365;15,40,19\pm0;0,0,30 \text{ days} \end{aligned} \tag{16}$$

This is interesting, as suggesting that the adaptation of the Babylonian algorithms from the Babylonian to the Egyptian calendar, entailing a shift in the fundamental time unit from synodic months to days, was not performed consistently for all the planets. Unfortunately these are the only planetary tables currently known to have used values of c precise beyond one sexagesimal place.

The examples we have of non-Ptolemy solar tables on papyrus also use the principle of tabulating epoch positions at regular intervals, the gaps between which are to be bridged by a second "template" table.[40] In this case it was sufficient for the epoch table to list dates, precise to a fraction of a day, when the Sun was supposed to be at a particular longitude marking the start of a longitudinal revolution, while the template table gave the longitude for each whole number of days after the epoch moment. Two fragments of solar templates are extant; neither gives the longitudes to sufficient precision to allow deduction of the length of the longitudinal period, which would be the constant interval between dates in the lost accompanying epoch table. *P. Oxy. astron.* 4162 counts days from an epoch such that the Sun is at

Sagittarius 13;30°, which is also the point of least daily motion. Since this epoch longitude is exactly 8° greater than the tropical longitude of the solar perigee according to the model of Hipparchus and Ptolemy, it is obvious that the set of tables to which the template belonged was based on a frame of reference such that the solstitial and equinoctial points are supposed to be at 8° in their zodiacal signs. On the other hand, *P. Oxy. astron.* 4163 counts days from an epoch such that the Sun is at Cancer 0°, implying a tropical frame of reference like Ptolemy's.

Our only solar epoch table, *P. Oxy. astron.* 4148, lists epoch dates with fractional days expressed to a precision of three sexagesimal places. The constant interval is 365;15,33,46 days, and the sequence is extrapolated from a fundamental epoch date in the year preceding Augustus' first regnal year, according to the unreformed Egyptian calendar month Epeiph day 1;0,0,0 exactly. This date is equivalent to 30 B.C., June 27, and the epoch dates are evidently meant to be summer solstices, though the year length is appropriate for a sidereal year. We note that it is *extremely* close to the year length derived from Theon's formula (equation 2 above) and that it expresses, to three sexagesimal places, the year length implied by the Babylonian tablet BCM A1845–1982.2.[41] Conversely, the shift per 365-day Egyptian year between Ptolemy's tropical frame of reference and the frame of reference of this papyrus' epochs is:

$$365\times(360°/365;14,48 - 360°/365;15,33,46) \approx 0;0,45,4,38° \approx 1/79.86 \quad (17)$$

so that if one wanted to construct a formula like Theon's to convert longitudes from Ptolemy's frame of reference to the papyrus table's, the variable term would be excellently represented by subtracting the number of years divided by eighty.

Though the papyrus does not tell us what solar longitude corresponds to the epoch dates, it is plausible to assume that the lost accompanying template used the 8° norm. Hence at the fundamental epoch date, 30 B.C. June 27, these tables would have put the Sun at Cancer 8°. However, we do not know what time of day was represented by a fraction of zero; it could have been as early as "sunset" (6 h past noon) of the preceding evening, or as late as noon of the day. Using the evening epoch (i.e. the convention of the Standard Scheme), we find the Sun's longitude from Ptolemy's tables as Cancer 0;23°, so that the difference of frames of reference is approximately 7;37°. Using the noon epoch, the longitude from Ptolemy's tables is Cancer 1;20°, and the difference is approximately 6;40°. Theon's formula gives 6;24° for this date. On balance I think the evening epoch is more likely.

P. Oxy. astron. 4179 is an ephemeris (a calendrically structured table of daily longitudes of all the heavenly bodies) for part of A.D. 348. It is the earliest ephemeris to contain a column for solar longitudes, which are given to a precision of one fractional sexagesimal place. The lunar longitudes of this ephemeris are from the Standard Scheme, computed for the "sunset" *concluding* the day; one can assume that the solar longitudes are computed for the same time. The preserved solar longitudes are tabulated below together with the corresponding longitudes from Ptolemy's tables:

Date	Ephemeris	Ptolemy	Difference	
May 19	Taurus 27;54	Taurus 26;17	1;37	
20	28;51	27;14	1;37	
21	29;49	28;11	1;38	
22	Gemini 0;45	29;8	1;37	
23	1;42	Gemini 0;4	1;38	
24	2;3[9]	1;1	1;38	
25	3;36	1;58	1;38	(18)
26	4;33	2;55	1;38	
27	5;30	3;52	1;38	
28	6;27	4;48	1;39	
29	7;23	5;45	1;38	
30	8;21	6;42	1;39	
31	9;17	7;39	1;38	

(Incidentally, the slight upward trend of the differences suggests that the solar model might not have been identical to the Hipparchus-Ptolemy solar model.) Moreover the ephemeris indicates the times and longitudes of syzygies, and on May 29 a full Moon is indicated at 2 1/4 h of day at Sagittarius 6;54°; Ptolemy's tables put the full Moon at about 18 equinoctial hours past the preceding noon and at Sagittarius 5;16°, so the offset is again 1;38°.[42] This is in very close agreement with the offset from Theon's formula for this date, 1;42°.

From the combined evidence of the papyri and the astrological texts (especially Vettius Valens) we can see that the astrologers of the first four centuries of our era knew and used a great variety of tables to compute the longitudes of the heavenly bodies and the cardinal points of their horoscopes: tables closely adhering to the algorithms of Babylonian astronomy, tables blending Babylonian-style arithmetical methodology with elements from Hipparchus-style geometrical modelling, and Ptolemy-style tables based on tabulated uniform motions and trigonometrical analysis of geometrical models. Yet there is a broad consistency in the longitudes, whether we look at the long-term pattern exhibited by horoscopes that were computed from unidentified tables, or we look at the parameters built into the extant tables. The longitudes tend to be greater than the ostensibly tropical longitudes yielded by Ptolemy's tables; and in the case of the Sun and Moon, for which we do not expect large discrepancies arising out of different theoretical models, the amount by which the longitudes exceeds Ptolemy's tends to be within 1 or 2° of the quantity prescribed by Theon's formula, ostensibly to account for the trepidation theory.

Theon obtains the trepidating frame of reference by adding an oscillating correction to Ptolemy's frame of reference. If we take Theon's chapter at its word, the corrections generated by Theon's formula behave like a linearly decreasing function over the centuries to which the papyri and Valens belong only because the model assumes that these centuries fall between extreme points of the trepidation, and because the correction is modelled as a linear zigzag function. The tables extant in papyri tell a different story. The planetary epoch tables simply perpetuate the algorithms of Babylonian planetary tables, preserving their sidereal period relations and, at least roughly, their sidereal alignment. The solar and lunar epoch tables and the lunar mean motion in *P. Oxy. astron.* 4220 embody essentially the same continuous frame of reference directly in the constant increments. It would be preposterous to imagine that the compiling of these tables also involved adding a periodic trepidation element that happened to be zero throughout the entire span of time with which we are concerned.

On the other hand, there is nothing in the papyri to suggest that their authors or users *thought* of the frame of reference primarily as sidereal. Like Valens, they probably computed the ascendant and other cardinal points of their horoscopes on the assumption that the vernal equinoctial point was fixed at Aries 8°, which would make the frame of notionally ostensibly tropical. A distinction between sidereal and tropical longitudes or years seems to be entirely absent. Different values for the length of the year were assumed, but these were almost always greater than 365 1/4 days, and values around 365 1/4 1/107 days appear to have seen especially widespread use.[43]

Such was the general practice of the astrologers, and it cannot be emphasized too strongly that up to the present we have not seen a single complete horoscope computed for a date before the late fourth century that, taken as a whole, fits Ptolemy's tropical frame of reference better than the common sidereal frame of reference, nor a single table other than Ptolemy's that assumes a solar longitudinal period less than 365 1/4 days. One might be inclined to assert categorically that the phenomenon of precession was wholly unknown among the astrologers were it not for the recent discovery of the astonishing papyrus *P. Fouad* 267A, discussed provisionally by A. Tihon in the preceding paper in this volume.

The text in the papyrus was composed during Ptolemy's lifetime by an astrologer for astrologers, for it is part of an explanation of the method of computing a horoscope, with a worked example for A.D. November 8. The side of the papyrus on which Tihon comments preserves part of the instructions and example for the calculation of the Sun's longitude from a set of tables that appears to have been associated with a treatise (σύνταξις) by an unknown author. Among the many remarkable points about this text, we may single out the following:

(1) Using the tables, solar mean motions are computed from an epoch date up to the given date according to *three* longitudinal frames of reference, corresponding to longitudinal periods slightly less than, equal to, and slightly greater than 365 1/4 days. The shortest period is close to, but slightly longer than, Ptolemy's tropical year, while the longest is close to, but again slightly longer than, the sidereal year implied by Theon's formula.[44] The shortest is also expressly designated as tropical, though the writer also seems in the

opening lines to confuse the tropical year with the one that looks sidereal (designated “from a point”).

(2) The epoch is set in the extremely distant past, 37500 Egyptian years before the year 159/158 B.C., during which the text states that Hipparchus observed a summer solstice (on 158 B.C. June 26). Thus these tables were an instance of the sort of tables that Ptolemy disparages as claiming to be accurate for intervals many times that of the observational record.[45] The period 37500 years is a numerologically appealing figure (a product of the smallest prime factors 2, 3, and 5), apparently chosen such that the differences between the three mean motions over the entire period could be made exactly 360° and 120° respectively.[46]

(3) There is an explicit mention of a displacement of the solstitial points since the Hipparchus solstice, and associated with this, a quantity equal to this displacement that is supposed to be subtracted from the Sun’s tropical longitude as found from the tables.

Thus the tables themselves represented a complex solar theory that appears to have recognized some form of precession, though with a sidereal year significantly longer than Ptolemy’s accurate value and near the one of Theon’s formula. There is also a possible suggestion of a correction analogous to Theon’s formula, to be applied to the tropical longitudes because of a presumed shifting of the solstitial points; whether this adjustment was prescribed by the author of the tables or by someone else is not clear. There has to be a connection between the role of Hipparchus’ solstice observation in 159/158 B.C.—both structurally in the mean motion tables and in the account of the shifting of the solstitial points—and Theon’s marking out of this very year as the year of maximum displacement of the solstitial points. This solstice observation, which is *not* mentioned by Ptolemy, must have been regarded by his contemporaries as in some way pivotal.[47]

Ptolemy’s Tables and Theon’s Formula in Practice

In 1956 Neugebauer published an edition and study of a Greek astronomical papyrus, *P. Heid. inv.* 34, which, as he showed, contains on both sides of the papyrus tables of planetary and lunar longitudes at regular intervals (five days for the planets, daily for the Moon).[48] Although indications of the year number do not survive, he succeeded in dating the planetary positions to A.D. 348/349, while finding unacceptable discrepancies if the lunar positions were supposed to pertain to that year. Two years later, in a postscript to a paper on a different papyrus, Neugebauer reported his realization that the Heidelberg papyrus preserved parts of a codex bifolium such that the planetary positions were originally on a different leaf of the codex from the lunar positions; still later, he identified the year of the lunar positions as A.D. 345/346, so that there must have been several lost leaves bound within the extant bifolium in the original codex.[49] In the meantime, however, J. J. Burckhardt and van der Waerden had made a more important discovery, that the planetary longitudes of the papyrus were computed by Ptolemy's tables, but with the longitudes corrected by addition of an approximately constant supplement averaging about 1;40°, in excellent agreement with Theon's formula which prescribes an supplement of 1;40° for that year.[50]

For a long time this papyrus was the only known Greek astronomical almanac tabulating planetary positions at fixed intervals of several days, and also the only known apparent instance of Theon's formula applied in practice.[51] Now, however, we have among the Oxyrhynchus papyri a further nine fragments of planetary almanacs using the same tabulation interval of five days, covering years ranging from A.D. 217/218 to 306/307, and every one of them was computed using Ptolemy's tables and Theon's formula.[52] The same turns out to be true of another group of five almanacs from Oxyrhynchus that use the same "sign-entry" format as the much earlier demotic almanacs discussed in section "Longitudes and Years in the Earlier Greek Horoscopes" above, but that are distinguished by the provision of a tabular column giving the hour of day or night when the planet is supposed to cross the boundary between zodiacal signs; the years covered range from A.D. 218/219 to 303/304.[53] Almanacs may in some cases have been compiled a few decades after the dates that they cover, but even so, these texts attest to a consistent practice extending over at least a century. The circumstance that, with the exception of the unprovenanced *P. Heid. Inv.* 34, the almanacs attesting to use of Theon's formula are all from Oxyrhynchus is probably due only to the abundance of third and fourth century astronomical papyri from that site.

I have deliberately described the tables on which the almanacs were based as "Ptolemy's" without further specification. The five-day almanacs given planetary longitudes to a precision of minutes of arc, while it is not clear what the underlying precision of the sign-entry almanacs. Ptolemy's planetary tables in the *Almagest* and the *Handy Tables* also have a precision of minutes, and in practice the user of the tables has so much discretion in rounding intermediate results that independent computations for the same date may differ by several minutes. Mean noon according to the *Almagest* is about 32 min later than mean noon according to the *Handy Tables* due to the equation of time between their epochs, but Ptolemy took account of this difference only in the tables for the Moon's mean motions; and the almanacs do not preserve lunar longitudes computed from Ptolemy's tables.[54]

Direct use of the *Almagest* tables in their original context, dispersed through Ptolemy's treatise, is the least likely possibility. The *Almagest* was of course not entirely unknown during the first centuries after its publication (about A.D. 150). An anonymous early third-century commentator on the *Handy Tables* drew on *Almagest* Book 4 and quotes a certain Artemidorus' somewhat confused criticism of Ptolemy's observational basis for the definition of the lunar epicycle's apogee in Book 5, while the slightly later *P. Rylands* 1.27 cites Ptolemy's solar observations of A.D.139/140.[55] Yet no fragment of a copy of the *Almagest* has to date come to light among the papyri, an absence that suggests that the work was seldom found on astrologers' shelves. By contrast fragments from five papyrus manuscripts of the *Handy Tables* copied during the third, fourth, and fifth centuries are known, with the tables more or less conforming to their counterparts in the medieval tradition.[56] But we also have fragments from several collections of tables from this same period that closely resemble the *Handy Tables* but with significant differences of content or format.[57] Some of these tables betray an independent derivation from the *Almagest*, either by employing the *Almagest*'s Era Nabonassar epoch instead of the *Handy Tables*' Era Philip epoch (*P. Lond.* 1278 frs. 1–3 and 5–6) or by presenting data with systematic numerical divergences or to a greater precision than the *Handy Tables* (*P. Lond.* 1278 fr. 4, *P. Ryl.* 3.522–523, *P. Oxy. astron.* 4173). So far as we can tell, these sets of tables operated with Ptolemy's tropical frame of reference, and any of them might as well have been used to compute the planetary longitudes in the almanacs as the "normal" *Handy Tables*.

The spread of Ptolemy's tables during the first two centuries after Ptolemy, as evinced by the extant copies on papyrus and the planetary almanacs dependent on Ptolemy, seems to have had surprisingly little effect on the methods of generating horoscopes. This is unfortunately a period for which we have comparatively poor documentation; in fact not a single certifiably genuine horoscope with precise (to the degree) longitudes for a date between A.D. 150 and 350 is known to have been transmitted through the medieval tradition.[58] There are, however, five known precise horoscopes from this interval on papyrus, and two more inscribed respectively on a gem and on a gold ring, and among these seven horoscopes only the one on the gold ring, for the date A.D. 327 August 17, appears to be based on Ptolemy's tables, with the longitudes increased by approximately 1;56° according to Theon's formula.[59] We may compare the longitudes on the ring, which are given to a precision of degrees, with recomputation using the *Almagest* tables for A.D. 327 August 17, four seasonal hours before sunrise on the parallel though Rhodes and meridian through Alexandria:

	Text	Almagest (tropical)	Almagest (with Theon's formula)	
Sun	Leo 23°	Leo 20;50°	Leo 22;46°	
Moon	Capricorn 27°	Capricorn 24;21°	Capricorn 26;17°	
Saturn	Cancer 27°	Cancer 25;22°	Cancer 27;18°	
Jupiter	Taurus 1°	Aries 29;9°	Taurus 1;5°	(19)
Mars	Scorpio 21°	Scorpio 18;55°	Scorpio 20;51°	
Venus	Cancer 8°	Cancer 6;5°	Cancer 8;1°	
Mercury	Leo 8°	Leo 5;59°	Leo 7;55°	
Ascendant	Cancer 10°	Cancer 7;25°	Cancer 9;21°	

The agreement is too close to be accidental, especially if we allow for some uncertainty about the exact time and location of the nativity, which would affect the longitudes of the Moon and the ascendant. Moreover, without more precise information, we cannot tell whether the ascendant was computed before the application of Theon's formula or after, with the solstitial points assumed to be at (say) 8° in their signs. It is tempting to connect the use of Ptolemy's tables in this horoscope but not in any of the papyrus horoscopes from this time with the fact that this was a luxury object made for a rich person.

The papyrological record is remarkably bleak for the interval from A.D. 350 to 450: there are no known almanacs in any format, and of the nine known papyrus horoscopes none give precise longitudes. Between 450 and 550, we have only four papyrus horoscopes, but two of them are precise; and four fragments of ephemerides, giving day-by-day longitudes of all the heavenly bodies, survive covering parts of A.D. 465, 467, 471, and 489.[60] The ephemerides have all been shown to be derived from the *Handy Tables* (definitely not the *Almagest*, because of the equation-of-time difference in the lunar longitudes) with no application of Theon's formula.[61] The two precise horoscopes also appear to derive from Ptolemy's tables, though with—mostly modest—discrepancies that may result from sloppy computation or interpolation. One of these horoscopes falls almost exactly at the point when Theon's formula vanishes; the other is a quarter century later, and shows no sign of a systematic shift of longitudes in either direction relative to Ptolemy's frame of reference.

If the papyri only hint at the possibility of new conditions for Ptolemy's tables in the fifth century, the "literary" sources leave us in no doubt. In contrast to the second century, where the large number of surviving horoscopes is due to a single author, Vettius Valens, this period's horoscopes come from several sources.[62] The genuineness of the majority of the horoscopes in the late sources is assured by their providing an explicit date. Among those that lack this information, a few can be eliminated as probably fictitious by the impossibility of finding any historically plausible date for which an acceptable number of the heavenly bodies were anywhere near the longitudes assigned to them in the text.[63] In striking contrast to the earlier periods, every one of the genuine late horoscopes preserved in a Greek source, and almost every one surviving in Arabic texts, provides longitudes to at least a precision of degrees, and in about a third of them we have minutes as well. The following table lists the genuine horoscopes grouped according to source. Dates in brackets are deduced from the planetary positions; unbracketed dates are explicit in the text. The column headed "computation" indicates whether a horoscope's longitudes are in close enough agreement with Ptolemy's tables to establish dependence. For these comparisons I did not apply Theon's formula to the recomputed positions, and the close agreement of longitudes for the horoscopes giving precision to minutes proves that Theon's formula was not used by the ancient astrologers, even for dates in the late fourth and early fifth centuries when the formula still yielded a significant correction. Lack of precision in the time of day or corruptions in the lunar longitudes prevent us in most instances from determining whether the *Handy Tables* or the *Almagest* tables were used.

Source and horoscope no.	Date	Minutes?	Computation
Hephaestio			
L380	380 November 26	Yes	Ptolemy (*Handy Tables*)
Marinus, *Vita Procli*			
L412[64]	[412 February 7]	Yes	Ptolemy
"Additimenta" to Vettius Valens			
L419	419 July 2	Yes	Ptolemy
L431	431 January 9	Yes	Ptolemy
Par. gr. 2506 ("Rhetorius Epitome IV")[65]			
L428	428 September 28	Yes	Ptolemy
L482	[482 March 21]	Sun only	Ptolemy
Par. gr. 2506			
L463[66]	463 April 25		Ptolemy
Par. gr. 2425, "Book 5"			
L440[67]	[440 September 29]	Yes	Ptolemy
Vind. phil. gr. 108			
L478	478 August 29		Ptolemy
L479	479 July 14		Ptolemy
L483	483 July 8		Ptolemy
L484[68]	484 July 18		Ptolemy?
L486[69]	486 March 17		Ptolemy
L487	487 September 5	Some	Ptolemy
Angelicus gr. 29 (pseudo-Palchus)[70]			
L474	474 October 1		Ptolemy
L475	475 July 16		Ptolemy?
Eutocius			
L497	497 October 28	Yes	Ptolemy (*Handy Tables*)
al-Qaṣrānī, *Jāmi' al-kitāb*[71]			
Pingree VI[72]	[475 January 12]		Ptolemy
Pingree VIII[73]	[483 April 9]		Ptolemy?
Māshā'allāh, *Kitāb al-mawālīd*[74]			
K–P 3.8	439 October 18		?
K–P 3.11	442 February 7		Ptolemy
K–P 3.9	464 November 25		Ptolemy?

Thus we can see a remarkable discontinuity in the astrologers' practice. Before about A.D. 350, although Ptolemy's tables as well as almanacs computed from Ptolemy's tables had wide circulation, the astrologers appear more often to have relied on other tables for computing horoscopes; and when they did depend on

Ptolemy, they applied Theon's formula. After 350, Ptolemy is practically the exclusive resource, and Theon's formula is abandoned.

It was not quite forgotten, though. The horoscope L497, which in its authentic form in the manuscripts *Laur. plut.* 2834 and *Par. gr.* 2425 is ascribed to the famous mathematical commentator Eutocius, is exceptionally detailed and elaborated even by the standards of its time, and in the prefatory section the author writes:[75]

> The example of a horoscope [θέμα] is as follows. Let there be assumed the 214th year from the beginning of Diocletian's rule, which is the 821st from the death of Alexander of Macedon, the 1st of the month Hathyr according to the Alexandrians, at the 7th and 1/12 seasonal hour for Alexandria in Egypt. So at this time and the stated place, that is Alexandria, I calculated the positions of the stars and the [astrological] centers accurately and I set them out without τροπή, since this seems right to the divine Ptolemy.

There can be no doubt that by τροπή Eutocius means Theon's trepidation correction—and thus we have the clarification of the chapter title in the *Little Commentary*. It must have been a jargon term among the astrologers for what we have been speaking of as Theon's formula. The word is revealing, since there is no reason why a monotonically decreasing correction for precession should be called "reversal of direction"; it must refer to the alternations of the trepidation theory. Theon seems to have known what he was doing when he associated the formula with trepidation, even if the formula, as he states it, does not bring about the changes of direction.

The Rise and Fall of Trepidation in Greek Astronomy

Theon's trepidation formula can be reduced to four elements: (1) Ptolemy's tropical year of 365 1/4 – 1/300 days that defines the frame of reference for longitudes calculated from the *Handy Tables*; (2) the year of approximately 365 1/4 + 1/107 days that defines the frame of reference for longitudes corrected by the formula; (3) the epoch date, 159/158 B.C.; and (4) the 8° maximum that the formula prescribes for that epoch. This last is, as we have already seen, surely related in some way to the Babylonian lunar System B norm placing the solstitial and equinoctial points at 8° in their zodiacal signs. At least two of the other elements might indicate a connection with Hipparchus.

Ptolemy insists in *Almagest* 3.1 that his tropical year was the value endorsed by Hipparchus, and he quotes from several of Hipparchus' writings (all now lost to us) to prove his point. The information that Ptolemy retails about Hipparchus' solar theory is deliberately selective, and we have some reason to believe that Hipparchus derived more than one estimate of the tropical year by comparing widely spaced observations of summer solstices; but the quotations leave no doubt that 365 1/4 – 1/300 days was the value that Hipparchus adopted in some of his latest works, written after 128 B.C.[76]

Though most of the dated observations of Hipparchus preserved in the *Almagest* fall within the interval between 147 and 127 B.C., in the list of equinox observations that Ptolemy extracted from Hipparchus' *On the Displacement of the Solstitial and Equinoctial Points* in *Almagest* 3.1 there are three distinctly earlier autumnal equinoxes, from 162, 159, and 158 B.C., that have usually been attributed to Hipparchus.[77] The latest of these equinoxes fell precisely in the epoch year (according to the Egyptian calendar) of Theon's formula. And now, from *P. Fouad* 267A, we know that Hipparchus also observed the summer solstice in this year.

Ptolemy does not tell us what sidereal year Hipparchus assumed, but in *Almagest* 7.2 he quotes a sentence from *On the Length of the Year* in which Hipparchus calculated the westward precessional shift (in a sidereal frame of reference) of the solstitial and equinoctial points taking place in 300 years on the assumption that the rate was "not less than" 1/100 of a degree per year. Ptolemy cites this as the best evidence he has that Hipparchus agreed with his own precessional rate of 1° in 100 years, though his case is weakened by the hypothetical character of the quoted sentence and the fact that it speaks only of a lower bound. If Hipparchus was confident that the tropical year was approximately 365 1/4 – 1/300 days when he wrote *On the Length of the Year* (and retrospectively, in a later summary of his own books, he said that he had demonstrated this year length in that work), he apparently believed that the sidereal year was *at least* about 365 1/4 + 1/147 days. However, in an apparently earlier work on lunar periodicities, as we know from *Almagest* 4.2, Hipparchus verified a lunar period relation incorporating the equation:

$$126007 \text{ d } 1 \text{ h} = 345 \times 360^\circ - 7\ 1/2^\circ \text{ of solar mean motion in longitude} \qquad (20)$$

where the frame of reference is sidereal. From this we can derive a fairly precise value for the sidereal year, approximately 365;15,35,29 days ± 0;0,3 days (assuming that the time is rounded to the nearest hour and the interval of mean longitude to the nearest half degree).[78] This is fairly close to the year of approximately 365;15,33,39 days implied by Theon's formula (and also close to the sidereal year in *P. Fouad* 267A, which that papyrus speaks of as "conforming to the observations of Hipparchus").

Neugebauer argued that Hipparchus could hardly have avoided comparing this sidereal year with his tropical year, thereby deriving a precessional rate of about 1° in 77 years.[79] Pointing out the resemblance of this to the rate in Theon's formula, as well as the circumstance that the formula's epoch coincided with Hipparchus' earlier equinox observations, he further suggested that Hipparchus invented the theory of trepidation, and that Ptolemy suppressed this aspect of his solar theory. But while the loss of all Hipparchus' writings on solar theory makes Neugebauer's conjecture impossible to disprove, the trepidation model's behavior during the period following 158 B.C. seems if anything to be a contradiction of his known deductions on precession (which he made some thirty years later).

I would suggest rather that the theory and associated formula were devised at a later date, as a response to the propagation of tables that yielded longitudes of the heavenly bodies according to an ostensible tropical frame of reference based on a tropical year like Ptolemy's; from the point of view of the originators of the theory, such tables were pseudo-tropical since they rejected the underlying tropical year. The primary object was to preserve the prevailing "true" frame of reference based on a year of about 365 1/4 + 1/107 days, which was assumed to be *both* sidereal *and* tropical; that is, precession was rejected. *P. Fouad* 267A hints that a correction analogous to Theon's formula might have been applied to tropically-based tables before Ptolemy's, and that 159/158 B.C. was adopted as the epoch for the correction formula because this year already had the status of an epoch (at least for calculations for recent dates) in the tables to which the correction formula was applied, or more fundamentally, because Hipparchus' observations from that year were accepted as valid. Hipparchus observed summer solstices in at least one other year (135 B.C.); we will probably never know just why this one was chosen.

Thus (still from the point of view of the theory's authors) if one believed that Hipparchus had observed the summer solstice at the correct date and time in 158 B.C., one would equate Cancer 0° in the frame of reference of the pseudo-tropical tables based on Hipparchus' solstice with Cancer 8° in the true frame of reference, because the true frame of reference by hypothesis placed the solstitial point at Cancer 8°. In subsequent years, therefore, one would continue to place the solstice at Cancer 8° in the true frame of reference, while its counterpart in the pseudo-tropical frame of reference of the tables would gradually decrease in longitude. In other words, it is in the *tables'* frame of reference, not the true one, that the solstitial and equinoctial points are imagined as shifting. It is doubtful whether any empirical considerations led to the stipulation that the direction of the shift should reverse whenever the correction reached 0° or 8°; this may just have been a factitious doctrine arising from a belief that the difference in norms between the Babylonian System B and Hipparchus' tropical system was astronomically significant.

It must be stressed that, while *we* tend to think of the frame of reference of Theon's formula as sidereal because its year length is longer than 365 1/4 days, the astrologers who employed it did not accept the distinction between tropical and sidereal. And since they were incessantly computing the cardinal points of horoscopes, which depend on oblique and right ascensions, but hardly ever operated with fixed stars, the tropical aspect was foremost. They were probably scarcely conscious of the fact that the longitudes of the fixed stars, if computed by the *Handy Tables* and corrected by Theon's formula, were not constant as they should have been in a truly sidereal frame of reference, but shifted very slowly because of the discrepancy between Ptolemy's sidereal year and theirs.

Why, then, did they not believe Ptolemy's panoply of arguments in *Almagest* 3.1 that the tropical year is shorter than 365 1/4 days, or those in 7.2–3 demonstrating the fact and rate of precession? Most users of the trepidation formula of course never read the *Almagest*, but the people who devised the formula in the specific form in which Theon presents it presumably did. Their motive was not laziness, since Ptolemy's tables were comparatively laborious to use, and the conversion formula just added to the number of arithmetical operations. They may have been sceptical of Ptolemy's empirical arguments, and particularly of his claimed observations of dates of equinoxes and solstices (and in this they would have had some justification), but probably they would not have had an alternative set of astronomical observations to back up their own theory.

The real objection to Ptolemy's precession theory was not astronomical in nature but astrological. Change the frame of reference for a horoscope, and you will find the Sun, Moon, and planets not only at different degrees, but often in different zodiacal signs possessing radically diverse qualities and influences; and when the equinoctial and solstitial points shift, this affects also the division of the zodiac by the ascendant and the other cardinal points. The interpretation of the horoscope will be utterly different. But the old methods resulted in successful astrological predictions, did they not?

Ptolemy's *Tetrabiblos* (or *Apotelesmatica*) does not directly mention precession, but of course the fact that with precession one could no longer treat position relative to fixed stars and position relative to the solstitial and equinoctial points as interchangeable concepts was very much on Ptolemy's mind. Ptolemy holds that the tropical frame of reference, with the zodiacal sign boundaries aligned with the tropical and equinoctial points, corresponds to physical reality. (From time to time, nevertheless, he endows zodiacal signs with attributes—"quadripedal," "watery," etc.—that are obviously derivatives of the zodiacal constellations that approximately coincided with the tropically-defined signs in Ptolemy's time.) The deeply reforming character of Ptolemy's astrology must have been offensive to contemporaries who believed that their astrology already was a successful science founded on ancient revelations as well as ancient observations, and this would only have compounded the difficulties his precession theory had in winning adherents.

An echo of the contra-precession arguments that would have been in the air around the second century of our era may be heard in commentary on Plato's *Timaeus* by the precession-sceptic Proclus:[80]

> And if [the believers in Ptolemy's precession theory] adduce the computations of the motion of the planets and the settings out of nativities [made] on the hypothesis that the fixed stars move with this motion in the direction of the trailing parts [i.e. the westward motion at a rate of 1° per 100 years] and think that they pronounce things in agreement with the phenomena, it has to be said to them that those who do *not* believe that the fixed stars move with this motion, they too, are in exceptional agreement with the phenomena, and they have published tables concerning the motions of the planets and labored diligently about the subject of horoscope interpretation [γενεθλιαλογίαν] without being in the least forced to adduce this [motion] in the setting out of tables or the discovery of nativities. I might say that the Chaldeans were such people *par excellence*, whose observations comprised entire cosmic periods and whose foretellings of things happening to individuals and the broad community [τῶν τε ἰδίων καὶ τῶν κοινῶν παθημάτων] were irrefutable. Why do we appeal then to the testimony of newfangled displays researched from a few observations and without such great accuracy when those others are testifying to the teachings of

> the ancients concerning the motion of the fixed stars? Do we not know this, that it is possible to arrive at a true conclusion also from false hypotheses, and that one ought not to consider the conclusion's agreement with the phenomena as sufficient evidence of the truth of the hypotheses?

But in Ptolemy's day and for some time after, horoscopy based on Ptolemy's frame of reference could not yet claim a successful track record to complete with the established standing of the old precession-free horoscopy.

We now have proof that Ptolemy's were not the first tables to employ a frame of reference tied to a tropical year close to 365 1/4 – 1/300 days, but the abundance of papyrus manuscripts of his tables and adaptations of them shows that somehow his planetary theories acquired the repute of being superior to anything else available, once one had made a systematic correction to the longitudes. (This was, in fact, true: excepting the case of Mercury, the error arising from Ptolemy's inaccurate placement of the solstitial and tropical points in his own time was the largest component of the error in longitudes computed from his models and tables.) What is striking is the unanimity of the response, the consistent application of the same correction formula by practically every astrologer who used the tables over a span of two centuries. At a very early stage, therefore, someone who had sufficient authority to persuade more or less the entire astrological community must have promulgated Theon's formula as a required supplement to Ptolemy.

I would guess that the original vehicle for this injunction was an early manual of instructions for the tables. Ptolemy's crabbed and unillustrated introduction to the *Handy Tables* left a need for something more accessible to the common run of astrologers (Theon writes in the preface of his *Little Commentary* that most were barely capable of understanding multiplications and divisions), and many handbooks of this genre were written, as we know from several papyrus fragments.[81] Theon probably consulted some of them while composing his *Little Commentary*, and his chapter περὶ τροπῆς would have been modelled on analogous sections in them, where, however, the formula would have prescribed without any protest that Ptolemy knew best.

The ancient reception of Ptolemy's astronomical tables went in two stages: initial, gradual and limited acceptance during the first two centuries following the *Almagest*'s publication, and an apparently abrupt transition to complete acceptance, to the exclusion of other sets of tables and without adjustment of the frame of reference, during the fourth century. We do not know the reasons for Ptolemy's belated triumph, but perhaps one can attribute it in part to two circumstances. First, it was during the fourth century that the *Almagest* seems to have acquired the status of a mathematical classic and schooltext, as we know from the Alexandrian commentaries of Pappus and Theon. The elevation of the theoretical foundations of Ptolemy's tables beyond the threshold of criticism must have endowed the tables themselves with additional credibility, even among users who did not themselves study the *Almagest* as part of a mathematical curriculum. Secondly, the drop in the number of surviving horoscopes from the fourth century, whether archeologically recovered or transmitted through the medieval tradition, as compared to the second and third centuries hints at a significant diminishing in astrological activity at this time, perhaps reacting to the empire-wide bans on astrology during the reigns of Diocletian and the early Christian emperors.[82] A partial suppression of the practice and practitioners of astrology could have had a differential effect, favoring the more sophisticated and better educated astrologers who are more likely to have followed Ptolemy in all things; when astrology rebounded during the fifth century, the old practices had been swept away.

Notes

1. Text: Tihon (1978, 236–237). Whether the τροπὴ of the chapter title designates a solstice, a solstitial point, or the phenomenon of trepidation is unclear—as is also whether the noun should be singular or plural.
2. Ragep (1996). The only other ancient descriptions of trepidation are Proclus, *Hypotyposis* 3.54 (Manitius 1909, 66–68), which expresses the general idea without specific parameters, and a scholion to the same work (no. 316 in Manitius 1909, 275), where however an 8° oscillation is confusedly attributed to Ursa Major and Minor.
3. Text: Heiberg (1908, 159–185).
4. Throughout the *Little Commentary* Theon assumes that dates given for astronomical computations are in the first instance civil Egyptian dates expressed according to the reformed (καθ' Ἕλληνας or "Alexandrian") Egyptian calendar with a year number according to the Era Diocletian, which was the customary convention for specifying the Egyptian year of a horoscope in the fourth and fifth centuries. Such dates must be converted to the unreformed (κατ' Αἰγυπτίους) Egyptian calendar for entry into Ptolemy's tables, but at that stage the Era Diocletian is no longer used. Theon's algorithm thus operates directly with civil calendar years.
5. Ragep (1996, 269–270) with note 9 misses this point. The interpretation of Theon by Ṣā'id al-Andalusī, al-Zarqāllu, and al-Biṭrūjī that Ragep (p. 276) reports, such that for a fixed star the alternating trepidational motion would be superimposed on Ptolemy's precessional motion, was absolutely correct. In the *Handy Tables* precession is built into the method of determining a star's longitude, since its star catalogue only provides elongations from Regulus, not absolute longitudes valid for an epoch date.
6. We disregard as practically insignificant the fact that the supposed motion of the solstitial point is continuous whereas the algorithm, if applied to integer y, describes a function decreasing by discrete steps. The algorithm entails a likewise negligible difference in its rate of change between the period before Augustus when the calendar year is a constant 365 days and the period after Augustus when the mean calendar year is 365 1/4 days.
7. Text: Heiberg (1898–1903).
8. Ptolemy offers empirical considerations for identifying the ecliptic as a great circle, but none are precise enough to rule out the possibility that the Sun might make small latitudinal deviations (say on the order of magnitude of a degree) from a great circle. He never addresses this possibility explicitly in the *Almagest*.
9. For numerical clarity I here treat the formula as if it prescribed a regression of 1° in 80 of Ptolemy's tropical years, not Egyptian calendar years. On this order of time the difference is not significant.
10. Ptolemy does not give a value for the sidereal year in the *Almagest*, but assuming his rate for precession of 1° in 100 years, one obtains the value 365;15,24,31,…. The planetary period

relations in his *Planetary Hypotheses* are based on a sidereal year of exactly 365;15,24,31,32,27,7 days, which results from assuming a precessional rate of 1° in 100 *tropical* years (cf. Neugebauer 1975, 902, where the parameter is misprinted). In the earlier *Canobic Inscription*, on the other hand, Ptolemy gives a precessional rate based on 1° in one Egyptian calendar year (365 days), from which one would derive a sidereal year of 365;15,24,33,… days, or about 365 1/4 + 1/147 days (Neugebauer 1975, 914; Jones 2005a, 88). Ptolemy obviously recognized that precessional rates of 1° in one Egyptian year or one tropical year were for all intents and purposes indistinguishable.

11. Van der Waerden in Burckhardt (1958, 86 and 92), Neugebauer (1975, 633), Jones (1999a, 49), Mercier (2007, 264).
12. Neugebauer (1942). Revised editions of the texts in Neugebauer & Parker 1960–1968, v. 3.
13. Neugebauer (1942, 229–231). The exception was Mercury, for which Neugebauer found modern theory tropical longitudes consistently a little higher than the sign boundaries.
14. Neugebauer and van Hoesen (1959).
15. Neugebauer and van Hoesen (1959, 171–172) with Figs. 37 and 38 on pp. 188–189.
16. Neugebauer and van Hoesen (1959, 172). In fact Neugebauer underestimated both the error of Ptolemy's tropical frame of reference for his own time (mid second century) and the accumulated further error over the three subsequent centuries. Hence longitudes from Ptolemy's tables should average about 2 1/4° less than modern theory values for the middle of the fifth century, which is consistent with Neugebauer's results for the late horoscopes.
17. Neugebauer and van Hoesen (1959, 179–183). These were "eyeball" estimates, not the result of any statistical analysis of the data. Most of the dated solar longitudes in Vettius Valens do not come from complete horoscopes but from examples of astrological calculations involving only the Sun, Moon, and ascendant.
18. Kollerstrom (2001).
19. Kollerstrom excludes Mercury's longitudes because of the comparatively large errors found in ancient theories for this planet.
20. Huber (1958).
21. For calculations according to Ptolemy's tables I have relied on the excellent Java Script programs of R. van Gent (http://www.phys.uu.nl/~vgent/astro/almagestephemeris.htm), which are based directly on Ptolemy's models and on the epoch positions of the *Almagest*. Calculations based on the *Handy Tables* would not be significantly different for our present purposes, except that lunar longitudes from the *Almagest* represent the Moon's position about half an hour later than longitudes computed for the same date by the *Handy Tables* because of the equation of time separating the epochs of the two sets of tables; see Neugebauer (1975, 984–989).
22. The most complete inventory of Greek horoscopes is Heilen (2006, 501–569). I exclude unreliably dated, grossly erroneous, and apparently fictitious horoscopes. The documentary horoscopes include Neugebauer and van Hoesen (1959), nos. 46, 81, 95, 137a, 137c, 258, 260, 284, and 478; Baccani (1992), nos. 215 and 327; *P. Oxy. astron.* 4237, 4239, 4245, 4274, and 4275 in Jones (1999a); and a horoscope gem and ring published in Neugebauer (1969) and Neugebauer and van Hoesen (1964, 69–70). The "literary" horoscopes include Neugebauer and van Hoesen (1959), nos. L–71, L–42, L40, L74IV, L75, L76, L110III, L114V, L115II, L127XI, L380, and all horoscopes later than A.D. 400 *except* L401, L412, L488, and L516. For the solar longitude of Neugebauer and van Hoesen (1959), no. 46 (*P. Oxy*. 307) I read on the basis of photographs Capricorn 15 instead of Capricorn 11 1/2. I also adopt the emendation of the solar longitude in L478 proposed by Neugebauer and van Hoesen (1959), 144 note 4, which is required by the subsequent astrological data.
23. Kollerstrom assumes that one should always round these longitudes *up*.
24. References to Vettius Valens are to Pingree (1986). Note that the division and numbering of chapters is often different in Kroll (1908). Divergences from Pingree's text and Julian date equivalences (cf. his index of *themata*, i.e. datable horoscopes whether complete or not,

xviii–xx): *thema* 66 (8.7.64), I assume year 18 of Trajan is a scribal error for year 14, with equivalent date A.D. 110 September 12; *thema* 25 (8.7.167), equivalent date A.D. 83 March 15, not May (misprint); *thema* 113 (8.8.36), I assume year 15 of Antoninus Pius is a scribal error for year 10, with equivalent date A.D. 146 November 22. I include *thema* 22 (8.7.194) as transmitted, with date equivalent to A.D. 79 November 28, with misgivings; for this date the text's lunar longitude is off by a full zodiacal sign, but I do not see how to remove the discrepancy by a simple emendation to either the date or the longitude.

25. The graph omits the wildly discrepant *thema* 22 (see preceding note).
26. The second "Apollonius" in this passage is a manuscript corruption of "Apollinarius," as is clear from the fact that Valens goes on to paraphrase his same disclaimer about possible errors of a degree or two. Whether the first "Apollonius" should also read "Apollinarius" is open to dispute. See Jones (1990, 12–17) for review of this crux and on Apollinarius in general.
27. I omit diacritical marks distinguishing whole numbers from fractions, since in my experience the testimony of manuscripts is practically worthless with respect to them.
28. Text from Maass (1892, 140). I do not know whether the version in Pingree (1986, 455), which reports the first year length as τξε θ ιε ("365 9 15") instead of τξε θι ε ("365 19 5"), derives from an independent reading of the manuscript.
29. The notation is not the common one expressing the fractional part as a series of unit fractions to be added together, but as a divisor and dividend as in a modern fraction. The variant in *Vat. gr.* 381 makes sense as it stands, and indicates that the original text was probably written with divisor first ("365 and a 19th part of 5"). It is not clear why Valens did not write, using the standard notation, 365 1/4 1/76.
30. See note 9 above. Neugebauer (1949b) suggests that a year length 365 1/4 1/288 days attributed by Galen, *On Seven-Month Children*, to Hipparchus (in the form 1/2 year = 182 days 15 h plus about 1/24 h) is an error, resulting from repeated halving, for 365 1/4 1/144 days; though it seems at least as plausible that a *tropical* half-year of 182 days 15 h *minus* about 1/24 h lies behind Galen's text.
31. The tablet, BCM A1845–1982.2, is published in Britton et al. (2007). Obverse lines 15′–16′ give a solar mean daily motion of 0;59,8,9,48,40°, from which one obtains a year of 365;15,33,45,39,… days.
32. Valens' ascension tables are of the System A variety in Neugebauer's nomenclature; see Neugebauer (1975, 712–721) (esp. 719).
33. Neugebauer (1955, 41–85).
34. Jones (2002a). While the mathematical structure of the System A ascension table was widely adapted in Greek astronomy, the System A norm according to which the equinoctial and solstitial points are at 10° in their signs is attested only in one Greco-Roman source, Manilius 3.681.
35. Neugebauer (1949a) and van der Waerden (1958a).
36. Jones (1997a).
37. Jones (1997a, 18–21). For an analytic treatment of this property of the zigzag function, see Mercier (2007).
38. A lunar mean daily motion of 13;10,34,51° is in fact attested in the Babylonian tablet BCM A1845–1982.2, Obverse line 9'. This is the same tablet that implies a year length close to that of Theon's formula (note 30 above), so that in this respect the tablet is internally consistent.
39. Jones (1998).
40. Jones (1997b).
41. See note 30 above.
42. The data for the conjunction on May 14 are imperfectly legible.
43. *P. Rylands* 1.27, the papyrus containing algorithms for generating the epoch dates and positions of the Standard Lunar Scheme, strangely continues with instructions for extrapolating solstice and equinox dates from a set of given dates in A.D. 139/140, assuming a tropical year of 365;14,48 days, i.e. precisely Ptolemy's value. The papyrus claims that the initial

dates were observed by Ptolemy, and in fact three of the four are reported in *Almagest* 3.1 and 3.4. The papyrus does not associate specific longitudes with the solstices and equinoxes.

44. At present is not clear what meaning should be assigned to the intermediate year length of 365 1/4 days, unless it is perhaps a period of solar anomaly. There does not seem to be a connection with the curious, precession-free solar model of *P. Oxy. astron.* 4174a, in which the Sun has distinct periods of longitude (365 1/4 days), anomaly (365 1/2 days), and latitude (365 1/8 days); cf. Jones (2000).
45. *Almagest* 3.1 (Heiberg 1898–1903, v. 1, 202; cf. Toomer 1984, 137 with note 18).
46. The tables evidently broke time up into tens of thousands and thousands of Egyptian years, as well as the shorter units familiar from the *Handy Tables*. The increments in mean motion appear to be based (notwithstanding some doubtful readings) on daily motions of exactly 0;59,8,9,36° (359;44,38,24° per Egyptian year, thus 37473 × 360° + 120° in 37500 Egyptian years), approximately 0;59,8,15,16,52° (exactly 359;45,12,57,36° per Egyptian year, thus 37474 × 360° + 120° in 37500 Egyptian years), and approximately 0;59,8,17,10,29° (exactly 359;45,24,28,48° per Egyptian year, thus 37474 × 360° + 240° in 37500 Egyptian years). These imply year lengths slightly different from the values stated explicitly in the papyrus. The principle resembles that of a "great year" and in particular the 29160 Egyptian year combined planetary period of the Keskintos Inscription, for which see Jones (2006).
47. In *Almagest* 3.1 Ptolemy does cite Hipparchus' book *On the displacement of the solstitial and equinoctial points* for observations of the autumnal equinoxes of 162, 159, and 158 B.C., and Neugebauer (1975, 633), already suggested a connection between these and the trepidation epoch. For what Neugebauer thought this connection was, see section "The Rise and Fall of Trepidation in Greek Astronomy" below.
48. Neugebauer (1956).
49. Neugebauer (1958, 112), Neugebauer and van Hoesen (1964, 385–386).
50. Burckhardt (1958). For example the mean difference between the papyrus readings and Burckhardt's recomputations using the *Handy Tables* for 65 legible positions of Saturn is approximately 1;39°, with a standard deviation of approximately 0;3°.
51. Neugebauer (1956, 14–16) very plausibly suggested that *P. Heid. Inv.* 34 was representative of a class of ancient tables from which the medieval Almanacs descended.
52. *P. Oxy. astron.* 4205–4213. In addition a small undatable fragment, *P. Oxy. astron.* 4205a, employs ten-day as well as five-day intervals. Detailed comparison of the texts with recomputation in the relevant commentaries in Jones (1999a).
53. *P. Oxy. astron.* 4190, 4192, 4194, 4195, and 4196; 4196a has the same format but is undatable. Comparisons of text with recomputation in Jones (1999a).
54. Neugebauer (1975, 984–988). The lunar longitudes in *P. Heid. Inv.* 34 are not in agreement with Ptolemy's model. There are no lunar positions preserved in any of the other almanacs that have planetary longitudes from Ptolemy's tables.
55. Jones (1990, 10–12); note 42 above.
56. Jones (1999a, 38–39).
57. Jones (1999a, 39–40).
58. Two undated horoscopes discovered by Pingree in an anonymous commentary on Ptolemy's *Tetrabiblos* published in Wolf (1559, 112–115 and 98 with 168–171) would, if genuine, best fit respectively A.D. 175 December 22 and 213 June 13 (not 241 July 29 as reported in Neugebauer and van Hoesen 1964, 66). I strongly suspect, however, that they are contrived illustrations. Aside from the 1559 edition, which is not free of textual inconsistencies, only a partial summary of the first horoscope has been published in Pingree (1982).
59. The horoscope ring has been independently dated twice: see Neugebauer and van Hoesen (1964, 69), Rea (1980) with Rea (1984). According to Stutzinger (1984, 557–558) (where there is a good photograph of it), the ring is from Tartus, Syria.
60. *P. Oxy. astron.* 4180; *P. Mich. Inv.* 1454 (Curtis and Robbins 1935); *P. Vind. G.* 29370b and 29370 (Gerstinger and Neugebauer 1962).

61. *P. Mich. Inv.* 1454: Burckhardt (1958, 87–92); *P. Vind. G.* 29370 and 29370b: Jones (1994); *P. Oxy. astron.* 4180: Jones (1999a, 190).
62. Some horoscopes appear in more than one extant source. It is impossible to tell how many distinct astrologers were responsible for producing them.
63. On these grounds I regard as fictitious Neugebauer and van Hoesen (1959) nos. L401, L488, and L516, all of which are undated horoscopes from “Rhetorius Epitome IV” preserved in *Par. gr.* 2506 (Pingree 1977, 216–219); an undated horoscope from Book 5 of the astrological compilation in *Par. gr.* 2425 attributed to Rhetorius by Pingree (Pingree 1977, 221), dated by Pingree to A.D. 601 February 24 (Pingree 1976b, xii, and Burnett and Pingree 1997, 134–135); and three undated and incomplete horoscopes in the commentary on Paulus Alexandrinus attributed to Olympiodorus that Pingree dated to A.D. 492–493 (Pingree in Boer 1962, 149). The genuineness of L401, L488, and L516 was already questioned by Neugebauer and van Hoesen (1959, 134, 152, and 158), and that of the Olympiodorus examples by Toomer (1963) and Neugebauer (1975, 1044) note 11. The supposed A.D. 601 horoscope, which has serious discrepancies for four out of seven heavenly bodies for the date in question, appears to be the chief basis for Pingree’s belief that Rhetorius was active around A.D. 620 (Pingree 2001, 6–13).
64. See Jones (1999b).
65. Pingree (1977, 216–219).
66. New edition in Pingree (1976a, 146–148).
67. New edition in Pingree (1976a, 144–146), where the source is described as “the collection mistakenly ascribed to Rhetorius of Egypt,” though Pingree (2001, 12) takes it to be an authentic part of Rhetorius’ composition.
68. New edition, with Arabic version from al-Qaṣrānī, in Pingree (1976a, 139–142).
69. New edition in Pingree (1976a, 148–149).
70. This collection also includes L483, L484, L486, and L487 which are in *Vind. phil. gr.* 108.
71. This work also contains L484 which is in *Vind. phil. gr.* 108.
72. Pingree (1976a, 137–138).
73. Pingree (1976a, 142–144).
74. Apparently extant only in a Latin translation, though the horoscopes are also reported by Māshā’allāh’s pupil Abū ‘Alī al-Khayyāṭ; the degree numbers in the two versions often disagree, suggesting extensive corruption or deliberate alteration. This work also contains L428 which is in “Rhetorius Epitome IV” in *Par. gr.* 2506.
75. Text: Olivieri (1898, 170–171). Neither Olivieri nor Neugebauer and van Hoesen (1959, 188–189), were aware of the copy in “Book 6” of *Par. gr.* 2425 (ff. 216v–219v, see Pingree 1977, 222; and Pingree 2001, 12).
76. The first work in which Hipparchus deduced a tropical year of approximately 365 1/4 – 1/300 days was *On the Length of the Year* (although the parameter may not have been explicitly stated in that work); according to *Almagest* 7.3 this was written after his *On the Displacement of the Solstitial and Equinoctial Points*, which cited an observation of the vernal equinox in 128 B.C. For Ptolemy’s selectivity see Jones (2005b, 18–27).
77. For doubts, see Toomer (1984, 133) note 8.
78. This derivation of a Hipparchian sidereal year was first found independently by Biot and Sédillot (Sédillot 1845, 11–14), and subsequently rediscovered by Petersen (1966).
79. Neugebauer (1975, 297–298). Toomer (1980, 108) note 7 dismisses this hypothetical precession rate as “of no historical significance for anyone who has examined the evidence for the chronology and basis of Hipparchus’ discovery of precession.”
80. Diehl (1903–1906 v. 3, 125–126).
81. *P. Oxy. astron.* 4142 and 4143; I know of one further unpublished example.
82. For frequency of horoscopes according to date see Neugebauer and van Hoesen (1959, 162) Fig. 23 (for the “literary” horoscopes) and Jones (1999a, 6) Fig. 1 (for papyri).

Ptolemy's Doctrine of the Terms and Its Reception

Stephan Heilen

This contribution is devoted to Ptolemy's astrological treatise which is commonly called *Tetrabiblos* ("treatise in four books"), a Greek surname to be found in various branches of the medieval manuscript tradition and used by most editors starting with the first edition by J. Camerarius (1535).[1] The original Greek title, however, seems to have been *Apotelesmatiká* (*biblía*), "(books on) effects," and I shall follow the authoritative edition of Hübner (1998) in using this.[2] The authenticity of this work, which was first questioned by Arabic scholars in the Middle Ages and later also by Western humanists, is now generally acknowledged.[3]

The modern enlightened distinction between the "science" of astronomy and the "superstition" of astrology did not exist in antiquity. The Greek terms ἀστρονομία and ἀστρολογία (*astronomía* and *astrología*) were both used to denote either one, and it was not uncommon that one and the same scholar wrote treatises on either one.[4] In his introduction to the *Apotelesmatika*, Ptolemy asserts that there are two important and valid methods of making predictions through *astronomía*.[5] The first is concerned with the movement of the heavenly bodies (κίνησις, *kínēsis*), the second with the effects of these movements on Earth (ἀποτελέσματα, *apotelésmata*). The first part had been treated in the *Syntaxis* (Arabic: *Almagest*), and the *Apotelesmatika* were intended by Ptolemy as a complement to the *Syntaxis* in order to give a thorough account of both areas of *astronomía*.

There is one special problem in the *Apotelesmatika* to which Ptolemy devotes considerable space: the doctrine of the Terms (ὅρια, *hória*), in Apotel. 1.21.[6] Terms are one among several dignities that a planet can enjoy according to Hellenistic astrological doctrine. The five canonical dignities are called "Houses," "Triangles," "Exaltations," "Terms", and "Decans."[7] Suffice it to say that whenever a planet's longitude is such as to make the planetary deity enjoy one or more of its dignities, it is supposed to have a stronger and more benefic astrological influence on Earth. Greco-Roman astrologers never established a universally accepted hierarchy of the planetary dignities, but it is fair to say that "Houses," "Triangles," and "Exaltations" were usually considered to come first, and to be more important than Terms

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Table 1 Ptolemy's treatment of the planetary dignities (Apotel. 1.18–21)[8]

	Lines
"Houses" (1.18)	45
"Triangles" (1.19)	62
"Exaltations" (1.20)	36
Terms (1.21)	169
"Decans" (–)	0

and "Decans." Therefore it is interesting to see that Ptolemy devotes more space to the treatment of the Terms than to all other dignities taken together (Table 1).

Why did Ptolemy pay so much attention to the seemingly marginal doctrine of the Terms? And how did his treatment of this topic influence later writers? In my account of the ancient reactions I shall aim at completeness. However, no such claim can be made for the later reception from the Arabic, Byzantine and Latin Middle Ages through the Renaissance to the very present, and I shall but outline that vast array of post-antique reception to the best of my knowledge.

The English word Terms goes back to Latin *termini*, which in turn is the translation of Greek ὅρια (*hória*), meaning "boundaries."[9] The classical version of this system was attributed to "the Egyptians," that is: to Nechepso the King and Petosiris his High Priest.[10] Under these pseudonyms, which evoke the idea of a far remote age, the core of Hellenistic astrology had in truth been created and dispersed by unknown, Greek writing authors in the second or first century BCE.

Table 2 The Egyptian terms[11]

♈	♃ 6	♀ 6	☿ 8	♂ 5	♄ 5
♉	♀ 8	☿ 6	♃ 8	♄ 5	♂ 3
♊	☿ 6	♃ 6	♀ 5	♂ 7	♄ 6
♋	♂ 7	♀ 6	☿ 6	♃ 7	♄ 4
♌	♃ 6	♀ 5	♄ 7	☿ 6	♂ 6
♍	☿ 7	♀ 10	♃ 4	♂ 7	♄ 2
♎	♄ 6	☿ 8	♃ 7	♀ 7	♂ 2
♏	♂ 7	♀ 4	☿ 8	♃ 5	♄ 6
♐	♃ 12	♀ 5	☿ 4	♄ 5	♂ 4
♑	☿ 7	♃ 7	♀ 8	♄ 4	♂ 4
♒	☿ 7	♀ 6	♃ 7	♂ 5	♄ 5
♓	♀ 12	♃ 4	☿ 3	♂ 9	♄ 2

Within the 30° of each zodiacal sign, each of the five planets known to antiquity is allotted a delimited area.[12] These Terms vary with regard to their sequence and extension. When Ptolemy embarked upon the arduous task of finding rational, physically convincing explanations of astrological doctrine, the Egyptian Terms posed a serious problem because they follow no apparent rational order, neither with regard to their sequence (τάξις, *táxis*) nor with regard to their extension (ποσότης, *posótēs*).[13] As Bouché-Leclercq put it, "l'incompréhensible […] atteint sa pleine floraison dans le système des ὅρια."[14] Despite the lack of a consistent rationale in the Egyptian Terms, it was unthinkable simply to dismiss them as Ptolemy did in the case of some other astrological tenets. This easy solution was impossible for two reasons: firstly, the Terms were allegedly based on empirical evidence, namely on certain exemplary nativities that "the Egyptians" (i.e. Pseudo-Nechepso and Petosiris) had recorded in their manual(s),[15] and secondly, what is more important, because they were linked to the most important task of ancient astrology, the prediction of life expectancy.[16] It was an adamant tenet that the sum of the Terms of each planet in the Egyptian system equals the maximum number of years that this planet grants if it is particularly well positioned in a person's horoscope. Due to the tension between the importance of the Egyptian Terms on the one hand and their lack of order on the other hand, this topic is among the most interesting chapters of Ptolemy's attempt at rationalizing astrological doctrine.

Table 3 Years of life granted by the planetary deities[17]

	Maximum (τέλεια ἔτη)	Mean (μέσα ἔτη)	Minimum (ἐλάχιστα ἔτη)
Saturn	57	43	30
Jupiter	79	45	12
Mars	66	40	15
Venus	82	45	8
Mercury	76	48	20
Sun	120	69	19
Moon	108	66	25

Ptolemy's main discussion of the Terms (Apotel. 1.21) falls into three parts.[18] First he explains the unsatisfactory characteristics of the widespread Egyptian system (Apotel. 1.21.1–11). He explicitly refutes the attempts of certain unnamed people who had argued that this system was based on the evenly progressing ascension tables of the zodiacal signs in the various *klimata*,[19] a caveat that did not keep some modern scholars from dabbling in the same sort of futile explanations.[20] The implicit information that "the Egyptians" did not explain the rationale of their system with respect to Ptolemy's leading criteria (i.e. order and extension) is later made explicit in Apotel. 1.21.19.[21]

Second, Ptolemy presents an alternative system which he attributes to "the Chaldeans" (Apotel. 1.21.12–19). As Neugebauer pointed out, the assignment of the degrees in this system "is made in good Babylonian spirit", strictly linear, with difference 1 from Term to Term, which does not mean, however, that the whole pattern really is of Babylonian origin; on the contrary, it seems to be a Hellenistic invention.[22] This second system is perfectly orderly but over-schematic and unknown from other sources; still worse: the totals of each planet's Terms are much different from the Egyptian totals (Apotel. 1.21.17), with an unwelcome emphasis on the astrological evil-doers Mars and Saturn. Therefore Ptolemy concludes that this is not a useful alternative to the Egyptian system (Apotel. 1.21.18).

Lastly, he presents a third system (Apotel. 1.21.20–30) which, like the Chaldean one, is unknown from other sources. Even if Ptolemy does not explicitly say so, this third system is obviously his favorite,[23] not only because of its final position in his presentation of the competing systems but also because it is credited with uniting no less than three advantages: (1) it contains a natural and consistent explanation of the order and extension of the Terms, (2) it agrees with the degrees reported in the aforesaid exemplary nativities put forth by the "men of old," and (3) it also agrees with the Egyptian totals of each planet's Terms.[24]

The only disadvantage of this third system, one might object, is its lack of authority compared to the widespread Egyptian system. This leads us to the third system's origin which will be examined shortly. First, however, let us quickly round off the picture by looking at other systems of Terms that are known from antiquity, though not mentioned by Ptolemy. A fourth one which assigns Terms not only to the five true planets but also to the luminaries is described by Vettius Valens. It seems to be Valens' own invention and was analyzed by Bouché-Leclercq.[25] Besides, three more Greek systems have come to light that were still unknown to Bouché-Leclercq.[26] One of these goes back to the early astrologer Critodemus who assigned Terms to the five planets and to the Sun, but not to the Moon.[27] Critodemus is also credited with having devised an enlarged version of the basic Egyptian system that specifies the names and apotelesmatic characteristics of the Egyptian Terms.[28] Entirely different is a system of unknown authorship that is preserved in a second century Michigan papyrus. This system is based on the epicycles of the planets.[29] Outside the Greek astrological tradition we have two more ancient systems of Terms, an indigenous Egyptian one on a Demotic papyrus from Tebtunis which seems to be a combination, devised for mnemonic purposes, of the traditional Egyptian system with that of Critodemus,[30] and an Indian one (of Greek origin?) that Sphujidhvaja included in his work on Greek horoscopy (*Yavanajātaka*, 269/270 A.D.).[31] Both the Demotic and the Indian system allot Terms only to the five true planets.[32]

Now back to the origin of Ptolemy's preferred system. He says that he recently came upon an ancient manuscript, much damaged (ἀντιγράφῳ παλαιῷ καὶ τὰ πολλὰ διεφθαρμένῳ) which contained an astrological treatise that was very lengthy in expression and excessive in demonstration. The book's damaged state made it hard to read, so that he could barely gain an idea of its general purport. Fortunately, certain tabulations of the Terms were better preserved because they were placed at the end of the book. This last information points to the form of a papyrus roll, for there the last leaves would be protected. The first and last pages of a codex, instead, would be liable to damage, since they would be outermost.[33] One more detail on the material appearance of this manuscript is given later, towards the end of Ptolemy's analysis of its astrological rationale (Apotel. 1.21.22–27), namely that certain extra degrees allotted to this or that planet's Terms were marked with dots (1.21.26 οἷς καὶ παρέκειντο στιγμαί).

While earlier readers had generally not questioned the authenticity of that old manuscript described in such detail by Ptolemy, modern scholars tend to doubt that it ever existed.[34] Their critical remarks are mostly brief and unsupported by detailed argument. But it makes an important difference whether Ptolemy really found such a document or if he only pretended in order to bestow dignity and authority on a system that is actually his own invention. A fair assessment of this question requires that various aspects be taken into consideration. I shall first discuss the topic of book finds in general and then address the specific case of Ptolemy's old manuscript.

As Speyer (1970) has amply demonstrated, pretended finds of books are a widely spread phenomenon in the ancient world, especially in the geographical area of Egypt and in the thematic areas of magic, astrology, and religion. The surviving evidence is so vast that Speyer overlooked some instances that deserve examination, including Ptol. Apotel. 1.21. Generally speaking, the situation is complicated by the fact that there are numerous certain cases of forgeries and pretended book finds, but also cases of real book finds, mostly from Egyptian tombs and temple libraries.[35] In most cases, we are informed about the alleged provenance of the book, be this from heaven ("Himmelsbrief"), from a tomb viz. from the Earth, or from a temple, library, or archive. In his section devoted to pagan antiquity (as opposed to Christian book finds in late antiquity), Speyer presents three detailed reports.[36] Among these, one is particularly interesting: the pretended find, in 181 BCE, of the coffin of Numa containing this early Roman king's books, that is: papyrus rolls with writings of Pythagorean philosophy and pontifical law.[37] Pliny the Elder describes this case (Plin. nat. 13.84–87) following the report of various Roman annalists, especially Cassius Hemina (first half 2nd c. BCE). According to Roman chronology, 535 years had elapsed, at the time of the find, since the reign of Numa in the late eighth century BCE. Therefore the good status of preservation of the rolls called for explanation. For this reason, the forgers adduced a detailed account of how the rolls had allegedly been protected. Pliny quotes literally from the fourth book of Hemina: "Other people wondered how those books could have lasted so long, but Terentius's explanation [*i.e. the discoverer's who allegedly turned up the coffer when digging over his land on the Janiculum*] was that about in the middle of the coffer there had been a square stone tied all round with waxed cords, and that the three books had been placed on the top of this stone; and he thought this position was the reason why they had not decayed; and that the books had been soaked in citrus-oil, and he thought that this was why they were not moth-eaten."[38] In antiquity no one has doubted the authenticity of the find. Only the teacher-student relationship between Pythagoras and Numa was rejected on chronological grounds. Today we know that those books were a forgery by Neo-Pythagorean circles, but we do not know whether the story about their discovery is entirely faked or if someone really buried the forged books in order to have them discovered in a more realistic and convincing fashion.[39] This example is all the more striking because it seems to have had a considerable public dimension: Pliny reports that these volumes were burned by the praetor Quintus Petilius who thereby executed a Resolution of the Senate that had been taken because the content of the newly found books threatened traditional religion.[40] In comparison, Ptolemy had a much easier task, if his is a forgery. Similar pretended book finds described by Speyer in the same fashion are diaries of the Cretan Dictys and the Apocalypse of Paul.[41] Note that through the whole period of the Middle Ages and even in early modern times Dictys and his Trojan counterpart Dares were considered to be historians of a remote age who deserved unconditioned faith.[42]

In the specific field of the exact sciences in antiquity, the case of the geometrician Dionysodorus of Melos (he lived sometime between 240 and 25 BCE) deserves to be mentioned, even if it is not exactly comparable to Ptolemy's. Pliny reports that on the day after Dionysodorus's burial his female relatives, while carrying out the due rites, found in the tomb a letter signed with his name and addressed to those on Earth, which stated that he had passed from his tomb to the center of the Earthly globe and that it was a distance of 42,000 stades. Probably the geometrician himself had devised this literary fraud shortly before his death in order to bestow plausibility onto his scientific claims. While the content of this message reflects Greek science, the literary form follows Egyptian models. Pliny mockingly introduces the anecdote as an *exemplum vanitatis Graecae maximum*.[43]

In the field of magic and alchemy,[44] the most famous forgery is from the Arabic Middle Ages, the Emerald Tablet (*Tabula Smaragdina*).[45] Containing the pretended teaching of Hermes Trismegistus, this text was the foundation of the alchemic belief in the possibility of transformation of metal, a revelation of highest divine truth and the key to the ultimate secrets of nature. Allegedly the Emerald Tablet had been found by Apollonius of Tyana (1st c. A.D.) in a cave underneath a statue of Hermes.[46] Its actual date of composition is sometime between the sixth and the middle of the eighth century A.D. It was only in 1603 that N. Guibert first attempted to prove that the *Tabula Smaragdina* is a forgery. The alchemists themselves have defended its authenticity until the middle of the nineteenth century and, far from being content with that, invented even the alleged original text in Phoenician language.

In the field of astrology, one may think of the pretended find of an Apocalypse of Daniel under the reign of Constans II (642–688 A.D.).[47] Remotely similar is the opening of the fifth book of the astrological poet Pseudo-Manetho (of uncertain date) who promises to sing of what he learned "from the books of the temple sanctuaries and the hidden (!) steles, which all-wise Hermes erected and inscribed with his own forecasts of the heavenly stars."[48] Festugière 1950, in his chapter on "Révélation par la découverte d'un livre ou d'une stèle," further mentions a *lunarium* that had allegedly been composed in hieroglyphs under the pharao Psammeticus and found in the inner sanctuary of the temple of Heliopolis.[49]

Seen against that background of numerous pretended book finds in Greco-Roman culture which spans from the early fourth century BCE[50] into the Middle Ages, it seems well possible that Ptolemy is only pretending in Apotel. 1.21. But we should distinguish more clearly between two kinds of forgeries, on the one hand that of which a material product existed that could be examined or even burned, like the pretended books of Numa, and on the other hand forgeries without a *corpus delicti*, those that served literary purposes only, like the *Tabula Smaragdina*.[51] Speyer adduces ample evidence that the motif of book finds as a mere literary device to certify authenticity began already in the Hellenistic period (i.e. the last three centuries BCE).[52] Ptolemy may well be standing in this latter tradition. Although he does not say in which locality he found the old manuscript, one may most reasonably think of a library, an archive, or a temple with its own library like the Alexandrian *Serapeum*.[53] These cases are typical of the Egyptian environment where libraries and their archives garantueed the authenticity and value of a text.[54] One might object that Ptolemy's detailed information on the old manuscript, like the precarious state of preservation and the dots marking certain kinds of degrees (Apotel. 1.21.20–21 and 1.21.26), points to a real book find, but this argument is ambiguous: it is the shrewd forger's method to impress the reader with such details invented for the purpose of plausibility, and to emphasize the deteriorated state of preservation of their pretended manuscripts while assuring the reader that all the essential features were still readable.[55] In other words: even if we find, for instance, other papyri with dots similar to those described by Ptolemy,[56] that can be taken as an argument for the existence of his old manuscript as well as for his ability as a forger.

But all this dos not *prove* that Ptolemy was lying. That may be the reason why different scholars hold different views. While Houlding (2007) takes the old manuscript's authenticity for granted and does not even mention the wide-spread doubts of other scholars, Festugière is convinced that Ptolemy is only pretending: "Le prestige du livre très ancient est si grand que Ptolémée, dans la *Tetrabible*, ne craint pas d'user lui-même de cette fiction."[57] Boll takes a curious intermediate view point which leans towards authenticity: he thinks that Ptolemy's story is "certainly credible" yet "unconsciously following those widespread concepts of revealed knowledge" (because he finds it noteworthy to adduce those typical details of old book finds, whether true or not).[58]

We must therefore look for additional arguments *pro* and *contra*, related more specifically to Ptolemy as an author, to the doctrine in question, and to the typical features of astrological manuals. Four points deserve being made:

1. There is no supporting evidence that the third system really existed before the time of Ptolemy. And no one of the other ancient systems of Terms that are known to us is suited to fulfill Ptolemy's criteria.[59]

2. Elementary matters like the dignities of the planets typically belong to the beginning of astrological manuals, including the relevant tables. Compare, besides Ptolemy's own arrangement in Apotel. 1.21, Paulus Alexandrinus, Chapter 3, on the Terms. I know of no parallel case of an astrological manual with tables related to elementary matters put separately at the end of the manual. If, however, Ptolemy is only pretending, then his description provides a perfect explanation why the tabularized summary is preserved while the description of the underlying rationale is too damaged to be understood thoroughly.
3. Ptolemy's description is that of a papyrus which is at least one, or rather two centuries old.[60] This takes us back to the period of early Hellenistic astrology which is characterized by mystification and revelation,[61] a circumstance that makes the detailed—even excessive—demonstration[62] adduced by Ptolemy's old manuscript look anachronistic. Again, the problem disappears when viewing the find as made up: Ptolemy could not of his own question the overwhelming authority of "the Egyptians." In order to stand up against that well established tradition and to propose an acceptable alternative to their system of Terms, he needed an authority dignified by similarly old age.[63]
4. There might be a parallel case for Ptolemy's creating a non-existant ancient authority as a cover for his own invention. As the independent scholar John P. Britton points out to me,[64] he suspects that Ptolemy's reference to "the ancients" in Book IV, Chapter 2 of the *Almagest* may also be made-up. In that chapter Ptolemy attributes to "the ancients" efforts to find a constant interval between eclipses which would therefore also be a period of return in lunar anomaly. Two paragraphs later, however, he says that Hipparchus had "already" (πάλιν) solved the problem, making it highly doubtful that anyone subsequently pursued the theoretically logical but utterly impractical methodology proposed by Ptolemy.[65]

In conclusion, a definitive answer is impossible, but there is a considerable probability that Ptolemy himself invented the third system of Terms.

It is now time to compare Ptolemy's Terms to the Egyptian ones (Table 4). One finds an overlap of 184 out of 360° (51%). The discrepancy in the remaining 49% grows still bigger when taking into account the fact that Ptolemy (Apotel. 1.22) wants his table of Terms to be applied to the tropical zodiac, not to the sidereal one which was used by all contemporary practitioners. This point tends to be overlooked but is an important contribution that Ptolemy made to the history of astrology.[66] At the time when he wrote the *Apotelesmatika*, the shift between sidereal and tropical zodiac was still some 3.5°.

Table 4 Egyptian vs. Ptolemaic terms (discrepancies are greyed)

<table>
<tr><th></th><th>1</th><th>2</th><th>3</th><th>4</th><th>5</th><th>6</th><th>7</th><th>8</th><th>9</th><th>10</th><th>11</th><th>12</th><th>13</th><th>14</th><th>15</th><th>16</th><th>17</th><th>18</th><th>19</th><th>20</th><th>21</th><th>22</th><th>23</th><th>24</th><th>25</th><th>26</th><th>27</th><th>28</th><th>29</th><th>30</th></tr>
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<tr><td>♎</td><td colspan="6">♄</td><td colspan="8"></td><td colspan="5">♃</td><td colspan="9"></td><td colspan="2">♂</td></tr>
<tr><td>♏</td><td colspan="6">♂</td><td colspan="21"></td><td colspan="3">♄</td></tr>
<tr><td>♐</td><td colspan="8">♃</td><td colspan="4"></td><td colspan="2">♀</td><td colspan="3"></td><td colspan="2">☿</td><td colspan="2"></td><td colspan="4">♄</td><td colspan="1"></td><td colspan="4">♂</td></tr>
<tr><td>♑</td><td colspan="6"></td><td colspan="1">☿</td><td colspan="5"></td><td colspan="2">♃</td><td colspan="11"></td><td colspan="1">♄</td><td colspan="4"></td></tr>
<tr><td>♒</td><td colspan="6"></td><td colspan="1">☿</td><td colspan="5"></td><td colspan="1">♀</td><td colspan="17"></td></tr>
<tr><td>♓</td><td colspan="8">♀</td><td colspan="4"></td><td colspan="2">♃</td><td colspan="2"></td><td colspan="3">☿</td><td colspan="1"></td><td colspan="6">♂</td><td colspan="2"></td><td colspan="2">♄</td></tr>
</table>

The above table is correct as long as one takes the sequence and extension of the Terms as edited by Hübner 1998 for granted. However, here lurks a problem. Besides the table given by the direct manuscript tradition we have several indirect sources that give slightly different versions of the Ptolemaic table of Terms.[67] Which one is correct? And how did the variants originate? These questions will be addressed more fully in the now following, chronologically arranged survey of the reception of Ptolemy’s doctrine of the Terms. It falls into two sections: first, Greco-Roman Antiquity, and second the later reception from the Middle Ages to the present. Regrettably, there is not yet an article on Ptolemy available in the *Catalogus Translationum et Commentariorum*.[68] It is to be hoped for that the following survey contain some useful suggestions for those who will investigate the reception of Ptolemy, especially of his *Apotelesmatika*, on a broader scale.

Greco-Roman Antiquity

By Greco-Roman antiquity I mean the time-span down to the Arabic conquest of Egypt in 641 A.D. Within this section, I shall first discuss the evidence from original documents for the Terms, and then the literary sources.

Original Documents

My analysis is based on the following list of texts (Table 5). They are all Greek horoscopes.[69] Exact calendar dates refer to the planetary alignments discussed by the authors, not to the unknown dates of composition. In each case, the authoritative modern discussion is referred to in parenthesis (“GH” stands for the collection of *Greek Horoscopes* by Neugebauer and van Hoesen 1959).

Table 5 Original Greek horoscopes indicating the terms

P. Oxy. II 307 (GH, pp. 19–20, n. 46)	Jan. 3, 46 A.D.
P. Oxy. XXXI 2555 (Baccani 1992, pp. 81–95, n. 1)	May 13, 46 A.D.
P. Oxy. astron. 4236 (Jones 1999a, v. 1, pp. 250–251; v. 2, pp. 372–373)	Nov. 25, 63 A.D.
P. Lond. I 130 (GH, pp. 21–28, n. 81)	Mar. 31, 81 A.D.
P. Lond. I 98 (GH, pp. 28–38, n. 95)	Apr. 13, 95 A.D.
P. Oxy. astron. 4279 (Jones 1999a, v. 1, p. 287; v. 2, pp. 428–429)	c. 100 A.D.
P. Oxy. astron. 4280 (Jones 1999a, v. 1, p. 287; v. 2, pp. 428–429)	c. 100 A.D.
P. Paris 19 = P. Lond. I 110 (GH, pp. 39–44, n. 137a,b)	Dec. 4, 137 A.D.
P. Princeton II 75 (GH, pp. 44–45, n. 138/161)	138/161 A.D.
P. Oxy. astron. 4281 (Jones 1999a, v. 1, p. 288; v. 2, pp. 430–431)	Second century A.D.
P. Oxy. astron. 4276 (Jones 1999a, v. 1, pp. 282–283; v. 2, pp. 418–419)	c. 200 A.D.
P. Oxy. astron. 4277 (Jones 1999a, v. 1, pp. 284–286; v. 2, pp. 420–427)	c. 200 A.D.
P. Oxy. astron. 4245 (Jones 1999a, v. 1, pp. 258–259; v. 2, pp. 382–383)	218 A.D.
P. Oxy. astron. 4285 (Jones 1999a, v. 1, p. 290; v. 2, pp. 434–435)	Third century A.D.
P. Oxy. astron. 4284 (Jones 1999a, v. 1, p. 289; v. 2, pp. 434–435)	Late third century A.D.
P. Oxy. astron. 4282 (Jones 1999a, v. 1, p. 288; v. 2, pp. 430–431)	c. 300 A.D.
P. Oxy. astron. 4283 (Jones 1999a, v. 1, p. 289; v. 2, pp. 432–433)	c. 300 A.D.
PSI I 23,a (GH, pp. 65–67, n. 338)	Dec. 24, 338 A.D.
P. Kell. I Gr. 84 (De Jong and Worp 1995)	May 16, 373 A.D.

Most of these 19 papyri have been found in the ancient rubbish mounds of Oxyrhynchus in Upper Egypt. The horoscope for January 3, 46 A.D., contains our earliest datable reference to astrological Terms. All texts on the above list refer to the Egyptian Terms, including those that would, chronologically speaking, allow for a reception of Ptolemy's *Apotelesmatika*. There is only one dubious case, P. Oxy. astron. 4281, the fragment of a deluxe horoscope from Oxyrhynchus which was dated on palaeographical grounds to the second century A.D. All that is left of the text are three lines on Saturn: "Saturn in Aquarius, 15° and 36 min, its own house and terms of Venus, at its first station."[70] That is correct only with reference to the Ptolemaic system of Terms because in the Egyptian one the 16th degree of Aquarius is assigned to Jupiter:

Table 6 Egyptian vs. Ptolemaic Terms in Aquarius

Egyptian

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
♒	☿							♀						♃							♂					♄				

Ptolemaic

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
♒	♄						☿						♀								♃					♂				

There is no doubt about the reading because the numerals iota epsilon for "15" are easily visible in the right upper angle of the papyrus (see Fig. 1).

However, it is possible that the scribe was negligent or incompetent, as is clearly the case in a few completely preserved horoscopes. Since this would be the

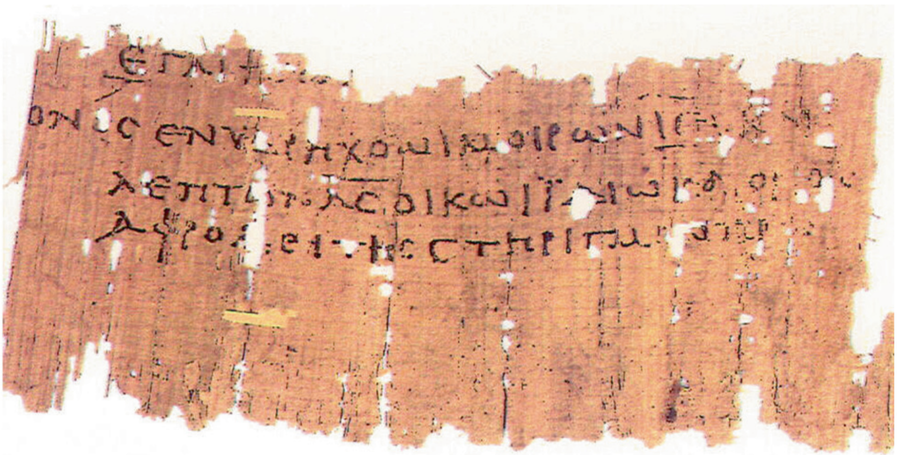


Fig. 1 P. Oxy. astron. 4281 (copyright Egypt Exploration Society)

only known case in which a documentary text refers to the Ptolemaic Terms, it is better not to stress the meager evidence, especially because the early date (2nd c. A.D.) makes it unlikely that we are dealing with a reception of Ptolemy.[71]

Besides the original horoscopic texts, horoscopic boards deserve our attention. These so-called πίνακες (*pínakes*) were used by astrologers during consultations with their clients to illustrate the heavenly alignment at a person's birth.[72] Take, for instance, the astrological tablets from Grand (Fig. 2).[73] These ivory tablets were produced in Egypt and made their way to northern France where they were destroyed c. 170 A.D. and thrown into an ancient sanctuary's well.[74] On such boards, various semi-precious stones were used as planetary markers in order to visualize the positions of the planets and luminaries in the twelve zodiacal signs.[75] On the tablets from Grand, the Terms are clearly indicated with Greek numerals on a ring between the zodiacal signs and the Egyptian decans (Fig. 2). In the signs Aquarius, Pisces, and Aries, the sequence is (beginning counter-clockwise from Aquarius): ζ ς ζ ε ε / ιβ δ …, i.e. 7 6 7 5 5 / 12 4 … Although the respective planets are not specified, this sequence clearly refers to the Egyptian Terms, as the above Table 6 shows. A similar find is the fragment that remains of the *Tabula Bianchini* (Fig. 3), a marble board found on the Aventine Hill in Rome in 1705 and now in the Louvre; it can be dated to the second or third century A.D.[76] In each of these cases the ring that indicates the respective extension of the Terms is positioned between the inner ring(s)[77] of the zodiacal constellations and the outer ring of the Egyptian decans. Whenever such boards indicate the Terms, they refer to the traditional Egyptian system, not to Ptolemy's.

Fig. 2 Tablets from Grand (detail)[78]

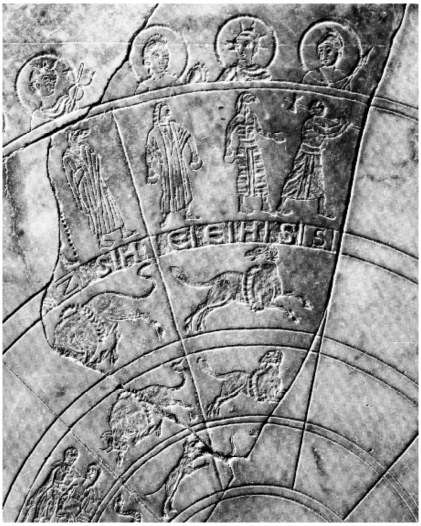


Fig. 3 *Tabula Bianchini* (detail)[79]

Literary Texts

In the field of literary texts, our earliest sources on astrological Terms are difficult to date: Besides the already mentioned Critodemus (early 1st c. A.D.)[80] there is Teucer of Babylon who must have written around the same time, certainly before Manilius (c. 14 A.D.) who drew on him. However, it is only through a late excerpt by Rhetorius (7th c. A.D.) that we know Teucer's chapter about the twelve zodiacal signs in which the complete table of the Egyptian Terms is quoted.[81]

Our first reliable testimony of the Terms is the astrological poem of Dorotheus of Sidon from around 70 A.D.[82] The preserved Greek fragments of this poem include a full set of mnemonic verses.[83] This complete hexametrical versification of the Egyptian table of Terms leaves no doubt about the authenticity of both the sequence and the extension of each single Term. We can therefore be sure that the irrational order of the Egyptian Terms is not due to scribal errors in the course of textual transmission.[84]

Antigonus of Nicaea (c. 150 A.D.) is the author of the earliest preserved literary horoscopes that mention the Terms. Since Antigonus stands in the tradition that harks back to Pseudo-Nechepso and Petosiris, he employs the Egyptian system.[85] As far as we can tell from the preserved fragments of his manual, Antigonus did not know Ptolemy.

Vettius Valens of Antioch (c. 175 A.D.) is slightly later than Ptolemy, but his *Anthologiai* (ed. Pingree 1986) contain no reference at all to Ptolemy's *Apotelesmatika*. Apparently the two authors did not know each other. Valens reports the

Egyptian Terms in full, but those of Libra are corrupt in the only preserved manuscript (Marc. gr. 314) on which the edition of the relevant chapter (1.3) is based: while they should be ♄6 ☿8 ♃7 ♀7 ♂2 (see above Table 2), Val. 1.3.31–35 gives ♄6 ☿5 ♃8 ♀7 ♂4. The total is still 30.[86]

The first two authors who possibly mention the Ptolemaic Terms are Antiochus of Athens and Sextus Empiricus (both late second century). Note, however, that both cases are far from being certain. As to Antiochus, we do not have the original text of his astrological manual called *Treasures* (Θησαυροί, *Thēsauroí*), but an epitome of it made by Rhetorius in the early seventh century. This late epitome explicitly mentions the disagreement between Ptolemy and the Egyptians on the Terms, but a few chapters of this epitome are evidently taken from sources later than Antiochus. Therefore we cannot be sure that the chapter on the Terms is genuine. The relevant lines read as follows: "Now Ptolemy did not agree with the Egyptians on some Terms. Therefore I had to mention these, too. And his Terms agree only in their effects" (i.e. with the Egyptian ones, not in their sequence and/or extension).[87] As to Sextus, we do have his original text, but Sextus never in his whole work mentions the name of Ptolemy. The chapter in question only says that there is a considerable disagreement among astrologers on the Terms.[88]

The first certain reception of Ptolemy's *Apotelesmatika* is Porphyry's introduction to it, written in the late third century. However, Chapter 49 which deals with the Terms is a very late appendix to this introduction, made by Demophilus in the tenth century, who drew the material from Rhetorius (7th c.).[89] Therefore Porphyry adds nothing, as far as the Ptolemaic Terms are concerned, to what has already been said on behalf of Antiochus. There is, however, a curious reference to the Egyptian Terms in a fragment of Porphyry's treatise "On what depends on us" (Περὶ τοῦ ἐφ' ἡμῖν), whose authenticity cannot be doubted.[90] In the myth of Er at the end of Plato's *Republic*, the choice of their future lives that the souls take is most important with regard to their freedom. Porphyry wonders how Plato came to imagine his technical explanation of the correspondence between each single soul's choice and its subsequent individual live on Earth. He suggests that Plato was inspired by Egyptian astrology, especially by the Egyptian doctrine of the Terms.[91] The decisive lines say that the first degrees of each zodiacal sign, being allotted to the governor of that sign, were traditionally considered to be of large extension while the last degrees in each sign were given to the so-called evil-doers among the planets (i.e. Mars and Saturn). These two conditions are then reflected in the lives of the souls that pass through the respective degrees of the revolving zodiac into the world, the former ones being privileged, the latter ones straitened.[92] There can be no doubt that Porphyry means the traditional Egyptian system of Terms.[93] No matter how bizarre this association with Plato's *Republic* is, it proves that the Egyptian system of Terms, which was in all likelihood devised in the second or first century BCE, enjoyed the reputation of a much higher age.

Roughly contemporary with Porphyry is Pancharius who wrote a commentary on the *Apotelesmatika*.[94] This text is lost except for some fragments from the section that was devoted to Ptolemy's famous chapter on the length of life (3.11).[95] It would be interesting to know if and to which effect Pancharius discussed the Ptolemaic Terms because the preserved fragments show that his general attitude towards Ptolemy was very critical.

Firmicus Maternus (c. 335 A.D.) and Paulus Alexandrinus (378 A.D.) both give the full table of the Egyptian Terms without mentioning the existence of the Ptolemaic system.[96] Note that there are scholia (explanatory notes) of uncertain date to the astrological manual of Paulus Alexandrinus.[97] One of these informs the reader that Paulus' Terms are the Egyptian ones, not those from the old book found by Ptolemy.[98] Another one points to the relevance of the totals of the terms to the maximum life expectancy.[99]

Hephaestio of Thebes

It is only in the fifth century that we find the first explicit, securely datable references to Ptolemy's Terms in the work of Hephaestio of Thebes.[100] Hephaestio was born on Nov. 26, 380 A.D.[101] and must therefore have written in the early fifth century.[102] He draws most of his material from Ptolemy and Dorotheus. Therefore it is no wonder that he quotes, for each single zodiacal sign, first the Egyptian Terms according to Dorotheus[103] and then the alternative arrangement according to Ptolemy.[104] Hephaestio abstains from any sort of commentary or judgement on the two systems. On closer examination, one finds that his report is in partial disagreement with the sequence and extension of the Ptolemaic Terms as printed in the authoritative edition of Ptolemy's *Apotelesmatika* by Hübner 1998.[105] Since a versified and indisputable account like Dorotheus' of the Egyptian Terms does not exist in Ptolemy's case, and since no ancient board (πίναξ) with the Ptolemaic Terms inscribed on it has ever been found, it is difficult to verify the authenticity of the transmitted sequence and extension of his Terms. This difficulty is further increased by the circumstance that Ptolemy's explanatory remarks on which he believes to be the principles underlying the "old manuscript's" system (Apotel. 1.21.22–27) provide some insight into its rationale but are insufficient to account satisfactorily for each single detail. In other words: We cannot verify the data transmitted in Apotel. 1.21.28–29 with certainty by applying the principles laid out in Apotel. 1.21.22–27. Under these circumstances the testimony of Hephaestio is to be taken into serious consideration, especially because we know that in his time the textual transmission of Ptolemy, which originated from a common ancestor ω,[106] had already split up into three branches designated ψ, α, and β by Hübner[107] and that Hephaestio's quotations and excerpts from Ptolemy represent an independent testimony that lends support now to this, now to that branch of the direct transmission, sometimes even being the only source for the correct reading, against all direct manuscripts.[108]

Before comparing the Ptolemaic Terms in Ptolemy's and Hephaestio's manuscripts (see Table 7 below), some further remarks on these manuscripts are needed. In a recent article on the Ptolemaic Terms, D. Houlding (2007, p. 266 n. 10 and p. 277) emphasizes the fact that the extant Greek manuscripts of the *Apotelesmatika* are mostly late, from the fourteenth and fifteenth centuries, and that only one manuscript (Vat. gr. 1038, = **V**), the oldest of all, is from the thirteenth century, still more than a millennium after the date of Ptolemy's composition of the text. Therefore, Houlding argues, the indirect testimony of earlier sources is particularly valuable. This inference sounds convincing,[109] and it does so even more when considering the fact (overlooked by Houlding) that the 13th c. manuscript **V** omits the table of Ptolemaic Terms in Apotel. 1.21.28–29,[110] thereby leaving the testimony to manuscripts from the fourteenth and fifteenth centuries. But Houlding's premise calls for correction. Her statement that we have no direct manuscript tradition of Ptolemy's *Apotelesmatika* older than the thirteenth century, for which she refers to the English introduction of Robbins 1940, is true only for the *complete* manuscripts, not for the *incomplete* ones. Robbins was ignorant of the Florentine manuscript Laur. gr. 28,34 (**L**) from the eleventh century, the most important copy of that lost ninth-century anthology of astrological texts commonly known as *Syntagma Laurentianum*.[111] **L** contains various excerpts from the *Apotelesmatika*, including Chapter 1.21 on the Terms. The decisive table of the Ptolemaic Terms is on fol. 149^{r}, preceded by the explanatory text of Apotel. 1.21.22–27 on fol. 148^{v}.[112] It is on **L**, which has not been used at all by Robbins 1940, that Hübner 1998 based his edition of the Ptolemaic table of Terms. Hübner did so because **L** stems from the highly valuable subarchetype **ψ**, as does **V**.[113] The total agreement of **L** and **V** wherever their readings can be compared shows that we have here the text of the *Apotelesmatika* as it was in **ψ** in the early ninth century.[114] And there is more: We have an excerpt of nothing but Apotel. 1.21.28–29, the Ptolemaic table of Terms, in ms. Vatic. gr. 1291, a deluxe manuscript of Ptolemy's *Handy Tables* that can securely be dated to 813–820 A.D.[115] Hübner 1998 used this important manuscript (plus **L**) in his edition of Apotel. 1.21.28–29[116] but did not assign it a *siglum* (identification letter) in his list of incomplete manuscripts.[117] I shall henceforth call it **z**. The excerpt containing the Ptolemaic table of Terms is on fol. 3^{v}, written—according to Boll—in a somewhat later uncial script ("etwas spätere Unciale") then the rest of **z**.[118] This seems to indicate a date still in the ninth century. The sequence and extension of the Ptolemaic Terms are identical in **L** and **z** (and confirmed by the later manuscripts from the fourteenth and fifteenth centuries). To conclude this excursus, it deserves to be emphasized that within the direct transmission of Ptolemy's *Apotelesmatika* we have plenty of evidence from the ninth century—five centuries earlier than the manuscripts used by Robbins 1940!—for the data contained in the Ptolemaic table of Terms.

Now back to the indirect transmission in Hephaestio's manual. The only two existing manuscripts of Heph. 1.1 are Paris. gr. 2841 (**A**) and 2417 (**P**), both from the thirteenth century. **L** and **z** agree with **A** and **P** insofar as the total of all Terms is always 360°. But the individual sums of Terms assigned to each of the five planets present a small discrepancy insofar as **AP**, compared to **Lz**, assign 1° more to Mars and 1° less to Venus. This slight deviation from the canonical values in Table 3 (above) tends to discredit Hephaestio's account.[119] Interestingly, among the four Byzantine epitomes of Hephaestio's manual, which are neglected by many scholars and have not been included in Schmidt's English translation of 1994, there is one (n. 4) that contains the relevant first chapter of the first book. In this epitome the account of the Ptolemaic Terms[120] is basically identical with the direct transmission in **L** and **z**, as the following table shows.

Table 7 Comparison of the Ptolemaic Terms in Ptolemy's and Hephaestio's manuscripts ("*a*" stands for *agreement*)[121]

<table>
<tr><th></th><th>Ptol. Apotel. 1.21.28-29
(Lz)</th><th>Heph. epit. 4.1
(IJKM)</th><th>Heph. 1.1
(AP)</th></tr>
<tr><td>♈</td><td colspan="3">a a a a a</td></tr>
<tr><td>♉</td><td colspan="2">a a a ♄4 ♂4</td><td>a a a ♂6 ♄2</td></tr>
<tr><td>♊</td><td colspan="3">a a a a a</td></tr>
<tr><td>♋</td><td colspan="3">a a a a a</td></tr>
<tr><td>♌</td><td colspan="2">♄6 ☿7 ♀6 ♃6 a</td><td>♃6 ♀6 ☿7 ♄6 a</td></tr>
<tr><td>♍</td><td colspan="3">a a a a a</td></tr>
<tr><td>♎</td><td>a a ♃8 ☿5 a</td><td>a a ☿8 ♃5 a</td><td>a a ☿5 ♃8 a</td></tr>
<tr><td>♏</td><td colspan="2">a ♃8 ♀7 a a</td><td>a ♀7 ♃8 a a</td></tr>
<tr><td>♐</td><td>a ♀6 a ♄6 ♂5</td><td>a ♀6 a ♂6 ♄4[122]</td><td>a ♀5 a ♄6 ♂6</td></tr>
<tr><td>♑</td><td colspan="2">a a a ♂6 ♄5</td><td>a a a ♄6 ♂5</td></tr>
<tr><td>♒</td><td colspan="3">a a a a a</td></tr>
<tr><td>♓</td><td colspan="2">a a a ♂6 ♄4</td><td>a a a ♂5 ♄5</td></tr>
</table>

The manuscripts of Hephaestio's fourth epitome (**IJKM**, 14th c.) are independent of and often more reliable than those of the main stream of transmission (**AP**, 13th c.), especially when it comes to numerical values.[123] Nevertheless they may in this particular case be of little or no value because it is well possible that the common ancestor of **IJKM** shared the data given by **AP** and was then corrected by someone using a manuscript of the direct transmission of Ptolemy.[124] This might explain why the epitome, unlike Hephaestio's main text, gives also the intermediate totals after addition of each new Term, just as the table in Ptolemy's *Apotelesmatika* does.[125]

If we assume, instead, that epitome n. 4 preserves the original values given by Hephaestio, it is difficult to explain how the data in **AP** which are scattered over many pages of text[126] have been modified here and there with regard to various signs without ending up far from the required total of 360° and the canonical sums of each planet's Terms. Therefore it seems that Hephaestio did actually offer a table of the Ptolemaic Terms which was different from the direct transmission in **L**, **z**, and later manuscripts. It is an issue of current scholarly debate which one of the two is to be preferred. In the course of this ongoing review of the reception of Apotel. 1.21, we shall return to this question.

Eutocius of Ascalon

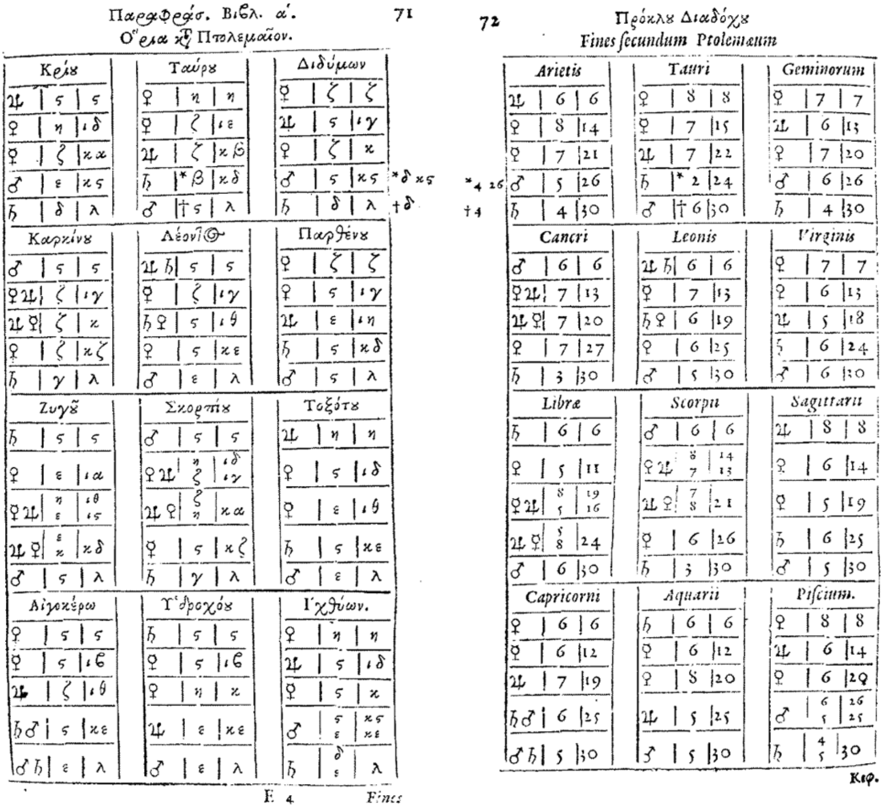
An important case of the reception of the Ptolemaic Terms in Greco-Roman antiquity is a horoscope of considerable length that Eutocius of Ascalon composed in the early sixth century as a didactic example in his work *Astrologumena*.[127] Eutocius is better known for his commentaries on various works of Archimedes and for his edition with commentary of the first four books of the *Conics* of Apollonius of Perge.[128] However, he is also credited by al-Nadīm (10th c.), the biographer of Islamic writings, with being the author of a commentary on the first book of Ptolemy's *Apotelesmatika*[129]—an interesting piece of information, provided that it does not refer to a pseudepigraphon. We cannot tell if this commentary is identical with the *Astrologumena* mentioned above. From this latter work, which is now lost, the horoscope was excerpted by Rhetorius in the early seventh century and thus preserved (Rhet. 6.52). Only a small part of this eight page horoscope has been published a century ago in the *Catalogus Codicum Astrologorum Graecorum* (CCAG). The full text will first become available in print in the late David Pingree's edition of Rhetorius.[130] Eutocius' didactic horoscope is for the alignment of Oct. 28, 497 A.D.,[131] which may be his own birthdate.[132] It does not contain predictions of the future but an extensive, exemplary discussion of the planetary positions, the cardinal points, and the astrological lots (Rhet. 6.52.7–38). Eutocius specifies, in the case of each ecliptic longitude that he mentions, first the Terms according to Ptolemy and then the Terms according to the Egyptians.[133] He thereby inverts the sequence that we encountered in the work of Hephaestio of Thebes. Both authors express their admiration for Ptolemy by calling him "divine."[134]

Eutocius' juxtaposition of the Ptolemaic and Egyptian Terms is curious insofar as it is, methodologically speaking, slightly inconsistent. Eutocius explicitly states that he calculated all longitudes without converting the tropical data into sidereal ones (a practice that was widely spread among astrologers in late antiquity), thereby following the authority of Ptolemy.[135] By doing so, he refers his tropical data to one system of Terms that is meant to be employed thus (Ptolemy's)[136] and at the same time to another system of Terms (the Egyptian one) that had been devised for sidereal data and allegedly grown out of the analysis of nativities computed in sidereal longitudes.

This practical application will provide us with a unique opportunity to check the single data against the competing versions of the Ptolemaic table of Terms.[137] First, however, we need to take two more texts into consideration, the so-called *Proclus Paraphrase* and the *Anonymous Commentary*.

The Proclus Paraphrase

Under the name of the philosopher Proclus (412–485 A.D.) a paraphrase of Ptolemy's *Apotelesmatika* has come down to us, but its authenticity is very doubtful.[138] This paraphrase aims to provide a more easily understandable version of the difficult original text, while keeping the first person of Ptolemy speaking.[139] There is no modern critical edition of this text. The oldest preserved manuscript (Vatic. gr. 1453) dates from the tenth century, which means that it does not—as some think—antedate all manuscripts of the direct transmission, at least not in the chapter on the Terms (1.21).[140] The text was first printed in 1554 by Philipp Melanchthon in Basel and then again in 1635 by the Elzevir typesetters in Leyden together with Leo Allatius' Latin translation of it. Regrettably, on both occasions the old MS Vatic. gr. 1453 was not used.[141] The anonymous preface to the edition of 1635 states that text and translation were printed from a manuscript copy that escaped Allatius' control and had neither been authorized nor intended for print.[142] Ptol. Apotel. 1.21 corresponds to Procl. paraphr. 1.23–24 pp. 60–72 in the edition of 1635. As to the Ptolemaic table of Terms, the paraphrase briefly mentions its alleged source, the old manuscript. But all information about its physical appearance (Apotel. 1.21.20–21) is omitted,[143] except for the later reference to certain dots marking a specific sort of extra degrees.[144] The Ptolemaic table of Terms (Fig. 4) is puzzling because in several cases it assigns alternative sequences of planets or extensions of their Terms.



Παραφράσ. Βιβλ. α΄. 71

Ὅρια κατὰ Πτολεμαῖον.

Κριοῦ			Ταύρου			Διδύμων		
♃	ς	ς	♀	η	η	☿	ζ	ζ
♀	η	ιδ	☿	ζ	ιε	♃	ς	ιγ
☿	ζ	κα	♃	ζ	κβ	♀	ζ	κ
♂	ε	κς	♄	*β	κδ	♂	ς	κς *δ κς
♄	δ	λ	♂	†ς	λ	♄	δ	λ †δ
Καρκίνου			**Λέοντος**			**Παρθένου**		
♂	ς	ς	♃♄	ς	ς	☿	ζ	ζ
♀♃	ζ	ιγ	☿	ζ	ιγ	♀	ς	ιγ
♃☿	ζ	κ	♄♀	ς	ιθ	♃	ε	ιη
♀	ζ	κζ	♀	ς	κε	♄	ς	κδ
♄	γ	λ	♂	ε	λ	♂	ς	λ
Ζυγοῦ			**Σκορπίου**			**Τοξότου**		
♄	ς	ς	♂	ς	ς	♃	η	η
♀	ε	ια	♀♃	η ζ	ιδ ιγ	♀	ς	ιδ
☿♃	η ε	ιθ ις	♃♀	ζ η	κα	☿	ε	ιθ
♃☿	ε η	κδ	☿	ς	κζ	♄	ς	κε
♂	ς	λ	♄	γ	λ	♂	ε	λ
Αἰγοκέρω			**Ὑδροχόου**			**Ἰχθύων.**		
♀	ς	ς	♄	ς	ς	♀	η	η
☿	ς	ιβ	☿	ς	ιβ	♃	ς	ιδ
♃	ζ	ιθ	♀	η	κ	☿	ς	κ
♄♂	ς	κε	♃	ε	κε	♂	ς ε	κς κε
♂♄	ε	λ	♂	ε	λ	♄	δ ε	λ

E 4 Fines

72 Πρόκλου Διαδόχου

Fines secundum Ptolemæum

Arietis			*Tauri*			*Geminorum*		
♃	6	6	♀	8	8	☿	7	7
♀	8	14	☿	7	15	♃	6	13
☿	7	21	♃	7	22	♀	7	20
*4 26 ♂	5	26	♄	*2	24	♂	6	26
†4 ♄	4	30	♂	†6	30	♄	4	30
Cancri			*Leonis*			*Virginis*		
♂	6	6	♃♄	6	6	☿	7	7
♀♃	7	13	☿	7	13	♀	6	13
♃☿	7	20	♄♀	6	19	♃	5	18
♀	7	27	♀	6	25	♄	6	24
♄	3	30	♂	5	30	♂	6	30
Libræ			*Scorpii*			*Sagittarii*		
♄	6	6	♂	6	6	♃	8	8
♀	5	11	♀♃	8 7	14 13	♀	6	14
☿♃	8 5	19 16	♃♀	7 8	21	☿	5	19
♃☿	5 8	24	☿	6	26	♄	6	25
♂	6	30	♄	3	30	♂	5	30
Capricorni			*Aquarii*			*Piscium.*		
♀	6	6	♄	6	6	♀	8	8
☿	6	12	☿	6	12	♃	6	14
♃	7	19	♀	8	20	☿	6	20
♄♂	6	25	♃	5	25	♂	6 5	26 25
♂♄	5	30	♂	5	30	♄	4 5	30

Κεφ.

Fig. 4 Procl. paraphr. 1.24 pp. 71–72 ed. 1635 (Allatius)[145]

These alternative values are already present in the oldest existing manuscript, Vatic. gr. 1453, fol. 50r–v. On closer inspection one finds that they clearly fall into two chronologically discernable categories. I shall start with the later one: Whenever a cell contains two different planetary symbols, the right one corresponds to the data in MSS Laur. gr. 28,34 (**L**) and Vatic. gr. 1291 (**z**),[146] on which the Ptolemaic table of Terms in Hübner's edition (1998) is based, and whenever a cell contains two different numerical values, the upper one corresponds to **L** and **z**.[147] In the oldest manuscript, moreover, one can still observe a feature which is absent from the edition of 1635: some of the right / upper values are rather tiny and squeezed into the upper or lower right corner of the respective cell. See, for example, the data for Scorpio (upper row, middle) or Capricorn (lower row, left) in the following illustration (Fig. 5).

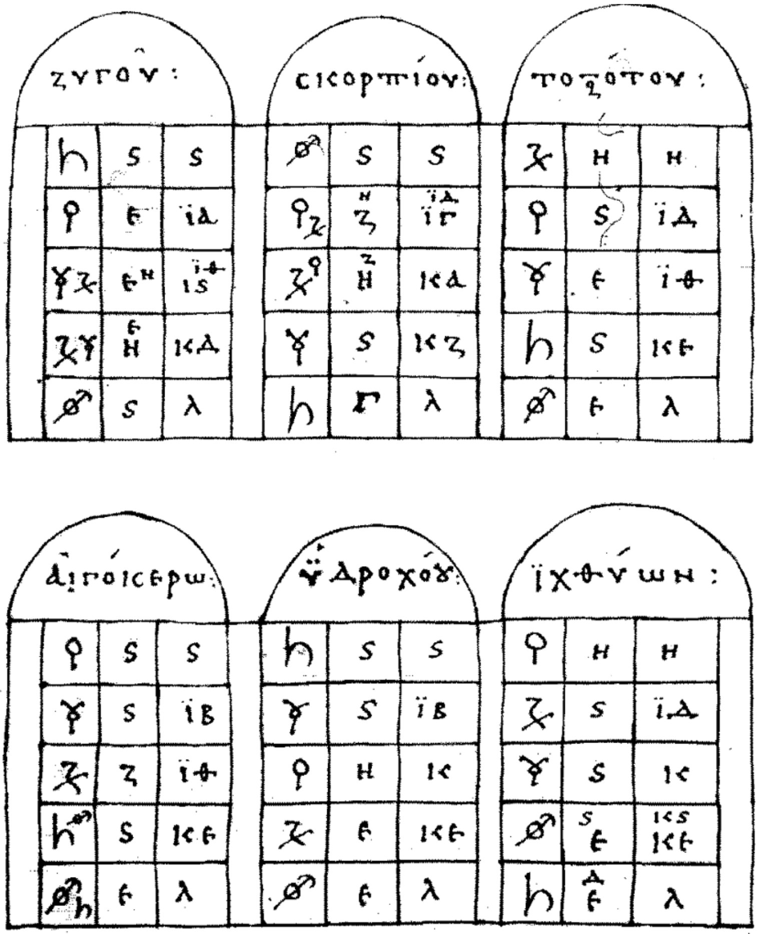


Fig. 5 Vatic. gr. 1453 (10th c.), fol. 50v (Libra—Pisces)

Altogether there can be no doubt that these right/upper values have been added in a second step to the already present left/lower values, and that they are the work of the same scribe. Although it is theoretically possible that the scribe of Vatic. gr. 1453 most faithfully reproduced the appearance of an earlier, now lost manuscript from which he was copying, the simpler and more convincing explanation is that he compared, while copying, his exemplar of the paraphrase to a manuscript of the direct transmission of the *Apotelesmatika*, and that the addenda to the paraphrase's table originated with him, in the tenth century.[148] The fact that the earlier data are not expunged indicates that the addenda are not meant to be corrections but alternative values, added for the sake of completeness.[149]

But what about the earlier values, those that are left/below in a cell? One finds striking correspondences between these values and the Ptolemaic table of Terms as transmitted by Hephaestio Chapter 1.1.[150] These lead me to believe that both Hephaestio and the author of the paraphrase drew on the same (now lost) branch of manuscript tradition. In order to substantiate this hypothesis which—to my knowledge—has not occurred to other scholars so far, it is sufficient to compare Fig. 4 with Table 7 above. One finds that the paraphrase's data concerning the twelve zodiacal signs fall into three groups:

1. Five signs without addenda, because Ptol. Apotel. 1.21.27–28 and Heph. 1.1 agree with each other. These are Aries, Gemini, Virgo, Sagittarius,[151] and Aquarius.
2. Four signs with addenda where all the added values (left/below) agree with Hephaestio's data. These are Libra, Scorpio, Capricornus, Pisces.
3. Three signs with addenda where Hephaestio and the paraphrase disagree because one of them incurred in a lapse while copying. These are Taurus,[152] Cancer,[153] and Leo.[154]

The above hypothesis can further be substantiated through reference to those numerous readings in other chapters of the *Apotelesmatika* on which Hephaestio and the *Proclus Paraphrase* agree against the manuscripts of the direct transmission.[155] However, it would go beyond the purpose of this article to examine those instances systematically.[156] Altogether it looks as if the *Proclus Paraphrase* not only goes back to the same branch of manuscript transmission as Hephaestio's manual but that it underwent, probably at the time when Vatic. gr. 1453 was written (10th c.), the same kind of correction of the Ptolemaic table of Terms based on a comparison with manuscripts of the direct transmission which occurred in the case of the fourth epitome of Hephaestio's text.[157]

The Anonymous Commentary

Another large text in the reception of Ptolemy's *Apotelesmatika* is the so-called *Anonymous Commentary* which was written in Greek at an uncertain date in either late antiquity or, less likely, the Byzantine period.[158] It is preserved in numerous manuscripts, often following or preceding Porphyry's introduction.[159] There is no modern critical edition. The text was printed in 1559 by Hieronymus Wolf from a heavily corrupted manuscript (we do not learn which one) that required numerous conjectures and emendations, and it was accompanied by a Latin translation that was done by a scholar whose name the editor does not reveal.[160] In the title, Wolf informs the reader that some believe the author of the Greek original is Proclus. However, that attribution looks like guesswork, originating from the temptation to ascribe further works to the same author who was already credited with having composed the *Paraphrase.* Probably none of the two works is Proclus', and it seems best to attribute them to two distinct Anomymi because of factual disagreements in both texts, for instance with regard to the Ptolemaic Terms. Since the anonymous commentator quotes Porphyry, he must be later than c. 300 A.D.[161] Two horoscopes contained in this commentary have been dated by D. Pingree to Dec. 22, 175 A.D., and July 29, 241 A.D.[162] This may, with all due caution, be taken as an argument for a date of composition before the end of antiquity. The section devoted to Apotel. 1.21 is on pp. 39–47 of Wolf's Renaissance edition.

This author is the first to assert explicitly that the Egyptian Terms are false, "a product of their inventors' quest for idle glory," while the Ptolemaic ones are true.[163] He does not question the existence of Ptolemy's old manuscript and remarks that its author seems to have combined the Egyptian Terms with the Chaldean ones, thus bringing the doctrine to perfection.[164] This may actually be what Ptolemy himself did. The commentator rightly interprets Ptolemy's remark about the dots for extra degrees as referring to the old manuscript.[165] Eventually he declares that Ptolemy's general explanation of the rationale underlying the old manuscript's Terms (Apotel. 1.21.22–27) calls for a discussion of each single instance, in order to make things clear.[166] What follows is a complicated discussion on which Bouché-Leclercq remarked: "Ce chapitre du scoliaste (pp. 44–47) est un spécimen curieux des tours de force de la logique obligée de justifier un dogme préexistant".[167]

At the end the commentator announces, for the sake of still greater clarity,[168] a concluding table which however is missing in the manuscript tradition as well as in Wolf's edition.[169] The commentary's data are in perfect agreement with Hübner's table of the Ptolemaic Terms except for Gemini where Hübner 1998 (based on **L**, **z**, and the *Proclus Paraphrase*, and in agreement with Hephaestio) has ☿7 ♃6 ♀7 ♂6 ♄4 while the *Anonymous Commentary* assigns ☿7 ♃7 ♀7 ♄4 ♂5. Note that this means 1° more for Jupiter, at the expense of Mars. How the implicit violation of the canonical totals of the planetary Terms of Jupiter and Mars can be justified, the commentator does not say; on the contrary: he announces the final (now missing) table saying that it will show the agreement of his planetary totals with the canonical values of the writers of old.[170] Maybe the damaged state of the Greek manuscript used for Wolf's edition is to be taken into account.[171] Bezza & Fumagalli as well as Houlding 2007 made admirable efforts to explain the rationale of the Ptolemaic Terms, taking the anonymous commentator's application of the principles laid out by Ptolemy himself (Apotel. 1.21.22–27) a big step further.

Their analyses do shed light on many details, but they also demonstrate that a coherent explanation of the whole system remains impossible. We shall return to this later.

Concluding Remarks on Greco-Roman Antiquity

Besides the texts mentioned so far, there is only one left that deserves to be mentioned in passing: The lost Greek original of Chapter 25 of the *Liber Hermetis*, which we know through its medieval Latin and Picard translations, specified (among other things) the full set of Egyptian Terms.[172] It did not mention the Ptolemaic Terms.

We find, thus, that there are some, though not many references to the Ptolemaic Terms in ancient astrological manuals. And the didactic horoscope of Eutocius of Ascalon is the only practical application of the Ptolemaic Terms in all that remains of ancient literature, including the more than threehundred original and literary Greek horoscopes.[173] As a mathematician, Eutocius is a representative of the exact sciences in antiquity, which may account for his willingness and ability to comply with Ptolemy's teaching. All other practical applications follow the Egyptian system, and there are even traces of its reception outside the strictly speaking astrological area, as Porphyry's remarks on Plato have shown.[174] The only author to embark upon a critical comparison of the Egyptian and the Ptolemaic Terms is the Anonymous Commentator, who may already belong to the Byzantine period. No ancient author ever questions the existence of Ptolemy's old manuscript.

The data of the Ptolemaic table of Terms have not come down to us with the same degree of certainty as the Egyptian ones. Besides the main line of this table's transmission, which is represented by the direct manuscripts (esp. **L** and **z**), the addenda of the *Proclus Paraphrase*,[175] and—except for a peculiarity in Gemini[176]—also by the *Anonymous Commentary* (henceforth: group A),[177] there must have been, latest by c. 400 A.D., a slightly different version that is represented by Hephaestio of Thebes, the original recension of the *Proclus Paraphrase*, and—as we shall see—an Arabic tradition and its Latin followers (henceforth: group B).

It remains to check Eutocius' data against these competing versions of the Ptolemaic table of Terms. Fortunately, in three out of twelve cases the longitudes computed by Eutocius fall into Terms on which groups A and B disagree. In all three cases Eutocius agrees with group A, assigning the planetary deities' names as follows[178]: Mars at 17° 35′ Leo: Venus.[179] Group B wants Mercury. Midheaven at 19° 22′ Scorpio: Venus.[180] Group B wants Jupiter. Lot of Fortune at 16° 0′ Leo: Venus.[181] This confirms the first case (Mars). Again, group B wants Mercury. Since in Eutocius' didactic horoscope the Terms were computed and transmitted accurately in all twelve cases for both the Ptolemaic and Egyptian systems, the weight of evidence provided by the three critical cases above is sufficient to conclude that Eutocius lines up with group A.[182]

From the Middle Ages to the Present

In the Middle Ages, Ptolemy's *Apotelesmatika* were unknown to the Latin West,[183] but they enjoyed a wide reception in the Arabic world. They were first translated from Greek into Arabic in the eighth century by Abū Yaḥyā ibn al-Baṭrīq (d. c. 800 A.D.) and commented on by 'Umar ibn al-Farrukhān (d. 815 A.D.). A new commented translation was provided in the ninth century by Ibrāhīm ibn al-Ṣalt, whose translation was, in two successive steps, improved by Ḥunayn ibn Isḥāq (c. 809–873 A.D.) and then shortened by Thābit ibn Qurrah (836–901 A.D.). This last version is preserved. Further commentaries were composed by al-Nayrīzī (d. 922/23), al-Battānī (lat. Albatenius, d. 929/30), and 'Alī ibn Riḍwān (988–1068).[184]

As to the Terms, the astrologers used the Egyptian system, despite the generally high reputation of Ptolemy. This is true especially of Abū Ma'shar (Lat. *Albumasar*, 787–886 A.D.), one of the most influential Arabic astrologers. In Chapters 5.8–13 of his *Great Introduction to Astrology*,[185] he discusses five different systems:

> We found the terms to be of five kinds. The first of them is the terms of the people of Egypt, the second, those of Ptolemy, the third, those of the Chaldeans, i.e. the people of Babylon, the fourth, those of Astratu,[186] and the fifth, those of the Indians.[187]

As he asserts in his conclusion, "the most correct of these terms are those of the people of Egypt" (5.13.4).[188] Abū Ma'shar accepts Ptolemy's report on the old manuscript without discussion (5.8.9).[189] He presents a contaminated table of the Ptolemaic Terms (5.10) whose data match neither group A nor group B entirely.[190] Note that Abū Ma'shar tends erroneously to distinguish the author of the *Apotelesmatika* (*Tetrabiblos*) from that of the *Syntaxis* (*Almagest*), while identifying the latter one with one of the kings of Egypt.[191]

From about the same time we have an interesting didactic horoscope cast by a certain Aleim, son of the Jew Isaak, for an anonymous person born on September 30, 858 A.D. Aleim was possibly a disciple of Abū Ma'shar. Aleim's didactic horoscope is 35 printed columns long and based on Ptolemy, who is also the only authority explicitly mentioned in the text. However, the references to the Terms agree with the Egyptian system, a fact that Aleim does not deem worthy of explanation.[192]

It is again in the ninth century, in the astrological writings of al-Kindī (801–866 A.D.), that we can first grasp a hierarchy of the planetary dignities that had not existed in antiquity, at least not explicitly. This scale of numerical values is: "House" 5, "Exaltation" 4, Term 3, "Triangle" 2, "Decan" 1. It allows to add up each planet's various dignities and to compare them, so as to determine with mathematical accuracy which is pre-eminent.[193]

A modified version of this scale was devised by al-Qabīṣī (Lat. *Alcabitius*, fl. c. 950). In his *Introduction to Astrology*, he preferred the "Triangles" to the Terms while admitting that the sequence between these two is interchangeable. Al-Qabīṣī illustrates his hierarchy by linking it to human society, a metaphor that reminds us of the basically anthropomorphic character of the astrological planetary deities.[194] Al-Qabīṣī presents the Terms according to the Egyptian system.[195]

Al-Bīrūnī (973–1048 A.D.) discusses the Terms in Chapter 453 of his *Book of Instruction in the Elements of the Art of Astrology*.[196] He first mentions the three systems of the Chaldeans, of Astaratus,[197] and of the Hindus, adding that "none of these are employed by professional astrologers, who are unanimous in using the Egyptian terms, because they are more correct." Nevertheless Ptolemy's Terms deserve some attention, as the following makes clear: "Those who have expounded Ptolemy's works use the terms which he records having found in an old book, and which he has inserted in his Tetrabiblos. We have constructed a table showing both the Egyptian and the Ptolemaic terms: there is no use discussing any others."[198] The left half of the following table contains the Egyptian Terms, wich are all correct. The Ptolemaic data (right half) agree throughout with those of **L**, **z**, and Hübner 1998, especially in those cases where Hephaestio's group disagrees.[199]

'Alī ibn abī-r-Rijāl (Lat. *Abenragel*, fl. c. 1050 A.D.), in *De iudiciis astrorum* 1.5, summarizes the reception of the Terms in words that are reminiscent of Abū Ma'shar's assessment quoted earlier. Abenragel knows of five rivaling systems that he assigns to the Egyptians, Ptolemy, the Chaldeans, the Indians, and Attaratyh.[200] There is a hierarchical order that underlies this list because Abenragel explains: "The opinion of the majority of men which is more truthful and more based on empirical proof is the opinion of the Egyptians; and only a few use the Terms of Ptolemy. And the other systems have no followers at all, and people do not care about them because they are plainly wrong."[201]

Some further interesting remarks can be found in the commentary that 'Ali ibn Riḍwān (Lat. *Haly*, 988–1068 A.D.) wrote on the *Apotelesmatika*.[202] In his introduction, he adduces ample evidence for the authenticity of that work. He criticizes his predecessor Abū Ma'shar and certain unnamed historians for believing that the true author of the *Apotelesmatika* was not the scientist Claudius Ptolemy but one of the homonymous kings of Egypt, namely Ptolemy II Philadelphus (308–246 BCE), second king of the Ptolemaic dynasty.[203] With regard to Chapter 1.21, he thinks that the system of the Terms in Ptolemy's old manuscript is not reconcilable with Ptolemy's own criteria as put forth in his criticism of the Egyptian and Chaldean systems. Therefore Haly asserts that Ptolemy cannot be the author of the so-called Ptolemaic Terms and that he is rather to be praised for his honesty in reporting the find of the ancient book without any attempt at attributing its content to himself.[204] He eventually recommends that in astrological practice the Terms should first be analyzed according to the Egyptian system, next according to Ptolemy's, and last according to the Chaldeans.[205] At the end of his commentary, Haly discusses three exemplary nativities (the first one is his own) employing exclusively the Egyptian system of Terms.[206]

That the Egyptian Terms continued to be the most important system, as they had been in the Greek world, is also clear from a work by a contemporary of Haly, the philosopher Georg of Antioch (11th c.). The Arabic original of his treatise is lost, but a Latin translation—probably by Gerhard of Cremona—survived.[207] In this *Liber de astronomiae disciplinae peritia*, the full Egyptian table of Terms is given.[208] No other systems are mentioned.

As my Latin quotations show, most of the Arabic sources became available in Latin translations from the twelfth century onwards, thanks to an increasing interest of that period in the natural sciences. The *Apotelesmatika* themselves were translated three times from the Arabic, first in 1138 by Plato of Tivoli (Plato Tiburtinus, first printed 1551), next by an Anonymous in 1206 (unpublished), and last by Egidio de' Tebaldi (Aegidius de Thebaldis, 13th c., first printed 1484). It was only in the sixteenth century that the Greek original was first printed by J. Camerarius (Nürnberg 1535, 2nd revised ed. Basel 1553). Camerarius also provided the first direct translation from the Greek in his 1535 edition.[209] Other Latin translations from the Greek followed soon after.[210]

While scholars are generally aware of the fact that the first centuries of the *Apotelesmatika*'s reception in the Latin West rely on Arabic intermediaries, it has been pointed out only recently by Houlding that the values for the Ptolemaic Terms recorded by Hephaestio "found a relatively faithful line of transmission through Arabic sources".[211] On inspection of the evidence, I should prefer a slightly modified conclusion: the data of the Ptolemaic table of Terms in Hephaestio, in the *Proclus Paraphrase* (original recension) and in the Arabic version from which Plato of Tivoli's Latin translation originated[212] all seem to go back to a common source that must antedate Hephaestio. Apparently one or two copying errors occurred in each of these three branches of transmission.[213] This does not, however, obscure the fact that they all drew on the same version which, in its turn, is to be sharply distinguished from the transmission in **L** and **z** (supported by Eutocius).[214] Basically identical with **L** and **z** are the data of the *Anonymous Commentary*; they disagree only in Gemini.[215]

Table 8 Synopsis of the transmitted values in Apotel. 1.21.27–28

	A: Ancestor of A1,A2, and Eutocius										*B: Ancestor of Heph., Procl. Par., and Plato of T.'s Arabic source (= Robbins 1940)*[216]				
	A1: L, z (= Hübner 1998)					*A2: Anon. Comm. (= Wolf 1559)*									
♈	♃6	♀8	☿7	♂5	♄4	♃6	♀8	☿7	♂5	♄4	♃6	♀8	☿7	♂5	♄4
♉	♀8	☿7	♃7	♄4	♂4	♀8	☿7	♃7	♄4	♂4	♀8	☿7	♃7	♄2	♂6
♊	☿7	♃6	♀7	♂6	♄4	☿7	♃7	♀7	♄4	♂5	☿7	♃6	♀7	♂6	♄4
♋	♂6	♃7	☿7	♀7	♄3	♂6	♃7	☿7	♀7	♄3	♂6	♃7	☿7	♀7	♄3
♌	♄6	☿7	♀6	♃6	♂5	♄6	☿7	♀6	♃6	♂5					
♍	☿7	♀6	♃5	♄6	♂6	☿7	♀6	♃5	♄6	♂6	☿7	♀6	♃5	♄6	♂6
♎	♄6	♀5	♃8	☿5	♂6	♄6	♀5	♃8	☿5	♂6	♄6	♀5	☿5	♃8	♂6
♏	♂6	♃8	♀7	☿6	♄3	♂6	♃8	♀7	☿6	♄3	♂6	♀7	♃8	☿6	♄3
♐	♃8	♀6	☿5	♄6	♂5	♃8	♀6	☿5	♄6	♂5	♃8	♀6	☿5	♄6	♂5
♑	♀6	☿6	♃7	♂6	♄5	♀6	☿6	♃7	♂6	♄5	♀6	☿6	♃7	♄6	♂5
♒	♄6	☿6	♀8	♃5	♂5	♄6	☿6	♀8	♃5	♂5	♄6	☿6	♀8	♃5	♂5
♓	♀8	♃6	☿6	♂6	♄4	♀8	♃6	☿6	♂6	♄4	♀8	♃6	☿6	♂5	♄5

[216] The synopsis of the transmitted values in Apotel. 1.21.27-28 (Table 8) shows that the data of group B disagree in 15 out of 60 Terms with group A1. While the data in B for Leo, Libra, and Scorpio are different from A only with regard to the planetary sequence, which could theoretically be due to copying errors in either A or B, the data in B for Taurus, Capricorn and Pisces contain numerical discrepancies. This can hardly be due to negligence in copying because both peculiarities that B offers in Taurus, the subtraction of 2° from Saturn and the addition of 2° to Mars, are later made up in Capricorn and Pisces, so as to restore the planetary totals. In other words: Someone must have intentionally modified the data, probably as a result of Term by Term application of the rules laid out by Ptolemy (just as the Anonymous Commentator did). But this insight does not answer the question whether A1 represents Ptolemy's original values and B is the result of later modification or viceversa (the only certain thing is that the data in A2 are a modification of A1).[217] We cannot even exclude with certainty that Ptolemy himself revised the table in 1.21.28–29 at some point, but one should abstain from resorting all too easily to such hypotheses. We shall return to these questions later. Let us first examine the Western readers' reception of the Latin translation (*Quadripartitum*) in the context of other astrological works that had equally been translated from Arabic.

An early example is Guido Bonatti.[218] In his *De astronomia* (c. 1277 A.D.), which was reputed "the most important astrological work produced in Latin in the thirteenth century,"[219] he discusses the Terms in Chapter 1.14[220] with references to Abū Ma'shar's overview of the five systems of Terms, to Haly's remarks on Ptolemy's modesty, and to al-Qabīṣī's hierarchy of the planetary dignities.[221] Bonatti does not question Ptolemy's manuscript find and is (rightly) convinced that the Alexandrian scholar preferred that system which he presented last.[222] Unlike his Western predecessor Herman of Carinthia (1140 A.D.),[223] Bonatti is more impressed with systematic consistency than with empirical arguments. Therefore he embarks upon a long analysis of the Ptolemaic Terms, of which he provides a table, while dismissing all other systems.

Bonatti's contemporary Roger Bacon (c. 1214–1294) applies the Ptolemaic doctrine of the Terms to the Persian and Arabic doctrine of the Great Conjunctions. Bacon was particularly fascinated with Abū Ma'shar's teaching that a specific conjunction of Jupiter and Mercury in Virgo had brought about Christianity, and that a similar one of Jupiter and the Moon would bring about the arrival of Antichrist. For Abū Ma'shar the iconography of the decans had been important in this context, because the first decan of Virgo (1°–10° ♍) was represented as a woman holding a child, which Abū Ma'shar interpreted as Mary with the Infant Jesus.[224] This ultimately goes back to the ancient Egyptian iconography of the decans which depicted the first decan of Virgo as Isis holding the child Horus. Now Bacon in his *Opus magnum* reports how impressed he was with the discovery that both the Egyptians and Ptolemy assign the first 7° of Virgo to Mercury (see above Table 4), the planet assigned to Christianity.[225] In other words: Bacon finds an important detail of his religious world view confirmed through application of the Ptolemaic Terms to historical astrology which did not yet exist at Ptolemy's time.[226]

Cardinal Pierre d'Ailly (1350–1420), who undertook a much more systematic reconciliation of Christian religion and astrology,[227] refers to the Egyptian system when mentioning the Terms. See, for instance, his (hitherto unpublished) *Tractatus de figura inceptionis mundi et coniunctionibus mediis sequentibus* (c. 1414).[228] D'Ailly is representative of the fact that most astrologers followed the Egyptian Terms. Among the numerous pieces of evidence that could be adduced for this, suffice it to select the following manuscript illustration (Fig. 6). We have here exactly the same allocation of the Terms as more than a millennium earlier on the tablets from Grand and on the *Tabula Bianchini*.

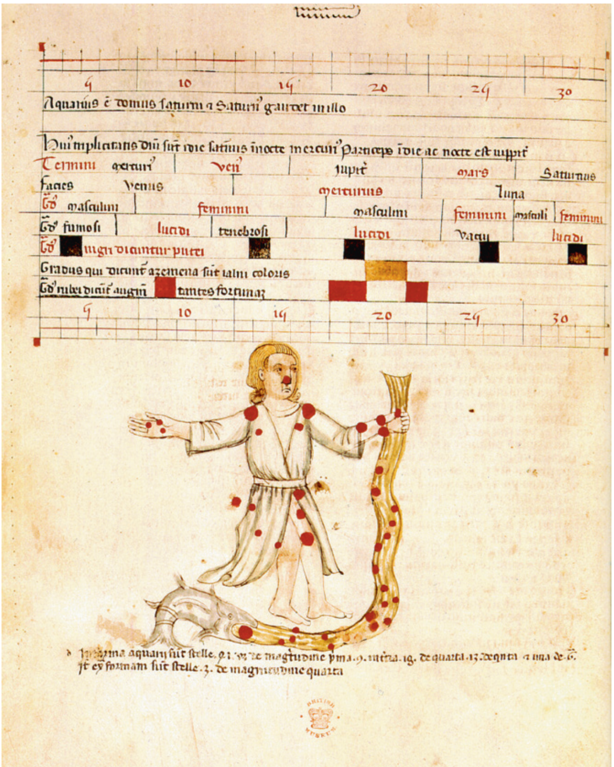


Fig. 6 The classification of Aquarius, c. 1350 (British Library, Add. 23770, f. 20v).[229] Courtesy of the British Library

For Giovanni Pico della Mirandola (1463–1494),[230] the doctrine of the Terms is an easy victim, or at least he himself believes so. Pico's attack on the Terms in Chapter 6.16 of his *Disputationes adversus astrologiam divinatricem* is logically questionable because his key argument is the disagreement on their sequence and extension among astrologers (this does not exclude that one system may be right).[231] Pico's argument ultimately goes back to Sextus Empiricus (c. 200 A.D.). After his opening words on the *discordia* among the astrologers, Pico devotes the rest of this chapter to scornful examples of the discrepancies between the existing five systems.[232]

Since much of the Renaissance reception of Ptolemy was based on Arabic commentaries, it is not surprising to find that both the correct insights of those commentators and their misinterpretations, as well as their additions to the ancient tenets,[233] were absorbed by their Western disciples. The Italian humanist Giuliano Ristori (1492–1556), for instance, embraced Haly's hypercritical view of the Ptolemaic Terms (see above) and pushed it even further, so as to assert in his public lectures on the *Apotelesmatika* that Ptolemy disapproved of the old manuscript's Terms and that he tacitly approved the Egyptian Terms.[234]

At the same time, Agostino Nifo (1473–1546) in his *Ad Apotelesmata Ptolemaei eruditiones* (Naples 1513), proclaimed the opposite, namely that the last system of Terms explained by Ptolemy is really Ptolemy's and that every good astrologer who wishes to predict the truth must follow this system. Then Nifo, being carried away by his enthusiasm, lets the discussion of the Terms culminate in the astonishing statement that "this is the system followed by myself, by Porphyry, by the Greek commentator and by all astrologers who care (*omnes curiosi Mathematici*)."[235] This last point about the practice of all astrologers is clearly contradicted by the historical evidence.

Girolamo Cardano (1501–1576) devotes no less than ten pages of his commentary on Ptolemy's *Apotelesmatika* to the Terms.[236] He begins noting the *maxima confusio* among astrologers with regard to the Terms and continues on a high level of self-confidence: First, he criticizes Haly for not undertstanding, then Porphyry for being no good at all, and eventually all Latin contemporaries and their Arabic predecessors for having messed up the art of astrology.[237] Only Ptolemy is praised for his clear reasoning (*claram rationem*) in refusing the Egyptian system.[238] Cardano wonders if the Ptolemaic Terms are an invention of Thrasyllus (1st c. A.D.), the court-astrologer of Tiberius, but then he rejects the idea.[239] He correctly states that in antiquity only the Egyptian Terms were widely practiced, and he follows Ptolemy (Apotel. 1.22) in stressing the need of measuring the Terms in tropical, not sidereal longitudes.[240] However, in his own countless horoscopes one does (to my knowledge) not find any practical application of any system of Terms, not even in the extremely detailed analysis of his own nativity.[241] At the theoretical level, he is inconsistent: In his "Book on the seven planets" he explicitly follows the Egyptians,[242] but a surprising thing awaits the reader of his "Book on the judgement of nativities" where Cardano says that his own rationale of the Terms is profoundly different from all the others.[243] After a brief display of the Egyptian table of Terms with some Latin mnemonic verses he presents the reader with a new system devised by himself which assigns Terms not only to the five planets but also to the luminaries.[244]

Francesco Giuntini's (1523–1590) *Speculum Astrologiae* is disappointing with regard to the Terms because this 2,500 page work offers nothing but the Greek text of Apotel. 1.21 with the Latin translation of John Camerarius.[245]

More stimulating is what the philosopher Tommaso Campanella (1568–1639) has to say in his *Astrologicorum libri VI* (Lyon 1629). During his long imprisonment for heresy, he let his mind wander widely. His astrological perspective extends to America and also to the southern hemisphere where, as he rightly observes, the Terms must all be shifted by 180°.[246] This is an implicit application of Ptolemy's request in Apotel. 1.22 that the Terms ought to refer to the tropical zodiac. However, Campanella admits freely that he is unable to judge the qualities of the rivaling systems of Terms. He also doubts that empirical knowledge of the Terms (as "the Egyptians" had claimed, being followed by Ptol. Apotel. 1.21.18–20) is possible.[247]

Claude Saumaise (1588–1653) does not comment on Ptolemy's old manuscript, nor does he give explicit preference to any system of Terms. After briefly noting the disagreement between Ptolemy and his forerunners, Saumaise moves right on to discuss the Egyptian system of Terms, because, as he says, Ptolemy's opinion is well known.[248]

The monk Placido Titi (1601–1668), in his *Quæstionum physiomathematicarum libri* (Milan 1650),[249] Chapter 2.12, makes an attempt that implicitly threatens the Ptolemaic Terms, namely to provide a rational explanation of the Egyptian Terms. His explanation is based on geometrical relations and harmonic proportions. Titi's disciple Gerolamo Vitali (1623–1698) is so impressed with what he calls the "majestic energy" of Titi's explanation that he quotes the long text in full in his *Lexicon Mathematicum Astronomicum Geometricum* (Paris 1668), the most comprehensive historical dictionary of astrological terminology ever made.[250]

My last author of the seventeenth century is William Lilly (1602–1681) who had an enormous influence on later astrologers right to the present day, especially in the English speaking world. In his *Christian Astrology* (London 1647), Lilly gives just one table of the various dignities of the planets, and that is based on Ptolemy.[251] His interesting, yet misleading commentary reads as follows:

> There hath been much difference between the *Arabians, Greeks* and *Indians* concerning the *Essential* Dignities of the Planets; I meane how to dispose the severall degrees of the Sign fitly to every *Planet,* after many Ages had passed, and untill the time of *Ptolomey,* the *Astrologans* were not well resolved hereof; but since *Ptolomey* his time, the *Grecians* unanimously followed the method he left, and which ever since the other Christians of *Europe* to this day retain as most rationall; but the *Moores* of Barbary at present and those *Astrologans* of their Nation who lived in *Spaine* doe so somewhat at this day vary from us.[252]

The incorrect statement that the Greeks unanimously followed Ptolemy may be a reminiscence of Pico della Mirandola[253] or Agostino Nifo.[254]

After the enlightenment of the eighteenth century had inflicted an almost deadly blow to astrology, it slowly recovered in the nineteenth and twentieth centuries. The Terms do no longer play a role in contemporary astrology, not least because the discovery of new planets in modern times, beginning with William

Herschel's (1738–1822) discovery of Uranus in 1781, was incompatible with those old systems of Terms.[255] But a formerly unknown historical interest among some astrologers makes it worth tracing our survey down to the very present. The ultimate roots of this new historical interest seem to lie in the Romanticism and in the work of the Grimm brothers who brought about, in the late nineteenth century, the scholarly interest in the history of religion and astrology. Scholars like Hermann Usener (1834–1905), Auguste Bouché-Leclercq (1842–1923), Franz Boll (1867–1924), and Franz Cumont (1868–1947) laid the foundations of all subsequent research into the history of ancient astrology. The best known product of their efforts is the *Catalogus Codicum Astrologorum Graecorum* (1898–1953), a 12 volume catalogue of Greek astrological manuscripts that contains, in its large appendices, editions of many formerly unknown texts. It seems to be a response to these scholars' groundbreaking work that a number of practicing astrologers started in the 1990s to do their own research in ancient astrology. Among the most active groups is the American *Project Hindsight*, founded 1993.[256] Somewhere in between the world of traditional scholarly research and historically interested practitioners is the Italian *Associazione Cielo e Terra*, founded 1999.[257] Both are non profit organizations with a wide range of activities such as seminars and conferences. They have both produced a considerable amount of translations of and commentaries on hitherto untranslated Greek and Latin originals.

The *Warburg Institute* of the University of London has responded to this new trend and taken the opportunity of bringing traditional scholars and historically interested practitioners together in a workshop on the history of ancient astrology.[258] It is in this context that Houlding's contribution on the Ptolemaic Terms (2007) has its roots.[259] A large part of it is devoted to the discussion of each single Ptolemaic Terms planetary ruler and extension, in the hope of overcoming the disagreements in the manuscript tradition. A similar attempt has recently been made by Bezza & Fumagalli (*Associazione Cielo e Terra*). These two contributions stand in and are the most rigorous products of an exegetic tradition that extends back in time over the Renaissance (Agostino Nifo) and the Byzantine and late antique periods (Anonymous Commentator) to Ptolemy's own brief account of the rules that allegedly underly his preferred table of Terms (Apotel. 1.21.22–27). Both parties—Houlding as well as Bezza & Fumagalli—discuss each one of the 60 (12×5) Terms separately, and with painstaking accuracy, basing their analyses on previous discussions of the criteria to be applied.[260] They come independently, and with slightly different focuses of interest, to the same result, namely that no consistent explanation of any of the transmitted versions of the Ptolemaic table is possible. Particularly useful are the results concerning the reasoning of the Anonymous Commentary, whose table is (except for three data in Gemini) identical with that transmitted in **L** and **z** and printed by Hübner 1998: although persuasive in being able to explain most of the table, the anonymous commentary is "fundamentally flawed in offering two alternative approaches, neither of which is capable of justifying the arrangement of all of its signs."[261] This ultimately shows that Ptolemy (or the author of that old manuscript, if it ever existed) struggled unsuccessfully with the conflicting requirements of applying a reasonable set of rules consistently while respecting the traditional planetary totals of the Terms. The general rules laid out by Ptolemy in Apotel. 1.21.22–27 give the misleading impression that they are suitable to explain the following table (1.21.28–29) with the same perfection as it had been the case with the preceding Terms attributed to the Chaldeans (1.21.12–17).[262]

Although the Ptolemaic table cannot be reconstructed with certainty,[263] the above analysis has shown that only 15 out of 60 Terms may be the object of reasonable doubt. These are the Terms where groups A1 and B in Table 8 above disagree. Figure 7 is an attempt to visualize the various relationships between manuscripts and printed books that have been discussed so far. Note that this diagram is concerned exclusively with the Ptolemaic table of Terms (Apotel. 1.21.28–29), not with the whole Chapter 1.21, and still less with the whole *Apotelesmatika*.[264] It remains to be investigated to which extent these insights can shed light on the complicated transmission of the *Apotelesmatika* as a whole.

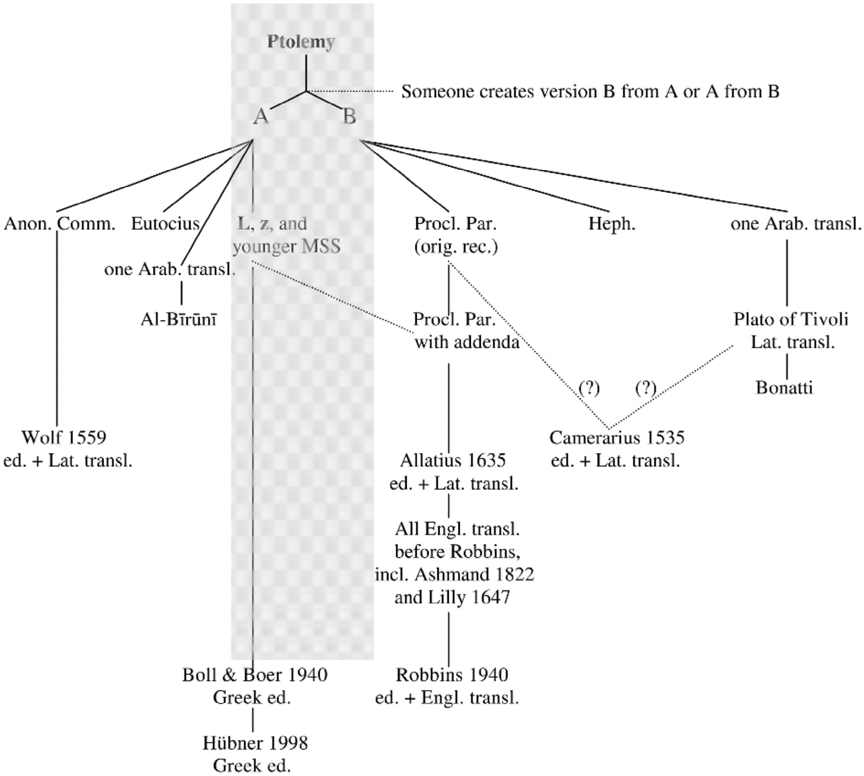


Fig. 7 Ptol. Apotel. 1.21.28–29: groupings and relationships (the area of direct transmission is greyed, as opposed to paraphrases, commentaries, translations, etc.)

In conclusion, it is to be emphasized that the reception of Ptolemy's doctrine of the Terms is entirely theoretical, not practical. As part of this theoretical interest, an anonymous reader before Hephaestio, that is: before c. 400 A.D., must have checked and revised the data of Ptolemy's table in Apotel. 1.21.28–29 so as to give rise to the 25% discordance between the recensions A and B. Neither A nor B can be securely validated as reflecting the original. The only historical horoscope based on the Ptolemaic Terms that I know of, that of Eutocius of Ascalon, is not a practitioner's analysis of a real client's nativity but a didactic example devised for a manual; this horoscope follows recension A. Over two millennia, astrologers made considerable efforts trying to understand the principles of the Ptolemaic system of Terms, but whenever it came to practical applications, the authority of the revered Egyptians and the alleged empirical basis of their system were stronger than Ptolemy's old manuscript with its more rational and orderly approach. It is the merit of modern scholarship to have unmasked the fake character of the former authority[265] and to have seriously shaken the credibility of the latter. Independently of the fact that all ancient and medieval practitioners seem to have preferred the Egyptian system to Ptolemy's, there is an interesting historical misapprehension among commentators in the Latin West that extends from Pico della Mirandola (maybe even earlier) all the way down to William Lilly and his followers: these authors wrongly assume that the Greeks unanimously followed Ptolemy and his system of Terms.[266] This phenomenon ties in with the broader misapprehension, widely spread in modern times, that Ptolemy's *Apotelesmatika* are representative of Greek astrology, which is in many respects not true.[267] They rather are a hybrid, yet fascinating and highly influential attempt to rationalize traditional astrological lore, and their intrinsic tension between science and superstition in the modern sense is best perceptible in the Ptolemaic doctrine of the Terms.

Notes

1. The Latin equivalent of *Tetrabiblos* is *Quadripartitus* (sc. *liber*) or *Quadripartitum* (sc. *opus*).
2. See the discussion of the original title in Hübner (1998, pp. XXXVI–XXXIX).
3. For the Arabic period, see below note 191 on Abū Ma'shar. In the Renaissance, Luca Gaurico (1475–1558) and Pierre Gassendi (1592–1655) discussed the idea that there might have been two Ptolemies, one who wrote the *Almagest* and one who wrote the *Apotelesmatika* (see Tester 1987, p. 232). See also what the editor of Leo Allatius' translation of the *Proclus Paraphrase* says on fol. 2*^{v} (quoted in note 142 below). Boll 1894 argued convincingly that all doubts concerning the authorship of the *Apotelesmatika* are to be dismissed. He has been followed by all subsequent scholars.
4. See Hübner (1989).
5. Ptol. Apotel. 1.1.1.
6. Recent commentaries on this chapter: Feraboli (1985, pp. 391–394); Bezza (1990, pp. 338–350); Houlding (2007). Note that Apotel. 1.21 is counted as *two* chapters (1.20–21) in the obsolete edition of Robbins (1940).

7. See Bouché-Leclercq (1899, pp. 182–240). Especially on the Terms see ibid. pp. 206–215 and Gundel and Gundel (1950, coll. 2125–2128).
8. The line count is based on the edition of Hübner (1998).
9. Besides *termini*, there is a second Latin technical term for Greek ὅρια, *fines*.
10. See Riess (1891–1893) (incomplete, and partly obsolete), Pingree (1974, 1978, v. 2, pp. 436–437), Fournet (2000). An updated, thorough discussion of this ancient manual, its pretended authors, and the preserved fragments will be given in Heilen (forthcoming).
11. Symbols: ♈ Aries, ♉ Taurus, ♊ Gemini, ♋ Cancer, ♌ Leo, ♍ Virgo, ♎ Libra, ♏ Scorpio, ♐ Sagittarius, ♑ Capricorn, ♒ Aquarius, ♓ Pisces; ♄ Saturn, ♃ Jupiter, ♂ Mars, ♀ Venus, ☿ Mercury.
12. Still smaller units recommended (but probably not used) for prognostic purposes were the zodiacal dodecatemories (2.5° each, see Manil. 2.693–737) and the planetary dodecatemories (0.5° each, see Manil. 2.738–748 and below note 82). The highest degree of differentiation was reached in the *Myriogenesis* ("Ten-thousand-Nativity"), a system mentioned several times by Firmicus Maternus that took its name from the assignment of specific effects to each single minute of arc (360×60 = 21600).
13. Ptol. Apotel. 1.21.1: ὁ μὲν οὖν Αἰγυπτιακὸς ὁ τῶν κοινῶς φερομένων ὁρίων οὐ πάνυ τι σῴζει τὴν ἀκολουθίαν οὔτε τῆς τάξεως οὔτε τῆς καθ' ἕκαστον ποσότητος. (Engl. transl.: Robbins 1940, p. 91).
14. Bouché-Leclercq (1899, p. 199).
15. Ptol. Apotel. 1.21.18.
16. Bouché-Leclercq (1899, p. 404): "Le calcul de la durée de la vie, avec indication du genre de mort préfixé par les astres, est le grand œuvre de l'astrologie, l'opération jugée la plus difficile par ses adeptes, la plus dangereuse et condamnable par ses ennemis."
17. Note that in the ancient geocentric model of the cosmos the furthest known planet was Saturn. The luminaries (Sun and Moon) were both counted as planets revolving around the central Earth. Since the luminaries are not allotted any Terms in the Egyptian system, the years of life given by them are based on a different rationale. For more details, see Bouché-Leclercq (1899, pp. 407–410). The figures concerning the maximum numbers of years for each of the five true planets (they are surrounded by a bold frame in the above table) are explicitly mentioned by Ptol. Apotel. 1.21.11. Robbins (1940) omitted this paragraph which is absent from part of the manuscripts. For its authenticity, however, compare Apotel. 1.21.17 where the corresponding figures of the Chaldean system are given, and esp. Apotel. 1.21.30 where it is said that the Ptolemaic totals for each planet's Terms are *again* such and such, which does not make sense without a previous reference to the same figures. The figures mentioned in Apotel. 1.21.11 are also given by Vettius Valens, Firmicus Maternus (partly flawed), Paulus Alexandrinus, Olympiodorus, and Rhetorius.
18. He further mentions the Terms in Apotel. 1.18.1; 1.22.1–2; 2.8.2; 3.3.3; 3.11.13; 4.9.2; 4.10.18; 4.10.23. None of these passages adds details that might be relevant for the present analysis, except for 1.22.1–2 on which more will be said in note 136.
19. On the seven climates, see Honigmann (1929).
20. For instance, Tester (1987, p. 76), argues that the Egyptian Terms are based on the rising times of the Babylonian system Ba (on which see Neugebauer 1975, v. 2, p. 732).
21. In this paragraph Ptolemy speaks of σύνταξις (coordination) and ἀριθμός (number), thereby slightly modifying his hitherto consistent use of τάξις (order) and ποσότης (quantity).
22. Neugebauer (1975, v. 2, p. 690). See also ibid. p. 606.
23. I disagree with Houlding (2007, p. 279), who interprets Ptolemy as taking "a fairly neutral stance" in regard to the value of the data in the "old manuscript."
24. Apotel. 1.21.20: ἤδη μέντοι περιτετυχήκαμεν ἡμεῖς ἀντιγράφῳ παλαιῷ καὶ τὰ πολλὰ διεφθαρμένῳ, περιέχοντι φυσικὸν καὶ σύμφωνον λόγον τῆς τε τάξεως καὶ τῆς ποσότητος αὐτῶν, μετὰ τοῦ τάς τε τῶν προειρημένων γενέσεων μοιρογραφίας καὶ τὸν τῶν

συναγωγῶν ἀριθμὸν σύμφωνον εὑρίσκεσθαι τῇ τῶν παλαιῶν ἀναγραφῇ. (Engl. transl.: Robbins 1940, p. 103).

25. Val. 3.6; see Bouché-Leclercq (1899, pp. 213–215).
26. There may even be a fourth, by Erasistratus; see below note 186.
27. As Pingree (1978, v. 2, pp. 212–213) has shown, this system can be reconstructed from the table at the end of Chapter 9 in the eighth book of Valens. Its only known application is in a didactic horoscope for Oct. 7, 2 A.D., by Critodemus himself (Val. 8.9.5–22 = Val. 3.[6,5–22]). A more detailed discussion of these systems will be given in my forthcoming commentary on the fragments of Antigonus of Nicaea, esp. on Antig. Nic. F1 § 26 ἐν ἰδίαις μοίραις (= Heph. 2.18.26, ed. Pingree 1973–1974).
28. See CCAG v. VIII 1 (1929), pp. 257,21–261,2. The new authoritative edition of this text is Hübner (1995, v. 1, pp. 193–203).
29. P. Mich. III 149, coll. VII,27–VIII,15. See the commentary of Robbins (1936, pp. 98–99), and also Neugebauer (1972) and Neugebauer (1975, pp. 805–808).
30. P. CtYBR inv. 1132(B) in the Beinecke Rare Book and Manuscript Library, Yale University. See Depuydt (1994) and Bohleke (1996, esp. pp. 34–46).
31. See Pingree (1978, v. 2, p. 211), with a table of these ὅρια (triṃśāṃśas).
32. For more details, see my forthcoming commentary (as in note 27).
33. I closely follow the translation of Ptol. Apotel. 1.21.20–21 in Robbins (1940, p. 103) (and note 1 *ibidem*).
34. See esp. Bouché-Leclercq (1899, pp. 206–207): "Ptolémée … finit par en proposer un troisième, qu'il n'ose pas donner comme sien, mais qu'il pretend avoir trouvé dans un vieux livre […] Cette page de Ptolémée est un document psychologique de haute valeur; elle nous montre l'état d'esprit des croyants et les moyens, bien connus des fabricants d'apocryphes, dont il fallait se servir pour capter la foi. Enfin […] Ptolémée, en dépit de toutes ses précautions, ne réussit pas à remplacer le vieux système égyptien par le nouveau, donné comme plus vieux que l'autre." (cf. ibid. p. 208). This judgement has been adopted by many, often without explicit reference to Bouché-Leclercq. See Abry 1993b, p. 147, n. 13: "Selon Ptolémée (*Tetrab.* I,21) il existe […] un autre système égyptien, ancient et infiniment plus satisfaisant pour l'esprit, qu'il expose sans oser dire que c'est sa creation." Feraboli (1985, p. 392), says the same in Italian, with this moralizing addendum: "Tolomeo non ha resistito alla tentazione di avvalorare il proprio sistema con l'autorità del passato; ma il tempo ne ha fatto giustizia."
35. Speyer (1970, pp. 142–144). For a recent, concise survey on scholarly research on pseudepigraphy and literary forgery in ancient literatures, see Baum (2001, pp. 1–3). As to Speyer's valuable monograph, note that what he (p. 142) takes as the best known authentic book find in the Greco-Roman world, the discovery of Aristotelian writings in a basement of a house in Scepsis as reported by Strabo, is an issue of scholarly disagreement. Aristotelian expert D. Frede writes that "der Bericht Strabons vom Verfall des Corpus der Lehrschriften in einem Keller in Skepsis in Kleinasien weithin als Legende angesehen wird, weil es ganz unwahrscheinlich ist, daß es in Athen im Lykeion keine weiteren Exemplare der Lehrschriften gegen haben sollte" (Frede 1996, col. 1143); Engl. transl. in Frede (2002, col. 1145): "Strabo's report that the corpus of didactic writings lay mouldering in a cellar in Secpsis [*read:* Scepsis] in Asia Minor is still [*read:* by many*; translator's confusion of German* weithin *and* weiterhin] seen as a legend, because it is improbable that no copies of these works existed in the Lyceum in Athens."
36. Speyer (1970, pp. 51–65).
37. Speyer (1970, pp. 51–55).
38. Plin. nat. 13.86 = Cassius Hemina frg. 37 in Peter (1914, pp. 109–110). The English translation is that of Rackham (1945, p. 151).
39. Speyer judges the latter possibility less likely.
40. Plin. nat. 13.87 (quoting from the third book of the historian Antias).

41. Speyer (1970, pp. 55–59 and 60–65).
42. Speyer (1970, pp. 58–59).
43. Plin. nat. 2.248; see Speyer (1970, p. 49).
44. Speyer (1970, pp. 72–76).
45. Speyer (1970, pp. 74–75).
46. Burnett (2001, p. 118).
47. See CCAG v. VIII 3 (1912), pp. 171–179, and Speyer (1970, p. 76, n. 53).
48. Ps.-Maneth. 5[6],1–3: Ἐξ ἀδύτων ἱερῶν βίβλων, βασιλεῦ Πτολεμαῖε, / καὶ κρυφίμων στηλῶν, ἃς ἤρατο πάνσοφος Ἑρμῆς / οὐρανίων τ' ἄστρων ἰδίαις ἐχάραξε προνοίαις, κτλ. (ed. Koechly 1858). Ps.-Manetho pretends to be writing to king Ptolemy II. Philadelphos (3rd c. BCE). The real date of composition is much later. See further Speyer (1970, p. 115), who mentions (besides Ps.-Manetho) the pretended transcriptions of the writings of Agathodaemon and their Greek translations.
49. Festugière (1950, pp. 319–324, here: p. 320). The reference is to CCAG v. VIII 4 (1921), p. 105, 4–5: τὸ δὲ ἕτερον βιβλίον εὑρέθη ἐν Ἡλιοπόλει τῆς Αἰγύπτου ἐν τῷ ἱερῷ ἐν ἀδύτοις γεγραμμένον ἱεροῖς γράμμασιν ἐπὶ βασιλέως Ψαμμητίχου. See also the introduction of F. Cumont ibid. pp. 102–103 and also CCAG v. VII (1908), p. 62, fol. 177.
50. Interestingly, these earliest pretended book finds are already related to Egypt. See Speyer (1970, p. 70).
51. Speyer (1970, p. 65).
52. Speyer (1970, pp. 77–80), with special emphasis on myth, poetry, entertainment and satire.
53. On this provenance of pretended book finds see Speyer (1970, pp. 125–141).
54. See Speyer (1970, p. 134). The very reference to such an archive served in some cases to bestow credibility on a pretended book find. One such forger successfully deceived the church father Eusebius with regard to the pretended correspondence between Jesus Christ and the prince Abgar of Edessa (see Speyer 1970, p. 135).
55. Besides the early case of the books of Numa (above p. 49), see, for instance, Odo of Glanfeuil (9th c.) who falsified the history of St. Maurus pretending that at the opening of the saint's tomb (845 A.D.) a small strip of parchment was found whose text was so badly faded that it could barely be read; eventually, however, thanks to most astute and patient analysis, its full text could be deciphered (see Speyer 1970, pp. 95–96, with quotation from the Latin original). See further Otloh's *Vita S. Magni* (10th c.?) on the pretended find of the saint's life in his tomb: it was allegedly written on an almost rotten strip of parchment (*pitacium pene putidum*), but the text was still readable (Speyer 1970, pp. 100–101).
56. Such papyri have actually been found, but they have nothing to do with astrological Terms.
57. Festugière (1950, p. 320). See also the authors mentioned above in note 34.
58. Boll (1914, p. 7): "Wie diese Buchoffenbarung um sich griff, lehrt am besten ein so profaner und grundgelehrter Mann wie Ptolemaios, der ein Stück seiner astrologischen Lehre einem alten sehr zerstörten Buch entnommen zu haben versichert—gewiß ganz glaubhaft, und dennoch unbewußt in der Gefolgschaft solcher Offenbarungsvorstellungen." See further references ibid. n. 5.
59. See above p. 48 (before and after note 24).
60. Some 200–300 years were considered to be a long lifetime for a papyrus roll (Speyer 1970, p. 52, n. 7). The worst enemy of the papyrus was the bookworm.
61. See Cumont (1937, p. 156).
62. Apotel. 1.21.21 μετὰ περιττῆς τινος ἀποδείξεως. For περιττῆς (followed by Robbins 1940 and Hübner 1998) there is a variant reading πολλῆς which has been adopted by Boll and Boer (1940).
63. See below p. 58 (after note 93) on Porphyry's belief that the Egyptian Terms had been devised long before the time of Plato. According to Bouché-Leclercq, Ptolemy presents his old manuscript as even *older* than the revered Egyptians, and *antedating* their writings (see the end of my quotation in note 34 above), but this may be an overinterpretation: see above note 60.

64. Oral communication at the Conference, confirmed by email on July 12, 2009.
65. There is a considerable published literature on this topic, all of it referenced in the article by Toomer (1980). It is to be hoped for that Britton's paper on Almag. IV.2 be published soon.
66. It is emphasized by Holden (1996, p. 44); for details see note 136.
67. This table (for which see my synopsis on p. 70, Table 8) must be an authentic part of Ptolemy's Chapter 1.21 because it is firmly anchored in the context. See the introductory words 1.21.27: ἔστι δὲ καὶ ἡ τούτων τῶν ὁρίων ἔκθεσις τοιαύτη ("the tabulation of these terms is as follows," Robbins 1940, p. 107).
68. Catalogus Translationum et Commentariorum: Mediaeval and Renaissance Latin Translations and Commentaries. Annotated Lists and Guides, Washington, DC 1960—(so far 8 vols.). CTC editor Prof. Virginia Brown (Toronto) informs me (e-mail Nov. 28, 2006) that the article on Ptolemy was originally assigned to Prof. David Pingree (Brown Univ., Providence). When he died on Nov. 11, 2005, Prof. Charles Burnett of the Warburg Institute, London, kindly took it over.
69. No original Latin horoscopes have been preserved from antiquity. As to the very few literary examples (esp. Firm. math. 2.29.10–20), they do not mention the Terms.
70. Κρόνος ἐν Ὑδρηχόωι μοιρῶν $\overline{\text{ιε}}$ καὶ | λεπτῶν $\overline{\text{λς}}$, οἴκωι ἰδίῳ καὶ ὁρίοις | Ἀφροδείτης, στηριγμῷ πρώτῳ (Jones 1999a, v. 2, pp. 430–431).
71. I am grateful to Dirk Obbink, Oxford, for his palaeographical expertise of the papyrus in question. He comes to the same conclusion as the editor (A. Jones). Obbink informs me (*per litteras*, Jan. 6, 2008): "I am convinced that it is of the second century. Whether it is earlier or later than 160 [*the approximate date of composition of Ptolemy's Apotelesmatika*] is perhaps more precise than we are able to be without any external dating criteria like archaeological data or another document on the back [...]. But I wouldn't rule out the possibility that it could be later than 160."
72. The most famous such πίναξ (*pínax*) is not an archaeological find but a literary fiction. It is described in the late Greek novel of Pseudo-Callisthenes on Alexander the Great where Nectanebo shows Olympias the state of the heavens on a precious horoscopic board, urging her to endure her labour a little longer until the moment is suited to give birth to a world ruler. See Ps.-Call. Hist. Alex. Magn. 1.4.5 (ed. Kroll 1926, Engl. transl.: Stoneman 1991).
73. They are amply described and analyzed in Abry (1993a).
74. Bertaux (1993, p. 44).
75. See Evans (2004).
76. Gundel (1992, pp. 110–111) (with fig. 51) and p. 226, catalogue n. 63. Another famous pinax is the Daressy tablet which, however, provides no information on the Terms. See Daressy (1916, pl. 2), = Abry (1993b, pl. II,1). See also Gundel (1992, p. 226), catalogue n. 62, with plate on p. 227.
77. The *Tabula Bianchini* has two such rings.
78. Diptyque A, lunar half, from: Béal (1993, pl. 3).
79. From: Boll (1903, pl. V).
80. See above notes 27 and 28. For the date, see Pingree (2001, p. 10).
81. CCAG v. 7 (1908), pp. 194–213 (= Boll 1903, pp. 16–21). On this text, see Pingree (1977, p. 220), and Hübner (1995, v. 1, pp. 92–93 and 104–107).
82. Still earlier is the reference of Manilius to the *fines* within each single zodiacal constellation (Manil. 2.747–748) which, however, does not mean the Terms but planetary dodecatemories, a subdivision of the zodiacal dodecatemories. See Goold (1997, p. liv), and (on the textual problems involved) Feraboli (1992, p. 167).
83. See Dorotheus, Appendix II B, in Pingree (1976b, pp. 429–430). The twelve relevant fragments have been transmitted by Hephaestio Thebanus in various paragraphs of the long first chapter of the first book of his *Apotelesmatika* (ed. Pingree 1973–1974): Heph. 1.1.9; 28; 47; 66; 86; 105; 124; 144; 164; 183; 202; 222.

84. The Egyptian Terms were much later versified a second time by John Camaterus (12th c.) in political verses in *De zodiaco* vv. 96–132 (ed. Miller 1877, pp. 57–59).
85. See Antig. Nic. **F1** § 26 and **F2** § 54 (= Heph. 2.18.26 and 2.18.54) with my forthcoming commentary ad loc. (see above note 27). These two nativities can be dated to the years 76 A.D. (Emperor Hadrian) and 40 A.D. (anonymous).
86. The manuscript readings of Pingree (1986) are confirmed by the older edition of Kroll (1908). See further the table of Egyptian Terms in Valens, Additamentum 6 (Pingree 1986, p. 358) which is again (though in a different way) partially corrupt.
87. This is my translation of Rhet. 5.12.5–6 in the forthcoming edition of the late David Pingree. The Greek text is: ὁ οὖν Πτολεμαῖος ἔν τισιν ὁρίοις οὐ συνήνεσε τοῖς Αἰγυπτίοις, διὸ ἠναγκάσθην καὶ τούτων ὑπόμνησιν ποιήσασθαι. συμβάλλονται δὲ τὰ ὅρια αὐτοῦ ἐν τοῖς αὐτῶν ἀποτελέσμασι μόνοις. These lines are preceded by a threefold casuistry explaining how the effects of the planets depend on and are modified by the Terms in which the planets happen to be (Rhet. 5.12.3).
88. Sext. Emp. adv. math. 5.37 (ed. Mau 1961): ὅρια δὲ ἀστέρων προσαγορεύουσιν ἐν ἑκάστῳ ζῳδίῳ ἐν οἷς ἕκαστος τῶν ἀστέρων ἀπὸ ποστῆς μοίρας ἐπὶ ποστὴν μοῖραν πλεῖστον δύναται· περὶ ὧν οὐχ ἡ τυχοῦσα παρ' αὐτοῖς ἐστι καὶ κατὰ τοὺς πίνακας διαφωνία.
89. Therefore the text of Ps.-Porph. introd. 49 pp. 222,18–21 in the edition of Boer and Weinstock (1940) is essentially identical with Rhet. 5.12.5–6 (see note 87 above). Note, however, that in the last sentence Boer and Weinstock, following MSS **SDM**, read τὰ ὅρια αὐτῶν ("their terms") whereas Pingree, following Ms. **L**, reads τὰ ὅρια αὐτοῦ ("his terms," i.e. Ptolemy's). Pingree (2001, p. 8) adduces convincing evidence that Chapters 47–50 of Porphyry's introduction are spurious (and probably also Chapters 51–52).
90. It was transmitted by Stobaeus 2.8.42. I shall, in the following, quote from Smith (1993, pp. 302–308) (Porph. Frg. 271F).
91. Porph. Frg. 271F, lines 38–104. The text is partly difficult to understand. See the comments of Boll (1894, pp. 114–116), Bouché-Leclercq (1899, pp. 601–602), Gundel and Gundel (1966, p. 214), Deuse (1983, pp. 148–159), Dörrie and Baltes (2002, pp. 264–271, esp. 268–269), Hübner (2006, p. 36, n. 142). The only modern translation that I know of is French, by Festugière (1970, pp. 349–357, here: pp. 356,11–19; I disagree on important details).
92. Porph. Frg. 271F, lines 79–85: Ζῳδίων δὲ ὄντων δώδεκα, δι' ὧν ἡ ὁδὸς ταῖς ψυχαῖς πεπίστευται τοῖς Αἰγυπτίοις γίγνεσθαι τῇδε πανταχοῦ σχεδόν, αἱ μὲν πρῶται ἑκάστου ζῳδίου μοῖραι, ὡς ἂν αὐτῷ νενεμημέναι τῷ κυρίῳ τοῦ ζῳδίου, παρεδόθησαν εἶναι ἀμφιλαφεῖς· αἱ δὲ τελευταῖαι ἐπὶ πάντων τοῖς κακοποιοῖς λεγομένοις ἀστράσιν ἀπενεμήθησαν. Ἐντεῦθεν οὖν ἡ τῶν πρώτων κλήρων εὐμοιρία ἀποδοχῆς ἠξίωται καὶ ἡ τῶν ὑστέρων ἐστενοχωρεῖσθαι λέγεται. Note that ἑκάστου ζῳδίου is my conjecture for the MSS reading τοῦ ζῳδιακοῦ. Heeren's conjecture ἀγαθῷ for the MSS reading αὐτῷ (after ὡς ἂν) has rashly been accepted by Festugière (1970, p. 356), and Smith (1993, p. 306).
93. Explanation: As Table 2 (above) shows, the first Egyptian Term in each sign is always larger than the last one, or at least of equal size, and the last Term is always occupied by either Mars or Saturn. Besides, the first Term is, indeed, usually given to the planet with the highest dignity in that sign, in other words: to its governor, a feature that recurs almost unchanged in Ptolemy's table of Terms (see his explanation of the rationale at Apotel. 1.21.22).
94. See Gundel and Gundel (1966, p. 215), and Pingree (1978, v. 2, p. 437, n. 36).
95. We owe these fragments to Hephaestio of Thebes who quotes them in his long chapter 2.11.
96. Firm. math. 2.6 (ed. Kroll et al. 1968) and Paul. Alex. 3 (ed. Boer 1968).
97. Ed. Boer (1968, pp. 102–134). Scholia n. 3–8 (γ–η) on pp. 103–104 belong to Chapter 3 on the Terms.
98. Paul. Alex. schol. 4, p. 103,12–14: ταῦτα τὰ ὅριά εἰσι κατ' Αἰγυπτίους, ⟨οὐκ⟩ ἀπὸ τοῦ παλαιοῦ βιβλίου τοῦ εὑρεθέντος τῷ Πτολεμαίῳ.
99. Paul. Alex. schol. 6, p. 103,18–22.

100. It is the Egyptian Thebes, not the Greek one.
101. He gives his own horoscope in Heph. 2.11.6–7 and 2.11.9–15 (ed. Pingree 1973–1974). See Neugebauer and Van Hoesen (1959, pp. 131–132, n. L 380), and Frommhold (2004, pp. 151–162).
102. Up to the present day, many scholars erroneously date Hephaestio's manual to the 4th century.
103. See above note 83.
104. Heph. 1.1.10; 29; 48; 67; 87; 106; 125; 145; 165; 184; 203; 223.
105. Hübner (1998) agrees exactly with the edition of Boll and Boer (1940). Robbins (1940), instead, professes (p. 106, n. 1) to be following the *Proclus Paraphrase*. See the diagram at the end of this article.
106. On this archetype's errors which are shared by all extant manuscripts see Hübner (1998, p. XVII).
107. See the *stemma codicum* in Hübner (1998, p. XXV).
108. On the importance of Hephaestio for the textual criticism of Ptolemy's *Apotelesmatika* see Hübner (1998, pp. XXIV and XXVIII). See also Pingree (1973–1974, v. 1, pp. VI–VII).
109. I agree in principle, although older manuscripts are not *necessarily* better. Remember Pasquali's maxim *recentiores, non deteriores* (Pasquali 1934, p. 41, title Chapter IV).
110. See the *apparatus criticus* of Hübner (1998, p. 80). On the importance of **V** see ibid. p. XVIII.
111. See the analysis of Boll (1899, pp. 85 and 88–110).
112. See A. Olivieri's codicological description of **L** in CCAG v. I (1898), pp. 60–72 (n. 12), esp. p. 70, and Hübner (1998, p. 68), app. test. (read "fol. 148^{v}<–149^{r}>").
113. See the *stemma codicum* in Hübner (1998, p. XXV).
114. This is emphasized by Boll (1899, p. 85). On the very high quality of **ψ** and the purity of the Ptolemaic text once contained in it see ibid. pp. 82–84. Incidentally, **ψ** was also the ancestor of Vatic. gr. 1594, that immensely valuable 9th century manuscript of the *Almagest*.
115. See Boll (1899, pp. 110–138) (on Vatic. gr. 1291), here: 114–115 (on its content and date of composition).
116. See his *apparatus criticus* on p. 80.
117. Hübner (1998, pp. XV–XVII).
118. Boll (1899, p. 113).
119. More about this in note 151 below.
120. See esp. Heph. epit. 4.1.10; 28; 45; 62; 80; 98; 116; 135; 153; 170; 187; 204. This epitome is printed in Pingree (1973–1974, v. 2).
121. This abstention from specifying *all* the data serves to highlight the disagreements.
122. Instead of ♂6, it ought to be ♂7 to save the total. This is the only case in which the epitome (Heph. epit. 4.1) gives a worse value than Hephaestio's main text (Heph. 1.1).
123. This will be demonstrated in my forthcoming commentary (see above note 27), esp. on Antig. Nic. F1 § 22 (= Heph. 2.18.22 = Heph. epit. 4.26.12).
124. Cf. Pingree (1973–1974, vol. 1, p. xx, n. 1).
125. For example in Aries (Heph. epit. 4.1.10): ♃6, 6, ♀8, 14, ☿7, 21, ♂5, 26, ♄4, 30. The same can be observed in the epitome's report of the Egyptian Terms which again coincides with how the data are presented in Ptolemy's *Apotelesmatika* (and which again departs from **AP**).
126. See above note 104.
127. The attribution to Julian of Laodicea, which was the dominant view in the early 20th century and is still followed by some (for instance, Bezza 1990, pp. 339–340), is obsolete.
128. See Bulmer-Thomas (1971, esp. p. 488).
129. Al-Nad. Fihr. 7,2 pp. 638 and 640 Dodge.
130. I am currently preparing this edition for publication with De Gruyter.
131. See Neugebauer and van Hoesen (1959, pp. 152–157, n. L 497).
132. This hypothesis has been advanced by Toomer (1976, p. 18, n. 2).

133. Rhet. 6.52.10; 13; 17; 21; 25; 29; 33; 34; 35; 36; 37; 38. I think that Eutocius does so merely for the sake of completeness. Bezza (1990, p. 350, n. 21), prefers a more complicated explanation, drawing on a passage in Haly's (i.e. 'Ali ibn Riḍwān's) commentary.
134. Heph. 1.3.1. 1.20.1. 2.2.8. 2.2.42. Eutoc. ap. Rhet. 6.52.5. Even the superlative (ὁ θειότατος Πτολεμαῖος, "the most divine Ptolemy") occurs in Greek astrological literature. See, for instance, the Anonymous of the year 379 in CCAG v. V 1 (1904), p. 204,9 and John Lydus (c. 550 A.D.) in Chapter 2 of his book *On signs* (*De ostentis*).
135. Rhet. 6.52.5: καὶ ἐξεθέμεθα αὐτὰ [sc. τάς τε ἐποχὰς τῶν ἀστέρων καὶ τὰ κέντρα] ἄνευ τροπῆς διὰ τὸ οὕτως καλῶς δοκεῖν τῷ θείῳ Πτολεμαίῳ. ("And I put them down [i.e. the positions of the planets and the cardinal points] without change because thus it seems right to the godly Ptolemy"). I thank A. Jones and A. Tihon for clarifying the meaning of ἄνευ τροπῆς to me and for directing my attention to Chapter 12 Περὶ τροπῆς in Theon's *Shorter Commentary* on the *Handy Tables* (Tihon 1978, pp. 236–237; cf. Jones 1999a, v. 1, p. 343).
136. Eutocius' reference in Rhet. 6.52.5 (see previous note) is to Ptol. Apotel. 1.22.2 where Ptolemy insists that it is reasonable to reckon the beginnings of the signs and the 'Terms' from the equinoxes and the solstices: ἐκεῖνο δὲ ἐπιστάσεως ἄξιον τυγχάνον οὐ παραλείψομεν ὅτι καὶ τὰς τῶν δωδεκατημορίων καὶ τὰς τῶν ὁρίων ἀρχὰς ἀπὸ τῶν τροπικῶν καὶ τῶν ἰσημερινῶν σημείων εὔλογόν ἐστι ποιεῖσθαι. Note that the words καὶ τὰς τῶν ὁρίων which refer to the 'Terms' are missing in branch α of the manuscript tradition. Robbins (1940, p. 109) follows these MSS. That leads to his mistranslation of the one remaining καὶ: "to reckon the beginnings of the signs also from the equinoxes" (correct: "to reckon the beginnings of both the signs and the terms from the equinoxes"). Note that the reading καὶ τὰς τῶν ὁρίων is supported by the oldest preserved MSS. of the text, **L** (saec. XI) and **V** (saec XIII). Still earlier is the testimony of the *Proclus Paraphrase*, which has the same words (Allatius 1635, p. 73) and whose oldest MS., Vatic. gr. 1453, dates from the 10th century. The words καὶ τὰς τῶν ὁρίων are further supported by the Arabic tradition of the text which goes back to Ibrahim ibn al-Ṣalt (9th c.) and lead to the inclusion of the *termini* in the Latin translations from the Arabic by Plato Tiburtinus (12th c.) and Aegidius de Thebaldis (13th c.), both to be found in Haly (1493, p. 23r-v). Hephaestio Thebanus and the Anonymous Commentary do not provide additional evidence. Note that also Bezza (1990, p. 351) considers the reference to the terms to be genuine ("e dei confini").
137. See below, p. 67.
138. In favor of authenticity: Boll (1899, p. 86), Gundel and Gundel (1966, pp. 213 and 215). Against authenticity: Boll (1903, p. 219, n. 1); Mansfeld (1998, p. 81); Hübner in Folkerts et al. (2001, col. 568): "wohl aus byz. Zeit"; Hübner (1998, p. LXXV): "Procli quae dicitur paraphrasis." Undecided: Robbins (1940, p. xvi); Beutler (1957, col. 204, n. 33).
139. See, for instance, ἡμεῖς / *nos* in note 143 below.
140. See above p. 60, on **L** and **z**. The general remarks of Robbins (1940, p. xviii), are inappropriately quoted with regard to Ptol. Apotel. 1.21 by Houlding (2007, p. 266, n. 10).
141. Boer (1959, col. 1833).
142. It says (fol. 2*r): *Sumpserat* [sc. *Allatius*] *autem hunc laborem privatim sibi, & amicis quibusdam. verum, quod sæpe alias contigit, hujuscemodi scripta ubi semel ex autoris elapsa sunt manibus, pariter ex ejusdem potestate exiisse deprehenduntur. Hinc adeo evenit ut, relicta Roma, fœtus hic* [!] *Venetias pervenerit, atque inde à viro summo, & in illustri posito dignitate, ad me excudendi gratia fuerit transmissus.* […] *Proinde non destiti Elzevirios nostros, optimos accuratissimosque Typographos* […], *serio ad ejus editionem sollicitare. qui, ut publici litterarum boni amantes sunt, hac, quam vides, forma id excudi curarunt.* See also the interesting remarks later on (fol. 2*v): *Obiter moneo, esse nonnullos qui dubitant, utrum Ptolemæi genuinum hoc scriptum sit. Porphyrio certe & Proclo, Philosophis quidem, sed religionis Christianæ hostibus, dignum visum cui lucem aliquam affunderent commentando.* ("In passing, I wish to draw your attention to the fact that there

are some who doubt whether this work is genuinely Ptolemy's. To Porphyry and Proclus, who were philosophers but enemies of Christian religion, it certainly appeared worth the effort of shedding some light on it by way of commenting.") The reference to Porphyry means the introduction published by Boer and Weinstock (1940).

143. All that remains of Apotel. 1.21.20–21 is Procl. paraphr. 1.23 (ed. 1635 p. 68): ἡμεῖς δὲ ἀντιγράφῳ παλαιῷ ἐνετύχομεν, φυσικὸν λόγον καὶ σύμφωνον καὶ τῆς τάξεως καὶ τῆς ποσότητος αὐτῶν περιέχοντι. ἔχει δὲ οὕτως · / *Nos vero in antiquum volumen incidimus in quo naturalis ratio ordini & numero eorum congruens, & apta continebatur, ea autem hæc erat.* What follows (Chapter 1.24) equals Apotel. 1.21.22–29.
144. Apotel. 1.21.26 οἷς καὶ παρέκειντο στιγμαί = Procl. paraphr. 1.24 p. 70 (ed. 1635) οἷς καὶ παρέκειντο στιγμαί / *& punctis hæc interstinguebantur*.
145. In the fourth Term of Libra the nonsensical figure κ (20) is an obvious yet overlooked misprint for η (8) (ms. Vatic. gr. 1453, fol. 50^v, has η). This is a standard letter confusion in Greek manuscripts, due to the very similar palaeographical appearance of κ and η. Compare, in the Egyptian table of Terms (Procl. paraphr. 1.23 pp. 64–65 ed. 1635) the erroneous sum of the first three Terms in Leo = ικ instead of (correct) ιη (the Lat. transl. correctly reads *18*). There is one more mistake in the Egyptian table which gives the sum of the first four Terms of Sagittarius as κδ (= *24* in the Lat. transl.), while it should be κς (26).
146. See above notes 112 and 115.
147. This different spatial allocation of planetary symbols and numerical values has an obvious reason: zodiacal degree numbers from eleven to thirty are rendered with two consecutive letters like κδ (= 24) which would make the corrected/expanded version four letters long and thereby not fit into one cell.
148. A theoretically possible, but unlikely variant of this scenario is that the scribe in question is himself the author of the whole paraphrase, and that he made some addenda to his own work.
149. I agree on this with Robbins (1940, p. 106, n. 1), and Houlding (2007, pp. 267–268, 270, 275). Note, however, that Houlding's main argument on which her correct conclusion is based does not hold. She writes (p. 270): "That the first options are intended to be the main values is proven by a comment under the table where the total term values for each planet are listed. These are accurate only if the first planets and their associated numbers are used." (The "comment" is Apotel. 1.21.30, a short paragraph that is present in the Vatic. gr. 1453, fol. 50^v, but absent from Allatius' printed version.) The truth is that both sets of values lead to the same planetary totals: In Taurus, 2° are taken from Mars and given to Saturn; in both Capricorn and Pisces 1° is taken from Saturn and given back to Mars. It is important not to overlook the obvious fact that in Leo the scribe forgot to add the symbol of Jupiter as an alternative to that of Venus. (See the reproduction of the Terms of Leo from Vat. gr. 1453, fol. 50^r, at Houlding 2007, p. 271, Fig. 5.) This lapse happened because only in Leo three planets are affected by the addition of new data, not two planets as in all other cases, and the scribe had already made two addenda in Leo. The principles of the system require that also this third addendum be made, because otherwise there would be no Terms of Jupiter in Leo but two Terms of Venus. Houlding's statement is true only when giving both Terms of 6° each to Venus, and none to Jupiter.
150. Houlding and I made the same observation independently from each other. However, she does not discuss the stemmatic consequences.
151. Sagittarius belongs into this group after correction of an obvious mistake in Heph. 1.1.165: It is here that Hephaestio unduly subtracts 1° from Venus, giving it instead to Aries, which leads to an impossible deviation from the canonical planetary totals put forth in Table 3. See above p. 61 (before note 119).
152. The degree numbers of the last two Terms of Taurus agree with Heph. 1.1.29 (against Ptol. Apotel. 1.21.28). However, in Heph. 1.1.29 the order of the respective planets is reversed. Probably the lapse is Hephaestio's, because the Arabic line of transmission agrees with the

paraphrase (see below note 213 and the following Table 8). The marginal addenda concerning Taurus in the printed edition (Fig. 4) are meant to be located on top of the figures that the three respective cells already contain, as is clear from ms. Vatic. gr. 1453, fol. 50r, where the three cells in question are filled in the same spatial arrangement as, for instance, the corresponding three cells of Pisces (the data themselves are, of course, different).

153. Since Heph. 1.1.67 agrees with Ptol. Apotel. 1.21.28 and with the Arabic line of transmission, the reversed sequence of planetary symbols in the second and third Terms of Cancer is the paraphrase's mistake.
154. In Leo, Hephaestio's and the paraphrase's allocations of planets to the single Terms disagree with each other as well as with **Lz** and also with the Arabic line of transmission. There may have been something seriously wrong in Leo at an early date.
155. The relevant chapters that Hephaestio excerpted are: Apotel. 1.1; 1.3–7; 1.9; 1.15–17; 1.21; 1.23; 2.4–10; 2.12; 2.14; 3.2–15; 4.1–10 (see Pingree 1973–1974, v. 1, p. VII).
156. All the relevant agreements can be found in the critical apparatus of Hübner 1998 where they are attributed to "Heph. Procl."
157. See above p. 61, before note 125.
158. See Boll (1899, p. 87): "ein nicht werthloser, aber unsäglich breiter anonymer Kommentar, gehört jedenfalls auch dem ausgehenden Alterthum an."
159. See above p. 58, after note 88.
160. Wolf calls him *doctum quendam amicum* and explains that this learned friend's motive for preferring anonymity was to escape reproaches for dabbling in this sort of literature: *ac ne nomen quidem suum uoluit adijci, quòd aliquorum reprehensionem uereatur, qui non in meliore scriptore elaborarit. Non quod hos commentarios contemnendos esse putet (inesse enim multa quæ studiosis haud dubiè utilia & iucunda futura sint) sed quod turpe uideatur ab equis quodammodo ad asinos descendere*. As to the badly damaged manuscript, Wolf reports the following complaints of the translator: *Nam & scripturam Græcam esse pessimam lectuque difficillimam, & authoris* [i.e. Ptol.] *atque interpretis* [i.e. Anon.] *uerba sæpiss(ime) confusa, & ipsum opus plurimis locis adeò deprauatum & mutilum ut nulla ex eo certa sententia possit elici. Se tamen solerti* [sic] *diuinatione multa loca citra temeritatem correxisse & distinxisse: in cæteris id præstitisse quod potuerit, atque optare, ut suus labor studiosis sit utilior quàm sibi iucundior* (Address to the Reader, fol. a3). See also the preceding epigram which invites the reader to "dig out hidden jewels from the excrement": *Quas tibi relliquias dedimus, studiose Mathesis, / Accipe. Barbaries non meliora sinit. / Omnia sunt mendis corrupta, nec Oedipus ipse / Soluerit hos griphos, æquior esto mihi. / Forsitan effodies tamen hoc è stercore gemmas: / Nullus enim liber est quin aliquando iuuet.*
161. See Gundel and Gundel (1966, p. 215).
162. See Neugebauer and van Hoesen (1964, p. 66), on Anon. comm. in Ptol. apotel. 3.5.4 et 3.11.5 (pp. 98 et 112 Wolf).
163. He does so at the beginning of his next chapter (Anon. comm. in Ptol. Apotel. 1.22 p. 47 Wolf): Ἄριστός ἐστι διδάσκαλος ὁ μὴ μόνα τὰ ἀληθῆ λέγων καὶ ὀρθὰ δόγματα ἀλλὰ καὶ τὰ διαπατᾶν μάτην τὴν ψυχὴν τοῦ νέου δυνάμενα διελέγχων. τοιοῦτος οὖν καὶ ὁ παλαιὸς [i.e. Ptol.] ἐν τῇ περὶ τῶν ὁρίων ἐφάνη διδασκαλίᾳ ἐλέγχων τὰ ψευδῶς παρὰ τοῖς αἰγυπτίοις δοξαζόμενα. καὶ νῦν δὲ ἐθέλει καὶ ἕτερα αὐτῶν ἐλέγξαι δόγματα πρὸς κενοδοξίαν ὁρῶντα./ *Optimus est præceptor, qui non uera tantum & recta præcepta explicat, sed falsa etiam, quæ adolescentis animum distrahere possunt, coarguit. Talem se Ptolemæus quoque in doctrina finium præbet, dum falsas Aegyptiorum de finibus opiniones reprehendit. ac nunc alias quoque sententias eorum ex falsæ gloriæ studio ortas refutat.*
164. Anon. comm. in Ptol. Apotel. 1.21 pp. 41–42 Wolf: Λοιπὸν δεῖ καὶ τὴν ἐκκειμένην ὑπὸ τοῦ Πτολεμαίου τῶν ὁρίων διδασκαλίαν ἐπεξελθεῖν, ἣν οὐκ αὐτὸς φησὶν εὑρηκέναι, ἀλλ' ἐξ ἀντιγράφου τινὸς [p. 42] διεφθαρμένου, καὶ εἰς αὐτὸν ἐληλυθότος, δυνηθῆναι μόλις ποτὲ ἀνευρεῖν. ὁ γὰρ γράψας τὸ εἰρημένον βιβλίον δῆλός ἐστι τάς γε τῶν χαλδαίων καὶ αἰγυπτίων δόξας εἰς ἓν συναγαγὼν καὶ οὕτω τὴν διδασκαλίαν

τελειῶσαι συλλογισάμενος. / *Restat ut expositam a Ptolemæo finium doctrinam recenseamus: quam non ipse sese inuenisse sed in uetusto* [p. 42] *& corrupto codice uix reperisse profitetur. Libri autem autorem apparet opiniones Aegyptiorum & Chaldeorum coniunxisse, & sic ex utrarunque collocatione doctrinam perfecisse.*

165. Anon. comm. in Ptol. Apotel. 1.21.26 οἷς καὶ παρέκειντο στιγμαί, p. 44 Wolf: ἐν τῷ βιβλιδίῳ φησὶ τῷ διεφθαρμένῳ προσθέσεσιν τῶν ἀστέρων πλείονας ἐχόντων λόγους παρέκειντο στιγμαὶ ἐνδεικνύμεναι δηλονότι ὅτι τοῦτό γε αὐτὸ, ὡς πρόσθεσις, αὐτοῖς τούτοις ἐγειόνει [read ἐγεγόνει]. / *In codice corrupto, inquit, stellarum additionibus quæ plus quam ratio postulat habebant, puncta erant adiecta, quæ indicarent scilicet idipsum esse adiectitium.*
166. Anon. comm. in Ptol. Apotel. 1.21.27 ἀφαιροῦνται δὲ αἱ προστιθέμεναι, p. 44 Wolf: διὰ τούτους τοίνυν τοὺς προσδιορισμοὺς ἀναγκαζόμεθα πολὺν τῆς σαφηνείας λόγον ποιούμενοι τὴν καθόλου μέθοδον ἐπὶ τῶν κατὰ μέρος γυμνάσαι καὶ δεῖξαι τὰς αἰτίας ἐφ' ἕκαστον. / *Ob has igitur declarationes perspicuitatis ratione in primis habita cogimur doctrinam hanc per partes tradere, & ostendere causas in singulis.*
167. Bouché-Leclercq (1899, p. 212, n. 1).
168. Anon. comm. in Ptol. Apotel. 1.21 p. 47 Wolf: σαφηνείας δὲ πλείονος ἕνεκεν / *ob maiorem perspicuitatem.*
169. Wolf's edition provides summaries of the data for each single zodiacal sign *in margine*, comparable to the content of each cell of the missing table, but in Taurus and Cancer these marginalia contain some inaccuracies compared to the text of the commentary itself.
170. Anon. comm. in Ptol. Apotel. 1.21 p. 47 Wolf: καὶ εὑρήσεις τὴν ἐπισυναγωγὴν τῶν ὁρίων ἑκάστου ἀστέρος συνᾴδουσαν τῇ τῶν παλαιῶν συγγραφέων συναγωγῇ. / *in qua* [sc. *figura*] *summam finium stellæ cuiusque cum ueterum scriptorum summa consentire inuenies.*
171. See above note 160.
172. Lib. Herm. Chapter 25 ed. Hübner (1995, v. 1, pp. 36–91).
173. A full catalogue of all preserved Greek and Latin horoscopes will soon be published, either in my forthcoming edition of Antigonus of Nicaea (see above note 27) or separately. As to Latin horoscopes, see above note 69.
174. See above p. 58, at note 90.
175. See above p. 63–65.
176. See above p. 66, after note 169.
177. See below, Table 8.
178. The manuscript tradition of these names is unanimous.
179. Rhet. 6.52.25: ὁρίοις κατὰ μὲν Πτολεμαῖον Ἀφροδίτης, κατὰ δὲ Αἰγυπτίους Κρόνου.
180. Rhet. 6.52.36: ὁρίοις κατὰ μὲν Πτολεμαῖον Ἀφροδίτης, κατὰ δὲ Αἰγυπτίους Ἑρμοῦ.
181. Rhet. 6.52.37: ὁρίοις κατὰ μὲν Πτολεμαῖον Ἀφροδίτης, κατὰ δὲ Αἰγυπτίους Κρόνου.
182. Note, however, a curious habit of Eutocius, namely to omit fractional rests. On examining all 24 specifications of Terms, one detects two such instances, one in each of the two systems: the position of Mercury (14° 32′ ♏, cf. Rhet. 6.52.30) should be assigned to the first degree of the Ptolemaic Term of Venus (14°–21° ♏) but is actually given to the last degree of the preceeding Term of Jupiter (6°–14° ♏), and the position of the upper culmination (19° 22′ ♏, cf. Rhet. 6.52.36) which should be assigned to the first degree of the Egyptian Term of Jupiter (19°–24° ♏) is actually given to the last degree of the preceeding Term of Mercury (11°–19° ♏).
183. W. Hübner in: Folkerts et al. (2001, p. 567).
184. Honigmann (1929, p. 116). We owe our knowledge on the early translators and commentators to Chapter 7.2 of the famous *Fihrist* of Ibn al-Nadīm (ed. Dodge 1970, v. 2, p. 640).
185. In this section, a general discussion of the Terms (5.8) is followed by chapters on the different systems of the Egyptians (5.9, a flawless table), Ptolemy (5.10, a contaminated table), the Chaldeans (5.11), Astratu (5.12, see next note), and the Indians (5.13).

186. This is in all likelihood the distorted form of a Greek personal name. The Latin translation of Herman of Carinthia reads *Aristotua* (Albumasar 1493, fol. e2^{v} = p. 67 of the BNF online version). No certain identification is possible. Boll (1894, p. 160) (quoted by Bezza 1990, p. 338), thought of Adrastus, but Wright (1934, p. 265, n. 3) adduced a variety of arguments that rather point to the Greek astrologer Erasistratus who is known from other sources (his date, however, is unclear and no fragments are preserved). A third Greek name, Straton, is brought into play by the Byzantine translation (see the following note). According to Abū Ma'shar, Introd. mai. 5.8.3, this *Astratu* was the only authority to assign Terms to all seven "planets," including the luminaries.
187. This is Chapter 5.8.2 in the forthcoming edition of the Arabic original with English translation by Ch. Burnett and K. Yamamoto. I thank the editors for sharing the unfinished draft of their translation with me. The corresponding passage of the somewhat shorter Byzantine Greek translation (Ἀποτελεσματικὰ μυστήρια τῆς ἐπιστήμης, Lat.: *De mysteriis*), which is edited by D. Pingree in the same volume, says (myst. 3.15.1): Εὕραμεν τοὺς ἀρχαίους πενταχῶς ἐκλαμβάνοντας τὰ ὅρια· εἰσὶ γὰρ ὅρια κατ' Αἰγυπτίους καὶ εἰσὶν ὅρια κατὰ Πτολεμαῖον καὶ εἰσὶν ὅρια κατὰ Χαλδαίους καὶ εἰσὶν ὅρια κατὰ Στράτωνα καὶ εἰσὶν ὅρια κατ' Ἰνδούς. Herman of Carinthia expanded Chapter 5.8.2 by remarking that the Egyptian Terms are of most frequent use and that experience proves them to be right. See his Latin text in Albumasar (1493), fol. e2^{v} (= p. 67 of the BNF online version; Engl. transl. in Burnett's and Yamamoto's apparatus to 5.8.2). Herman's Latin translation, which includes numerous addenda, is a personal interpretation of the Arabic original (see Burnett 2007, p. 74).
188. See also 5.8.9 where Abū Ma'shar says, without expressing his own preference, that all the early learned astrologers used the Terms of the people of Egypt.
189. In this paragraph, Herman of Carinthia adds several details of his own imagination (Albumasar 1493, fol. e3^{r} = p. 68 of the BNF online version).
190. For groups A and B, see above p. 67, after note 176, and (in more detail) below, p. 70, Table 8. I compared the Ptolemaic table (Chapter 5.10) as edited and translated by Burnett and Yamamoto with the earlier edition of Lemay 1995–1996, vol. II, p. 326 (I am grateful to G. Bezza, Bologna, for translating the Arabic data into English). From both editions it appears that Abū Ma'shar's table (Chapter 5.10) agrees with the consensus of A1 and B in ♈, ♊, ♍, ♐, ♒; it agrees with A1 against B in ♎ and with B against A1 in ♑ and ♓; deviations from both A1 and B: ♉ T4/5: ♄6 ♂2 (= closer to B = ♄2 ♂6 than to A1 = ♄4 ♂4); ♋ T4/5: ♀6 ♄4; ♌ T4/5: ♂6 ♃5 (close to A1 = ♃6 ♂5); ♏ T3/5: ♀6 … ♄4 (close to A1 = ♀7 … ♄3).
191. Abū Ma'shar, Introd. mai. 4.1.4: "There were a number of Greek kings immediately after the Two-Horned, Alexander, son of Philip, each of whom was called Ptolemy, namely ten, nine men and a woman. They lived in Egypt and their rule lasted 275 years. The majority of them were wise, and one of them was Ptolemy, the wise, who composed the book of the *Almagest* on the causes of the motion of the sphere and all the planets within it. Another of them composed a book on astrology and attributed it to Ptolemy, the author of the book of the *Almagest*. It is sometimes said that the very learned man who wrote the book of astrology also wrote the book of the *Almagest*. The correct answer is not known because of his (?) error, but the one who was the author of the book of astrology mentioned the natures of the planets and their causes in his book." The corresponding passage of the Greek translation (see above note 187) is *Myst.* 3.14.1–4. See also below on Haly (p.69 before note 203).
192. Chapter XIa of this long horoscope (p. 321 in the edition of Bezza 2001) wants the Lot of Fortune to be 9° Cancer and to fall into the Terms of Venus which is correct only in the Egyptian system, not in Ptolemy's. In Bezza (2001, p. 294, n. 10), correct "Xa" to "XIa."
193. The same can be found later in al-Bīrūnī's Chapter 494 (transl. Wright 1934, pp. 306–307). For al-Kindī, I rely on the account of Bezza (1995, v. 1, p. 286).

194. See the *fortitudines planetarum* in Alcab. introd. 1.22–23 in Burnett et al. (2003, pp. 239–240) (= Lat.; Arab./Engl.: ibid. pp. 32–33). The highest power (5) is that of the "House" (*domus*: *similis viro in domo atque in dominatione sua*), next (4) comes the dignity of "Exaltation" (*exaltatio*: *similis viro in regno suo atque gloria*), next the "Triangle" (3, *triplicitas*: *sicut vir in honore suo et inter auxiliatores suos atque ministros*) and the Terms (2, *terminus*: *sicut vir inter parentes suos et cognatos atque gentes*), with an option for reversal of the sequence (2–3 instead of 3–2). The least power (1) is that of the "Decan" (*facies*: *sicut vir in magisterio suo*). Still different—though incomplete—is a hierarchy that can be found in Firm. math. 8.32.2 (1: "Exaltation"; 2: Terms; 3: "House").
195. Alcab. introd. 1.19 (ed. Burnett et al. 2003).
196. Wright (1934, p. 265). For al-Bīrūnī's hierarchy of planetary dignities see above note 193.
197. On this astrologer see above note 186.
198. Wright (1934, p. 265).
199. Only Al-Bīrūnī's data in Scorpio are strangely flawed, but without shaking the evident grouping with **L**, **z**, and Hübner 1998. Al-Bīrūnī does not mention each single Term's extension but only the increasing sums within each sign. If one such figure is flawed, both the previous and the following Term are affected. Assuming that Wright's English translation is correct, Al-Bīrūnī's data in Scorpio are: ♂6 ♃12 ♀21 ☿24 ♂30. The single extensions are then: ♂6 ♃6 ♀9 ☿3 ♂6. (Hübner 1998 has: ♂6 ♃8 ♀7 ☿6 ♄3). Despite the apparent flaws it is clear that in the second and third Terms Al-Bīrūnī does not join Hephaestio's group in inverting the order of the planets. —Note that Bezza (1992) provides a new Italian translation from the Arabic text edited by Wright (1934). Bezza (p. 80, Chapter 453) has the Ptolemaic Terms of Scorpio in agreement with Hübner (1998). Bezza's commentary (p. 158) does not mention the different data in Wright's translation.
200. This is the same as Aristotua in the Latin translation of Abū Ma'shar and as Al-Bīrūnī's Astaratus. See above note 186.
201. *Opinio maioris partis hominum que est magis veridica et magis experta est opinio Egyptiorum; et pauci utuntur terminis Ptolemei. Et de nihilo utuntur aliis opinionibus, nec ad eas inspiciunt, quia sunt a veritate remote* (Abenragel 1523, fol. 5ra-b; my transl.).
202. Haly (1493, fol. 24^{v}–27^{v}). The correct year of birth (988, not 998) is clear from Ibn Riḍwān's analysis of his own horoscope which Seymore (2001, pp. 222–232) translates from the Arabic MS Bodleian I 992 (Marsh. 206). The horoscope is for Jan. 15, 988 A.D.; see esp. Seymore (2001, p. 222, n. 2).
203. See Seymore (2001, pp. 207–210) (Engl. transl. of the Arab. orig.) and Haly (1493, fol. $2^{va\text{-}b}$) (Latin transl. which omits the name of Ptolemy II Philadelphus) as well as note 191 above.
204. Haly (1493, fol. 26^{vb}) ad Apotel. 1.21.22: *Ad hoc intelligimus quod isti termini de quibus pthole. loquitur non fuerunt sui. quia si sui essent non preponeret dominum exaltationis etc.* "We understand right away that these Terms of which Ptolemy speaks are not his own, because if they were his own, he would not put the lord of the exaltation first etc."; ibid. fol. 26^{va} ad Apotel. 1.21.21: *Hec verba demonstrant magnam bonitatem ptholemei, qualiter multum diligebat veritatem, quoniam de verbis alienis noluit sibi attribuere gloriam, nec verecundatus fuit palam dicere, quod in illis rationibus longis, que scripte erant in libro ipso,* [*et*] *non poterat reddere rationem.* "These words demonstrate the great goodness of Ptolemy, how much he loved the truth, because he did not want to attribute to himself the glory that was due to other people's words. And he also did not shy away from saying publicly that he was unable to explain those long reasonings that were written in that book." (my transl.).
205. Haly (1493, fol. 27^{va}): *Semper tamen conuenit quod ante inspicias ad terminos egyptianorum. Postmodum ad terminos tphole.* [*sic*] *propter rationem quam dixit et subsequenter ad terminos caldeorum.* See the commentary on this by Bezza (1990, p. 340).
206. See Haly (1493, fol. 105^{ra}–106^{vb}) (Lat. transl.) and Seymore (2001, pp. 222–242) (Engl. transl. of the Arab. orig.). In his autobiographical horoscope, the use of the Egyptian Terms

is clear from Ibn Riḍwān's remarks on the Lot of Fortune (3° 48′ ♑), on the MC (0° ♑), on Jupiter (0° 28′ ♑), and on the Sun (0° 23′ ♒): all four positions are said to fall into a Term of Mercury, which is in all cases true for the Egyptian system while false for the Ptolemaic one. See esp. Seymore (2001, pp. 228–229, n. 12, 17, 18), as well as the tabular arrangement of the horoscope data ibid. p. 245. Further confirmation comes from the second horoscope (Haly 1493, pp. 105^{vb}–106^{rb}, Seymore 2001, pp. 232–238), that of a boy whose Lot of Fortune is 0° 36′ ♑. This is again a Term of Mercury, as Ibn Riḍwān says (p. 235); he emphasizes the agreement between the boy's and his own Lots of Fortune (p. 237). Note that Seymore's note 37 on p. 235 is confused; it ought to read: "According to the Egyptian system the native's Lot of Fortune is in the term ruled by Mercury."

207. It was edited by M.A.F. Šangin, CCAG v. XII (1936), pp. 216–229.
208. Ibid. pp. 217,26–218,9.
209. This translation covers the whole of books I and II plus parts of III and IV.
210. One by Antonio Gogava was printed in 1548, one by Philipp Melanchthon was printed with Camerarius' revised edition of 1553. For details on the early editions and translations, see Boer (1959, col. 1832), and Hübner (1998, pp. LII–LIV).
211. Houlding (2007, p. 276).
212. In the 1533 edition of Plato of Tivoli's translation which I have used, the Ptolemaic table of Terms is on p. 19 (wrongly numbered 17). Plato of Tivoli's Arabic source is to be distinguished from al-Bīrūnī's version which has the same table of Ptolemaic Terms as **L** and **z** (see above p. 69). It would go beyond the purpose of this article to investigate the Arabic transmission of the *Apotelesmatika* systematically, so as to determine (if possible) the number of translations that were available, and their characteristics.
213. Hephaestio gets the data in ♉ T4/5 and ♐ T2/5 wrong; see above notes 151 and 152. The *Proclus Paraphrase* (1st rec.) gets the data in ♋ T2/3 and in ♌ T2/3/4 wrong; see above notes 153 and 154. Plato of Tivoli's Latin translation from the Arabic incorrectly anticipates ♌ T5 before ♌ T3/4 and is followed in this by Bonatti (1550). Plato of Tivoli 1533 further has an obvious numerical mistake (5 instead of 7) in ♈ T3, but I cannot tell wether this fault is to be attributed to the typesetter, to Plato himself, or to his Arabic source (in the two latter cases Bonatti's correct value would be an intelligent restoration of the required figure).
214. Since Eutocius (ap. Rhet. 6.52.25 and 6.52.37) assigns both Leo 17° 35′ and Leo 16° to Venus (see above notes 179 and 181), he belongs to group A. No agreement with any of the discordant planetary sequences reported in group B is possible, as the following table shows:

A: **L**, **z**	♄6 ☿7 ♀6 ♃6 ♂5	(i.e. 13°–19° = ♀: this agrees with Eutocius)
B: Heph.	♃6 ♀6 ☿7 ♄6 ♂5	(i.e. 12°–19° = ☿: contradicts Eut.)
B: Procl. Par. (1st rec.)	♃6 ☿7 ♄6 ♀6 ♂5	(i.e. 13°–19° = ♄: contradicts Eut.)
B: Plato of Tiv., Bonatti	♄6 ☿7 ♂5 ♀6 ♃6	(i.e. 13°–18° = ♂: contradicts Eut.)

215. The *Anonymous Commentary* gives 1° too much to Jupiter (violation of planetary totals, see p. 66 after note 169). His values in Gemini are disproved by the Hephaestio-group.
216. Since Robbins follows the *Proclus Paraphrase*, his data in Leo are ♃6 ☿7 ♄6 ♀6 ♂5 (see above note 214).
217. A remotely similar problem is raised by the last paragraph of the *Apotelesmatika* (4.10.27) of which the manuscripts transmit two different versions.
218. For Bonatti see Bònoli and Piliarvu (2001, pp. 39–43) with extensive references to further literature.
219. Thorndike (1923–1958, v. 2, p. 826).
220. Bonatti (1550, coll. 46–48) (47–48 are wrongly numbered "49"–"50").

221. Only the first reference is explicit (col. 46 *Albumasar recitauit*). Haly is alluded to in col. 46 *ipse enim nolens se iactare, non attribuit sibi inuentionem illorum terminorum* (cf. note 204), al-Qabīṣī in col. 47 *quia dicitur quòd cum Planeta est in termino suo, est similis uiro existenti inter parentes suos & inter gentes quæ attinent ei, & diligent eum siue sint cognate siue agnati siue affines* (cf. note 194).
222. Bonatti (1550, col. 46): *& postea posuit suam opinionem, & fuit secutus dicta quorundam uetustorum, quæ ipse inuenit, in quodam libro uetustissimo, prout ipse refert, & approbauit dicta illorum* ("and then [i.e. after the exposition of the Egyptian and Chaldean systems] he put forth his own opinion, and he was following the sayings of certain men of old, which he personally discovered in a certain very old manuscript, as he himself reports, and he approved their sayings").
223. See note 187 above.
224. Abū Ma'shar, Introd. mai. 6.1.31: "In its [i.e. Virgo's] first decan there ascend a maid whom Tinkalos (Teucer) called *dūšaybuh*. She is a pretty, clean, longhaired, and good-looking virgin, with an ear of corn in her hand. She is sitting on a chair on which is a mat, and she is nourishing (?) a little boy, and feeds him with broth in a place called 'atria.' Some people call that boy *Īsū'*, whose meaning is Jesus." The corresponding passage of the Greek translation (see above note 187) is *Myst.* 3.21.46.
225. On the doctrine of the "Great Conjunctions" and its assignment of one planet to each world religion see, as introduction to the subject, North (1980) and Bertozzi (1996).
226. Roger Bacon, *Opus Maius*, *pars quarta*, p. I 260 Bridges: *Famosiores autem termini sunt Aegyptiorum. Jupiter habet sex primos gradus Arietis, Venus sex sequentes, Mercurius octo, Mars quinque, Saturnus quinque, Venus adhuc octo primos Tauri, Mercurius sex sequentes. Et sic mira diversitate variantur isti termini, ut patet in tabula terminorum, ita quod Mercurius habeat septem primos gradus Virginis pro termino, non solum secundum Aegyptios, sed secundum Ptolemaeum, et hoc est quod nunc quaerimus.* "More famous, however, [i.e. than the triplicities and exaltations] are the Terms of the Egyptians. Jupiter has the first six degrees of Aries, Venus the next six, Mercury eight, Mars five, Saturn five, Venus then the first eight of Taurus, Mercury the six following ones. And so those Terms vary with remarkable diversity, as can be seen in the table of the Terms, so that Mercury has the first seven degrees of Virgo as his boundary, not only according to the Egyptians but (also) according to Ptolemy. And this is what we shall investigate now." (my transl.).
227. See Smoller (1994).
228. On this work, see Smoller (1994, p. 59). I thank Laura Smoller for sending me photocopies of one of the two existing manuscripts, ms. Vienna (5266, fol. 46^{r}–50^{v}). The table of the Egyptian Terms is on fol. 49^{v}.
229. The illustration is from Page (2002, p. 22, Fig. 14).
230. On Pico, see the contribution by Darrel Rutkin in this same volume.
231. Pico disp. 6.16 (Garin 1946–1952, v. 2, p. 128): *Reliquum est de finibus, quos non aliter quidem breviter confutabo, quam ex discordia ipsorum adhuc de finibus inter se litigantium* […]; eventually he concludes (ibid. p. 130): *Vanissima igitur dogmata astrologorum, quae nec rationibus firmant, nec experimentis, quando in illis nugantur, in istis non concordant.*
232. In the same vein, he returns to the topic of the Terms in Chapter 10.10, but without adding anything of substance (Garin 1946–1952, v. 2, pp. 414–416).
233. For instance, al-Qabīṣī's hierarchy of the planetary dignities (see note 194) can be found in numerous printed books and manuscripts from the Renaissance like Johannes Stöffler's *Almanach Nova* (1499), fol. $D1^{v}$, and ms. Sloane 332, fol. 9^{v}, of the British Library (see the photograph in Page 2002, p. 24, fig. 15).
234. *Reverendi ac eximij magistri Iuliani Ristori Pratensis per me Amerigum Troncianum, dum eum publice legeret in almo Pisauri ginnasio currente calamo collecta*, MS Florence, Bibl. Ricc. Lat. 157, fol. 137^{r}–138^{v} (*non vidi*; I quote from Bezza 1990, pp. XXVII and 339).
235. Nifo (1513, fol. 36^{vb}, cf. 37^{ra}) (*non-vidi*; see previous note).

236. Cardano (1578, pp. 190–199) (= Cardano 1663, v. 5, pp. 156a–162b). This is almost 10% of the length of Cardano's entire commentary on the *Apotelesmatika*, a huge disproportion compared to the 3–4% that Apotel. 1.21 makes of the whole text.
237. Cardano (1578, pp. 192 and 194) (= Cardano 1663, pp. 157a and 159a): *Haly nullo modo intellexit ...Porphyrius in nulla alia re utilis quàm in commemorandis huiusmodi nominibus Trasibuli & Petosiridis ...ut Porphyrius totam artem ut nunc etiam Latini & Arabes olim deturpari*[*n*]*t*. The criticism of Porphyry is particularly inappropriate because that chapter on the Terms is not authentic; see above note 89 on Ps.-Porph. introd. 49.
238. Cardano (1578, p. 192) (= Cardano 1663, p. 157a).
239. Cardano (1578, p. 194) (= Cardano 1663, v. 5, p. 159a).
240. Cardano (1578, pp. 194 and 196) (= Cardano 1663, v. 5, pp. 158b and 160a).
241. *Liber duodecim geniturarum* […], Basel ^{2}1578 (^{1}1544), n. VIII. This autobiographical horoscope fills 50 columns in the folio format of the *Opera omnia* (Cardano 1663, v. 5, pp. 517a–541b).
242. *De septem erraticis stellis liber*, Cardano (1663, v. 5, pp. 369–432). He opens each of the seven sections with a tabular set of astronomical and astrological data for the planet in question, beginning—as usual—with the outermost planet Saturn (pp. 369–382) whose data sheet (369–370) lists the Terms of Saturn with the remark: *Notandum circa hos fines quod secutus sum opinionem Ægyptiorum, qui alios volet legat in Ptolemæo supra*. ("Note about these Terms that I followed the opinion of the Egyptians. Who wishes other ones, may read in (my commentary on) Ptolemy above"; my transl.).
243. *Liber de iudiciis geniturarum*, Cardano (1663, v. 5, pp. 433–457), here: Chapter 27 (pp. 453–456) *De viribus & dignitatibus planetarum*, esp. pp. 455–456 (*Terminorum vero ratio alia longe est nobis ac aliis …*). Cardano's own table is on p. 456.
244. In this regard Cardano's system is indeed profoundly different from all three systems discussed by Ptolemy. Remember, however, that some ancient systems included the luminaries (see above p. 48 on Valens and the Michigan papyrus, and also note 186 on Erasistratus).
245. Junctinus (1581–1583, v. 1, pp. 73–77). Giuntini's index (end of v. 2) has no further references for the Terms.
246. See esp. Campanella (1629, Chapter 1.7.6, p. 41) (appendix with a reference to America), and Chapter 1.7.7.4, p. 42: *Aduertendum quod termini non sunt vbique locorum iidem* […]: *proptereáque in hemicyclo vltra œquatorem terminos Arietis ponemus in ♎, & è contra: sic ♉ in ♏, & è contra: & ♊ in ♐, & sic de cœteris seriatim etc.* ("Note that the Terms are not in all locations the same […]: and therefore in the hemisphere beyond the equator we shall place the Terms of Aries in Libra, and viceversa; in the same fashion Taurus in Scorpio, and viceversa; and Gemini in Sagittarius; and so with the other ones in accordance with the series").
247. Ibid. Chapter 1.7.7.1, p. 41: *At equidem nisi à Deo reueletur hoc negotium, haud credam potuisse singulorum graduum plurima experimenta facta fuisse satis vt inde scientia prodiret.* […] *Ego enim de his nil certi me habere profiteor, nisi quod ratiocinatio prœdicat: In omnibus terminis, & bonos & malos euentus experiri.*
248. Salmasius (1648, pp. 288–292, esp. p. 289): *Circa hanc ὁρίων distributionem Ptolemæus ab antiquis dissentit. Sed antiquiorem ad exemplum sumemus opinionem, cum Ptolemæi nota sit.*
249. There was a posthumous reprint in 1675 under the title *Physiomathematica sive Cœlestis Philosophia*.
250. See Vitali (1668, p. 190) s.v. *Finis* (F17) and pp. 495–497 s.v. *Termini* (T19). In the reprint by Bezza and Faracovi (2003), see pp. 230 and 531–533.
251. Lilly (1647, p. 104): "A Table of the Essentiall Dignities of the PLANETS according to *Ptolomy*."
252. Lilly (1647, p. 103).
253. Pico disp. 6.16 (Garin 1946–1952, v. 2, p. 128): *iuniores Aegyptiis adhaerent, Graeci Ptolemaeum sequuntur*.
254. See p. 73 above, at note 235.

255. The few remaining astrologers who continued to use the Ptolemaic Terms in the 19th and 20th centuries unanimously followed Lilly (see Houlding 2007, p. 269, with bibliographical references in note 15).
256. See http://www.projecthindsight.com (it originated form *The Golden Hind Press*, founded 1985).
257. See http://www.cieloeterra.it. This association is centered on the scholarly expertise of G. Bezza. My article profited greatly from Bezza's learned commentary on the first book of the *Apotelesmatika* and their reception (Bezza 1990).
258. The proceedings have been published in vol. 11 of Culture and Cosmos (2007). A similar conference was held in Amsterdam in 2004; for its proceedings see Oestmann et al. 2005.
259. Houlding works from a practitioner's perspective. Her research project came to my knowledge while I was preparing my own paper on the same topic for *Caltech*. The present article profited greatly from Houlding's kind permission to read hers before it went to the press, especially with regard to the host of valuable material that she presents. As to the insights that she draws from it, my notes make it clear where I am indebted to her. Note, however, that the two articles are dissimilar in purpose and method, mine being chronologically arranged, strictly philological, and interested in the problem of the authenticity of the Ptolemaic Terms (while renouncing to the laborious discussion of each one of them).
260. See Houlding (2007, pp. 286–306), and the twelve pages in Bezza and Fumagalli (online), one for each sign, starting with Aries at http://www.cieloeterra.it/articoli.confini/confini01.html.
261. Houdling (2007, p. 293).
262. Houlding (2007, p. 307), concludes her article providing an experimental table (Fig. 15) of what the Ptolemaic table of Terms *should* look like when one consistently applies "the rules that seem most reliably expressed" (p. 306) in the transmitted values.
263. Houlding (2007, p. 306) comes to a similar conclusion: "it now seems impossible to validate any historical table of Ptolemaic terms as demonstrably accurate and consistent in its logic."
264. This is particularly important with regard to Camerarius (1535) and Robbins (1940), who except for Apotel. 1.21.28–29 do *not* follow the *Proclus Paraphrase*, but manuscripts of the direct transmission.
265. See above p. 46 after note 10.
266. See the quotation from Lilly p. 74 above and the reference to Pico in note 253.
267. See, for instance, the correct explanation of the substantial disagreements between Dorotheus of Sidon and Ptolemy in Holden (1996, p. 33): "The reason is simple: Dorotheus was in the mainstream of Greek astrology, and Ptolemy was not."

The Tradition of Texts and Maps in Ptolemy's *Geography*

Florian Mittenhuber

The Preserved Manuscripts

There remain today fifty-three Greek manuscripts of Ptolemy's *Geography*,[1] none of which were written before the late thirteenth century. There is, therefore, a time span of about 1,000 years between the original work and the earliest extant manuscripts. The main lines of the textual tradition were outlined in the 1930s, in particular by Cuntz (1923), Schnabel (1930, 1938), and Fischer (1932a), and are—more or less—undisputed. However, debate on the relationship between text and maps, and especially the tradition of the maps themselves, is far from over.

The different viewpoints can be summed up as follows:

> Cuntz (1923), Fischer (1932a), Schnabel (1938), Polaschek (1965), and Schmidt (1999) all argue that Ptolemy's original work contained maps. Most of them are also of the opinion that Agathodaimon designed or reworked the maps: Cuntz, Schnabel and Polaschek believe that he worked on the world map and 26 regional maps, Fischer on the world map in the first projection.
>
> By contrast, Polaschek (1965) – for the maps of manuscripts U, K, and F – Bagrow and Skelton (1985), Harley and Woodward (1987–1992), Aujac (1993), and Berggren and Jones (2000) do not see a tradition in the maps dating back to antiquity but rather believe that the maps were reconstructed by Planudes in Byzantine times. Sezgin (2000) is even of the opinion that Planudes reconstructed all the maps from Islamic models.

The tradition of Ptolemy's *Geography* can be divided into two main recensions: Ω and Ξ (Fig. 1). Recension Ω, which includes the majority of the manuscripts, is subdivided into two further groups: Δ and Π. Group Δ contains parchment manuscripts from the end of the thirteenth century, which are the earliest extant manuscripts of the *Geography*, and, therefore, the most important. They are:

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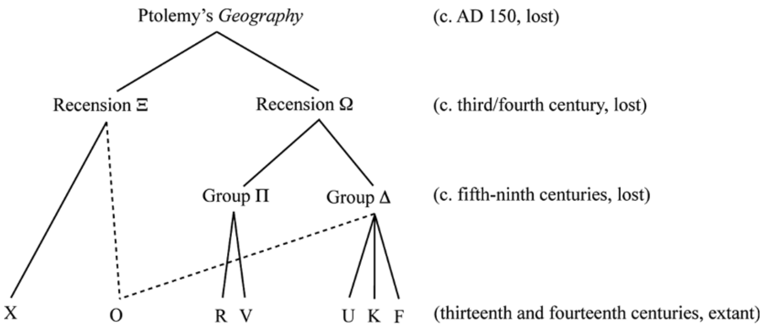

Fig. 1 The tradition of Ptolemy's *Geography* (Stemma)

U: Codex *Vaticanus Urbinas Graecus 82* (Vatican);[2]
K: Codex *Seragliensis GI 57* (Istanbul);[3]
F: Fragmentum *Fabricianum Hauniensis Graecus 23* (Copenhagen).

Group Π includes:

R: Codex *Marcianus Graecus 516 (=904)*, beginning of the fourteenth century (Venice);[4]
V: Codex *Vaticanus Graecus 177*, probably end of the thirteenth century (Vatican).[5]
The other recension, Ξ, is represented by one codex only:

X: Codex *Vaticanus Graecus 191*, middle/end of the thirteenth century (Vatican).[6]

The so-called Codex X is of particular significance, because it contains many local names and coordinates that differ from the other manuscripts mentioned above, and which cannot be explained by mere errors in the tradition. Unfortunately, none of the coordinates from *Geography* 5.13.17 onwards of this codex were ever copied.

There are also a small number of so-called *Mischhandschriften* that have been influenced by both recensions. With regard to the tradition of the maps, the most important manuscript of this group is:

O: Codex *Florentinus Laurentianus Graecus 28.49,* beginning of the fourteenth century (Florence).[7]

As regards the maps, we can establish from the start that the textual differences between the manuscripts in recensions Ξ and Ω as well as the subdivision of the latter recension into groups Δ and Π are apposite to the tradition of the maps.

There are basically two different types of map sets: the first has a world map at the end of Book 7 and twenty-six regional maps in Book 8, and is the classical set on which Ptolemy's entire work is conceptionally based. A second group of manuscripts shows a later redaction, with one world map and sixty-four regional maps, included in Books 2 to 7.[8]

Altogether, sixteen of the fifty-three preserved Greek manuscripts still contain maps. However, not all these sets are complete, and their quality is uneven. Of these sixteen manuscripts, only four are of relevance to the tradition of the maps: in the classical set with twenty-six regional maps, manuscripts U and K and the fragment F are the most important, since their maps have been drawn with great accuracy and care; they also follow Ptolemy's instructions concerning the maps' proportions. The maps of Codex R are far less carefully drawn, and do not follow the proportions prescribed by Ptolemy.

The following investigation focuses mainly on these four primary manuscripts. Of the other twelve map-containing manuscripts, only Codex O will be examined, since it is undoubtedly the earliest surviving manuscript with sixty-four regional maps.[9] The other eleven manuscripts (seven with twenty-six regional maps, four with sixty-four) are derived, either directly or indirectly, from manuscripts U and O, and are, therefore, of secondary importance.

Manuscripts V and X have no maps. Nevertheless, they are also relevant to the tradition of the maps, because they contain notes and scholia which refer to the maps of their master copies.[10]

The Theoretical Conceptions of Map Drawing: The General Concept

Ptolemy's *Geography* can be divided into three sections:

Part 1: Theoretical Introduction (Chapters 1.1–1.24).
The first part contains the theoretical outlines for drawing world maps. In the introduction to Book 1, Ptolemy reflects on the reliability of geographical data in general (Chapters 1.1–1.5), and follows this with a long critical discussion of the work of his predecessor, Marinos of Tyre (Chapters 1.6–1.17). The book closes with Ptolemy's own conception of drawing a world map on a globe and on a plane surface (Chapters 1.18–1.24).

Part 2: Catalogue of localities (Chapters 2.1–7.4).
The second part covers Books 2 to 7.[11] It begins with a short introduction (Chapter 2.1), and then continues with antiquity's most detailed geographical database, that is, a catalogue of eighty-four provinces with about 8,000 localities (cities, rivers, mountains, and so on), among them around 6,300 localities that are determined by coordinates, and about 1,400 peoples and 200 names of regions and seas for which no coordinates are given. The *oikoumene* described by Ptolemy is bound by the Islands of the Blest (Canary Islands) to the west, China to the east, Scotland and southern Scandinavia to the north, and Central Africa and Indonesia to the south.

Part 3: Atlas (Chapters 7.5–8.30).
The third part of the *Geography* is a kind of atlas, containing one world map (originally there were three) at the end of Book 7 and twenty-six regional maps in Book 8. The maps are supplemented by instructions for constructing and drawing the maps, the so-called *katagraphai* and *hypographai.* Although they complement each other, they do not always follow on from each other. For instance, the *katagraphe* of Chapter 1.24, which contains Ptolemy's detailed instructions for constructing the world maps in two different conical projections, is followed by the *hypographe* of Chapter 7.5, which consists of a brief description of the borders of the *oikoumene* and their corresponding parallels, with the addition of two drawn world maps in the first and second projections. The following chapter (7.6) contains a *katagraphe* for constructing an armillary sphere with an inscribed *oikoumene*, and ends with the *hypographe* of Chapter 7.7, with an additional world map in the third projection.[12]

The same system is applied to the regional maps: the *katagraphe* of Chapter 8.1, which contains detailed instructions for constructing the regional maps, corresponds not only to the general *hypographe* of Chapter 8.2 but also to the special *hypographai* of Chapters 8.3 to 8.28. These twenty-six chapters (one for each map) contain captions for the maps and the lists of "noteworthy cities" (*poleis episemoi*). [13] Chapters 8.29 and 8.30, which contain lists of the provinces and the frame borders of the maps, are probably not Ptolemaic.

The Appearance of the Maps

World Maps

Almost all the surviving world maps are drawn in Ptolemy's first projection, that is, in a simple conical projection with straight meridian lines; only the world map in manuscript K shows the second projection, that is, the modified conical projection with curved meridian lines. All the world maps are divided by thirty-six meridians at intervals of five degrees from each other and by twenty-four parallels, starting from the equator and counting twenty-one parallels to the north and two parallels to the south. The position of the parallels follows the length of the longest day on the corresponding geographical latitude and agrees with the information of the corresponding text in the *katagraphe* of Chapter 1.23.[14]

Table 1 Technical information on the parallels in the world maps

Parallels	Longest day	Reference location	Klima	Length in stades
21st	20 h	Thule*		40,854
15th	16 h	Borysthenes	7	
14th	15 h 30′	Central Pontos	6	
12th	15 h	Hellespont	5	
10th	14 h 30′	Rhodes*	4	72,812
8th	14 h	Alexandria	3	
6th	13 h 30′	Syene*	2	82,336
4th	13 h	Meroë*	1	86,333
0	12 h	Equator*		90,000
1st south	12 h 30′	Cinnamon country		
2nd south	13 h	Anti-Meroë*		86,333

Additional information on the parallels is given in the left margins of the world maps (Table 1). The simplest form mentions the number of the corresponding circle and the length of the longest day: for instance, tenth parallel, longest day 14 h and 30 min.[15] In some cases, the length of the parallel is also given in stades, in the appropriate ratio to the equator, i.e., 90,000 stades multiplied by the cosine of the latitude.[16]

Together with the number of hours, a reference location is also given: the original additions (marked in the table by an asterisk), which appear in all the manuscripts in the text of Chapter 1.23, cite the principal parallels of Thule, Rhodes, Syene, Meroë, the equator and the opposing parallel of Meroë (Anti-Meroë), and are, therefore, indispensable for constructing the world maps. They are mentioned several times in the different *katagraphai* and *hypographai* (Chapters 1.24 and 7.5–7). The reference locations that were added later appear only on the maps and in some of the texts of the secondary manuscripts, and are not relevant for constructing the world map.

These later reference locations are related to the geographical tradition of the seven climes or *klimata*,[17] which are zones of the same geographical latitude that extend over an area of half an hour of daylight time. These *klimata* do not appear in the text of the *Geography* but are only inscribed on the maps in relation to the corresponding parallels. Thus, we can assume that they did not appear in Ptolemy's original world maps but were added by a later draftsman.

All the surviving world maps show the twelve zodiacal signs, together with a stylized Sun, in the right margin of the map (Fig. 2). The zodiacal signs are grouped into two rows and extend between the Winter and the Summer Tropics according to the Sun's position in the zodiac during one year. The names of the signs are given in Greek; in some maps, the names of the months are given in Egyptian and Latin. All the drawings of the signs on the different maps resemble each other and show typical late antique features: for example, the figures are naked. The star catalogue in the so-called *Aratus Leidensis*, a Carolingian manuscript containing the Latin translation by Germanicus, which clearly dates back to a master of late antiquity, has very similar illustrations.[18]

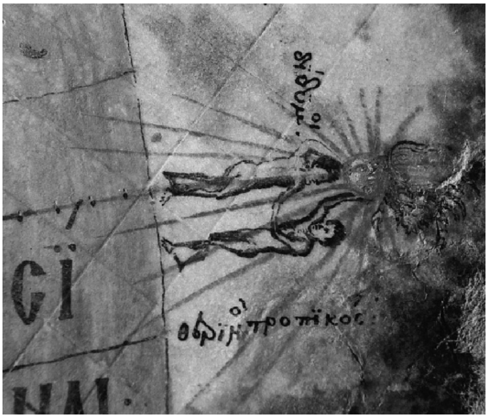

Fig. 2 Zodiacal signs (Codex K, fol. 74r.)

Fig. 3 Winds (Codex K, fol. 73v.)

Surrounding the maps are twelve human heads that symbolize the winds (Fig. 3). The names of the winds follow the twelve-part wind rose of Timosthenes of Rhodes, mentioned by Ptolemy as an author reference in Chapter 1.15. In a twelve-part wind rose, the distance between the single orientations is 30°. In this case, they mainly follow the cardinal directions north, east, south and west as well as the directions towards the polar circles and the Tropics. Ptolemy explicitly states that "the names of the Winds [shall be inserted on the map] in accordance to their indications on the armillary sphere at the five mentioned parallels and the poles" (7.16.15). However, these instructions are only appropriate to a map of the third projection. When this system, which was developed for the armillary sphere, is assigned to a world map of the first or second projections, the orientations change slightly. Thus, the names of the winds seem originally to have been written on a map of the third projection.[19] Furthermore, all the surviving world maps are based on this reworked version of Ptolemy's world maps. The original maps, in particular those of the third projection, can, therefore, be regarded as lost.

Redaction with Twenty-Six Regional Maps

Almost all the surviving manuscripts with maps follow Ptolemy's original conception of twenty-six regional maps, divided into ten maps of Europe, four of Africa and twelve of Asia. This concept is explained by Ptolemy in the general *hypographe* of Chapter 8.2. In manuscripts U and K, all of these twenty-six maps have been preserved; in the bifolium of manuscript F, only three half maps have survived. In these manuscripts the maps are displayed in their original position following the special *hypographai* of Chapters 8.3–28; that is, after the list of "noteworthy cities", the corresponding map is shown.

The construction of the maps in manuscripts U, K, and F follows the instructions given in Book 8 extremely closely, carefully taking into account the ratio of the center parallel to the meridian as described in the captions of Chapters 8.3–28, as we can see in the recalculated graticule (Fig. 4).[20]

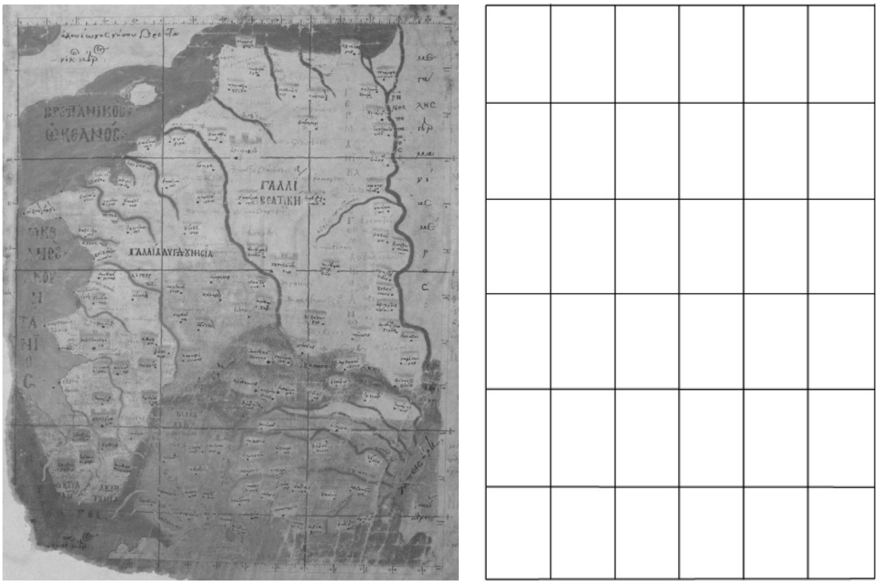

Fig. 4 Third map of Europe from manuscript K (fol. 80r.): correct graticule

By contrast, it is clear that, in manuscript R, in which all the maps have been assembled at the end of the *Geography*, the instructions of Chapters 8.3–28 regarding the correct ratio have not been understood: as the repetition of the maps' format in manuscript R reveals, these maps have been drawn simply by following the framing format of the double page, resulting in an inappropriate distortion of the maps (Fig. 5).[21]

Redaction with Sixty-Four Regional Maps

Unlike the twenty-six maps of the aforementioned manuscripts, Codex O and its copies contain sixty-four smaller maps. These maps do not appear at the end of Book 8 but in the catalogue of localities in Books 2–7, following the description of each country. The maps of these versions also use the cylindrical projection.

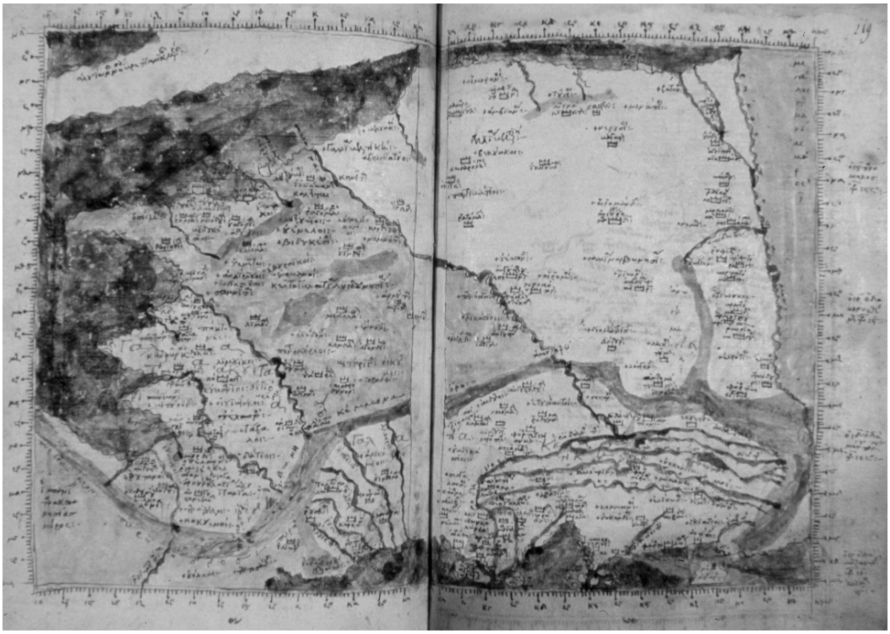

Fig. 5 Third map of Europe from manuscript R (fol. 118v./119r.): distorted graticule

This raises an important question. Could these sixty-four maps have resulted from the division of the original twenty-six maps? If so, the ratio of the center parallel to the meridian of the corresponding maps of O (for example, the partial maps of Spain) should be the same for each map, even though the latter have different center parallels.[22] In the case of an independent drawing of the corresponding maps, different ratios of the center parallel to the meridian would be expected.

Table 2 Calculated and true ratios in the second and third maps of Europe in MSS U, K, and O

Maps of MSS U and K	Theoretical ratio (Chapters 8.3–8)	Maps of MS O	Calculated ratio (maps of MS O)	True ratio (maps of MS O)
Second map of Europe	3 : 4 = 0.750	Hispania Baetica	37.375° = 0.795	0.800
	3 : 4 = 0.750	Hispania Lusitania	39.5° = 0.772	0.763
	3 : 4 = 0.750	Hispania Tarraconensis	42° = 0.743	0.740
Third map of Europe	2 : 3 = 0.667	Gallia Aquitania	45.75° = 0.698	0.693
	2 : 3 = 0.667	Gallia Lugdunensis	48.5° = 0.663	0.667
	2 : 3 = 0.667	Gallia Belgica	49.625° = 0.648	0.662
	2 : 3 = 0.667	Gallia Narbonensis	44° = 0.719	0.704

The data in Table 2 closely corresponds to the recalculated values of the different center parallels and the related map, which implies that these sixty-four maps were, without any doubt, constructed independently. It seems clear that the copy editor of manuscript O decided from the start to create a completely new edition of the *Geography,* integrating all the maps into the catalogue of localities in Books 2–7 —perhaps because a smaller page format was to be used. For this purpose, the space needed for the text and the maps had to be recalculated and, therefore, the frames of the maps had to be determined using the maxima and minima of the coordinates, with the correct ratio also recalculated. In this way, the space needed for the maps or their respective formats could be determined.

This conjecture can be backed up by examining the map of Gedrosia, where the land does not fit into the frame and extends into the text, protruding into the frame of the northern border (Fig. 6). This error obviously resulted from a mistake made by the copy editor of manuscript O. He did not recognize the coordinates of this point in the text, which is—significantly—not part of the description of the borders of Gedrosia (6.21.1f.) but can be found in an earlier part of the text, in the border description of the Karmanian Desert (6.6.1). From this mistake, an incorrect format for the map followed. Such an error would not have occurred had there been a map model.

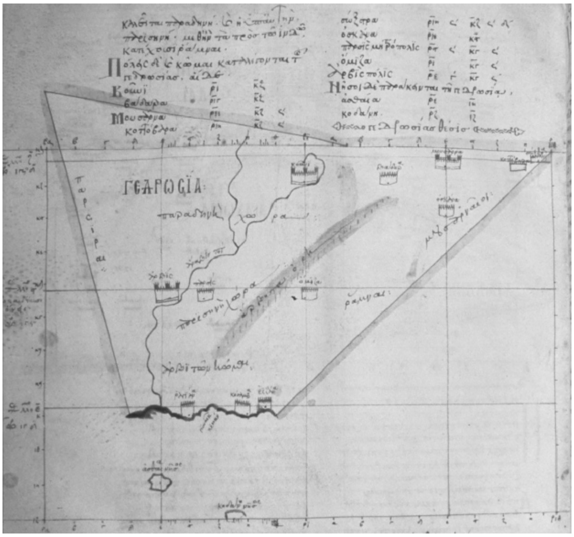

Fig. 6 Map of Gedrosia in manuscript O (fol. 86v.): text and map clearly overlap

Internal Manuscript Agreements and Differences Between Text and Maps

In the broadest sense, the drawing of a map is the translation of a text into an image. When the text contains instructions on how to create the drawing, and the original maps have also survived, the text and maps can be checked against each other. This is particularly advantageous when—as in the case of the *Geography*—some of the connecting links may have been lost and transcription errors may have accumulated. Then, agreements and differences found on the maps allow one to reconstruct the history of the map tradition.

Their relevance is twofold: first, agreements and differences between the text and the maps in the same manuscript prove that the map has been drawn according to the text; second, agreements among the maps of different manuscripts indicate an older tradition, especially when these agreements cannot be derived from the text.

The Common Ancestry of Manuscripts U, K, and F

As shown above, the construction of the maps in manuscripts U, K, and F and their positioning in Chapters 8.3–28 are very similar. The technical execution of the drawings with their frame borders and the subdivision of the degrees of longitude[23] also strongly indicate a common archetype.

Furthermore, features in the maps common to U, K, and F show additional indications of a common ancestry: for example, the maps contain some needless but exactly concordant entries—such as “the two Balearic islands, called *Gymnesiai* in Greek” (2.6.78), which were copied directly from the corresponding text.

Especially interesting are entries on the maps that have been erroneously taken from a misleading textual source. On the fourth map of Europe in manuscripts U and K, for instance, we can read the tribe’s name “Protoi Sidones” (the first Sidons), whereas the texts of both manuscripts speak of “the first [living south of these] are the Sidons” (2.11.21).

Similar errors can be found in the transmitted coordinates. A good example is the legendary Island of Thule on the northern boundary of Ptolemy’s *oikoumene* (Fig. 7): following the transmitted data in the texts (2.3.32), the center of the island is supposed to be situated at 33° of longitude, that is, more than one degree further east than the easternmost part of the island (31° 40′ of longitude). Because the maps in manuscripts U and K have been drawn according to the coordinates given in the texts, the center of the island is located on the eastern edge, while the eastern edge has been placed in the center. This inconsistency has clearly been caused by a transcription error in the separation of degrees and minutes. By making a paleographically simple correction, the coordinates can be changed from

$\lambda\gamma° = 33°$ to $\lambda° \; \gamma' = 30° \; 20'$, and the correct outlines of the island can then be restored.

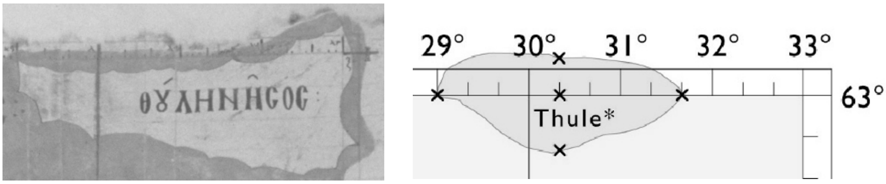

Fig. 7 The island of Thule in manuscript K (fol. 77r.), and the corrected version

These examples may be evidence that the draftsman closely followed the master text and that the maps of manuscripts U and K—all showing the same errors—have the same ancestry.

In addition to the textual evidence, drawing characteristics can also be used to prove a common tradition for the maps: many characteristics of the drawings in the maps of U, K, and F, which cannot be explained by the texts, show significant agreement, for instance the system of coloration, the drawings representing rivers,[24] and the tendency to connect the mountain ranges.[25]

On all the maps of manuscripts U, K, and F, the so-called city vignettes[26] have two forms: "noteworthy cities" are shown as large vignettes with three pinnacle-crowned towers, and are marked with a small cross indicating their location. The other cities have smaller vignettes without towers but with pinnacles at the top, and their location is indicated by a simple dot. The drawings of these vignettes are virtually identical. In addition to the location points and their names, the vignettes in all three manuscripts have symbols indicating their relationship to a people.[27] With only a few exceptions, these symbols are the same in all the manuscripts. As they only appear on the maps, we can assume that they share a common source.

The maps of manuscripts U, K, and F also concur in the course of their coastlines, which cannot be taken from the text: where there are long stretches between points determined by coordinates, the course of the coastline was left to the fantasy of the draftsman. However, on the maps of manuscripts U and K the coastlines are identical, as the example of the Novantes Peninsula in the north of mainland Britain illustrates, even though only three points with coordinates are given in the texts. By connecting these points, a simple triangle would have resulted. Therefore, the almost identical coastline on the maps of manuscripts U and K again strongly indicates a common source (Fig. 8).[28]

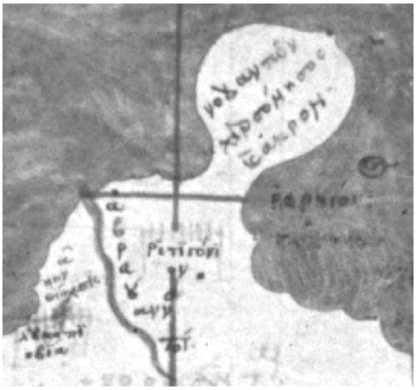
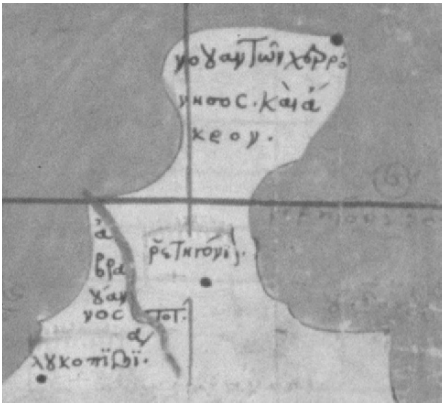

Fig. 8 The identical coastline of the Novantes Peninsula in manuscripts U (fol. 64r.) and K (fol. 77r.)

Maps in Manuscript R, Copied from the Text of This Manuscript

It has been demonstrated that solely the available space was used for the map drawings in manuscript R.[29] The maps in this manuscript, therefore, differ significantly in their conception from those in manuscripts U, K, and F and were not constructed following the instructions of Book 8.[30] This is also true of the drawing characteristics. For example, typical features of the maps in U, K, and F, such as the drawing of coastlines or rivers, cannot be found on the maps of manuscript R.[31] On the other hand, the latter show some unique characteristics, such as many small mountains, not mentioned in the text, that are depicted as river sources. The size of the city vignettes in the maps of manuscript R has been determined by the available space; no differentiation has been made between normal and "noteworthy" cities.

Indications of the source for the maps in manuscript R are provided by many seemingly meaningless entries, such as *to metaxy* (the middle), showing that the draftsman closely followed his textual source. However, this source often seems to have been misinterpreted, as can be concluded from the many entries of regions or peoples that are also mentioned several times in the text. Where the text displays incorrect or missing information, the maps mirror these as well.

Especially significant are cases where the text of manuscript R contains conspicuous errors. For instance, on the second map of Europe, the cities of Juliobriga and Morika are missing because in the master text the scribe entered the coordinates of Morika in the line of Kamarika, which is mentioned earlier in the text; this has resulted in a gap in the text (2.6.51). As Kamarika now occupies the position of Morika on the map and the other cities have been omitted, this map can only have been derived from this text.[32] Therefore, the inscriptions as well as the drawing characteristics of the maps of R clearly point to an R text master as their source. In short, the maps in manuscript R are the result of a remarkable but poorly executed attempt to arrive at a new conception of Ptolemy's maps.

Maps in Manuscript O, Drawn from the Text of This Manuscript

As has been shown above, manuscript O is clearly the source of the redaction with sixty-four regional maps.[33] This opinion is supported by the drawing characteristics of its maps: the basic design concept of the maps in U and K can be recognized in many of manuscript O's regional maps—for instance, in the coastlines and city vignettes. There are also numerous traces of a source in recension Ξ that point to a working process in which the texts of both recensions were used as sources and in which attempts were made to eliminate errors or correct misleading text passages.[34] In addition, there are many places in the text and on the maps which reveal that the author of manuscript O also used scholia and other textual sources, in particular those of Dionysius Periegetes.[35] All these characteristics reveal that, although the author of manuscript O closely followed the text of O in composing the maps, he also had considerable cartographical knowledge. From these observations it seems plausible to infer that Codex O was the prototype of the conceptually new redaction with sixty-four regional maps that emerged in the fourteenth century, and is—in contrast to the maps in manuscript R—of extremely high quality.

Therefore, even if we put the evidence from the drawings aside, there are numerable and unequivocal indications (when the maps in the different manuscripts show common characteristics that cannot be deduced from the preserved texts, as is the case of the maps of manuscripts U, K, and F, but not of manuscripts R and O) that suggest that the maps descend from an independent tradition. From this we can conclude that the maps of U, K, and F are descended from a common ancestor. The maps in manuscripts R and O, however, closely follow their texts and do not lead back to a long tradition of maps; therefore, they are of no relevance to a map tradition dating back to antiquity.

Evidence for a Map Tradition Dating Back to Antiquity

Evidence from the Extant Maps

It was shown in the previous section that most of the common characteristics of the maps in manuscripts U, K, and F point to a common archetype of this group. However, there are also many indications suggesting a map tradition dating back to the time before Planudes. The following details on the maps of manuscripts U, K, and F are of special interest, because they not only show a bias on the part of the archetype's draftsman but also provide additional information, and document actualities from antiquity, that are no longer part of the text and therefore indicate a map tradition dating back to this period.

In the circumstances, it is worth mentioning that the maps in manuscripts U, K, and F have avoided many of the errors in the coordinates of localities that can be found in the accompanying texts, sometimes even providing a better version. For example, the preserved texts in manuscripts U and K locate the mouth of the River Trisanton in Britain at 23° longitude (2.3.4), although this position is impossible, since it is located on the other side of the English Channel in the province of Gallia Belgica. However, by positioning the mouth of this river at a longitude of 20° 20′, the maps of U and K give a more realistic position. The error is clearly palaeographical, that is, the copyist accidentally wrote κγ° instead of κ°γ′, resulting in a reading of 23° rather than 20° 20′.[36]

Especially revealing in this case is the fact that the maps of manuscripts U and K and the text of manuscript X match perfectly. Therefore, the copies of the maps of manuscripts U and K seem to date back to an earlier version than the corrupted copies of the corresponding texts, thereby suggesting that the maps belong to an independent and older tradition.

Differences between the texts and the maps in the localities of peoples point in a similar direction, as the positioning of peoples in the catalogue is not given by coordinates but by indications relative to other peoples or structures. For instance, in the description of Scotland, it is claimed that the "Vacomagi (1) live south of the Caledonians (2)" (2.3.13), which is also represented on the maps of manuscripts U and K (Fig. 9). However, in the texts of these two manuscripts *hyper* (above) has been mistaken for *hypo* (below), which would mean that the area inhabited by the Vacomagi would be inaccurately located on the maps. While the texts again contain an easy-to-explain error in the transcription of their source,[37] the maps of manuscripts U and K—again in accordance with the text in manuscript X—have the correct position.

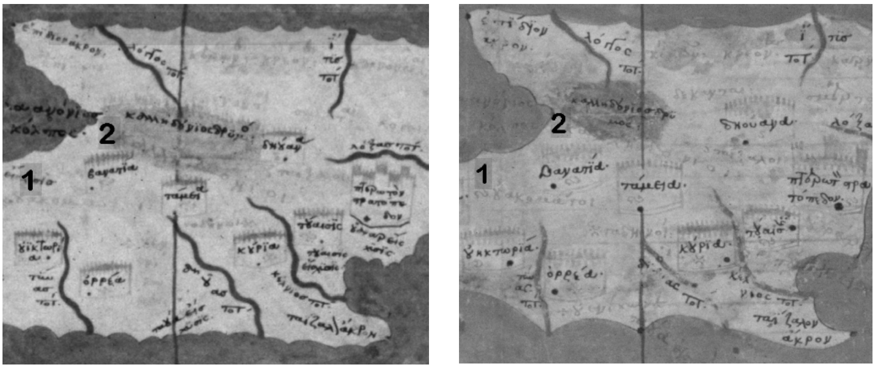

Fig. 9 The positioning of the Vacomagi in manuscripts U (fol. 63v.) and K (fol. 76v.)

In addition to the textual evidence for a map tradition dating back to a point before recensions Ξ and Ω first evolved, there are also indications in the drawings themselves. Perhaps the most important indication can be found by examining the representations of the two mouths of the Anas and Baetis rivers in Spain (Fig. 10), since they appear on the maps of all the manuscripts (including R and O). However, the corresponding texts (2.4.3 and 2.4.5) only mention the eastern mouth, neglecting any reference to the western mouth. The reason for this seems to be a line skip at an early stage of the tradition, as this line is already missing in manuscript X. Since the fact that the Anas and Baetis had two mouths is often documented in the geographical literature of antiquity,[38] it seems reasonable to conclude that this was already the case in the Ptolemaic original (which is supported by the division into east and west). As the maps show the correct information, they must reflect an earlier stage of the tradition. If Markianos of Herakleia,[39] who used sections of Ptolemy's *Geography* in the fifth century, only knew the eastern mouths of the rivers, we probably then have a *terminus ante quem* for the first occurrence of the textual error and, therefore, also for the maps.

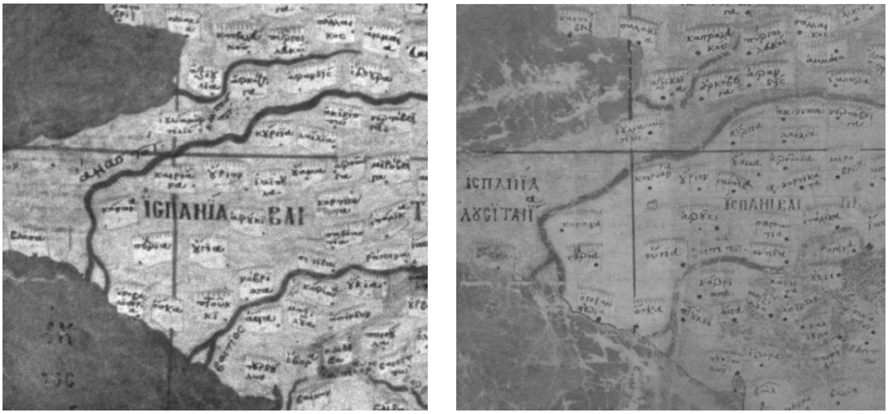

Fig. 10 The two mouths of the Anas and Baetis rivers in manuscripts U (fol. 65v.) and K (fol. 78v.)

Indications in the Manuscripts

Besides the aforementioned graphical elements, there are also several notes and scholia in the manuscripts that point to the maps of their master copies.

The most important note seems to be the so-called Agathodaimon Subscriptio at the end of the texts in the manuscripts of recension Ω:

> Ἐκ τῶν Κλαυδίου Πτολεμαίου γεωγραφικῶν βιβλίων ὀκτὼ τὴν οἰκουμένην πᾶσαν Ἀγαθὸς Δαίμων Ἀλεξανδρεὺς μηχανικὸς ὑπετύπωσα.

> On the basis of the geographical books of Klaudios Ptolemaios, I, the engineer Agathodaimon from Alexandria, have sketched the drawings of the whole *oikoumene*.

From this it follows that this otherwise unknown *mechanikos*[40] Agathodaimon[41] from Alexandria seemed to be responsible for drawing the maps.[42] The note also suggests that a specialist was needed to work on this challenging duty. Because manuscript X does not have this note, the work on this redaction might be related to the development of recension Ω and may even be its origin. A clear dating of the new recension is difficult, but such an undertaking would seem to have been rather unlikely after the destruction of the *Serapeion* of Alexandria in A.D. 391, so that one could plausibly date it to the end of the fourth century.[43] This dating can also be confirmed by the transcription of almost all literature from papyrus rolls to the far better durable and easier-to-handle parchment codices, which began, on a large scale, in the fourth century.[44]

A note at the end of manuscript X (fol. 169v.) states that the master copy of X contained twenty-six or even twenty-seven maps:

> Ἐνταῦθα κς πίνακες (sic) κα<τα>τάσσει, ἐν αὐτῃ δὲ τῇ καταγραφῇ κζ· τὸν γὰρ ι πίνακα τῆς Εὐρώπης εἰς δύο διαιρεῖ, εἰς ἕνα μὲν τάσσων τὴν Μακαιδονίαν, εἰς δὲ ἕτερον Ἤπειρον καὶ Ἀχαίαν καὶ Πελοπόννησον καὶ Κρήτην καὶ Εὔβοιαν.
>
> Here he prescribes twenty-six maps, but in the *katagraphe* itself there are twenty-seven. For he divides the tenth map of Europe into two maps, putting Macedonia in one, Epirus, Achaea, the Peloponnese, Crete and Euboea in the other.

Evidently, the draftsman of the maps divided the tenth map of Europe because of the numerous entries and the resulting lack of space. Therefore, the master copy must have contained twenty-seven regional maps,[45] which also proves that at least one of the manuscripts of recension Ξ must have once contained maps, and—as in recension Ω—twenty-six regional maps. Unlike the members of group Δ, these regional maps all seem to have been assembled at the end of Book 8 (as in R), which might explain why they did not survive.

The following scholia in manuscripts V and R are less well known: at the beginning of Book 8 (fol. 213r.) and at the end of the original text of the *Geography* after Chapter 8.28 (fol. 237r.) in manuscript V, two scholia can be found pointing to the maps of its master copy. The first scholion reads:

> Ἐνταῦθα καταγράφεται ὁ πίναξ ὁ περιέχων ὅλην τὴν οἰκουμένην, μεθ’ ὃν γράφεται ταῦτα ἕως τοῦ ‘ὁ πρῶτος πίναξ τῆς Εὐρώπης’. Εἶτα γράφεται ἐκεῖνο ἑπόμενου τοῦ πίνακος μετὰ τοῦ ’ὁ δεύτερος πίναξ’.
>
> Here the world map is shown, after which this text [i.e. Chapters 8.1–3] is written up to the heading *First Map of Europe*. Then, the following [i.e. Chapter 8.4] is written, followed by a map in relation to the heading *Second Map <of Europe>*.

The second scholion reads:

> Ταῦτα ὀφείλει γραφῆναι μετὰ τὸν τελευταῖον πίνακα.
> The following has to be written after the last map [i.e. the twelfth map of Asia].

Therefore, both scholia indicate not only that the master copy of V included a world map and twenty-six regional maps but also that the maps were placed in the same order as the maps in manuscripts U, K, and F.[46]

In manuscript R, the eastern half of the first map, the entire second map and the western half of the third map of Asia are missing. Because the eastern half of the third map of Asia comes immediately after the western half of the first map of Asia (fol. 129v./130r.), it can be assumed that the maps which should have been positioned between these two were included in one folio, which was later removed or fell out of the volume.

However, we cannot explain the non-existence of the fourth map of Africa in this way, because the surrounding maps have survived. A clue lies in a note positioned below the third map of Africa (fol. 129r.):

> Γύρισον τὸν πρῶτον πίνακα τῆς Ἀσίας καὶ εὑρήσεις τὸν τέταρτον πίνακα τῆς Λιβύης, ἐπεὶ διὰ τὴν σμικρότητα τῶν μοίρων καὶ τὴν στενότητα τοῦ τεύχους ἐτέθη ἐάσαι ὡς ἔχει.
> Turn over the first map of Asia and you will find the fourth map of Africa, since because of the smallness of the grid and the density of entries it has been decided to leave it as it is.

Clearly, in the codex to which this scholion refers, the fourth map of Africa could be found on the reverse side of the first map of Asia. This map was taken from the master copy, without any changes having been made to it, and placed in the new codex, perhaps because the draftsman found the densely inscribed map too onerous. In addition, we can assume that this decision was made after the maps had been completed. In any case, according to the scholion, the fourth map of Africa was placed where the folio containing the second map of Asia, which is now lost, should have been. Maybe this folio was pasted over or damaged and therefore removed for repairing. However, the folio was never reinserted.

As Codex R is not damaged here, the scholion cannot relate to the state of manuscript R itself. Therefore, it must have already been included in the master of R and points to the master's master, which may have been written on a larger format, which would then explain the difficulties the draftsman had copying all the information from the larger maps onto smaller ones.[47] Furthermore, the small format might explain why there is no world map. Essentially, though, it can be concluded that the maps in R can be traced back to at least two generations of transcriptions.

In addition to such direct indications to maps in master copies that are now lost, the preserved maps contain other clues that cannot be dated exactly; usually, they can be found at the end of the main text of the *Geography*.

In recension Ω Chapter 8.29 contains a list of ninety-four provinces. The same list also survives in one version of recension Ξ: it can be found in manuscript X at the end of Chapter 7.6. If we compare the different versions of this list, their common origin becomes clear. By contrast, the maps show only eighty-four provinces, which are color-coded. These are exactly the same provinces given in the catalogue of localities in Books 2–7, with a description of their borders (*periorismos*), which proves that the number of provinces on the maps is the original number. Since neither the number nor the names on the accompanying maps correspond to those in the list of provinces, it follows that the maps cannot stem from this list. Therefore, the map tradition must predate the composition of Chapter 8.29 and go back to a point before the development of recensions Ω and Ξ. Therefore, the list of provinces in this chapter seems to come from a world map, perhaps because it was deemed necessary to have a kind of index of the world maps. This may have occurred when the two recensions first evolved, when the lists got their current form and position inside the *Geography*.

Additional indications from the manuscripts regarding the tradition of the maps can be derived indirectly from the chapter headings in the catalogue of localities and from the tables of contents of the individual books, which have a larger number of countries and regions. None of the maps, however, have been touched by changes made to the texts. Clearly, during the tradition of the texts several subheadings seem to have been misread as chapter headings, which would explain the rise in the number of names of regions.[48] These chapter headings, as well as the tables of contents, do not belong to the original version of the *Geography*, but are rather a result of later technical amplifications to the book.[49]

External Testimonia for the Maps in the Texts of Other Authors

These findings can be supported by several external *testimonia* from late antiquity and Byzantine times that relate to lost codices of Ptolemy's *Geography* containing maps.

A testimony relating to Ptolemaic maps can be found in Cassiodorus' *Institutiones*[50] (c. A.D. 560), in which the author recommends that his monks read the *Geography*

> *Tum, si vos notitiae nobilis cura inflammaverit, habetis Ptolemaei codicem, qui sic omnia loca evidenter expressit, ut eum cunctarum regionum paene incolam fuisse iudicetis. Eoque fit, ut uno loco positi, sicut monachos decet, animo percurratis, quod aliquorum peregrinatio plurimo labore collegit.* (*Institutiones* 1, 25).

> If you are taken by the noble interest in the knowledge (of geography), you have the codex of Ptolemy at your hand, which represents all locations so illustratively that one can get the impression he would have been at home everywhere. Therefore, you can visit in your mind all that others have collected information about in burdensome travels – staying at home, which is the duty of monks.

If we admit that the text of the catalogue of localities consists, in large part, of topographical names and their coordinates only (which do not lend themselves to being illustrated), it seems hard to believe that Cassiodorus would have used the expression *omnia loca evidenter expressit*. However, his choice of words are particularly apt for describing the maps. Therefore, this testimony can only point to a sample of Ptolemy's *Geography* illustrated by maps.

There are also other clues, dating from late antiquity, from which we can deduce a usage of Ptolemaic maps: evidence in the writings of Markianos of Herakleia,[51] of Ammianus Marcellinus (fourth century)[52] and of Jacob of Edessa (seventh century),[53] as well as in the anonymous books *Diagnosis* and *Hypotyposis*, which seem to have originated in the fifth or sixth centuries.[54] The evidence that can be deduced from these works is related to passages of text that cannot be understood without using the maps. Therefore, at the end of the fourth, during the fifth and at the beginning of the sixth centuries, the existence of late antique (that is, written in majuscule) codices of the *Geography* can be confirmed by several sources. It would have been during this period that the carrying out of learned work would have moved from Alexandria to Constantinople. It is no coincidence that the well-known palimpsest of Strabo, written in slightly slanted majuscule, dates from exactly the same period, that is, the end of the fifth and the beginning of the sixth centuries. What remains of the palimpsest are contained in the two codices rescripti *Vaticani Graeci* 2306 and 2061A. That Ptolemy's *Geography* was copied during this period due to a reawakening of interest in geographical works is entirely plausible.[55] In any case, the "very old manuscript" that Planudes rediscovered and which formed the archetype of the so-called group Δ (manuscripts U, K, and F and their copies) must have been written during this period.

Another unmistakable *testimonium* of a map-containing manuscript is given by the Byzantine scholar Maximus Planudes (c. 1255–1305), who lived—as we know from an *ex libris* in Codex V[56]—in the Chora monastery (Istanbul). In a poem, written in hexameter, Planudes enthuses over the recovery of an extremely old and splendid manuscript of the *Geography* that "had been hidden for countless years".[57] This manuscript obviously contained maps the colors of which Planudes compares to a woven dress of Athena or a meadow of flowers.

If Planudes, who had undoubtedly seen many extremely precious manuscripts during his work as a philologist, praises the rediscovered manuscript so enthusiastically, we can assume that it must have been an exceptional codex. It seems reasonable to suppose that it had survived from late antiquity and that it had been written in majuscule.

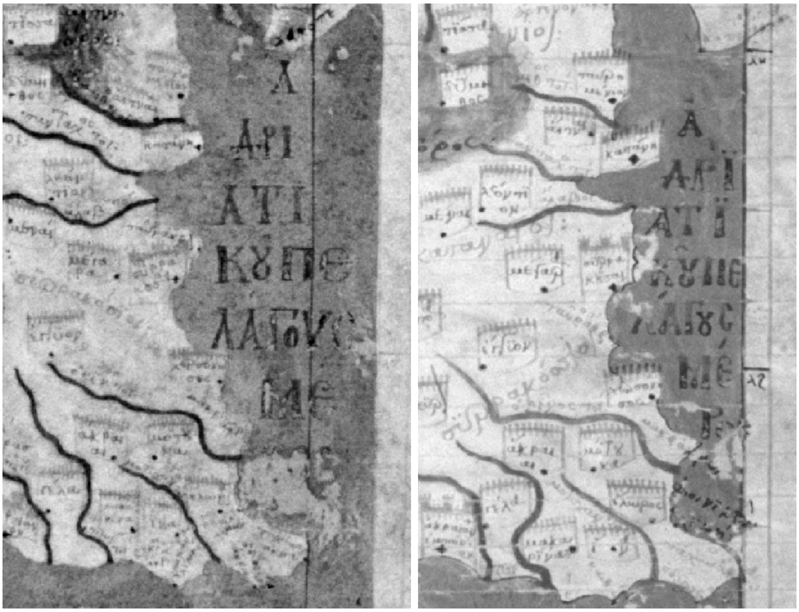

Fig. 11 Calligraphical entries in majuscule in manuscripts U (fol. 74r.) and K (fol. 85r.)

This might explain the many majuscule errors in manuscripts U and K, and the almost identical calligraphical entries in majuscule on the maps of U and K, which were quite rare in the time of Planudes (Fig. 11).

One can only conjecture the origin of this manuscript: it is highly likely that it came from the area of activity of Athanasios II, Patriarch of Alexandria. In this region we can also find traces of a Ptolemaic map tradition in Islamic culture. The Islamic polymath al-Mas'ūdī, for example, mentions a colored Ptolemaic atlas in his work *Muruj al-dhahab* (*Meadows of Gold*),[58] written in about A.D. 950 in Egypt. In addition to several citations, there are also Islamic maps that point to the use of Ptolemaic maps.

In an eleventh-century manuscript[59] of the *Kitab surat al-ard* (*Book of the Image of the Earth*), written by al-Khwārizmī (ninth century),[60] there are four regional maps, among them one of the Nile (Fig. 12). The drawn representation of the river's course as well as the position of the *klimata* strikingly resemble the Ptolemaic maps; therefore, this Islamic map must go back to a Ptolemaic master.

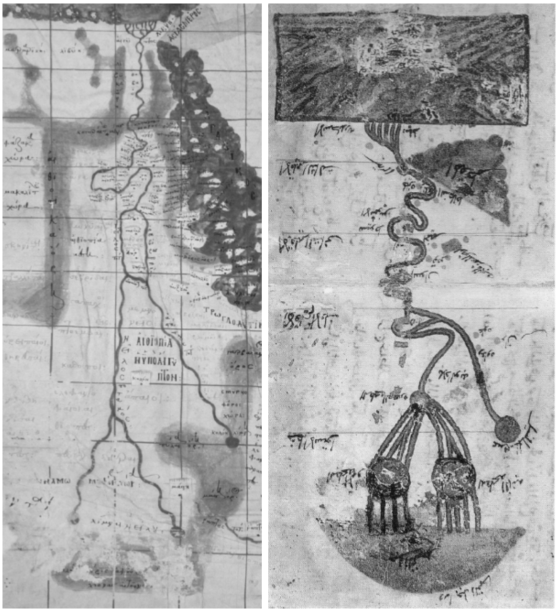

Fig. 12 Left, the River Nile by Ptolemy (MS K, fol. 99r.) and, right, by al-Hwarizmi (MS Strasbourg 4247)

Reconstruction of the Different Stages of the Map Tradition

To sum up our conclusions: at first glance, most of the clues mentioned in the manuscripts only prove that the drawn map is based on the text of the accompanying manuscript. This can be seen when textual errors occur on the maps as well as in the texts, as happened in R and O, and on occasion in U, K, and F, for example in the case of Thule.

However, where the different maps show common characteristics that cannot be deduced from the texts, as frequently occurs on the maps of manuscripts U, K, and F, it is clear that they must descend from a common archetype. As examples of this, the following characteristics can be cited from the maps of U, K, and F: the color-coding for different regions, the identical symbols for cities and peoples, the identical course of the coastline of the Novantes Peninsula, and the similar representations of rivers and mountains.

In addition, there are many indications on the maps of manuscripts U, K, and F that not only show a bias of the archetype's draftsman but also provide additional information and document actualities from antiquity that are no longer part of the texts and therefore point to a copying process based on a set of maps that must predate the master copy that was used for the text. Of special interest is evidence common to recensions Ω and Ξ, for example, in the instances of the mouth of the River Trisanton or the locating of the peoples in Scotland, where the maps of U and K, together with the text of X, provide a more accurate version of names and coordinates than the texts of U and K. Presumably, they can be dated to a period before the main lines of tradition of Ω and Ξ were separated in late antiquity. The same can be deduced from the information concerning the two mouths of the Anas and Baetis rivers, where the maps have not be affected by errors introduced into the texts and, therefore, show the state of the rivers (the fact that they then had two mouths) as it was in antiquity.

Furthermore, other evidence suggests that the maps surely predate Planudes, for instance the mentioned scholion in R, which points to procedures in the copying process of the ancestor of R. In general, the very fact that the maps have survived or that their existence is confirmed by scholia in all the primary manuscripts point to a map tradition dating back to before Planudes. Therefore, one can assume from the findings in the preserved manuscripts that the masters for the maps date back to late antiquity, which can be substantiated further by the zodiacal signs and the wind heads on the world maps and from the majuscule inscriptions on the maps, which are preserved in manuscripts U and K. Planudes' description of the extremely old manuscript "that had been hidden for countless years" fits this conclusion nicely.

The external evidence provided by Agathodaimon, Cassiodorus, al-Mas'ūdī and others strongly supports a map tradition that can be traced from antiquity up to the time of Planudes. The belief held by some that Ptolemy's maps are new constructions from Byzantine times, and even the proposition that Ptolemy's *Geography* did not contain any maps at all, clearly contradict the facts that have been presented here. All the evidence suggests that—similar to the star constellations in the tradition of the *Almagest*—a continuing map tradition, dating back to Ptolemy himself, must have existed.[61]

Notes

1. Schnabel (1938, pp. 5–37).
2. Fischer 1932a (investigation of the maps) and 1932b (facsimile of Codex U).
3. Deissmann (1933, pp. 89–93), see also Diller (1940, pp. 62–67).
4. Furlan (1981, pp. 30–48), Mioni (1983, pp. 57–67).
5. Burri (2003, pp. 127–136).
6. Müller (1880, pp. 300–305).
7. Bandini (1764–1770, vol. 2, p. 71).
8. Some of the manuscripts from this redaction contain four maps giving an overview of the continents. However, a scholion published by Fischer (1932a, p. 105, note 1) reveals that these maps are new constructions taken from the Codex *Mediolanensis Ambrosianus 997* (middle of the fourteenth century). See also Schnabel (1938, pp. 17–18).
9. See section "Redaction with Sixty-Four Regional Maps" below.

10. See section "Indications in the Manuscripts" below.
11. The books have been ordered in such a way as to facilitate the drawing of the maps (2.1.4), i.e., the descriptions of the regions usually run from the top left-hand corner (north west) to the bottom right-hand corner (south east). Thus, Book 2 covers northern and western Europe, Book 3 southern and eastern Europe, Book 4 Africa, Book 5 western Asia, Book 6 Arabia and Central Asia, and Book 7 India and southeast Asia.
12. Some of the later Latin manuscripts, e.g., Codex *Parisinus Latinus 4801* (fol. 74r.), contain a representation of an armillary sphere.
13. Related to this list is the *Kanon of Noteworthy Cities*, which is transmitted as a part of Ptolemy's *Handy Tables (procheiroi kanones)*. A complete edition of this list can be found in Stückelberger & Mittenhuber 2009, 136-217; see also Mittenhuber & Koch 2009.
14. The maps in manuscripts U, K, and F have the same errors in noting down degrees as the text in Chapter 1.23 of recension Ω, e.g. at the seventh parallel, 27° 30′ instead of the correct 27° 10′; or at the sixteenth parallel, 51° instead of the correct 51° 30′; the maps in O correspond to the text (seventh parallel 27° 40′ instead of the correct 27° 10′) and in this respect joins recension Ξ. See also Schnabel (1938, pp. 47–49), Diller (1941, pp. 4–7).
15. These data appear on the right-hand side of the world map in O, while the left side contains the latitude data for the corresponding parallel in degrees.
16. The length of the tenth parallel on 36° of latitude can be calculated according to the following formula: $90{,}000 \times \cos 36° = 72{,}812$ stades.
17. The word *klima* is mentioned in the *Geography* 1.15.5f. in relation to Marinos, and in 5.9.16 as well as in 7.5.15 as a general term for the most northerly regions of the Earth. The grouping of the German cities into four zones separated by parallel circles (2.11.27ff.) is also not related to the seven *klimata* (which would be *klimata* 7–10). The word is used in a similar way in the *Almagest* (*Syntaxis* 1.5): where the parallel circles are defined numerically, the word *klima* is not used (but the tables in *Syntaxis* 2.13 follow the order of the first seven *klimata*. On the question of the *klimata* in general, see Honigmann (1929, pp. 4–24 and 55–60); on its usage in the *Almagest*, see Toomer (1984, p. 19).
18. Cod. *Leidensis Lat. Q 79*, ninth century; cf. Bischoff and Eastwood (1987).
19. In a similar way, the same is true of the scholia on the armillary sphere following Chapter 8.29 (in manuscript X following Chapter 7.7) as well as of the table of the Sun's path, which only survives in recension Ω (8.29.31a).
20. The special *hypographai* in Chapters 8.3–28 lack important data for constructing the maps: for instance, every map has information on the ratio of the center parallel to the meridian, but not on the borders of the maps, the position of the center parallel, or the map's format. Therefore, the maps in manuscripts U, K, and F occasionally differ from each other.
21. One argument in favour of this assumption is that the frames of the empty double pages that follow the twelfth map of Asia have the same dimensions as those of the other maps.
22. This would also be the case in a drawing following the specific *hypographai* in Chapters 8.3–28, which in Codex O were also designed for 26 regional maps.
23. The degrees of longitude in U, K, and F are divided into twelve parts by red and black lines according to the following system: full degrees are in long, black lines; half degrees are in long, red lines; one-third degrees are in black, half long; one-quarter degrees are in short, red lines; sixth and twelfth part degrees are in short, black lines.
24. A fitting example is the River Danube in Pannonia, where the course of the river in the maps of U, K, and F is drawn skilfully around the vignettes of the Pannonian cities in very similar ways. Comparable cases can be found in the drawings of Spain's rivers, which show remarkable meanders in the maps of U and K, even though no meanders are mentioned in the texts.
25. In the maps of U and K mountain ranges have been connected and are often used as areas of the sources of rivers. This is especially striking in the mountainous source of the Dorias and Tagos rivers on the second map of Europe, which is not mentioned in the text. This un-

named mountain region, which does not belong to the original text of the *Geography*, is shown in the maps of U, K, and F in exactly the same way.

26. Fischer (1918, pp. 49–52; 1932a, pp. 143–146) claims that the cities on the original maps were at one stage differentiated into even more classes, containing "second-" and "third-class cities". I am not of this opinion.
27. The maps with symbols for different peoples are: Europe 1 (Britain), Europe 2 (Spain), Europe 3 (Gallia), Europe 6 (Italy), Europe 10 (Greece), Asia 1 (Asia Minor), Asia 4 (Syria), and Asia 10 and 11 (India).
28. A clear indication of this are the drawn characteristics of the hypothetical land bridge connecting Africa and southeast Asia on the southern border of the *oikoumene*, as is shown in the world maps of manuscripts U and K.
29. See section "Redaction with Twenty-Six Regional Maps" above.
30. The maps contain no meridians or parallels; the remarks in the right margin for the parallels are often inaccurate or incorrect.
31. In manuscript R, some structures have not been carefully separated and others have been omitted altogether; this has resulted in either an inexact or incongruent course of the provincial borders or a non-conforming coloration of the regions.
32. A similar example can be found on the fourth map of Europe, where, because of an error in the catalogue of localities in the text of manuscript R (2.11.1), the name of the Cimbrian Peninsula has been inserted between the mouths of the Visurgios and Albis rivers; as a consequence, the people living on the peninsula have been wrongly located on the maps of manuscript R.
33. See section "Redaction with Sixty-Four Regional Maps" above.
34. This can clearly be seen by examining the representation of the Kemmena Mountains in Gallia, where the editor of O has amended a long-standing mistake in the coordinates and correctly repositioned the concerned cities and peoples.
35. This is proven by the inclusion of ὃς καὶ Πεπηγὼς καλεῖται ἢ Κρόνιος ἢ Νεκρός ([the Hyperborean Sea], which is also called the Frozen Sea, Sea of Kornos or the Dead Sea) on the map of Hibernia, which can only be found in the text of O and follows Dionysius v. 32. Other examples are the city of Tartessos in the Baetica (Dionysius v. 337) or the two Liburnian peoples of the *Hylleioi* and *Boulimeis* (Dionysius vv. 386f.), which are also only mentioned on the maps of manuscript O.
36. As in the case of Thule (see section "The Common Ancestry of Manuscripts U, K, and F" above), a simple transcription error in the separation of degrees and minutes occurred. Another typical error is when a single number sign has been mistakenly omitted, e.g., μ° γ' (40° 20′) instead of μγ° γ' (43° 20′).
37. This error can be even more easily understood when one takes into account that in manuscripts words often ended in ligatures.
38. Strabo 3.1.9 (Anas and Baetis); Avienus vv. 208 (Anas) resp. 288f. (Baetis), Mela 3.5 (Baetis).
39. See Müller (1882a, pp. 515–562).
40. A *mechanikos* could be a technician like Heron of Alexandria (c. first/second century A.D.), who was renowned for his construction of machines and their descriptions, illustrated by drawings (on the illustrations of the Heron manuscripts, see Stückelberger 1994, pp. 99–109).
41. Agathodaimon's name appears in antique literature several times; an overview is given in Ganschinietz (1918).
42. The view that Agathodaimon was the draftsman only of the world map (see Fischer 1932a, pp. 118–119) is unconvincing: Agathodaimon explicitly mentions "all of the eight books of the *Geography*".
43. The oldest known subscriptions date from the end of the fourth century. See the *subscriptio* by an unquestionably antique specialist of gynaecological drawings (*gynaikeios hypozogra-*

phos) in a Soranus manuscript (Cod. *Paris. Graec. 2153*, fol. 218v.), mentioned by Stückelberger (1994, pp. 92–93).

44. On this point, see also Hunger (1961, pp. 47–49). It cannot be merely coincidental that, in his study guide, Cassiodorus explicitly mentions a *Codex Ptolemaei* (for more on this, see below).
45. Cf. Müller (1880, p. 301), Schnabel (1938, p. 10).
46. Presumably, the codex had a folio format, which would explain why in the smaller Codex V the maps have been omitted; see also the notes in the scholion to Codex R.
47. The fact that, in regions with a high density of entries (e.g., Italy, Greece or Egypt), the cities are only marked by a dot on the map itself, and their names are noted in the margins, confirms this assumption.
48. For example, Armenia Minor appears in the texts of many of the manuscripts as a separate chapter, even though there is no description of its borders; on this topic, see especially Diller (1939, pp. 228–238).
49. The same is true of the instructions to the draftsman that can be found at the end of the tables of contents of Books 2, 4, and 5.
50. Mynors (1937).
51. See end of section "Evidence from the Extant Maps" above.
52. Ptolemy is mentioned explicitly in Ammianus' *Res Gestae* (22.8.10); see Fischer (1932a, pp. 483–487), Polaschek (1965, pp. 764–772), den Boeft et al. (1998, p. 130).
53. Hjelt (1892). On the topic, see Fischer (1932a, pp. 452–462), Schmidt (1999, pp. 57–66).
54. Müller (1882b, pp. 488–493; 1882c, pp. 494–509); new editions by Mittenhuber 2009c and 2009d
55. The Strabo tradition converges with the Ptolemy tradition several times: the Strabo Codex *Parisinus Graecus 1393* (end of the thirteenth century) was written by the same hand as the Ptolemy Codex *Seragliensis GI 57* (see Diller 1975, pp. 70-71. and 89-97.); the Ptolemy codex Athous *Vatopedi 655* (fourteenth century) also contains the text by Strabo (see Diller 1937, pp. 174–184).
56. *Claudii Ptolemei liber Geographie et est proprius domini maximi philosophi greci ac monachi in monacerio Chore in Constantinupli. Emptus a quodam Andronico Yneote.* It is not certain whether Planudes really did own Codex V (see Burri 2003, pp. 131–136); but in any case it is not this codex to which the poem refers.
57. Poem by Planudes vv. 28f.; Stückelberger (1996, pp. 197–205).
58. de Meynard and de Courteille (1861–1877).
59. The manuscript is dated 1037 and today belongs to the University and Regional Library of Strasbourg (Codex *4247*).
60. See also von Mzik (1926), Wieber (1974).
61. The topic of the present paper is treated in more detail, in German, in Mittenhuber (2009a); see also Stückelberger and Mittenhuber (2009), esp. pp. 34-108 and 322-357.

Islamic Reactions to Ptolemy's Imprecisions

F. Jamil Ragep

Consider the following quotation from the author of the treatise *Fī sanat al-shams* ("On the Solar Year"), most likely written in Baghdad in the first part of the ninth century:

> Ptolemy, in persuading himself that the period of the solar year should be taken according to points on the ecliptic, also persuaded himself as to the observations themselves and did not in reality perform them; coming from his imagination, this was of the greatest harm for what was described for the calculations (Morelon 1987, p. 61; my translation).

Or the following from Ibn al-Haytham in the eleventh century:

> When we investigated the books of the man famous for his attainment, the polymath in things mathematical, he who is [constantly] referred to in the true sciences, i.e. Ptolemy the Qlūdhī, we found in them much knowledge, and many things of great benefit and utility. However when we contested them and judged them critically (but seeking to treat him and his truths justly), we found that there were dubious places, rather distasteful words, and contradictory meanings; but these were small in comparison with the correct meanings he was on target with (Ibn al-Haytham 1971, p. 4).

As the quotation from Ibn al-Haytham indicates, there was a real ambivalence towards Ptolemy among Islamic scientists. Widely respected, he was held by many of them to be a paragon of the mathematician whose truths transcended cultural and religious difference. And yet it was also clear that there were many flaws in his various works, many of which were puzzling and led to a variety of doubts (*shukūk* [ἀπορίαι]). There has been a great deal written in recent years about the doubts regarding his models. (For a summary, see Sabra 1998). In this paper, I would like to turn to another aspect of the Islamic doubts toward Ptolemy and other Greek astronomers, namely observations.

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For quite some time, I have had the impression that there is a significant difference between the types of observations one finds in antiquity and those one finds in the Islamic world, beginning sometime in the early ninth century during the ʿAbbāsid period. In what follows, I shall first try to give a sense of the differences by providing some examples. I will then try to characterize these differences. And lastly I will provide some reasons, admittedly speculative, that might account for these differences.

Before continuing, let me explain a few terms that I will be using. By *exact methods*, I mean those mathematical and observational procedures that could potentially lead to accurate results. By *accurate results*, I mean those that are in accord with modern values. Now exact methods may or may not lead to accurate results, depending on the underlying mathematical and observational tools that are used. Results may be *precise*, i.e. to several digits, without being accurate, since many of these digits could be spurious, i.e. the result of carrying out calculations to a greater precision than supported by the original data or measurements. In order to determine accuracy, one needs to engage in *testing*, i.e. checking received values by some means to determine their accord with newer observations or theories. I distinguish between *confirmation* of earlier parameters or results that leads to the acceptance of a received value, and the testing of parameters or results that may or may not lead to the revision of those values. (I'll have more to say about this later.)

Let us take as our first example the measurement of the size of the Earth.

The Measurement of the Earth

There is a heroic story that is well-known in the secondary literature about the early measurements of the Earth. Eratosthenes (3rd c. BCE), head of the library of Alexandria, is said by Cleomedes (1st c. BCE) to have measured the size of the Earth using a simple but effective means (see Fig. 1). This consisted of taking a known distance along a meridian in linear distance, finding its equivalent angular distance, and then setting up a proportion that would yield the meridional circumference. Eratosthenes is said to have taken the linear distance between Alexandria and Syene (modern day Aswan) to be 5,000 stades, and he found the angular distance to be 1/50 of a complete circle. In addition, Eratosthenes evidently made the following assumptions:

(a) Syene is on the tropic of Cancer, so there would be no shadow cast by the Sun at noon on the day of the summer solstice.
(b) The Sun is at an infinite distance, so all its rays are parallel.
(c) Alexandria and Syene are on the same meridian.

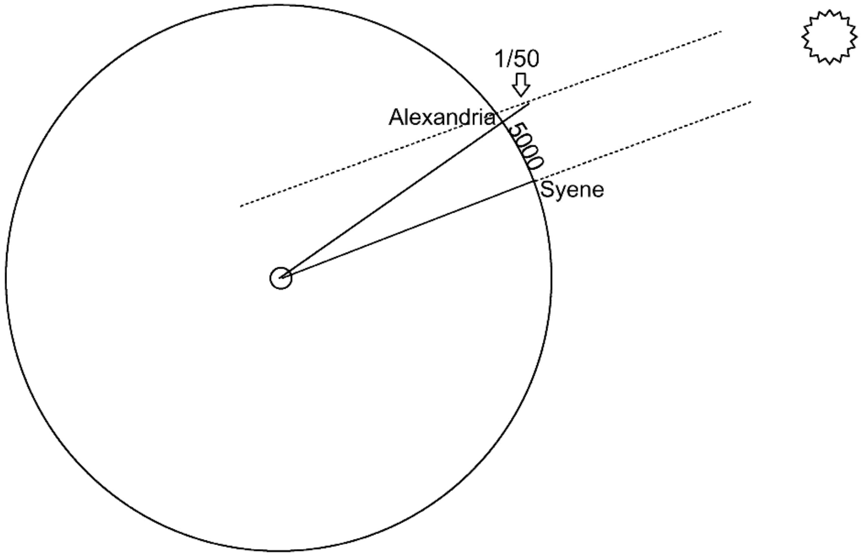

Fig. 1 Eratosthenes' measurement of the Earth's circumference

Now all three assumptions are false; the effect of (b) is negligible, but (a) and (c) could cause some distortion. But of more effect on the accuracy of the final result are the "observations" of 5,000 stades and 1/50 of a circle. Now the roundness of these numbers, as well as the final result of 250,000 stades, immediately puts one (or should put one) on guard. These numbers are just too nice. But let's give Eratosthenes the benefit of the doubt. The 5,000 stades could be rounded from some value close to 5,000 (and given the uncertainties involved this might be reasonable), and the 1/50 is said to have been from an observation of a shadow cast in a bowl at the summer solstice. But several modern authors have cast doubt on whether these numbers were the result of actual observations. R.R. Newton, for example, proposed that the 1/50 was calculated based on latitude differences, or more likely on equinoctial noontime shadow differences, between Alexandria and Syene (Newton 1980, p. 384). And others have pointed out that a survey of linear distance between Alexandria and Syene would have been difficult to attain in antiquity to any degree of accuracy and that Eratosthenes was probably relying on travelers' reports (Dutka 1993, p. 62).

Other reports we have of Greek values for the Earth's circumference confirm the sense that we are dealing with "guesstimates" of various sorts (see Table 1). Besides the obviously rounded numbers, the post-Aristotle values are divisible by the standard Babylonian base 60. The one exception that proves the rule is the value that comes out of Eratosthenes' reported observations, namely 250,000, which was changed to 252,000 (perhaps by Eratosthenes himself?) in order to be divisible by 60.

Table 1 Greek values for the circumference of the Earth (cf. Dutka 1993)

Authority	Circumference (stades)
Aristotle	400,000
Anon. (mentioned by Archimedes and Cleomedes)	300,000
Eratosthenes	250,000
Eratosthenes	252,000
Posidonius	240,000
Posidonius	180,000
Ptolemy	180,000

A number of historians have attempted to save these numbers by coming up with truly ingenious arguments to show how accurate they are, based upon one or another of the many modern equivalents for an ancient stade. But as D. Engels has show in the case of Eratosthenes, such tortuous reconstructions have little to do with the historical record and much to do with the wishful thinking of modern historians. In fact, Eratosthenes's stade is most likely the Attic stade, which has an approximate length of 185 m (1/8 of a Roman mile), resulting in a circumference of 46,250 km, about 15% too great (Engels 1985).

Despite the error in Eratosthenes' result, I am reluctant to say that this is simply a case of a calculated value based upon latitudinal intervals expressed either in stades or shadow ratios. It seems to me possible, and given the amount of ancient testimony likely, that Eratosthenes and others "confirmed" the calculated values using observations of various sorts. Now one might ask how one can confirm an error that is within the limits of observation (cf. Rawlins 1982), but here the distinction between a confirmation and a test is important to keep in mind. Science students confirm results all the time, and it is the naïve teacher indeed who thinks that all the confirmations are the result of rigorous testing. Testing assumes that the observer wants to modify the received values, but I don't think this is what was going on with the values listed in Table 1; rather, modifications are much more likely based upon changing equivalences of a stade.

The conclusion that these values were unreliable is, interestingly enough, the judgment reached during the early ʿAbbāsid period. We have very good evidence that indicates that the Caliph al-Maʾmūn (r. 813–833) was not happy with Ptolemy's 180,000-stade figure and wished to have it tested. (The following is a summary of a more extensive treatment in Ragep 1993, v. 2, pp. 501–510, which includes references; cf. King 2000 and Mercier 1992, both of whom evince a certain degree of skepticism regarding the Maʾmūnī measurement of the Earth. Though certain details are in doubt, in my opinion the amount of contemporaneous evidence makes a strong case for some sort of scientific observations ordered by Maʾmūn. Furthermore, there is no reason to distrust the evidence regarding Muḥammad ibn Mūsā, which is based upon his own words.) A text attributed to Muḥammad ibn Mūsā, one of the famous Banū Mūsā who was a protégé of Maʾmūn, as well as later sources, indicates that Muḥammad undertook a "confirmation" by simply taking the latitude difference of two Syrian cities, Raqqa and Palmyra (assumed on the same meridian) with Ptolemaic latitudes of 35°20′ and 34°, respectively. (The modern values are 35°58′ and 34°35′; in actuality, Raqqa is about 45′ east of Palmyra.) Since the Ptolemaic distance was given as 90 Roman miles, this did more or less confirm the Ptolemaic value of 66⅔ miles/meridian degree or 180,000 stades for the Earth's circumference. (Note this is based upon a Roman mile of 7.5 Ptolemaic stades rather than the 8 Attic stades presumably used by Eratosthenes; see above.) What is interesting about this story is that Maʾmūn seems not to have been happy with this "confirmation," perhaps because he was, correctly, not convinced that his astronomers knew the exact length of a Roman mile. Maʾmūn's reaction, judging from a number of reports, was then to order a scientific expedition to find a meridian degree by means of a survey. A group was sent to the Plain of Sinjār in upper Mesopotamia. (The Sinjār area is located in the northwestern part of Iraq and constitutes approximately 2,250 km^2 of a flat plain. Sinjār Mountain (1,460 m height) is the major geomorphological feature in the area.) The method we find described in Ibn Yūnus (d. 1009 CE) is instructive. Two groups, one going due north, the other due south, laid out survey lines using long ropes until the Sun's altitude descended or ascended one degree. The two groups then came back to the starting point and compared notes and arrived at an average figure of 56 Arabian miles. (There are other reports giving slightly different numbers.) Since we know that each of these miles was 4,000 cubits, and we also know that the cubit used at the time of Maʾmūn was approximately 49 cm, Carlo Nallino in the early 1900s concluded that the Maʾmūnī value for the circumference of the Earth was within a few hundred kilometers (off by less than 1%). It is instructive to compare this with a recent attempt by the MIT physicist Phillip Morrison and his wife Phyllis Morrison to measure a meridian line along 370 miles of US 183, running between Nebraska and Kansas. Taking two observations of Antares at the beginning and end of the trip and using the car's odometer to measure distance, they came up with a circumference of 26,500 statute miles, off by about 6% (actual value 24,900) (as reported by Dutka 1993, p. 64).

Here we can usefully distinguish, I believe, between the conventionalist attempt by Muḥammad ibn Mūsā to *confirm* the Ptolemaic value with Maʾmūn's demand to *test* that value. We can also say that Muḥammad was using an approach not all that different from what seems to have occurred rather frequently in antiquity—taking a received value and then using some observation or other means to confirm that it was approximately correct without seeking in any way to modify it. What seems new here is that a patron, in this case representing the state, is intervening to demand observational accuracy. While state patronage of science was certainly not unprecedented (one thinks of the Ptolemies and several Sasanian rulers not to mention Babylonian and Assyrian kings), this type of personal intervention by Maʾmūn as reported in contemporary accounts does seem to mark a new departure (Langermann 1985). We will return to this below.

The Length of the Year and the Sun's Motion

The Ptolemaic length for the tropical year, as well as others reported from antiquity, were clearly at variance with what was observed in the ninth century; the problem was how to interpret these conflicting values. Ptolemy's (and most likely Hipparchus's) length for a tropical year ($365^d5^h55^m12^s$) is about 6 min per year too long, so over the 300 years between Ptolemy and Hipparchus there would have been almost a 30-h disparity between, say, a predicted vernal equinox by Hipparchus for Ptolemy's time and an actual observation made by Ptolemy himself. And indeed Ptolemy's reports of the times of equinoxes and summer solstices are about a day later than they should have been, which is one of the bases for saying that he faked his observations in order to keep Hipparchus's value. By the time we reach the ninth century, this discrepancy would have reached well over 4 days! Of course, Maʾmūn's astronomers and Muḥammad ibn Jābir al-Battānī (d. 929 CE) had a longer baseline to work from than did Ptolemy, so it would be surprising, not to say shocking, if they hadn't modified Ptolemy's length for the tropical year. But let us look at this another way. Ptolemy decided not to tamper with the year he had inherited from Hipparchus, despite the fact that there would have been a discrepancy of more than a day. The Islamic astronomers of the ninth century had, in some ways, a more difficult problem to confront. How were they to understand the values they had inherited from the Ancients? Were they simply better observers than their predecessors or were there actual changes that had occurred in the intervening years in the motion of the Sun and, perhaps, in that of the stars as well that might account for the observed variations?

Thābit ibn Qurra (d. 901 CE) wrote his friend and collaborator Isḥāq ibn Ḥunayn asking him if he knew of a solar observation between the time of Ptolemy and Maʾmūn. (See Ragep 1996 for details on this (esp. pp. 282–283) and on what follows in this section.) There are several things at work here. Presumably, he wanted to check how well Ptolemy's tables would predict this intermediate position of the Sun, which might indicate whether changes in the Sun's motion and/or parameters had occurred in the years since Ptolemy. But I suspect he also wanted to ascertain whether this new observation might give a clue regarding the variation in year-lengths, which might then be coordinated with the varying precessional rates reported by Ptolemy and Maʾmūn's astronomers (1°/100 years for the former, 1°/66 years for the latter). Briefly, the reported differences in year-lengths could be the result of a speeding up of the rate of precession, here interpreted to mean a variable speed of the eighth orb containing the fixed stars that would be transmitted to the solar orbs, causing the Sun to reach the vernal equinox sooner than it would otherwise and thus resulting in a variation in the tropical year (see Fig. 2). Given this possibility, Battānī in his *Zīj* (astronomical handbook) entertains the idea that variable precession (whether or not connected with an oscillatory

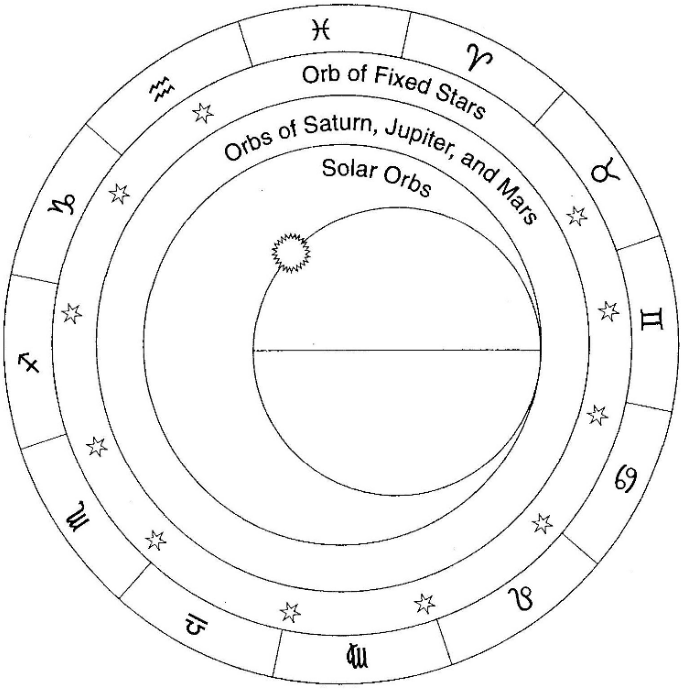

Fig. 2 A continuous speeding up (by trepidation or some other means) of the motion of the Eighth/Fixed Star Orb is here transmitted to the Sun's orbs, causing the Sun to reach the fixed vernal equinox sooner than it would with a simple monotonic precession. Battānī claims this might explain the differences in year-lengths reported by the ancients and early Islamic astronomers.

trepidation motion) could explain the observations. Here we may turn to Tables 2 and 3 for an indication of what Battānī had in mind. Table 2 lists the tropical year lengths (and corresponding solar speeds) from the ancients and his own observations. (Note the odd value for Hipparchus, which is at variance with the normal

Table 2 Year-lengths and solar motion as reported by Battānī

	Years since Nabonassar (Julian year)	Length of tropical year in days	Motion of Sun per Egyptian year
Babylonians	0 (–746)	365 1/4 + 1/120 (=365;15,30)	359°44′43″
Hipparchus	600 (–146)	365 1/4 (=365;15)	359°45′13″
Ptolemy	885 (+139)	365 1/4 – 1/300 (=365;14,48)	359°45′25″
Battānī	1,628 (+882)	365 1/4 – (3 2/5)/360 (=365;14,26)	359°45′46″

reading from the *Almagest*; Battānī, who elsewhere indicates that Ptolemy used the same year length as Hipparchus, may here be fudging the figures to indicate a steadily decreasing year-length.) Table 3 represents my reconstruction of the effect of variable precession, following Battānī's suggestion and using his year-length and reported precessional difference between him and Ptolemy to calculate the earlier values. Note the close relationship between the predicted year-lengths in Table 3 and the reported ones in Table 2.

Despite noting this correlation between an increasing rate of precession and an increased speed of the Sun (and thus a decreasing length of the tropical year), Battānī indicates his dilemma and that of the first generations of Islamic astronomers: how could he know whether Ptolemy's values were correct or whether Ptolemy was simply a bad observer and/or whether he was using an instrument that had been miscalibrated or had warped over time. So Battānī must leave the matter as undecided, with the hope that what he calls "true reality" will be attained over time. By the thirteenth century, most eastern Islamic astronomers, with several hundred years of reliable data behind them, were able to conclude that Ptolemy's year-length was bogus and that variable precession to account for the ancient values was unnecessary (Ragep 1993, v. 2, p. 396).

Table 3 Effect of variable precession on year-lengths (reconstructed according to the suggestion by Battānī, indicating the correlation between a shorter tropical year and an increasing rate of precession)

	Precession 1°/x years[a]	Precession y seconds/year[b]	Tropical year in days[b]	Motion of Sun per Egyptian year[b]
Babylonians	1°/261 years	14″/year	365;15,8 (365;15,22=sidereal year)	359°45′5″
Hipparchus	1°/125 years	29″/year	365;14,53	359°45′20″
Ptolemy	1°/100 years	36″/year	365;14,45	359°45′27½″
Battānī	1°/66 years	54½″/year	365;14,26	359°45′46″

[a]Rounded to the nearest year.
[b]In general, rounded to the nearest second.

The Obliquity of the Ecliptic

A third example concerns Ptolemy's value for the ecliptic, 23°51′20″, which has always been a bit mysterious inasmuch as it is off by almost 11 min. In a recent article, Alexander Jones provides us with a plausible and compelling argument for the origins of this number as well as another indication of Ptolemy's observational procedures (Jones 2002b). Jones shows that with a simple calculation one can get this result, or one very close to it, from a rounded value for the latitude of Alexandria of 31° (based upon an equinoctial shadow ratio of 3:5), the 5,000-stade distance of Alexandria to Syene (presumed on the Tropic of Cancer), and a circumference of the Earth of 252,000 stades. The ratio of the arc between the tropics, i.e. 47°42′40″, and 360° then translates by continued fractions into the enigmatic ratio 11/83 that is given by Ptolemy. Again we see the curious way in which Ptolemy has taken a Hellenistic value (probably from Eratosthenes) with evidently little attempt to verify it or its underlying parameters. (It is worth noting that Ptolemy's own latitude value for his hometown of Alexandria (30°58′), apparently taken from Eratosthenes' rather crude methods of equinoctial shadow ratios, is off by a quarter degree.)

Moving into the ninth century, we again have a familiar tale. Maʾmūn's astronomers arrived at a figure of 23°35′, which is accurate to about half a minute. But again there was confusion: was their value the correct one, allowing them to safely discard Ptolemy's, or had the obliquity actually been changing? In point of fact, the obliquity had been changing, but not so drastically as implied by Ptolemy's figure. There are reports of early attempts to deal with this by postulating an additional orb that would eventually lead to the obliteration of the obliquity entirely, leading to catastrophe in the opinions of some because of the subsequent lack of seasons. By the tenth century, there began to appear a number of creative attempts to deal both with a changing obliquity and a changing rate of precession, in part, no doubt, because early models meant to deal with a changing obliquity probably were seen (correctly) as interfering with the precessional rate (Ragep 1993, v. 2, pp. 396–408). While these attempts to provide models that would explain both the ancient and Islamic values for the obliquity were progressing apace, there were quite a few new measurements of the obliquity as we can see from Abū al-Rayḥān al-Bīrūnī's (d. ca. 1050) reports presented in Table 4 (al-Bīrūnī 1954–1956, v. 1, pp. 361–368). Note that most of these values are accurate to within a minute. (Bīrūnī himself notes that the two outliers, Abū al-Faḍl ibn al-ʿAmīd and Khujandī, were due to instrumental error.)

Bīrūnī describes the ecliptic ring needed to make the observations and remarks that it needs to be large enough in order to inscribe divisions in minutes. We also have a report from Ibn Sīnā (Avicenna; d. 1037), who gives a much less detailed account of earlier work in the appendix to his own *Almagest* that is part of his monumental work, *al-Shifāʾ*. There he merely reports that an observation of 23°34′ had been made after Maʾmūn's time. But then Ibn Sīnā gives his own observation to the nearest half minute, namely 23°33½′. This is a remarkably good value inasmuch as the estimate using modern tools gives 23°33′53″ for 1030. We have another report by Ibn Sīnā's long-term collaborator, ʿAbd al-Wāḥid al-Jūzjānī, who, writing after Ibn Sīnā's death, tells us that in Isfahan he obtained a value of 23°33′40″, which for 1040 would have been correct to within 8 or 9 s (al-Jūzjānī, *Khilāṣ kayfiyyat tarkīb al-aflāk*, Mashhad MS Āstān-i Quds 392 (=Mashhad 5593), p. 96). How they obtained such astonishing accuracy is not entirely clear, since they have not left us with detailed observational notes. We do, though, know that Ibn Sīnā was very interested in observations and invented an innovative observing device of some sophistication (Wiedemann and Juynboll 1927). It is also worth mentioning here that Ibn Sīnā claimed to have observed a Venus transit and also found the longitude distance between Jurjān and Baghdad to be 9°20′ [modern: 10°3′; traditional: 8°] (Ragep and Ragep 2004, p. 10). Although Bīrūnī did not think much of Ibn Sīnā's astronomical abilities, it is interesting that Bīrūnī basically ended up "confirming" the Maʾmūnī observations, whereas Ibn Sīnā and his circle seem to have embarked upon a serious observing program to test, and modify, previous results. Whether the remarkably accurate values they came up with are a matter of accident or due to innovative observational techniques remains a matter of conjecture. (It is worth noting that although the normal human visual acuity is limited to 1 min of arc, it is possible under certain circumstances involving the observation of a moving object to become hyperacute, with the capability to distinguish even 5 s of arc (Buchwald 2006, pp. 620–621)).

Table 4 Obliquity reports from Bīrūnī's *al-Qānūn al-Masʿūdī*

Observer	Obliquity value	Modern estimate
Euclid	24°	23°44′ (for –300)
Eratosthenes/Hipparchus	23°51′20″	23°43.5′ (–250)/23°43′ (–150)
Ptolemy	23°51′20″	23°40.5′ (140)
Indian Group	24°	23°38′ (500)
Yaḥyā b. Abī Manṣūr	23°33′	23°35′25″ (830)
Sanad ibn ʿAlī	23°34′ (23°33′52″ or maybe 23°33′57″ or 23°34′27″)	23°35′25″ (830)
Damascus tables	23°34′51″	23°35′25″ (830)
Banū Mūsā in Sāmarrāʾ	23°34½′	23°35′25″ (830)
Banū Mūsā in Baghdād	23°35′	23°35′25″ (830)
Manṣūr b. Talḥa/Muḥammad b. ʿAlī al-Makkī	23°34′	23°35′16″ (850)
Sulaymān b. ʿAṣma with parallax adj.	23°33′42″	23°35′5″ (875)
Sulaymān b. ʿAṣma without parallax	23°34′40″	23°35′5″ (875)
Battānī/Ṣūfī/Būzjānī/Ṣaghānī	23°35′	23°34′53″ (900)
Abū al-Faḍl ibn al-ʿAmīd	23°40′	23°34′30″ (950)
Khujandī	23°32′21″	23°34′19″ (970)
Bīrūnī	23°35′	23°33′58″ (1020)

Confirming vs. Testing

Let us look a bit more closely at the distinction I am trying to make between confirming and testing. (For the following, I am much indebted to Sabra 1968.) One often finds derived forms of the verb *iʿtabara* to indicate something like testing in the sense of checking whether a received value or parameter is correct; this is what Bīrūnī uses when saying that he wishes to test his predecessors' values for the obliquity. We also find another word, *imtiḥān*, which is used in the names of some *zīj*es such as the *Mumtaḥan Zīj* of the early ʿAbbāsid astronomer Yaḥya ibn Abī Manṣūr, and also in works that are meant to weed out incompetents, such as al-Qabīṣī's (10th c.) *Risāla fī imtiḥān al-munajjimīn* (treatise on testing the astrologers). Now Ptolemy, of course, also uses the idea of testing in various places in the *Almagest*. For example, in *Almagest* VII.1 he discusses the question of whether all stars or only those along the zodiac participate in the precessional motion. He proposes testing this by comparing his stellar observations with those of Hipparchus. Now the word used for comparison is σύγκρισις and for test πεῖρα. When the *Almagest* was first translated into Arabic by al-Ḥajjāj ibn Maṭar (early ninth century), he used *iʿtibār* for σύγκρισις and *tajriba* for πεῖρα. Later, in the second half of the ninth century, Isḥāq b. Ḥunayn would translate σύγκρισις as *muqāyasa* and πεῖρα as *al-miḥna wa-ʾl-iʿtibār* thus using two words for one. Since Isḥāq sometimes uses *iʿtibār* to translate σύγκρισις, A.I. Sabra has suggested that he may well have been trying to capture the idea of testing values over a longer interval by using the two words together. There are many examples in Islamic astronomy of the use of the conjoined *al-miḥna wa-ʾl-iʿtibār* or of one or the other alone to indicate testing. And Sabra has argued that *iʿtibār* from an astronomical context was used by Ibn al-Haytham for his idea of testing optical theories in his *Kitāb al-manāẓir*. (Note that the Latin translator of this work used *experimentum* for *iʿtibār*.)

Let me suggest that something more has been added in the translation process. When Isḥāq rendered πεῖρα as *al-miḥna wa-ʾl-iʿtibār*, he may well have meant to convey a stronger form of testing, one that was not simply a confirmation. Indeed, the word *miḥna* had attained a certain notoriety in the ninth century, since it was the inquisitory procedure used during the reign of the Caliph al-Maʾmūn to test adherence to the imposed state dogma of the createdness of the Qurʾān. Isḥāq was not translating in a vacuum. He was certainly aware that the author of *Fī sanat al-shams* believed that Ptolemy's πεῖρα for the solar year was suspect (see above). And his collaborator Thābit ibn Qurra was, as we have seen, suspicious as well. Thus this linguistic turn of phrase could well have reflected what had already happened in the first half of the ninth century, a felt need to critically test Ptolemy's parameters.

But what was the basis of this "need"? Given the many examples we have in Greek astronomy of confirmation rather than testing, I think we can safely say that there is nothing natural about testing with the intention to modify what has been received. Thomas Kuhn long ago made a persuasive case for the normalness of working within the paradigms of normal science, and though Kuhn did not necessarily have the safeguarding of parameters in mind, one can certainly understand the reluctance to change established values, especially something as entrenched as the length of the year. What seems to me in need of explanation are the many examples in early Islamic astronomy that point to a process not of confirming but of critical testing, with an intention and methodology that could result in revisions, sometimes drastic, to the received and heretofore accepted values.

Let us once again look at the case of measuring the Earth. Recall that Muḥammad ibn Mūsā seems to have followed the tried and true method of confirming earlier values in the way he went about using Ptolemy's *Geography* to show that Ptolemy's value was correct. But note the intervention of Maʾmūn, who exhibited a healthy skepticism and called for a new, indeed revolutionary, approach to the problem—he insisted upon each value being independently derived using reproducible methods that resulted in testable values. And from a modern perspective, the results are very good indeed.

Now the question arises: what could possibly have motivated Maʾmūn? Of course in the case of the size of the Earth, the obvious answer might be that he wanted to be able to have a basis for making maps of his vast empire, which was growing all the time. But to me this practical argument, though appealing, lacks a certain sufficiency. Didn't any ruler before Maʾmūn want a good value for the size of the Earth, going back to the Ptolemies and continuing through to the Romans, the Persians and many others? And this does not serve to explain the reports that show Maʾmūn riding his astronomers to produce better results on a whole range of observations (Langermann 1985). My own preference would be to see this as a kind of cultural transformation, one of many, that resulted from the appropriation of Greek science into Islam. Part of this transformation involved a much greater number of people involved in the enterprise, as is evidenced by Bīrūnī's list of observations of the obliquity. One can well sympathize with Ptolemy, who after all was a pioneer in many ways without a huge body of good observations at his disposal. But I think he also inherited an ambivalence about the phenomena that might well have stymied an excessive demand for accuracy. Though exactly what Ptolemy's philosophical and metaphysical stances may have been regarding ultimate reality is unclear, the Platonist strand at the time was strong, and Ptolemy may well have had to contend with attitudes such as we find in Proclus (4th c. CE):

> The great Plato, my friend, expects the true philosopher at least to say goodbye to the senses and the whole of wandering substance and to transfer astronomy above the heavens and to study there slowness-itself and speed-itself in true number. But you seem to me to lead us down from those contemplations to these periods in the heavens and to the observations of those clever at astronomy and to the hypotheses they devised from these, [hypotheses] which Aristarchuses and Hipparchuses and Ptolemies and such-like people are used to babbling about. For you desire indeed to hear also the doctrines of these men,

> in your eagerness to leave, so far as possible, nothing uninvestigated of what has been discovered by the ancients in the inquiry into the universe. (Proclus, *Hypotyposis*; translation by Lloyd 1978, p. 207, who also provides the Greek text).

What would the early Muslims have made of all this? I think, and here I must speculate, that they would have been profoundly puzzled. The religion of Islam reemphasized the concept of monotheism (*tawḥīd*) and the nobility of the created world. Thus in theory a Muslim so inclined could (some would say should) try to understand that world and its Maker's intentions. For a Platonist, this is a fool's errand, since what we experience through our senses is definitely not the Real. Furthermore Islamic law by its very nature emphasized the here and now to a remarkable extent despite the strong Islamic belief in the afterlife. How might these tendencies have influenced the course of Islamic science? In at least three ways. On the one hand, the earliest Islamic theological writings indicate an extensive interest in the material world and the type of world that would be compatible with God's will and intentions (Dhanani 1994). Another way in which interest in the mundane world could have been encouraged was in the demand for evidence brought by Islamic jurisprudence (*uṣūl al-fiqh*) and by the requirements needed to establish correct historical reconstructions to divine the Prophet's actual sayings and deeds (the *ḥadīth*). The third is the effect these religious aspects had on Hellenistic philosophy and philosophers in Islam. Though they were arch rivals, the *mutakallim*s (theologians) and *falāsifa* (Hellenized philosophers) grudgingly acknowledged the presence of one another and reacted to each other's doctrines. One of the ways that this manifested itself was in the striking transformation of what we can call the philosophy of science of Islamic philosophers. It has been customary to refer to such people, such as al-Kindī, al-Fārābī and Ibn Sīnā (Avicenna), as neo-Platonists. But these are very odd neo-Platonists. As should be clear from Ibn Sīnā, he had more than a passing interest in the phenomenal world held in such low esteem by the neo-Platonists of late antiquity. And even when those neo-Platonists wrote on astronomy, as Proclus did in his *Hypotyposis*, we can not help but notice his skepticism (as above), something one rarely finds in the philosophers of Islam. The insistence by Islamic philosophers and astronomers on the importance of empirical studies, manifested, for example, in Ibn Sīnā's striking observational program and in Fārābī's studies of contemporary musical practice, also bespeak a shift from late antiquity.

Could this shift in attitude account for Islamic astronomical exactitude? Here again we can only speculate since it is difficult to establish the relationship between ideological tendencies and actual practice. And we need to keep in mind that critical testing was episodic not universal in Islamic astronomy. Even Bīrūnī would seem to have succumbed to bouts of "confirmationism." And in the thirteenth century it is striking that no less a personage than Quṭb al-Dīn al-Shīrāzī was skeptical about the Maʾmūnī value for the Earth's circumference and thought it better to return to the authority of the Ancients (Ragep 1993, v. 2, pp. 509–510).

But the ongoing interest in observations and the ever increasing size of the instruments to make those observations—eventually culminating in the creation of the large-scale observatory—were often justified in terms of glorifying God's creation (Ragep 2001). If my suspicions are correct, it would seem that one of the unexpected consequences of the transplantation of ancient astronomy into Islamic soil was the subtle yet potent effect of monotheistic creationism in encouraging the astronomer to pay close attention to the sensual, phenomenal, and mundane world.

The Use and Abuse of Ptolemy's *Tetrabiblos* in Renaissance and Early Modern Europe: Two Case Studies (Giovanni Pico della Mirandola and Filippo Fantoni)

H. Darrel Rutkin

Not so very long ago, astrology was taught within the scientific curriculum of the finest European universities, especially in Italy, where it was taught from at least the beginning of the fourteenth through the middle of the seventeenth centuries. According to the University of Bologna's 1405 statutes, which articulate the basic structures of arts education in the premodern Italian universities, astrology was primarily taught in the four-year mathematics course, although it was also taught in different respects in the natural philosophy and medical courses. After prerequisites in arithmetic, geometry and elementary mathematical astronomy, the students began their study of astrology proper in the third year; in the fourth, they advanced to the higher levels of scientific astronomy and astrology by reading two of Ptolemy's fundamental texts, the *Almagest* and *Tetrabiblos*.[1]

Nevertheless, the actual teaching of Ptolemy's *Tetrabiblos* at medieval, Renaissance and early modern universities, in Italy and elsewhere, is not well understood. Indeed, the basic bibliographical and textual studies for understanding Ptolemy's Nachleben for this period in Europe have not yet been accomplished. The Ptolemy volume in the Kristeller-Cranz-Brown *Catalogus Translationum et Commentariorum* is a glaring desideratum.[2] To help develop the picture, I recently began to study a teaching manuscript from a year-long course on the *Tetrabiblos*. Delivered at the University of Pisa in 1585–1586, Filippo Fantoni (ca. 1530–1591) taught the entire text in 118 lectures during Galileo's last year as a student there, but we have no direct evidence that he actually took the course. I will discuss how Fantoni used Ptolemy in this teaching manuscript as the second case study.[3]

The first case study will explore how Giovanni Pico della Mirandola (1463–1494) attempted to use Ptolemy, a pro-astrological authority of the highest magnitude, to undermine astrology.[4] This was part of Pico's methodology to turn the astrologers' own arguments against their art in his extensive and multifaceted attack, the *Disputationes adversus astrologiam divinatricem* (*Disputations against*

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Divinatory Astrology), which was composed in 1493–1494 and published, posthumously, in 1496.[5] I will approach Ptolemy's use and abuse in Renaissance and Early Modern Europe by exploring two main themes of Pico's attack on and Fantoni's defense of astrology: (1) the disciplinary structure of astronomy-astrology, including its configuration within the broader map of knowledge, and (2) its natural philosophical foundations. Ptolemy discussed these issues in the first four chapters of the *Tetrabiblos*.

Pico

At the end of his short but intense life, Giovanni Pico della Mirandola attacked astrology with every means at his disposal, rhetorical, philological, empirical, and otherwise. His use and abuse of Ptolemy is part of the larger dynamic of Pico's use of authorities in the *Disputations against Divinatory Astrology*, a study yet to be undertaken. My first case study will provide an introduction to this rich and interesting subject; indeed, Ptolemy's *Tetrabiblos* looms large in Pico's *Disputations*, casting long shadows over the entire text.

Pico's attitude toward Ptolemy was respectful but mixed, with his complex usage deeply conditioned by his overall aim to destroy—not reform—astrology. Although truly respectful, Pico was ever willing to co-opt Ptolemy's arguments and authority to support his overall destructive aim, in part because Ptolemy himself often criticized the earlier astrological tradition. As one of the most influential mathematical theorists and practitioners of all time, Ptolemy's astrology was for Pico his only problematic feature. I will now explore how Pico used Ptolemy to attack and undermine astrology both explicitly and implicitly. Rarely launching a frontal assault, Pico normally attacked Ptolemy's views without naming him. When named, however, it was usually positive, emphasizing his role as astrological critic.

Pico discussed astrology's place within the map of knowledge at the very beginning of the *Disputations*. After insulting astrology as the mother of all superstitions, a loaded theological term, Pico defines what he means. His definition relates directly to Ptolemy's articulation in *Tetrabiblos* I, 1 of what became the standard premodern configuration of the science of the stars, but Pico does not mention this explicitly:

> But when I say *astrologia*, I do not understand that which measures the sizes and motions of the stars with mathematical argument, a certain and noble art, most honest in its benefits, and approved especially by the authority of the most learned men; but that which announces what will happen from the stars, a fraud of mercenary mendacity, prohibited by laws both civil and papal, retained by human curiosity, ridiculed by philosophers, worshipped by quacks, suspected by the best and most prudent, etc.[6]

Pico refers here to the standard distinction between the two sciences of the stars, what we call astronomy and astrology, where astronomy measures the heavenly motions and astrology treats their influence. His only description of the latter is that it announces what will happen from the stars (*quae de sideribus eventura*), which he then proceeds to abuse. Pico here differentiates the terms *astronomia* and *astrologia*, thus distancing himself from the practice of Ptolemy, Regiomontanus (1434–1474) and most others. In normal usage, the same term, either *astronomia* or *astrologia*, in either Greek or Latin, refers indifferently to *both* sciences of the stars, which are, nevertheless, conceptually differentiated in the same way Pico does here, but without his anti-astrological attitude.[7]

Pico then further distinguishes these subjects before rapidly devolving again into vituperation. The upshot of his discussion is that we should not be deceived by the similarity of names. Astrology has sneaked surreptitiously into the otherwise respectable company of the liberal arts, along with the legitimate science of mathematical astronomy, like a wolf in sheep's clothing. As Pico knew well, however, this disciplinary configuration was well-entrenched at the finest contemporary universities, including three he attended: Bologna, Padua and Ferrara. This context, with the odds of success stacked greatly against him, deeply informed Pico's attack and its vehemence.

In book one of the *Disputations*, Pico explicitly discusses this disciplinary structure in Guido Bonati and Ptolemy. After roundly criticizing all modern astrologers, Pico confronts the thirteenth century astrologer and writer:

> There is Bonatus among those of the highest authority; not only is he ignorant of philosophy, but he obviously raves and is delirious. Read the first book of his *On Judgments*, in which he himself introduces the work; I am lying if you do not judge this man worthy of hellebore. Where he is less stupid, he draws up certain arguments by which he proves that astrology is true; why should I call them false? One should rather call them puerile and ridiculous.[8]

After this abusive introduction, Pico turns to a criticism of interest for our purposes:

> But he thinks this is the most efficacious [argument]: that the *quadrivium* would be destroyed if astrology were removed; for it is one of the four mathematical arts. See how he does not even know what he professes! For this divinatory astrology, which we are refuting, is as far from that enumerated among the mathematical [arts] as light is from darkness, as truth from falsehood.[9]

After stating Bonati's claim, Pico again showers him with abuse. Nevertheless, within both Bonati's thirteenth- and Pico's fifteenth century contexts with their characteristic disciplinary structures, Bonati's claim is perfectly sound in using *astrologia* to refer to *both* sister sciences of the stars. Pico, on the other hand, provides a revisionary definition that terminologically distinguishes astronomy from astrology while delegitimizing astrology.

After further abuse, Pico presents an argument to support his interpretation:

> And this one indeed, which claims to predict the future, cannot stand, if that prior and truer one [mathematical astronomy] were removed; but the corresponding argument [does not follow], that, when the divinatory [part] has been removed, also the mathematical would be taken away. This is so clear, even to those who have only attained to the first elements of these arts, that it would be superfluous to declare this with more [arguments].[10]

Pico thus disposes of Bonati's argument by distorting his intention and ridiculing his results.

To support this interpretation, Pico then turns explicitly to Ptolemy's central text at *Tetrabiblos* I, 1:

> Ptolemy himself, in the introduction to the *Tetrabiblos* after he speaks of a twofold *astronomia*, nevertheless, referring to the prior mathematical discussion, he says thus: "The first of these, which has its own science, desirable in itself even though it does not attain the result given by the second ["astrology"], has been expounded to you as best we could in its own treatise [the *Almagest*]."[11]

This passage from Ptolemy supports Pico's misleading analysis of Bonati's argument, where the removal of mathematical astronomy destroys astrology but not vice versa. This, however, was not the intent of Bonati's argument, as Pico well knew. Bonati's point was that if you remove "the science of the stars," whether you call it *astrologia* or *astronomia*, you will remove one of the four mathematical arts (whose internal features he did not distinguish for this purpose), and thus destroy the fourfold nature of the *quadrivium*. Pico here thus appropriates Ptolemy's argument for his own anti-astrological purposes. Pico began by redefining Ptolemy's disciplinary structure without mentioning him, then he explicitly used a related element of that same structure to support his distortion of Bonati's argument, even though it coheres precisely with Ptolemy's analysis.

Pico attacked astrology's natural philosophical foundations in book III of the *Disputations*. His argument repays close analysis:

> At the same time as the astrologers say that every motion below depends on the motion of the heavens (*motum omnem inferiorem a caeli motu dependere*), they immediately contradict their teaching, since that commonplace among the philosophers follows from this, that the *caelum* is a universal cause of lower effects. Moreover, a universal cause does not distinguish effects, nor is why this comes-to-be or that sought from it, but from proximate causes, which are varied and different, to account for the difference and variety of effects; and since something makes different things from these [proximate causes], a universal cause makes everything with all [the proximate causes]. Since this appears obviously in things diverse in nature and species, it is amazing that they [the astrologers] do not understand by how much more the same thing should be believed about the variety of individuals, which, by how much more it is particular and, drawing its origin more from matter, it can be referred less particularly to a formal and universal cause. But who does not see that the heavens generate a horse with a horse, a lion with a lion, and that there is not any position of the stars under which a lion is not born from a lion, a horse from a horse.[12]

Pico here discusses the *caelum* as the most universal cause along with two types of proximate causes: the specific form (the formal cause) and the matter (material cause). The *caelum*, then, is the universal efficient cause, which must work with the specific form and matter to generate individuals. Just as differences of seeds determine different species (on the species level), so differences in matter differentiate the individual members of species.[13]

Ptolemy also treats these issues in *Tetrabiblos* I, 2, where he argues that astrology is a valid science, in the process treating different objections that might arise. In these discussions he offers a clear and nuanced picture of what astrology can and cannot provide insight into, and also what other factors must be taken into account to provide a full causal analysis; the details are very instructive. Ptolemy here defends astrology pre-emptively from those who would claim that the configuration of the heavens alone indicates everything. He is absolutely clear in the very first chapter that astrology is not an exact science (as mathematical astronomy is). It is conjectural and thus probabilistic, among other reasons, because one must also take into account the recalcitrant affects of matter. Nevertheless, what it can inform us about is so important, Ptolemy contends, that it is still a worthwhile scientific pursuit:

> In an inquiry concerning nativities and individual temperaments in general, one can see that there are circumstances of no small importance and of no trifling character, which join to cause the special qualities of those who are born. For differences of seed [with their specific forms] exert a very great influence on the special traits of the genus, since if the ambient [the celestial configuration] and the horizon [the place, with its unique horizon] are the same, each seed prevails to express in general its own form, for example, man, horse, and so forth; and the places of birth bring about no small variation in what is produced. For if the seed is generically the same, human for example, and the condition of the ambient the same, those who are born differ much, both in body and soul, with the difference of countries. In addition to this, all the aforesaid conditions being equal, rearing and customs contribute to influence the particular way in which a life is lived. Unless each one of these factors is examined together with the causes derived from the ambient, although this latter be conceded to exercise the greatest influence (for the ambient is one of the causes for these things being what they are, while they in turn have no influence upon it), they can cause much difficulty for those who believe that in such cases everything can be understood, even things not wholly within its jurisdiction, from the motion of the heavenly bodies alone.[14]

Ptolemy thus provides examples of the other causal factors involved, mainly differences of seed and the places of generation; he also presents the same and similar examples to those which Pico later used in his version of the astrologers' argument, but, of course, without citing an actual astrologer. In fact, Pico presents as the astrologers' the very same argument which Ptolemy himself had already criticized, and he used central features of Ptolemy's analysis to refute it. Pico's augmented argument is very coarse, stripped of all nuance; it is ultimately a caricature, as any informed reader would have realized.

It is no surprise, then, that Pico did not mention Ptolemy here. We know from *Disputationes* II, 1, however, that Pico knew this argument well (as we would have suspected anyway), because he himself provided a revised Latin translation of this key text directly from a Greek manuscript in the Laurenziana, which he used there to delimit astrology's domain.[15] Thus, where Ptolemy serves Pico's purposes, he holds him up as an authority. Pico is not, after all, attempting to reform astrology by returning it to a purified Ptolemaic basis, which the revised translation based on a Greek manuscript might imply; rather, Pico wants to obliterate astrology altogether while still retaining its Aristotelian cosmological and natural philosophical foundations.[16]

I will now briefly analyze Pico's attack on Ptolemy's view that the planets act through the four qualities, a central feature of Ptolemy's understanding of astrology's natural philosophical foundations from *Tetrabiblos*, I, 4. This argument is also not explicit. For Ptolemy, each planet's nature is characterized by a particular pair of the two primordial sets of contrary qualities: hot and cold, dry and moist. The planetary pairs (except Mercury) each involve one contrary from each set. The planets act by means of these qualities; for example: "the active power (*to poietikon*) of the Sun's essential nature is found to be heating and, to a certain degree, drying (Robbins 1940, p. 35)."

For Pico's revisionist views to be accepted, his analysis must also account for the same effects on Earth which the astrologers' analyses do; but for his anti-astrological purposes, he also needs to do so in a way that diminishes and preferably eradicates any individuating features the planets might have. Reducing planetary effects to motion, light and heat, Pico shows how the Sun and Moon in particular attain their perceived effects. This implicitly anti-Ptolemaic argument is more successful than the others we examined in that Pico does not first distort the pro-astrological argument. Rather, he provides a reasonable alternative analysis to account for the Sun's and Moon's heating and moistening, namely, the nature of the receiving matter and the intension and remission of light emanating from both luminaries.[17]

This move de-individuating each planet's particular nature by reducing them all to varying degrees of motion, light and heat is the primary structural metamorphosis Pico needs to co-opt and transform the normal astrologizing Aristotelianism to his anti-astrological ends. The heavens as the universal efficient working with the different limiting proximate causes—specific form and matter—where distinctions of place profoundly affect generation is precisely the same in both systems. For the astrologers, however, each planet has a unique nature, emitting its own characteristic radiating energy, which serves to individuate its effects.[18] For Pico, the planets and luminaries now all emit the very same radiations, modified only by degrees (more or less intense). To the extent that they can be measured—and their geometric relations to each other and to the place of generation—these reduced influences may still individuate what is generated, but for obvious reasons Pico himself does not make this turn.

I conclude this section on Pico by presenting his assessment of how the "astrologers" evaluated Ptolemy: "They easily concede that Ptolemy is their leader […]; for he is the most learned of the astrologers, and, with respect to mathematics, he is absolutely brilliant; but with respect to nativities, he should be called the best of the bad (*optimus malorum*)."[19] This seems more like Pico's editorializing than the astrologers', however. In general, then, Pico often presented an overly simplistic caricature as the astrologers' position. He then embraced their full argument—especially Ptolemy's—as his own more sophisticated natural philosophical analysis. Although structurally similar, Pico's analysis differed significantly at crucial points, thus turning his argument in an explicitly anti-astrological direction and abusing Ptolemy's status as a pro-astrological authority.

Fantoni

Ptolemy's *Tetrabiblos* was often used as a university textbook in medieval, Renaissance and early modern Europe. In this second case study I will offer a taste of what it was like to study astrology as a serious scientific subject at a distinguished Italian university.[20] To provide this taste, I will very selectively discuss the first six lectures of Filippo Fantoni's course on the *Tetrabiblos* at the University of Pisa in academic year 1585–1586.[21] Fantoni used not only his own wits to interpret Ptolemy. Rather, he drew on a rich tradition of translation and commentary, including earlier translations from Arabic and Greek, commentaries from medieval Arabic and Latin, as well as more modern works by Agostino Nifo and Girolamo Cardano. Here I will explore aspects of how Fantoni used Ptolemy, including his defense of Ptolemy from Pico's attacks. To do so, I will paraphrase and quote from the manuscript in Florence, which, as far as I know, has never been studied in detail.[22]

A Camaldolese monk, Filippo Fantoni, who lived from roughly 1530 to 1591, taught mathematics at the University of Pisa from 1560 to 1567, and then again from 1582 to 1589, when he was replaced by Galileo.[23] For his Ptolemy course, Fantoni used the Camerarius-Gogova translation of 1548 (Ptolemy 1548). One of Philipp Melanchthon's closest associates and a highly skilled textual scholar, Joachim Camerarius established the Greek text, publishing its *Editio princeps* in 1535 at Nuremberg with Johannes Petreius, who later published Copernicus's *De revolutionibus orbium coelestium* (1543) and some of Cardano's writings.[24] Indeed, Camerarius's Greek text was reprinted several times, but was only replaced with a modern edition in 1940, when it received two independently.[25]

In addition to providing the 1535 Greek text, Camerarius also translated the first two books of Ptolemy's treatise into Latin, providing brief summaries of books three and four. In 1548 Antonio Gogova, an associate of Gerard Mercator and Gemma Frisius at the University of Louvain, published a complete translation of the *Tetrabiblos*, where he took over Camerarius's translation whole cloth, and himself rendered books three and four into Latin (see Vanden Broecke 2003). Gogova's translation became standard in the Catholic world, whereas Philipp Melanchthon translated anew all four books in 1553, based on the revised edition of Camerarius's Greek text (Ptolemy 1553); this was the standard edition in Protestant lands. Cardano used the Camerarius-Gogova translation in his 1554 commentary on the *Tetrabiblos* (Cardano 1554), as did Fantoni for his course at the University of Pisa in 1585–1586.

When Cosimo I de' Medici refounded the University of Pisa in 1543, statutes were composed for the mathematics course that are clear and straighforward. I quote them in full: "The mathematics teacher in the first year will teach, [literally "read"], the author of the *Sphere*; in the second, Euclid; in the third, certain works of Ptolemy (*quaedam Ptolemaei*)."[26] Since the Ptolemy requirement was left open with respect to the specific work to be studied, we find that some professors taught from the *Almagest*, some from the *Geography*, and others from the *Tetrabiblos*.[27]

Fantoni's manuscript (Conventi Soppressi B.7.749)[28] at the Biblioteca Nazionale Centrale in Florence contains a complete lecture course on Ptolemy's *Tetrabiblos* in 118 lectures, written out in Latin in a fairly clear hand; there are many additions, corrections and deletions, seemingly in another hand (probably Fantoni's). The manuscript is very long, consisting of 673 double-sided folio pages.[29] Fantoni treats Ptolemy's prologue in his first six lectures. For the rest of this chapter, I will offer a glimpse into what a student in an advanced scientific astrology course at a fine Early Modern Italian university would have studied, and perhaps from the very lectures that Galileo himself heard as a student there. In providing a taste of Fantoni's course, I will focus once again on the disciplinary relationship between astronomy and astrology, and on astrology's natural philosophical foundations.[30]

In Chapter 1 of the *Tetrabiblos*, Ptolemy situates the current subject, astrology, in relation to another subject he had already treated thoroughly, namely, mathematical astronomy in the *Almagest*. Here is Fantoni's treatment:

> Since the study is twofold, *astrologia* is twofold: one is a speculative science concerned with motions, or the configurations of the luminaries and planets among themselves, and their relationship to Earth. This part is independent, as Ptolemy says in the *Almagest*. The other *astrologia* is concerned with prognostications and operations and effects coming forth from those motions. Therefore he says that his intention is to treat the science and doctrine of this part, and because someone could doubt whether this second science is equal to or less perfect than the first, comparing these he said that the first is more perfect because it is not dependent on the second. That is, the second *astrologia* is said to be less perfect than the first because we cannot know future events, unless we know the motions and the configurations among the planets. […] In fact, simply stated, the first contemplates the celestial bodies with respect to their differences of motion, and the second only with respect to their influences coming forth onto sublunary things. For which reason, both Aristotle and Ptolemy himself say that this lower world [the Earth] which is alterable by the first body [namely, the heavens] is the subject of this art.[31]

Despite Pico's attempts at restructuring, then, astrology was still intimately configured among the mathematical disciplines—as sister science of the stars with mathematical astronomy—in the 1580s.[32]

In Chapter 2 of the *Tetrabiblos*, Ptolemy argues strenuously that astrology should be considered a true science; but a conjectural one, not an exact science like mathematical astronomy. One of Ptolemy's principle arguments is that the Sun and Moon have obvious effects, which he then extends to the other planets. Here Fantoni fleshes out and supports Ptolemy's argument by discussing astrology's natural philosophical foundations. I will go into some depth here. First Fantoni raises a set of relevant questions:

> Therefore we shall enquire, first, whether celestial bodies act on the lower world; secondly, how they act, whether by light or by an occult power, and whether the entire heavens act, or just a part; and third, whether we can distinguish particular effects, because they say that the heavens are the cause of sublunar things.[33]

Before turning to specific arguments, however, Fantoni provides preliminary support for his case by presenting a two-fold set of authorities, natural philosophical and theological. First the philosophers, primarily Aristotle. Indeed, Fantoni here cites what have been called "the charters of scientific astrology":[34] First he cites a passage from book 2 of Aristotle's *Physics*: "The Sun and a human being generate a human being." Then he cites the second book of Aristotle's *De generatione et corruptione*: "Generations and corruptions come to be through the access and recess of the Sun in the oblique circle [namely, the zodiac]." Finally he cites the first book of Aristotle's *Meteorology*: "It is necessary that this lower world be contiguous with the higher motions [namely, of the luminaries and planets] so that all power (*virtus*) is regulated from there." He also adds a quote from Alexander of Aphrodisias, that not only in coming into being, but also in conservation, things below depend on superiors, by which he means the heavens.[35]

Then Fantoni turns to authoritative theological support. First sacred scripture, where he quotes the eighth book of *Deuteronomy*, which says that God made the planets to direct and govern human beings. Next he turns to Augustine, "a most excellent doctor of our Church," who says, in the fifth book of *De civitate Dei*—in an argument against the deterministic theories of the Stoics—that it is not absurd to say that different stellar influences act on different human temperaments. He then cites John the Damascene, who said that things below are governed by things above, and that different planets make different complexions and actions. Finally, he cites Thomas Aquinus (*divus Thomas*)—in the second book of his commentary on Peter Lombard's *Sentences*, and the second book of the *Summa Contra Gentiles*—that everything is referred causally to the celestial bodies, and he holds that they act on things below. Likewise, Fantoni concludes, Scotus and others hold this position as well.[36]

After this powerful display of authoritative support, Fantoni directly addresses how the celestial bodies act. He begins with motion:

> But let us see how they act. We should first note that celestial bodies act by motion, as is clear from the second book of *De caelo*, where Aristotle says that heat and light are generated by air agitated from the motions of the heavenly bodies, and therefore by compressing the air they act on things below, because thus they perfect their motions.[37]

Then, after arguing against the possibility (as some claim) that celestial bodies act by *virtutes occultae*, he aligns himself firmly with the Aristotelians, concluding that celestial bodies act only by motion and light, to which we will now turn.

The next question Fantoni tackles is fundamental, namely, whether astrology can distinguish particular or only general effects. In fact, this was one of Pico's most effective arguments in the *Disputations against Divinatory Astrology*, to which scientific astrologers responded from Lucio Bellanti ca. 1500 to Johannes Kepler and Placido Titi in the seventeenth century. Understanding how light works is key:

> To resolve this question, we say that light is understood in two ways, either qua genus or qua species. We say that light as light qua genus always has this distinguishing feature (*proprium*) to heat, just like an animal always has the *proprium* to be able to perceive (*sensibile*). Qua species, however, in so far as it follows the determinate species of stars,

> light cools, moistens, dries or heats, and it has different intentional and not only real actions, therefore, because the light of Saturn differs from the light of Jupiter, and the light of Jupiter from that of Mars, and likewise with the rest. And this argument from propriety is confirmed, for the different heats in light have different effects. The light of Saturn is a dark light, but that of Mars is firey, for the planet Mars appears to be ignited; likewise, the other planets have different lights, therefore they do different things, from which it follows that their properties and actions are distinct.[38]

Fantoni concludes this argument as follows: "[Therefore we can] say that celestial bodies act only by motion and light on these things below, and that motion and light are one qua genus. Moreover, light is many qua species, from which [it follows that] the same light in one species can cool, and in another, heat; nevertheless in itself it is the same."[39]

By arguing that the heavens act only by motion, light and heat, Fantoni thus situates himself within the now three hundred year old tradition in Europe of what I call an astrologizing Aristotelianism. In this system, astrological action is articulated in terms of Aristotelian natural philosophy operating within a Ptolemaic cosmographic framework with a geometrical optical model of planetary action.[40] With respect to natural philosophy, Pico was also heir to this tradition, but he, of course, wanted to profoundly reorient it by removing the astrological superstructure from its still solid Aristotelian foundations.[41]

The final argument to be examined concerns particular effects:

> It remains for us to see how the heavens distinguish particular effects, [since] Pico argued that there is no position of the stars under which a horse does not generate a horse, a human a human, [and] a lion a lion; therefore, the heavens do not distinguish particular effects. Again, two born in the same climate at the same longitude and latitude of cities and under the same position of the heavens; you cannot escape the argument that they are diverse in customs, in complexions, [and] in their *fortuna* [what happens to them]. What make the differences? Not the configuration of the heavens because it is the same, therefore the heavens do not distinguish particular effects.[42]

Fantoni meets this argument head on, but first by flipping it around and posing his own question:

> Turning the argument into its opposite, you would say that there are two children born from the same parents and raised at the same breast, disciplined by the same teacher in the same home and nourished on the same food, but nevertheless they have different habits, diverse complexions and fortunes; what then makes them different? I would say not only that the heavens are a universal cause, but also that all the stars qua species distinguish particulars, as Aristotle says. Since they either distinguish particulars immediately or mediately, we say, therefore, with secondary causes mediating, that the Sun and a man generate a human being, as with an instrument, for the heavens are, as it were, the artisan (*artifex*) and we are the instruments, as Averroes says in the 8th book of the *Physics*, just as it happens in art. For an artisan cannot make artifacts (*artificia*) without an instrument. Rather, he uses many instruments different in kind (*in specie*) and quantity for [making] different artifacts; likewise the heavens make different things with different instruments [namely, with different parents].[43]

The heavens working through different parents as with different instruments is mediated causality, whereas immediately or directly, the different planets qua species have different universal qualitative influences.

After establishing these principles, Fantoni turns directly to engage Pico's arguments *contra*: "There is no position of the stars under which a horse does not generate a horse. [This is denied], although some affirm and concede it. But I deny it (he says emphatically in the first person: *ego hanc nego*), because there is some gathering of the stars under which a horse will not generate a horse," but he does not press the point. Rather he discusses a more moderate position that most people would accept:

> But some say, conceding this nevertheless, that even if there is no position of the stars under which a horse does not generate a horse, there will be a difference in the generating because a horse can be born ferocious or sickly, that is, if it is not generated under a good constellation it will be a weak horse or aborted; regardless, I deny the major premise.[44]

The second argument, about twins, he makes short work of: "We say it is not possible for two children even twins to be born at the very same moment (*in eodem puncto et momento*)." He then provides evidence from his own experience: "I have observed all the births of twins in Florence in the Church of San Giovanni," namely, in the Baptistery, "and I have never seen nor has anyone seen twins born at the very same moment, but they can have different positions of the horizon and the meridian," namely different ascending and midheaven degrees, factors crucial for interpreting a horoscope.[45] Finally, he draws his conclusion:

> Let us say, therefore, that all these effects should not be referred only to celestial causes, because, as you have seen, one should also pay attention to the material subject, namely, this lower world, and laws, and customs, education and regions, as is clear from [the example of] a king's son and a peasant's. For the peasant's son is a peasant and born to labor. But a king's son is born a king and to rule.[46]

Thus Fantoni reiterates Ptolemy's fundamental point that although the celestial configurations are fundamental, they are by no means the only causal factor in accounting for why people are as they are.

Conclusion

This treatment of the use and abuse of Ptolemy's astrology in Renaissance and Early Modern Europe is merely an introduction to a rich and important subject that remains to be properly studied. I have only discussed Ptolemy's authentic *Tetrabiblos*. A full treatment would also discuss the extraordinarily influential but pseudonymous *Centiloquium* or *Karpos*, which Pico considered authentic but Cardano rejected.[47] Finally, the complex reception of—and responses to—Ptolemy's astrology is intimately connected to the broader question of astrology's long term acceptance within—but ultimate rejection from—the domain of legitimate natural and political knowledge, which took place during the seventeenth and eighteenth centuries in a complex process that is not yet fully understood.[48]

Notes

1. For the evidence and further bibliography, see Rutkin (2002, Chapter 2), Rutkin (2006) and Vol. I part II of Rutkin (forthcoming).
2. Francis Carmody stated that he was currently working on this volume in the 1950s (Carmody 1956). Now it would ideally be composed by a team of scholars.
3. Fantoni based his course closely—but with various alterations, some dramatic—on Giuliano Ristori's (1492–1556) earlier lecture course on the *Tetrabiblos* at Pisa delivered during the 1540s. For more on Ristori, see i.a. Schmitt (1972). When I offered this paper, I had not yet worked on the text of Ristori's lecture course in MS. Riccardiana 157. I will publish more on Ristori and the comparison with Fantoni in a forthcoming piece in a collection of essays originating from the conference, "From Masha'allah to Kepler: The Theory and Practice of Astrology in the Middle Ages and the Renaissance," held at the Warburg Institute, Nov. 2008. For now, however, much of what I present here under Fantoni's name also applies to Ristori.
4. This material is drawn from Rutkin (2002, Chapters 5 and 6), where much further bibliography can be found. For a splendid introduction to Pico, see Grafton (1997).
5. Garin's national edition (Pico della Mirandola 1946) relies on the *Editio princeps* (Pico della Mirandola 1496), our primary textual witness, since no earlier manuscript evidence exists. See Garin's introduction for more information on the text. Pico's page and line numbers cited here are also to this edition.
6. "Astrologiam vero cum dico, non eam intelligo quae siderum moles et motus mathematica ratione metitur, artem certam et nobilem et suis meritis honestissimam auctoritateque hominum doctissimorum maxime comprobatam; sed quae de sideribus eventura pronunciat, fraudem mercenariae mendacitatis, legibus interdictam et civilibus et pontificiis, humana curiositate retentam, irrisam a philosophis, cultam a circulatoribus, optimo cuique prudentissimoque suspectam, cuius olim professores gentilicio vocabulo Chaldaei, vel ab ipsa professione genethliaci dicebantur (40, 1–11)[.]" All translations of Pico and Fantoni are mine. No English translation exists for either. I am currently translating Pico's *Disputations* for the I Tatti Renaissance Library (Harvard University Press).
7. Regiomontanus discusses this in the extant inaugural oration for his course on al-Farghani's *De scientia stellarum*, given at the University of Padua in 1464, the year after Pico's birth, and at a university where Pico later studied. See Swerdlow (1993) and Rose (1975) for further discussion of Regiomontanus's oration, its context and significance.
8. Est Bonatus inter eos primae auctoritatis; is non ignarus modo est philosophiae, sed furit plane atque delirat. Lege eius primum librum *de iudiciis* in quo super opere ipse prooemiatur; mentior nisi helleboro dignum hominem iudicaveris. Struit, ubi desipit minus, rationes quasdam quibus astrologiam probet esse veram; illas quid dicam falsas? immo supra quam dici possit pueriles atque ridiculas (74, 17–76, 2).
9. Quam vero putat efficacissimam illa est: quadrivium destrui si astrologia tollatur; esse enim unam ex quattuor artibus mathematicis. Vide ut nescit etiam quid sit hoc ipsum quod profitetur! Astrologia enim haec divinatrix, quam confutamus, tantum distat ab ea quae mathematicis annumeratur quantum a tenebris lux, quantum veritas distat a mendacio (76, 2–7).
10. Et haec quidem, quae futurorum praedictionum usurpat, si prior illa veriorque tollatur, stare non potest; at non remeat ratio ut, divinatrice sublata, mathematica quoque illa auferatur. Quod adeo est perspicuum, vel his qui prima harum artium attigerit elementa, ut pluribus hoc declarare superfluum sit (76, 8–13).
11. "Ptolemaeus ipse, in prooemiis *Apotelesmaton*, postquam de duplici dicit astronomia, tamen ad priorem et mathematicam referens sermonem ita inquit: (76, 13–15)." Garin then prints the Greek here (76, 15–18); I quote Frank Robbins's translation (Robbins 1940, p. 3).

12. [N]am, quod ad primam attinet rationem, simul atque dixerunt astrologi motum omnem inferiorem a caeli motu dependere, statim dogmati suo contradixerunt, cum inde illud sequatur tritum apud philosophos, esse caelum universalem causam effectuum inferiorum. Causa autem universalis effectus non distinguit, neque cur hoc fiat, aut illud, quaeritur ab ea, sed a proximis causis, quae variae et differentes sunt, pro effectuum differentia et varietate; et cum ex his alia aliud faciat, universalis causa cum omnibus omnia facit. Quod cum manifeste appareat in rebus natura specieque diversis, mirum quomodo non intelligant multo magis idem credendum de varietate individuorum quae, quanto magis et particularis est et a materia plurimum trahens originem, minus referri potest in causam maxime et formalem et universalem. At quis non videt caelum cum equo equum generare, cum leone leonem, nec esse ullam siderum positionem sub qua de leone leo, de equo equus non nascatur (188, 18–190, 7)?
13. I reconstruct in much greater detail the basic structures of the astrologizing Aristotelian natural philosophy Pico attacks here in Chapter 1 of my dissertation and Vol. 1 part I of my book.
14. I use Robbins's translation (slightly modified) from Robbins (1940, pp. 17–19).
15. For a description of the manuscript (Plut. 28, 20) and further bibliography, see Gentile (1994, pp. 97–98).
16. This is one of the broader conclusions from Chapter 6 of my dissertation.
17. See 220, 20–222, 22, esp. 220, 20–222, 2: "Sic caelesti calori pro materiae conditionibus evenit, unde opinio nata Solem siccitatem facere, Lunam augere humiditatem, quia scilicet Solis calor ardentior, Lunae vero tepidior. Sed in idoneis affinibusque subiectis nec exsiccabit radius Solis humorem salutarem, nec generabit Luna inutilem noxiamve humiditatem, sed operabitur idem sua natura sidus utrumque, licet Luna remissius quod intensius Sol efficiet; ex accidenti vero, per infectionem peregrinae materiae, dissimiliter illis eveniunt, Sol ut exsiccet, Lunae ut humefaciat."
18. As Kepler does, for example, in *De fundamentis astrologiae certioribus (On the More Certain Fundaments of Astrology*, 1602), which I discuss in Chapter 7 of my dissertation and Vol. II of my book. There is an English translation in Field (1984).
19. Porro Ptolemaeum principem aliorum facile concedent; est enim doctissimus astrologorum et, quod attinet ad mathematica, vir ingeniosissimus; quod autem ad genethliacos, quemadmodum dici solet, optimus malorum (70, 5–8).
20. Much more research needs to be undertaken before a satisfactory account can be offered.
21. Charles B. Schmitt signalled but only briefly discussed this manuscript in two articles (1972 and 1978), the fruit of research undertaken while a Fellow at the Villa I Tatti, the Harvard University Center for Italian Renaissance Studies. This essay is offered to his memory. My thanks to Villa I Tatti for also making possible my research on Fantoni's manuscript.
22. My transcription of the manuscript presented here is preliminary; the text offered is a working text. I describe the manuscript just below.
23. My knowledge of Fantoni comes from Schmitt's articles, Pagano (1994) and the manuscript itself.
24. Camerarius (1535). For more on Camerarius, see Baron (1978). For Petreius, a printer worthy of greater attention, see Swerdlow (1992) and Brosseder (2004, pp. 147–149).
25. Robbins (1940) and Boll and Boer (1940). Hübner's recent Teubner text is the first to take both modern editions into account (Hübner 1998). I do not know of a satisfactory account of Ptolemy's textual history, especially of the Latin translations and commentaries. A proper critical edition of Ptolemaeus Latinus is a desideratum.
26. Schmitt (1972, p. 257). I discuss this material as context for understanding Galileo's study and practice of astrology in Rutkin (2005).
27. Lectures on the *Tetrabiblos*, were oriented, at least in part, to medical students, as we can see from Fantoni's course for 1585–1586 (Schmitt 1978, p. 57): "Primum Euclidis, primum librum Quadripartiti Ptolomei, quaestiones ad facultatem medicinae pertinent[es], et secundum planetarum delineationes et non aliud."

28. This corrects Schmitt's misprint of B.7.479 (1972, p. 259 n. 82).
29. This is my description of the manuscript, which I examined in situ.
30. I presented an earlier version of this section as part of a mini-colloquium at Villa I Tatti (May 2006). I offer my sincere gratitude for that year's fellowship, as well as to my colleagues in the colloquium and the audience.
31. Quod cum duplex sit studium, vel duplex sit Astrologia, una speculativa que versatur circa motus, configurationesve et luminum et planetarum inter se et habitudines circa terram, hec pars absoluta est, ut ipse dicit in altero libro magne compositionis. Alteram vero dixit Astrologiam, que versatur circa prognosticationes et operationes et effectus proficiscentes ex illis motibus. Ergo dic quod intenti sua est tradere scientiam et doctrinam huius partis. Et quia aliquis poterat dubitare an hec scientia secunda sit equalis vel minus perfecta quam prima, illas comparando dixit quod prima perfectior est quia non dependet a secunda. Secunda vero Astrologia eo quod a prima ea imperfectior dicetur, non enim futurorum eventus cognovere possemus nisi motus ac stellarum inter se configurationes cognoscamus. […] Etenim simpliciter celestia corpora quoad motum eiusque diversitatem contemplatur, et secunda impressiones tantum celestium corporum in sublunaribus proficiscentium. Unde dicitur et ex Aristotele et ipsomet Ptolomeo, quod mundus iste inferior alterabilis a corpore primo est subiectum huius artis (4v–5v).
32. The accurate study of Pico's influence is still in its infancy; for some preliminary indications, see Chapter 7 of my dissertation. For a broad range of evidence to establish that astrology was still normally configured closely with astronomy throughout the sixteenth and well into the seventeenth century, see Vol. II of my forthcoming book, which significantly supercedes my earlier discussions.
33. Querebamus ergo an corpora celestia agunt in hec inferiora, secundo quomodo agant, vel lumine, vel virtute occulta, an totum celum, vel pars eius, 3.o utrum distinguat effectus, quia dicunt ipsum esse causam rerum sublunarium, an propter hoc quod sit causa virtutis distinguat effectus particulares (17r–v).
34. This is Richard Lemay's phrase (1987, p. 57).
35. In oppositum tamen fuit Ptolomeus et Aristoteles 2.do physicorum Sol et homo generant hominem[.] Item 2.do de generatione tex.58. per accessum et recessum solis in circulo obliquo fiunt generationes et corruptiones[.] [I]n p[rim]o meth. dicit, necesse est mundum hunc inferiorem superioribus lationibus esse contiguum, ut omnis virtus inde regatur, ubi dicit Alexander quod non solum in fieri, sed etiam in conservari dependent hec inferiora a superioribus (18v–19r).
36. Habemus etiam sacros codices, nam 8.o deuteron. dicitur quod Deus fecit planetas ad ministerium hominis et ad gubernationem. Item ab Augustino doctore Ecc[ellentissi]mo nostre Ecclesie habetur in 5. de civitate Dei ad calcem eum disputavit contra stoicos. Dicit non est absurdum dicere, quosdam sidereos afflatus afferre ad varias ac varias hominum complexiones, et Damascenus in 4.o cap.o 4.o dixit inferiora gubernari a superioribus, item in secundo, alii et alii planete aliam et aliam faciunt complexionem et actionem, sic etiam divus Thomas in secundo sententiarum distinct. 24.a et 2.do contra gentes ab 80 usque ad 92.m omnia refert in corpora celestia, et tenet quod agant in hec inferiora, similiter et scotus, et alii (19r–v).
37. Sed videamus quomodo agunt. Est primum animadvertendum corpora celestia agere motu, quod autem ita sit patet in 2.do Celi ubi dicit quod calor, et lumen generantur aere attrito ex superiorum latione, igitur corpora celestia terendo, et comprimendo aerem agunt in hec inferiora, quia sic perficiunt motus (19v)[.]
38. "Pro solutione rationum dicimus quod lux dupliciter consideratur et accipitur, vel quo ad genus, vel quo ad species. Dicimus quod lux ut lux sub ratione generis semper est ei proprium calefacere, sicut animali, ut animal est ei semper esse sensibile. Ut autem capitur pro specie, pro ut consequitur determinatas species stellarum lux frigefacit, humectat, ex[s]iccat, urit et agit alias actiones intentionales, non solum reales, adeo quod lux saturni differt a luce [Jupiter], et lux [Jupiter] a [Mars], et similiter de reliquis. Et argumento sumpto a proprietate potest confirmari, diversi enim calores in luce agunt diversos effectus, modo lux

[Saturn] est obscura lux. [Mars] est quasi ignis, videtur enim stella [Mars] quasi ignita, similiter alii planete habent diversas luces, ergo videntur agere diversas res, ex quo sequitur quod proprietates, et actiones sint distincte (21r–v)[.]" I put the planetary names in English and in brackets to represent the glyphs in the manuscript.

39. […] et dicere quod corpora celestia agunt tantum motu et lumine in hac inferiora, et motus et lumen sunt unum secundum genus. Lux autem est plures secundum species, ex quo potest eadem lux in una specie infrigidare in alia calefacere, in se tamen est eadem (22v).
40. For a detailed reconstruction, see Chapter 1 of my dissertation and Vol. I part I of my book.
41. In this Giovanni Pico is to be distinguished from his nephew and editor, Gian Francesco Pico della Mirandola (1469–1533), who wished to destroy the entire edifice of Aristotelian natural philosophy; see Schmitt (1967).
42. Restat ut videamus quomodo celum distinguat effectus particulares. Arguit Picus non est positio siderum sub qua equus non generet equum, homo hominem, leo leonem, ergo celum non distinguit effectus particulares. Rursus nascantur duo in eodem clymate in eadem longitudine et latitudine civitatum et sub eadem positione celi, ne possis fugere argumentum, isti sunt diversi in moribus, in complexionibus, in fortuna. Quid facit diverso? Non constellatio celi quia est eadem, ergo celum non distinguit effectus in particulares (23r–v)[.]
43. Sed dicetis vertendo argumentum in oppositum sint duo filii orti et nati ex eisdem parentibus, educati eodem lacte, disciplinati sub eodem preceptore in eadem domo, eisdem cibis nutriti, et tamen habent diversos mores, diversas complexiones, et diversas fortunas, quid igitur facit illos diversos? Ideo dicamus quod non solum celum est causa universalis, et omnes stelle in specie tamen distinguuntur particularia sicut [Aristoteles] facit, quoniam aut distinguunt immediate, vel mediate non imediate, dicimus ergo mediantibus causis secundis, quia [sun] et homo generant hominem tamquam instrumento. Celum enim est tamquam Artifex, et nos sumus instrumenta. Dicit Averroes 8.o Phys: 47.o sicut in arte contingit. Non enim artifex sine instrumento conficit artificia, immo pluribus instrumentis diversis specie et quantitate utitur ad diversa artificia, similiter celum diversis instrumentis agit diversa (23v).
44. Non est positio syderum sub qua equus non generet equum, [hoc negatur] licet aliqui affirment et concedant. Sed ego hanc nego, quia est aliqua factio (sic) syderum sub qua equus non generabit equum. Dicunt aliqui concedendo ipsam tamen, quod si non sit aliqua positio siderum, sub qua equus non generet equum, sed erit differentia generationis quia poterit generari equus ferox vel debilis, quia si non generabitur sub bona constellatione erit equus debilis et abortivus, tamen ego nego maiorem (23v–24r)[.]
45. Ad aliud argumentum de duobus natis dicimus, quod non est possibile duos pueros et gemellos nasci in eodem puncto et momento[.] Ego observavi Florentie omnes nativitates gemellorum, que sunt in Ecclesia Divi Joannis, et nunquam vidi neque aliquis vidit gemellos notos in eodem puncto et momento, sed possunt habere alias positiones horizontis et meridiani (24r).
46. Dicamus igitur quod non sunt reducendi omnes isti effectus in causas celestes, quia ut vidistis animadvertenda est, et materia subiecta et mundus iste inferior et leges et consuetudines, educationes et regiones, sicut patet ex puero Regis et Rustici. Puer enim rustici natus est rusticus et ad laborandum. Puer vero regis natus est Rex et ad dominandum (23r–v)[.]
47. Grafton (1999, p. 137) discusses Cardano's rejection of Ptolemy's authorship.
48. I address this fundamental question in Vol. II of my book.

Tycho, Longomontanus, and Kepler on Ptolemy's Solar Observations and Theory, Precession of the Equinoxes, and Obliquity of the Ecliptic

N.M. Swerdlow

> *It therefore remains that either Ptolemy committed fraud with fabricated observations, or from a kind of awe and reverence for the ancients preferred to confirm rather than refute them, neither of which is likely in the philosopher Ptolemy, a defender of candor and truth, as is witnessed by many judgments, especially since he could expect no advantage or fame from this, but rather greater advantage and fame from correcting the ancients. But that he was not obsequious to the ancients, he left witnessed in many ways, refuting Hipparchus where it was required.*
>
> Johannes Kepler (1937–, 21.1.324).

It is well known that there are errors in Ptolemy's observations of the Sun with consequences for his own astronomy and for later astronomy up to some time in the seventeenth century. The principal problems and their consequences in Ptolemy's astronomy are the following:

(1) The latitude of Alexandria is taken to be $\varphi = 30;58^\circ$ when correctly it is $31;13^\circ$, an error of $-0;15^\circ$. In *Almagest* 2.5 Ptolemy describes, although does not recommend, a method of finding the latitude from the length of the Sun's shadow at both solstices or a solstice and equinox, which would make the latitude an error in measurement of the Sun's zenith distance. Indeed, for $\varphi = 30;58^\circ$, $\tan\varphi = 0;36,0 = 3/5$, so that where the length of a gnomon is 60, in the equator the length of the shadow is 36, which does suggest use of or adjustment to a rounded shadow length. The consequence is that the meridian altitude of the celestial equator is too high, or its zenith distance too low, by $0;15^\circ$, about 15 hours in the motion of the Sun in declination and $0;37^\circ$ in longitude near the

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equinoxes, and the error of 0;15° in meridian altitude and zenith distance affects the entire ecliptic.

(2) The obliquity of the ecliptic is taken to be $\varepsilon = 23;51,20°$ when by modern computation in 140 it was 23;40,39° an error of nearly +0;11°. From measurements of the zenith distance of the Sun at summer and winter solstice, Ptolemy found the arc between the tropics 2ε to lie between $47\frac{2}{3}^{\circ}$ and $47\frac{3}{4}^{\circ}$, and converted the lower limit to a ratio by $47\frac{2}{3}/360 = 143/1080 = (11 \cdot 13)/1080 = 11/83\frac{1}{3} \approx 11/83$, that is, the arc between the tropics is *about* 11 (parts) of which the meridian is 83. He notes that he derives *nearly* the same ratio as Eratosthenes, which Hipparchus also retained, a cryptic remark that has provoked a great deal of fanciful speculation. The derivation given here is by Delambre, although he attributes it to Eratosthenes rather than Ptolemy, which is scarcely possible. Before considering any other explanation of the ratio, it is first necessary to show that Delambre's is not correct. In any case, $2\varepsilon = 11/83 \cdot 360° \approx 47;42,40°$ and $\varepsilon = 23;51,20°$.

(3) The dates of Ptolemy's observations of three equinoxes and one summer solstice are from about 21 to 36 hours late. The consequences are to confirm exactly Hipparchus's length of the tropical years, $365\frac{1}{4} - \frac{1}{300}$ days $= 365;14,48^{d} = 365^{d}\ 5;55,12^{h}$, too long by $+0;6,26^{h}$, and to establish an epoch of the mean longitude of the Sun too low by −1;5° in 132, which indirectly affects the longitudes of the Moon, planets, and fixed stars. Because of the error in the length of the year, the error in the times of equinoxes accumulates at the rate of $+10;43^{h}$ per century and the error in the mean longitude of the Sun at −0;26,25° per century, and this too affects the longitudes of the Moon, planets, and fixed stars. The equinoxes and solstices cited by Ptolemy with specific dates and times are compared with modern computation in the Appendix and cited here by number.

(4) Ptolemy uses the same intervals as Hipparchus between the equinoxes and summer solstice, to one-half day, and derives the same eccentricity and direction of the apsidal line. Thus, from the vernal equinox to summer solstice $94\frac{1}{2}^{d}$, summer solstice to autumnal equinox $92\frac{1}{2}^{d}$, vernal to autumnal equinox 187^{d}, he finds that where the radius of the Sun's eccentric $R = 60$, the eccentricity $e = 2;29,30 \approx 2;30$ so that $e/R = 1/24$, the maximum equation $c_{m} = 2;23°$, and the direction of the apogee $\lambda_{A} = 65;30°$. He concludes that the eccentricity has not changed and the apogee is tropically fixed. Taking twice the modern eccentricity, in −145, the time of Hipparchus, $e = 2;6,22$, $c_{m} = 2;1°$, and $\lambda_{A} = 66;16°$; in 140, the time of Ptolemy, $e = 2;5,37$, $c_{m} = 2;0°$, and $\lambda_{A} = 71;6°$. Hence, e is in error by +0;24 and c_{m} by +0;23° and have barely changed, but λ_{A} is in error by −0;46° in −145 and −5;36° in 140, and its longitude has increased +4;50° in 285 years, of which about 4° is the precession of the equinoxes and 0;50° the proper or sidereal motion of the apsidal line.

(5) As a result of the error in the mean longitude of the Sun, Ptolemy's measurements of longitudes of fundamental stars have a systematic error of just over −1°. He therefore finds a difference in longitude of stars in the 265 years since Hipparchus of 2;40° when it should be just over 3;40°, and corresponding differences are found from other early observations. These confirm Hipparchus's

low estimate of the motion of the fixed stars, or precession of the equinoxes, of 1° per century or $36''$ per year, less by about $-14''$ than the correct $50''$ per year or 1° in 72 years. The error in the tropical longitude of stars accumulates at the rate of $-0;23,20^\circ$ per century.

So much for the errors and their consequences in Ptolemy's astronomy. The more interesting story begins several hundred years later when Arabic astronomers derived parameters from their own observations of the Sun and stars, and used Ptolemy's observations for finding motions over the intervening period. Without exception, compared to Ptolemy's parameters, it was found that: the obliquity of the ecliptic is smaller, the tropical year shorter, the eccentricity of the Sun smaller, the solar apogee advanced in longitude, and the motion of the fixed stars faster. What is to be done? One solution, for example by al-Battānī, was to accept the new parameters as correct and Ptolemy's, by implication, as erroneous.[1] And it was concluded, for example in *De anno solis* attributed to Thābit ibn Qurra, that Hipparchus's observations of equinoxes and longitudes of stars were preferable to Ptolemy's for finding the length of the sidereal and tropical year and the motion of the fixed stars, which accounts for their difference. But another, more complex solution was to assume that the parameters had changed over the intervening centuries and develop models and parameters for these long-period variations. Among those that came to be known in Europe are a model for a nonuniform motion of the "eighth sphere," of the fixed stars, in *De motu octavae sphaerae* attributed (incorrectly) to Thābit, included in the *Toledan Tables*, a model for a variation of the solar eccentricity by az-Zarqāl, not included in the *Toledan Tables*, and a very well-known nonuniform motion of the eight sphere in the *Alfonsine Tables*, for which there are tables but no model. The apogees of the Sun and planets were taken to be sidereally fixed, and thus to follow the motion of the eight sphere, and the apogee of the Sun was sometimes given its own proper sidereal motion. Implicit in models for the motion of the eight sphere is a variation of the obliquity of the ecliptic, although this was, it appears, not tabulated as a variable parameter, nor was an implied variation in the length of the tropical or sidereal year tabulated. The last thing these theories can be called is consistent. In the *Theoricae novae planetarum*, Peurbach described his understanding of the model in *De motu octavae sphaerae* and explained what may be his own model for the Alfonsine motion. Regiomontanus considered both theories to be false (*mendacem*), which was his opinion of the *Alfonsine Tables* in general.

All of these attempts to include long-period variation of parameters were superseded by Copernicus, who developed more or less consistent models of some complexity, based upon motions of the Earth rather than the sphere of the fixed stars and the Sun, for nonuniform variations of the obliquity of the ecliptic, rate of the "precession of the equinoxes" (Copernicus's own term), length of the tropical year, solar eccentricity, and sidereal and tropical motion of the solar apogee. Copernicus's models were described and the parameters derived, with some wishful thinking, in *De revolutionibus* (1543), and all the long-period motions were included in Erasmus Reinhold's *Prutenic Tables* (1551), which became the basis for the computation of ephemerides in the later sixteenth and early seventeenth centuries. And Copernicus's models were carried over into geocentric theory, as by Giovanni Magini, by transferring the various motions of the Earth to the sphere of the fixed stars and the Sun. Hence, the complex legacy of the errors in Ptolemy's solar observations was fundamental to the astronomy in this period in both theory and tables. But already questions were being raised about the reliability of the observations. Copernicus told Rheticus of his fear that very many of the observations of the ancients were not genuine but were accommodated to their theory, as Rheticus reports in his *Ephemerides novae* (1550), although these doubts may have come after Copernicus erected much of his astronomy upon these very observations. Girolamo Cardano, in *De restitutione temporum et motuum coelestium* (1543), attempted, after a fashion, to find the cause of error and correct some of Ptolemy's observations and parameters, although he was more critical of Thābit's motion of the eighth sphere and the *Alfonsine Tables*. A more expert examination did not come until the ancient observations and parameters were considered by Tycho, who intended more than he accomplished, Tycho's former assistant Christian Longomontanus, who set out the most radical criticism and correction, and Kepler, who had his own reasons for carrying out such an investigation. In this paper, we shall consider all three.[2]

Tycho Brahe

It is commonly said that Tycho did away with all the long-period variation of parameters that had so concerned Copernicus and established new and improved parameters for the obliquity, solar theory, and precession on the basis of his own observations, more accurate than any that came before. There is some truth in this, as he did all of these things, but in the *Progymnasmata* he explains several times that the parameters established here are only for his own time and he intends to investigate their variation over a long period in a complete restoration of astronomy, which was never written. In fact, Tycho always believed with Copernicus that the obliquity of the ecliptic and the solar eccentricity had decreased and the apsidal line advanced from antiquity to his own time, meaning that he took the observations and theory of Hipparchus and Ptolemy seriously, although he never worked out a hypothesis, model, of his own for long-period variation. Initially, he accepted Copernicus's hypothesis for the Sun, but because Copernicus's eccentricity was smaller than he found, he concluded that it must be erroneous. In a letter of 4 November 1580 (7.59–60) he tells Thadaeus Hagecius of a restoration of the motion of the Sun, which he investigated in preceding years, so careful that it agrees with daily observations, as (Paul) Wittich often tested with me, from which the computation of Alfonso and Copernicus deviates sometimes by half a degree, sometimes by a little more. For the motion of the center of the eccentric of the Sun in its small circle is far different than our predecessors found, or even Copernicus himself established, so that the eccentricity of the Sun is now 2;5 parts (where the radius of the eccentric is 60), 0;13 greater than the opinion of Copernicus, but the apogee of the Sun is near Cancer 5°, far before (west of) the hypotheses of Copernicus.[3] For otherwise the solar appearances do not agree, as I have demonstrated from many observations and will soon, God willing, communicate to the learned. Tycho thus accepts Copernicus's hypothesis with variable eccentricity and apsidal line, but is attempting to correct the parameters, and the same is true of the variable precession. For I have also discovered, he reports, that the motion of the eighth sphere (of the fixed stars) is now so much faster than Copernicus established that the equinox has precessed about one-quarter degree faster (in the period since Copernicus), which, by observation of Spica made in the same way as Copernicus, I have observed and demonstrated many times.

In the *Progymnasmata*, Tycho establishes a solar theory from his own observations in the years 1584–1588, which he considers more accurate than any earlier observations because he corrects for solar parallax, which he believes reaches $3'$ in the horizon, the standard value since Ptolemy, and refraction, reaching $34'$ in the horizon. In 1584 he found his definitive value of the obliquity, $23;31,30°$, which he continued to check in the following years. For his derivation of the solar eccentricity and direction of the apogee (2.19–23), he uses the two equinoxes and Taurus 15° or Leo 15°, following the method described by Regiomontanus in the *Epitome of the Almagest* 3.14. Copernicus had done something like this in *De revolutionibus* 3.16 using Scorpio 15°, which, Tycho points out, led to errors due to neglect of refraction at a low altitude although he believes that Copernicus did correct for parallax. From two derivations for 1588, he finds that where $R = 100{,}000$, $e = 3584$, or where $R = 60$, $e \approx 2;9$, $c_{\mathrm{m}} \approx 2;3,15°$, and $\lambda_{\mathrm{A}} =$ Cancer 5;30°, which he says are confirmed by yet other derivations. But he does not believe the parameters are permanent, indeed, with Copernicus, he had reason to believe that the solar eccentricity decreased and the longitude of the apogee increased since antiquity, as he explains (2.28). Hipparchus and Ptolemy found by observation at their times $\lambda_{\mathrm{A}} =$ Gemini 5;30° and $e = 415$ where $R = 10{,}000$, so $c_{\mathrm{m}} = 2;23°$, and since Ptolemy found these again in the same way as Hipparchus, before him by an interval of 260 years, he believed the apogee entirely immovable and the eccentricity to remain for ever the same. It may, however, be suspected that some error is concealed in the observations of both or at least one of them, which could easily happen in so sensitive an undertaking, especially because they began their demonstration in this investigation through equinoxes combined with transits of the solstice, which are observed with great difficulty. And it is likely that Ptolemy, because he did not find so great a difference, did not wish to disagree with the records of Hipparchus, but instead assigned to his own age the same eccentricity of the Sun and the same apogee, affirming too confidently for this reason that both are immovable. He goes on to review briefly, following the *Epitome of the Almagest* and *De revolutionibus*, the solar theories of al-Battānī and az-Zarqāl, and then carries out a detailed analysis of the errors in Copernicus's solar theory because Copernicus found a smaller eccentricity and a more advanced apsidal line for 1515 than he found for 1588, contradicting in a mere 73 years the record of nearly 1450 years since Ptolemy. Thus, Tycho still believes that a notable variation of the eccentricity and advance of the apsidal line have occurred, that the theory of Hipparchus and Ptolemy, although open to question, must still be taken seriously, but that Copernicus's own hypothesis and parameters are incorrect.

And there is more, for Tycho also believes, with Copernicus, that the length of the tropical year has varied from antiquity to his own time, as evidence for which he presents the following comparisons in days and hours to which we have added sexagesimal fractions of days (2.33):

From Hipparchus to Ptolemy	365^{d}	$5;55,12^{h}$	$365;14,48^{d}$
From Ptolemy to al-Battānī	365	5;46,20	365;14,26
From al-Battānī to our observations	365	5;49,29	365;14,34
From Ptolemy to our observations	365	5;47,52	365;14,29,40

That the year is nonuniform and has become shorter since antiquity is apparent, although it is also apparent that the values cited here are not consistent and something is wrong, as Tycho recognizes. What could cause such a variation? Tycho explains that the inequality of the tropical year is the result of the variation of the apogee and eccentricity of the Sun producing a motion of the ecliptic, on account of which the equinoctial points recede along the equator with respect to the fixed stars. Thus, the inequality of the year is the result of inequalities in the motion of the Sun affecting the location of the equinoxes, the precession of the equinoxes is a part of solar theory, and there is no motion of the sphere of the fixed stars, which he considers at rest except for the diurnal rotation of the heavens. This is a difficult subject, the interpretation and cause of precession, the theory of which Tycho never fully worked out although he later suggested something like his model for the regression of the nodes in the lunar latitude theory, and we shall return to it below.

Also with Copernicus, Tycho believes the sidereal year invariable, and this is of some interest as it is in finding the length of the sidereal year that he makes the most direct use of Ptolemy's observations and theory (2.33–37). That earlier values differ, he says, is because of errors in observation, failure to take account of solar parallax and refraction, insufficiently accurate locations of fixed stars, or from all of these causes coming together, as could easily happen in so sensitive an investigation. Of earlier values, he cites, from the Latin version of al-Battānī, "the most ancient Egyptians and Babylonians," $365\frac{1}{4} + \frac{1}{131}^{d} = 365;15,27,30^{d} = 365^{d}\ 6;11^{h}$; Thābit ibn Qurra, $365;15,23^{d} = 365^{d}\ 6;9,12^{h}$; and Copernicus, $365;15,24,10^{d} = 365^{d}\ 6;9,40^{h}$. Then, in order that we may find the length of the sidereal year more accurately, we have carefully compared Ptolemy's observations of the Sun and fixed stars with our own, for I am convinced that his observations are more accurate and secure than those of Hipparchus. (Delambre calls this a "choix singulier.") What Tycho does is use Ptolemy's solar theory and tropical longitude of fixed stars as correct for Ptolemy's time, and his own solar theory and longitude of fixed stars as correct for his own time, to find the sidereal longitude of the Sun at each time. He also assumes that Ptolemy's rate of precession, $36''$ per year, is correct for Ptolemy's time and his own rate, $51''$ per year, not yet set out, is correct for his own time. And like Copernicus, he takes the longitude of the first star of Aries in Ptolemy's catalogue, γ Arietis, as the measure of precession.

Thus, at Ptolemy's autumnal equinox (11) of 25 September 132 at 2^{h} after noon in Alexandria, the true longitude of the Sun $\lambda_s = 180^\circ$ and the mean longitude $\bar{\lambda}_s = 180^\circ + 2;10^\circ = 182;10^\circ$. Taking Ptolemy's longitude of Regulus on 23 February 139 of $122;30^\circ$ and the interval to γ Arietis of $-115;50^\circ$, the longitude of γ Arietis is 6;40°, as in Ptolemy's star catalogue. Since for 6 years and 7 months (corr. 5 months) earlier $\Delta\pi = -4'$, $\pi = 6;36°$ and the mean sidereal longitude of the Sun $\bar{\lambda}_s^* = 182;10° - 6;36° = 175;34°$. At Tycho's autumnal equinox of 12 September 1588 at 15;15^h after noon (13 Sep 3;15 AM) at Uraniborg, $\lambda_s = 180°$ and $\bar{\lambda}_s = 180° + 2;2\frac{1}{2}° = 182;2,30°$. From our observations, he says, at this time the precession of the equinoxes $\pi = 28;5\frac{1}{2}°$—as we shall show in the following chapter from accurate observations made earlier—so the mean sidereal longitude of the Sun $\bar{\lambda}_s^* = 182;2,30° - 28;5,30° = 153;57°$.[4] The difference in sidereal longitude $\Delta\bar{\lambda}_s^* = 153;57° - 175;34° = -21;37° = +338;23°$. Next, in his list of longitudes and latitudes of places (5.309–10), the difference in longitude of Alexandria and Uraniborg is $60;30° - 36;45° = 23;45° = 1;35^h$ (corr. $29;55° - 12;42° = 17;13° = 1;9^h$). Hence, at the meridian of Uraniborg, the time of Ptolemy's equinox is $2^h - 1;35^h = 0;25^h$ after noon. Now, between the two autumnal equinoxes, including complete revolutions and years, the difference of mean sidereal longitude $\Delta\bar{\lambda}_s^* = 1455^r + 338;23°$ and the difference of time in Julian years $\Delta t = 1455^{jy} + 355^d\ 14;50^h$. Thus, the mean sidereal motion of the Sun $\bar{v}_s^*$ and the length of the sidereal year sy are

$$\bar{v}_s^* = \frac{524138;23°}{531791;37,5^d} = 0;59,8,11,27,14,26,54^{°/d},$$
$$sy = 360°/\bar{v}_s^* = 365^d\ 6;9,26,43\tfrac{1}{2}^h.$$

The correct length of the sidereal year is $365^d\ 6;9,10^h$, about 17″ less, which accumulates to 1^h in 212 years and nearly 7^h in the 1456 years since Ptolemy's equinox. The principal cause of the difference is an error of about -30^h in Δt, from Ptolemy's equinox, which is 33^h late, compensated slightly by Tycho's, about 3^h late.

Tycho does better with the tropical year, for which his goal is more modest but the required work greater (2.37–45). He explains that he does not here attempt a complete restitution of the solar motion for all ages, which he decided to reserve for his complete work of restored astronomy, but only as suffices for the nearest periods, within 300 or 400 years, for in that time an inequality in the tropical year that disturbs what we propose to do cannot occur. Therefore, instead of using the sidereal year and separating out the precession of the equinoxes, which would here be very lengthy (because over long periods the precession is variable), we shall instead be satisfied with the equinoctial or tropical year confirmed for this very period. We shall investigate this from observations of meridian altitudes of the Sun a hundred years ago in Nuremberg by the learned Bernhard Walther, of lasting memory and especially worthy of praise, the distinguished student of Regiomontanus. What he then does is derive the parameters of solar theory for the year 1488 using Walther's observations of chords of meridian zenith distances of the Sun to locate the Sun at the equinoxes and at Taurus 15° and Leo 15°, and from two derivations settles on $e = 0.035481$, $c_m = 2;2°$, and $\lambda_A =$ Cancer 4;15°. Note that e and c_m are slightly smaller than Tycho's for 1588, and he also finds an obliquity of 23;31°, slightly less than his own 23;31,30°. He takes these differences seriously and remarks that it seems consistent that from that time the obliquity of the ecliptic has increased slightly, because meanwhile the eccentricity of the Sun has also increased somewhat, and not (as the Copernican reasoning erroneously maintains) decreased. Then, from the difference in time between the equinoxes, both vernal and autumnal, of 1488 and 1588—with small errors in the differences in longitude of Nuremberg and Uraniborg and in the mean longitudes of the Sun—he derives for the length of the tropical year 365^{d} $5;48,45^{\mathrm{h}}$ exactly, an excellent value. The mean daily motion is computed to no less than seven places, of which the first four are $0;59,8,19,49^{\circ/\mathrm{d}}$. The epoch for noon of 1 Jan 1588 is 290;4,50°.

Since Tycho considers the tropical year variable over longer periods, the mean motion is intended for a limited time, and he tabulates epochs only for the period 1400–1800, that is, 1600 ± 200 years. The rate of precession is later found (2.253) from the difference between the sidereal and tropical year, $365^{\mathrm{d}}\ 6;9,27^{\mathrm{h}} - 365^{\mathrm{d}}\ 5;48,45^{\mathrm{h}} = 0;20,42^{\mathrm{h}}$. Since the Sun moves about $0;2,28^{\circ/\mathrm{h}}$, the precession $\pi = 0;2,28^{\circ/\mathrm{h}} \cdot 0;20,42^{\mathrm{h}} \approx 51''$ per year, 1° in 70 years and 7 months, which will be confirmed for longer periods from observations of fixed stars. It was found that in 1488 $\lambda_A = 94;15°$ and in 1588 $\lambda_A = 95;30°$, a change of 1;15° in 100 years, from which the motion of the apogee is 45″ per year. Since the precession is 51″ per year, the sidereal motion of the solar apogee is −6″ per year, that is, retrograde, which Tycho does not mention. But since it may not be uniform over longer periods, perhaps at some other time it is direct. This then is the solar theory Tycho established for his own time and two centuries before and after. Although doubts have been raised about Ptolemy's solar observations and theory and observations of fixed stars, they have not been rejected, but in fact accepted for the determination of the sidereal year.

Tycho has more doubts about the observations of fixed stars used to confirm the rate of precession. He has no confidence in any earlier determination of precession: Ptolemy's 1° in 100 years is too slow, al-Battānī's 1° in 66 years is too fast, and Copernicus's variable precession is defective, as we shall see below. Nor does he consider earlier coordinates of stars reliable, although he does use some to confirm his own rate of precession. And he believes that Ptolemy's catalogue of stars is that of Hipparchus corrected for precession (2.151). "After these (Timocharis and Hipparchus), Claudius Ptolemy also, about the year 140 after the birth of Christ, and at Alexandria in Egypt, attempted to observe and commit to writing some amount in the advancement of these (stars, *nonnulla in harum progressione*), yet concerning the placement of them with respect to each other in longitude and latitude completely preserving the Hipparchan table." And the same is true of the catalogues of Battānī, Alfonso, and Copernicus, so in this sense, there has been only one star catalogue, that of Hipparchus, successively adjusted for precession.

We have seen that Tycho accepts long-period variation of parameters of solar theory, the eccentricity, direction of the apogee, length of the tropical year, and also, as we shall see, the obliquity of the ecliptic and the precession of the equinoxes, to which the variation in the length of the tropical year is related. Several times he states that the parameters derived here are only for the closest periods, and he also says that their definitive examination for all ages is deferred to his complete restoration of astronomy, which he never wrote. Although he admits the possibility of smaller errors, nowhere does he say that Hipparchus and Ptolemy were absolutely wrong about the eccentricity of the Sun, the direction of the apsidal line, the length of the tropical year, and the obliquity of the ecliptic, which thus have changed notably since antiquity. But the most important problem is the precession of the equinoxes or, as Tycho prefers, the (apparent) motion of the fixed stars: is it uniform or nonuniform over long periods and what is its cause? We begin with Tycho's treatment in the *Progymnasmata*, which comes after the establishment of locations of fundamental stars for the star catalogue and the explanation of how locations of other stars are found. The section is called "On the proper motion of the fixed stars corresponding to this age" (2.253–57). He begins with the derivation of the rate of precession from the difference of the sidereal and tropical years we have just shown. The length of the sidereal year is 365^{d} $6;9,26,43^{h}$, the length of the tropical year "in this age" is 365^{d} $5;48,45^{h}$, less than the sidereal year by about $0;20,42^{h}$. In so much time the Sun, after traversing an entire circle, again overtakes a fixed star which has advanced slightly, meanwhile passing over exactly 51″ in its motion, and therefore such a small amount is the annual advancement of the fixed stars "in our age."

He then sets out confirmations of this rate using pairs of locations of Spica and Regulus from observations of his own, Copernicus, Battānī, Ptolemy, Hipparchus, and Timocharis. We summarize these in the following table giving the observers, star, earlier and later longitudes λ_1 and λ_2, difference in longitude $\Delta\lambda = \lambda_2 - \lambda_1$, difference in time Δt in years, and the annual rate of precession $\pi = \Delta\lambda/\Delta t$ computed by Tycho.

Observers	*Star*	λ_1	λ_2	$\Delta\lambda$	Δt	π
Cop.-Tycho	Spica	197; 3,30°	198;3°	0;59,30°	70^{y}	$0;0,51^{\circ/y}$
Hip.-Tycho	Regulus	119;50	144;5	24;15	1713	0;0,50,59,47
Hip.-Bat.	Regulus	119;50	134;5	14;15	1006	0;0,51
Bat.-Tycho	Regulus	134; 5	144;5	10; 0	705	0;0,51,4
Tim.-Tycho	Spica	172;20	198;3	25;43	1879	0;0,49,15
Ptol.-Tycho	Spica	176;40	198;3	22;23	1446	0;0,53,15

The results are not quite straightforward and most of the values of π have small errors of little consequence.[5] To explain the discrepancies of about ±2″ in the comparisons with Timocharis and Ptolemy, he notes that the comparison with Hipparchus in between them is correct, which is confirmed by al-Battānī, that the mean of their values is about 51″, and that their observations are not sufficiently accurate for this purpose. For this reason, it is useless to give direct comparisons between Timocharis, Hipparchus, and Ptolemy, which would be close to Ptolemy's 36″ per year. Hence, it appears that 51″ per year is confirmed for nearly 1900 years. But Tycho is more cautious, for he writes that assuming that the annual motion of the fixed stars is exactly 51″, in no way shall we depart from the required goal in any experiences that can occur in the nearest three or four centuries (as concerning more I shall not speak). Like the epochs of the Sun's mean motion, the epochs of the motion of the fixed stars are tabulated only for 1400–1800, as they are in the *Restoration of the Fixed Stars* with the catalogue of 1,000 stars completed in 1598 (3.343,374). He goes on to say, and this seems to be his main point, that Copernicus's theory of the inequality of the precession of the equinoxes, to reconcile and preserve all the discoveries of his predecessors, is in no way correct, as in the motion of the seventy years from his first observation of Spica, which is much faster than he believed it would be, not one degree in about one hundred years but in seventy, and in the length of the tropical year, which is not as long as he believed, for according to Copernicus the two are connected such that the motion of the fixed stars is slowest when the year is longest. But the accurate observations of recent years refute this since they do not correspond in their periodic returns, meaning that the precession is not as slow or the tropical year as long as in Copernicus's theory.[6] He concludes (2.255–56):

> It is not now our intention to set out the universal motion of the eighth sphere (as it is called) and also corresponding to all periods in the age of the world, so that the inequality discovered by first some and then other practitioners will, as far as possible, be justified, leaving aside the undertaking of such labor to a special restoring of astronomy. Nevertheless, convinced in this matter by good reasons, I do not hesitate to affirm that so immense an anomaly is hardly concealed in the motion of the fixed stars as is come upon from the observations of Timocharis and Ptolemy compared with Hipparchus and al-Battānī. For it is not likely that sometimes they pass over 1° in 100 years, as Ptolemy reckoned, but sometimes in 66 years, as al-Battānī believed, but rather without doubt some error has escaped detection in the actual observations of the practitioners, which appears clearly enough from the fact that the longitudes of the very stars they report specifically to have observed are not distant from each other in heaven itself by the amount their record claims, so much so that a deviation from the arrangement of heaven is found of a third and even half a degree, which will be clear to anyone by comparing our intervals of longitude with their records regarding the same stars. We also see how little of more refined accuracy the moderns have shown in these matters, as is clear from the published observations of Regiomontanus and his student Bernhard Walther, and of Werner. Nevertheless, I shall not suppose that the observations of the ancients of the fixed stars were so erroneous that it cannot be gathered from them that some kind of inequality of motion is concealed in them, although I believe this takes place from some external cause and indirectly, and with good reason is not to be attributed to the stars themselves. Still, it is not yet suitable to make known a final judgment on this matter, considering more deliberately to reserve it to the comprehensive study of astronomy to be published in several years.

Although the comprehensive study of astronomy was never written, Tycho does say more on the question in his correspondence with Joseph Scaliger. The correspondence, of considerable interest on both sides, has been treated in detail by Anthony Grafton (1993) concerning Tycho's correction, or attempted correction, of Scaliger's notions about the sidereal year and precession, and our own examination of this curious subject owes much to Grafton's. Tycho knew Scaliger's work well. In 1584 he asked his friend Heinrich Brucaeus, Professor of Medicine at Rostock, for a copy of the recently published *De emendatione temporum*, the first of Scaliger's two great works on chronology, which Brucaeus promptly sent. In 1595, through his former assistant Johann Isaac Pontanus, then in Amsterdam, he sent Scaliger in Leiden several printed quaternions of the solar theory in the *Progymnasmata* so that he could compare his equinoxes with ancient equinoxes to find a more correct measure of the relation of the tropical and Julian year. He hoped that in this way the length of the tropical year, which he had established for the recent period from Walther's observations, could be found more accurately by extending the interval back to antiquity and in so doing refute Copernicus's theory of the variation of the tropical year and precession, which he says is not as great or as important as astronomers suspect (7.373–74). He seemed to think that Scaliger had original reports of ancient observations of equinoxes, by Hipparchus in particular, other than the citations in the *Almagest*, which of course he did not. In a letter of 14 March 1598 written from Wandesburg (8.31–33), he asks Scaliger to send him all Hipparchus's observations he has of vernal and autumnal equinoxes, perhaps from the *Commentary on Aratus* which contains no such equinoxes, set out in a table so that he could compare them with his equinoxes; if he has other very old observations of equinoxes, he would wish them, and also the most ancient epoch of the Jews, when it is believed the equinox took place on 21 April at 6 hours after noon. This would have been about 3800 BC, close to the date of Creation. Scaliger included Tycho's equinoxes in the second edition of *De emendatione temporum* (1598), and concluded from a comparison of Hipparchus's and Tycho's equinoxes that the Alfonsine tropical year of 365^{d} $5;49,16^{h}$ is correct and preferable to the year of "Gelalaeus."[7]

Now on 9 July 1598 Scaliger sent Tycho the second edition of *De emendatione temporum* with a letter setting out his ideas about the tropical and sidereal years and the precession (8.83–87). He believes that the sidereal year is not longer than, but equal to, the Julian year, because the same star always rises in the evening and sets in the morning on the same Julian date, which in truth the judgment of the Egyptians that decrees that Sirius always rises on the same Julian date proved to us, the evidence for which is that what we call the Julian year the Egyptians called "Canicular" because for more than 1500 years Sirius (Canicula) rose on the same date of the Julian year. This observation, as I hope, he tells Tycho, will not be unwelcome to you. He did not reach this conclusion from a record of Egyptian observations of the rising of Sirius, which does not exist, but, it appears, by interpreting the Sothic Cycle, 1461 Egyptian years = 1460 Julian years, in which 1 Thoth in the Egyptian calendar returns to the same date in the Julian calendar, plus an additional 44 Julian years for the effect of the precession of the equinoxes, advancing the equinox by 11 days in 1460 Julian years, as a period of more than 1500 Julian years in which Sirius rose on the same date.[8] Further, he continues, there is no trepidation nor motion of the eighth sphere, of the fixed stars, from west to east because it is the equinoctial points in the ecliptic that move from east to west, for the equinoctial circles (of the equator) are described as a consequence of them. These points are surely movable, and therefore the circles described as a consequence of their motion are movable and consequently the pole of the equator is movable. And thus in the time of Hipparchus, the pole of the equator was distant from the tail of Cynosura (Polaris) by 12;24°; now it is distant by less than 3°.[9]

Scaliger's theory is this: the pole of the world, meaning of the sphere of the fixed stars, passes through the Pole Star itself or is not far removed from it, and the pole of the world, the arctic and antarctic circles, and the fixed stars do not move at all—aside from the daily rotation—there is no motion of the eighth sphere. Instead, the pole of the equator is movable, and has never been the pole of the world although at some time it will be as it is approaching closer to the Pole Star. As a consequence of the motion of the pole of the equator, the equator moves along the ecliptic and the tropic circles also move parallel to the equator—these circles are not parallel to the arctic and antarctic circles—and it is this motion that produces the precession of the equinoxes and solstices. Just how Scaliger came up with this explanation of precession, which he regarded as eliminating the motion of the fixed stars, is not certain. He was no Copernican in the sense of holding the heliocentric theory and the motion of the Earth, but it may have been an attempt to adapt Copernicus's theory of precession, which is a motion of the equator along the ecliptic while the fixed stars and the ecliptic do not move, to an unmoving central Earth and unmoving sphere of the fixed stars, although without Copernicus's inequalities which Scaliger definitely rejects.

Tycho wrote a long, detailed, and patient answer from Wandesburg between 17 and 23 August 1598 (8.100–09). He had his work cut out for him. He says he cannot support Scaliger in his proposal concerning the equator and its movable poles and that they differ from the poles of the world as his experience from instruments is otherwise (8.102–02).

> For I have found from the change in latitude of fixed stars in accordance with the proportion of the change in the obliquity of the ecliptic from the times of Timocharis, Hipparchus, and Ptolemy up to the present (if only what they observed in the angle of the maximum obliquity and the rest are free of any error, concerning which, not without reasons I am in doubt) that it is the ecliptic that is unstable rather than the equator with its poles, the Sun not always describing the same ecliptic through a great interval of centuries, and at the same time successively anticipating the places at which it crosses the equator. Hence, it happens that the fixed stars appear to progress as much as the Sun returns earlier to these points. And since what fits the deficit of the tropical year from the Julian year is clearly not equal to that motion, it is not possible that the fixed stars rise or set with the Sun on the same days of the Julian year through intervals of several centuries, and likewise from other

concurrent causes which will give rise to a discrepancy. And although it (the rising of stars) can somehow coincide (with the same days of the Julian year) for some few stars for a long interval of years, nevertheless not always or for all stars. Also, there is no difference at all between the pole of the equator and of the world, as you infer both in your book and here, for they are one and the same. And the last star in the tail of Ursa Minor, called Polaris because it is near the pole, is not the pole of the world or the closest to it unless you understand that to mean the pole of the equator, which, as I said, does not differ from the pole of the world. This star was distant from the pole of the equator by $12\frac{2}{5}^{\circ}$ in the time of Hipparchus, but in this year according to our discoveries it has approached it within $2;51\frac{1}{2}^{\circ}$, as 25 years earlier we found it removed from the pole by precisely 3° with a quadrant 14 cubits in radius in the garden near the estate of Councillor Heinzel in Augsburg. The approach to the pole of the equator or of the world takes place, not because this star is the pole or near (the pole) of any sphere, but through its change of longitude about the poles of the ecliptic, by which its declination increases, since it is now near the end of Gemini but at the time of Hipparchus was near the end of Taurus, in the intervening time having covered a little more than 24° in longitude, but in latitude altered not more than the decrease of the obliquity of the ecliptic produces, through a third part of a degree (if it is even that much), for the (latitude) which the table of Ptolemy places at 66° exactly is approximately confirmed. And if this star is referred to the equator in our own age, it will not fall in the equinoctial colure, as perhaps you believe, but will be removed from it by $5\frac{3}{4}^{\circ}$ of the equator, as has itself been demonstrated by certain experience. But it can never be exactly united with the pole of the equator, for after about 500 years, when the beginning of Cancer reaches the solstitial colure, it will be distant from the pole toward the equator or ecliptic by $27\frac{1}{2}'$. For although the inclination of the ecliptic will perhaps then be increased a little (which, however, I scarcely think will come about), yet this will alter only the latitude of the star and not on account of that move it closer to the pole, as the stars definitely look to the fixed poles of the ecliptic while the Sun describes somewhat movable poles through the ages (i.e. a movable ecliptic with movable poles), in so far as the records of the ancients are worthy to be trusted.

Tycho goes on to explain that the heliacal rising of Sirius changed, according to his computation, by only one day in the Julian year for 1500 years before Ptolemy, which would have been difficult to detect, not because the sidereal year is equal to the Julian year, but by chance in that particular star because in the interval of so many centuries its declination changed by $2\frac{2}{3}^{\circ}$ such that this alteration of declination corrects and nearly eliminates the change that could occur from the difference between the Julian and sidereal year. This explanation, which Tycho was surely the first to formulate, is correct.[10] But our concern here is not so much Tycho's correction of Scaliger, as his own ideas concerning the precession, which he says is a result, not of the motion of the fixed stars, but of the ecliptic. He enlarges on this in correcting Scaliger's dismissal of the sidereal year and its relation to precession (8.103–04).

> The calculation of the sidereal year, introduced by the Babylonians and Egyptians and after that improved by Thābit, but restored in our age by Copernicus, is not so empty and useless as you think, if only it is determined exactly. We have corrected it still more accurately in our *Progymnasmata astronomica* in so far as Ptolemaic observations are compared with our own. But when through you I receive the Hipparchan observations in some quantity, I will examine this more precisely. For with you, I also consider it preferable to depend upon Hipparchus than Ptolemy. However, I believe no less than Copernicus that with respect to themselves the fixed stars remain forever unmoved. But I do not allow that progression, which they appear to make, through the precession and libration of the axis of the Earth, as he (Copernicus) preferred, since in truth nothing of the kind is suitable to the Earth. But if the reports of the ancients are worthy to be considered in every way certain, it will be very likely that the Sun itself describes one and another ecliptic in different ages. And however small the inequality concealed in it could be, in so far as it will be permitted to explain from past observations of the practitioners, we shall, God willing, save it through the universal hypothesis of the Sun. And the calculation of the sidereal year will also be of use for this purpose, as also for finding the simple motion of the planets from a fixed and immovable point and establishing it more accurately than up to now.

The "Hipparchan observations" refer to the original reports or additional observations, especially of equinoxes, Tycho requested earlier—the next year Scaliger sent him a copy of the *Commentary on Aratus*, in which he would have seen that it contained nothing of the kind—and again he asks for the most ancient equinox of the Jews. The meaning of the sentence about believing with Copernicus that the fixed stars are forever unmoved is, not that they do not move among themselves, which everyone believes, but that the fixed stars as a whole, the sphere of the fixed stars itself, is unmoved, as Copernicus alone believed and as Scaliger and Tycho now also believe, which is confirmed by the statement that the simple (sidereal) motion of the planets be found from a fixed, immovable point, as Copernicus also held. The only motion of the sphere of the fixed stars is the diurnal rotation about the pole of the equator, which Tycho considers the pole of the world and absolutely fixed. Instead, the precession is due, not to the motion of the stars, but to the motion of the Sun, describing different ecliptics in different ages, possibly with a small inequality. This agrees with his statement that the Sun does not always describe the same ecliptic through a great interval of centuries, and at the same time successively anticipates the places at which it crosses the equator, so that the fixed stars appear to progress as much as the Sun returns earlier to these points.

The discussion, the issue, between Tycho and Scaliger is this: Both believe with Copernicus that the fixed stars do not move at all, that there is no motion of the eighth sphere, although they also believe, differing from Copernicus, that the diurnal rotation is of the entire universe, including the sphere of the fixed stars, about an unmoving, central Earth. But again with Copernicus both believe there is a precession of the equinoxes with respect to the fixed stars which is not caused by any motion of the stars. Scaliger believes the precession is caused by a motion of the pole of the equator with respect to the pole of the world, of the sphere of the fixed stars, at or very near Polaris, shifting the equator and thus the intersections of the equator with the ecliptic, the equinoxes, with respect to the stars and the ecliptic, which is also fixed with respect to the stars. This appears to be an adaptation to a fixed, central Earth of Copernicus's theory, in which the equator moves with respect to the ecliptic and the fixed stars, although without the inequality in the motion of the equinoxes. Scaliger also believes that the sidereal year is equal to the Julian year, as shown by the constant Julian date of the rising of Sirius, and those who say it is longer are simply wrong. Tycho believes instead that the pole of the equator is the fixed pole of the world and that the precession is the result of the motion of the Sun, describing successively different ecliptics, that is, an ecliptic that moves, rotates, along the equator so that the Sun crosses the equator at successively different points, causing the equinoxes to precess with respect to the fixed stars and fixed equator. There is probably also some small inequality concealed in this motion of the equinoxes, which accounts for the variation of the length of the tropical year, but the sidereal year, the Sun's return with respect to the fixed stars, is constant, as the fixed stars do not move, and is longer than a Julian year. The nearly fixed Julian date of the rising of Sirius is fortuitous, because the star's change in declination nearly compensates the difference between the sidereal and Julian year, which is not true of most stars. Tycho believes that he has established the length of the constant sidereal year correctly for all times and the length of the variable tropical year for 100 years since Walther and probably for 200 years before and after his own time; more than that and a complete explanation of the precession is reserved for his universal hypothesis of the Sun.

Scaliger was not convinced by Tycho's arguments, indeed, he became more certain than ever that he, the philologist with a profound knowledge of antiquity, was correct, and Tycho, the astronomer, and all other astronomers, who know nothing of antiquity, were wrong. He was writing a *Diatribe on the precession of the equinoxes* (*De aequinoctiorum anticipatione diatriba*), completed in 1601 but probably never seen by Tycho, and only published posthumously in 1613.[11] On (NS) 10 March 1600, he wrote to Tycho in Wittenberg (8.261–64), referring to a letter he had received recently, and summarizing what he planned to write in his *Diatribe*. The word "diatribe" then meant a critical dissertation, not necessarily an invective, although that is hardly lacking in Scaliger's *Diatribe* or his letter to Tycho (8.262–63).

> For I intend to send you my diatribe on the precession of the equinoxes and refutation of the motion of the eighth sphere, in which we have both diligently assembled innumerable testimonies of the ancients and shall demonstrate by five clear testimonies of the most ancient authors that the star called Polaris has remained for 1966 years in the place where it is today. Further, we shall adduce so many incongruities and absurdities which follow from the

> hypothesis of the motion of the eighth sphere that there will be no one except the ignorant or envious who will dare speak to the contrary. I add also, because it has thus far been entirely unknown, that the precession of the equinoxes had been accepted from Thales and Anaximander up to the time of Hipparchus, and that Hipparchus was the first of all to reject it, having introduced the eastward motion of the eighth sphere, so that against the evidence of sight, he recorded the last star in the tail of Cynosura, or Polaris as it is commonly called, to be the most southern of all seven stars in that constellation, which nevertheless was before him, and is today, and will always be the most northern of all. See how the authority of such a man has misled posterity! For from him up to the present day, you believe such things, and for no reason other than that "the Master spoke" (αὐτὸς ἔφα), which should never have a place in mathematics. For to no sort of men has more harm been done by ignorance of antiquity than to the race of astrologers. Nothing occurs to me to be more surprising than that not one astrologer has had even a clue of the error of that hypothesis and how many and great are the absurdities necessarily born of it, if you except only Copernicus, who also recognized the precession of the equinoxes and the obliquity of the equinoctial circle (the equator), but through ignorance of antiquity took refuge in absurd hypotheses. In fact, the regular and uniform decrease of the maximum declination of the Sun necessarily follows from the precession of the equinoxes alone, which we have demonstrated completely, for otherwise it is not possible except by false hypotheses. Therefore it follows that the pole of the world differs from the pole of the equator, and that the meridian lines move and do not always remain in the same place, which we shall demonstrate perfectly from ancient authors.

Scaliger's history of precession may seem bizarre, but far more preposterous things have been written in our own time; indeed, precession always seems to inspire both learned and ignorant nonsense. The period of 1966 years during which the Pole Star has been in the same place is since Eudoxus as cited by Hipparchus, critically in fact, although Scaliger considers Eudoxus, with the likes of Thales and Anaximander, preferable to Hipparchus, the originator of the false understanding of precession as a motion of the fixed stars. This curious history, and there is far more of it in the *Diatribe*, has been treated at length by Grafton. It is clear that Tycho's attempt at correction had no effect, for Scaliger has changed his mind on nothing, and is certain that the testimony of some ancients correctly understood is of greater value to understanding precession than a sound knowledge of astronomy. Yet it can be said in Scaliger's defense that in the basic principle of moving the equator with respect to a fixed ecliptic and unmoving sphere of the fixed stars, in which he follows Copernicus, he is doing the right thing. Tycho answered in a letter written from Prague on (NS) 23 July 1600 (8.328–29), in which it appears that the great astronomer is not doing the right thing; the essential part is this:

> I eagerly await your thoughts, which you promised, about saving the equinoxes and the motion of the eighth sphere in another way. I readily grant you that

> the Pole Star, just as all the other stars of the eighth sphere (as it is called), remains in the same place in heaven, if only you acknowledge in this star, as in the others, changes in declination and right ascension, as well as in longitude and even some change in latitude. I also agree with you that this will take place, not by the advance of the eighth sphere, but by the precession of the equinoxes, as the great Copernicus likewise seems to have apprehended. But the critical point in this matter turns upon how this precession is to be understood and accomplished. That it takes place, as Copernicus theorized, through a motion of the axis of the Earth, reciprocated and librated and not entirely coincident with the annual revolution, is in error; rather, the assumption is utterly absurd and does not satisfy the appearances in this age, much less in all other ages. I am convinced that the Sun itself causes this variation as it describes one and another ecliptic in different ages, and moreover draws the intersections of the ecliptic with the equator backwards, and in fact not at all uniformly, as I intend to show more fully, God willing, in its proper place. For I have discovered that the lowest Moon also varies its orbit in single months in a way not much different such that, not only does its maximum latitude vary up to a third part of a degree (in fact just as much as the difference thus far discovered in the obliquity of the ecliptic), but I also learned that the nodes and intersections with the ecliptic, although they move westward with a uniform motion, yet this takes place reciprocally and by a nonuniform quantity and a fairly notable difference which can reach $1\frac{3}{4}^{\circ}$, as will be explained more completely, God willing, in publishing before long the restoration of the lunar motion in our *Progymnasmata*. If by chance there has become known to you a way different from ours by which these things can be explained properly, and it can be ascertained from ancient records and certain observations, I wish you to impart it to me. For the present, the matter is as I say, that I cannot comprehend what you have made known both in your letter and elsewhere: that the pole of the world is undoubtedly different from the pole of the equator and that meridian lines move. For it appears not quite suitable, unless perhaps I do not yet understand your meaning, which is rather obscure, so that concerning this matter I wish to be more fully instructed by you.

Tycho's answer is a lesson in gentle irony to a vain man who has nothing but abuse for those who do not submit to his teachings and acknowledge his genius. Since Scaliger's diatribe on precession has been considered by Grafton, we shall go on to our principal subject, Tycho's own explanation of precession in this letter, which appears to be the most complete statement of what he had in mind. The essential clue is the comparison to the model for lunar latitude, in which the inclination of the lunar orbit to the ecliptic is variable and the regression of the nodes along the ecliptic nonuniform. This model is to be transferred to the precession of the equinoxes, which, as we shall see, leads to problems.

Scaliger's and Tycho's hypotheses for precession are shown in Fig. 1. We are not concerned with Scaliger's ideas that the sidereal year is equal to the Julian year and that the pole of the equator will eventually reach Polaris, only that a motion of

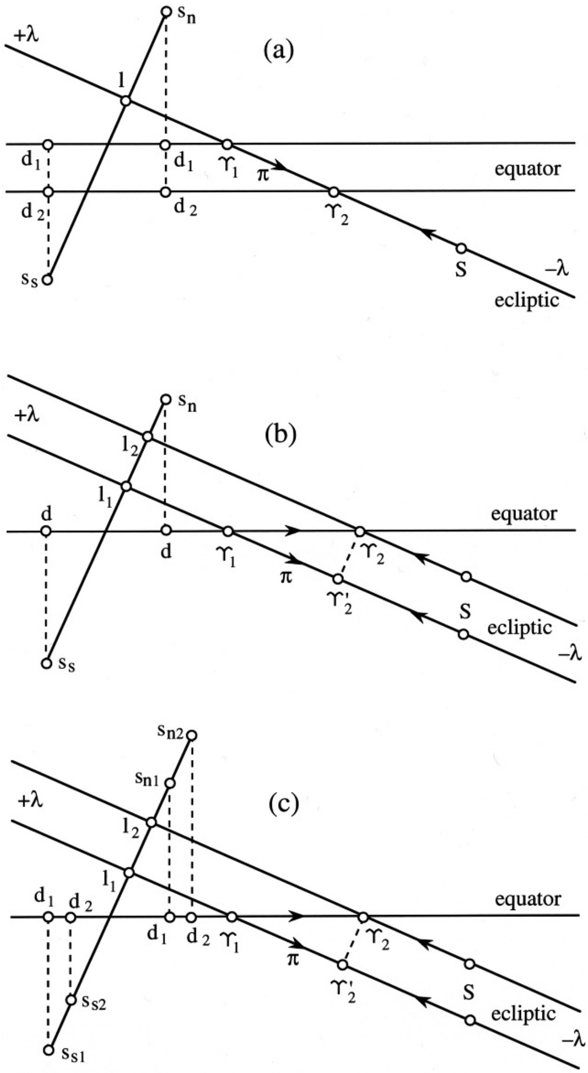

Fig. 1 Hypotheses for precession of the equinoxes according to (**a**) Scaliger, (**b**) Tycho, (**c**) correction of Tycho

the equator accounts for precession, which is shown in Fig. 1a. The Sun S moves on the ecliptic in the direction of increasing longitude $+\lambda$ and initially crosses the equator at the vernal equinox Υ_1; a northern star s_{n} and southern star s_{s} are shown with longitude $\lambda_1 = \Upsilon_1 l$, latitudes $\beta_{\mathrm{n}} = s_{\mathrm{n}} l$ and $\beta_{\mathrm{s}} = s_{\mathrm{s}} l$, and declinations $\delta_{\mathrm{n}} = s_{\mathrm{n}} d_1$ and $\delta_{\mathrm{s}} = s_{\mathrm{s}} d_1$. Now after some time the pole of the equator has moved, shifting the equator so that the Sun crosses it at the vernal equinox Υ_2, and the precession of the equinox along the ecliptic is $\pi = \Upsilon_1\Upsilon_2$ in the direction of decreasing longitude $-\lambda$.

The longitude of the stars has increased to $\lambda_2 = \Upsilon_2 l = \Upsilon_1 l + \pi$, the latitudes have remained unchanged, and the declinations have changed, s_n increasing to $\delta_\mathrm{n} = s_\mathrm{n} d_2$ and s_s decreasing to $\delta_\mathrm{s} = s_\mathrm{s} d_2$. And if the pole of the equator has moved closer to the pole of the ecliptic—this motion is not shown in the figure—the inclination of the equator to the ecliptic has decreased, which also affects the declination but not the latitude of stars. All of this is just as it should be, and is essentially Copernicus's model but without the inequality and without the motion of the Earth. But if the axis of the Earth does not move, how is the equator shifted? Scaliger seems to think that the equator and its poles are located on a sphere, which moves in relation to the unmoving sphere of the fixed stars. This does raise a problem. If the sphere is inside the sphere of the fixed stars, it may move in this way, but a sphere with identical equator and poles, and with the identical motion, must still be located outside the sphere of the fixed stars to produce the diurnal rotation of the heavens parallel to the equator. If the sphere is only outside the sphere of the fixed stars and also produces the diurnal rotation, then it is difficult to consider the poles of the sphere of the fixed stars as not moving with respect to the poles of this outer sphere moving the equator. There is nothing wrong with that, and it is a way of transferring Copernicus's model for precession to the heavens, but one can hardly then say that the sphere of the fixed stars is absolutely at rest, of course aside from the daily rotation, which strictly, or usually, is required to have yet another sphere of its own.

Tycho's model for lunar latitude produces both a variation of the inclination of the lunar orbit to the ecliptic and a nonuniform regression of the nodes, and he believes that both can be applied to the Sun to produce the variation of the obliquity of the ecliptic and the nonuniform precession of the equinoxes. Tycho's model for the precession is shown in Fig. 1b, in which the Sun S moves on the ecliptic, crossing the equator at Υ_1, and stars s_n and s_s have the longitude $\lambda_1 = \Upsilon_1 l_1$, latitudes $\beta_\mathrm{n} = s_\mathrm{n} l_1$ and $\beta_\mathrm{s} = s_\mathrm{s} l_1$, and declinations $\delta_\mathrm{n} = s_\mathrm{n} d_1$ and $\delta_\mathrm{s} = s_\mathrm{s} d_1$. The Sun, as Tycho says, "describes one and another ecliptic in different ages, and moreover draws the intersections of the ecliptic with the equator backwards, and in fact not at all uniformly," so that after some time the Sun crosses the equator at Υ_2, which is projected on to the previous position of the ecliptic at Υ_2' in the direction of decreasing longitude $-\lambda$ by the precession $\pi = \Upsilon_1 \Upsilon_2'$, which may be nonuniform, as the regression of the nodes in the lunar model, and thus the period of the Sun's return to the equinox, the tropical year, may be nonuniform. Along with this nonuniform motion of the equinoxes, as in the lunar model, there is a variation in the inclination of the ecliptic to the equator, that is, a variation of the obliquity, which is not shown in the figure. So far, so good, but when we consider the effect on stars, there are problems. If the ecliptic moves and the sphere of the fixed stars does not move, then the longitude of stars increases to $\lambda_2 = \Upsilon_2 l_2 = \Upsilon_1 l_1 + \pi$, which is correct. But the latitudes of stars also change, s_n reduced to $\beta_\mathrm{n} = s_\mathrm{n} l_2$ and s_s increased to $\beta_\mathrm{s} = s_\mathrm{s} l_2$, which is not correct and distinct from the change in latitude from the variation of the obliquity that Tycho has in mind, and the declinations do not change, which is also not correct. The solution to these difficulties, shown in Fig. 1c, is to make the fixed stars move with the ecliptic, so the latitudes $\beta_\mathrm{n} = s_{\mathrm{n}2} l_2 = s_{\mathrm{n}1} l_1$ and $\beta_\mathrm{s} = s_{\mathrm{s}2} l_2 = s_{\mathrm{s}1} l_1$ are unchanged, aside from the change produced by the variation of the obliquity, and the declinations are changed, from $\delta_n = s_{n1}d_1$ to $\delta_n = s_{n2}d_2$ and from $\delta_s = s_{s1}d_1$ to $\delta_s = s_{s2}d_2$. But that contradicts Tycho's belief that the fixed stars do not move and that the precession takes place, not by the advance of the eighth sphere, but by the precession of the equinoxes, which is the reason for applying his model for lunar latitude to the precession. Tycho would surely have discovered these difficulties had he worked out his model for the precession more carefully, but he did not do so, and all we have is this suggestion of applying the model for lunar latitude to account for a nonuniform precession and change of obliquity, which clearly fails.

Tycho's solar and precession theories, like so much he intended to do, were left unfinished. Both were established for about two hundred years before and after his own time, as shown by the tables in the *Progymnasmata,* and he clearly stated in that work and the letters to Scaliger that the consideration of long-period variations is deferred for his universal hypothesis of the Sun and complete restoration of astronomy. We have seen that in finding the length of the sidereal year, he applies Ptolemy's precession of 36″ per year to Ptolemy's observations even though in establishing his own rate of 51″ per year he shows that it applies not only to his own time, but is supported, more or less, by observations since antiquity. There would appear to be a contradiction, but Tycho does not see it that way, instead, perhaps, taking 51″ per year as close to a mean value over a long period, subject to an inequality of magnitude and period not yet known. He also accepts, at least as more or less correct, Ptolemy's eccentricity and direction of the apogee, also used to find the length of the sidereal year, and length of the tropical year for the period between Hipparchus and Ptolemy, and he believes the obliquity varies over a range of about 20′, meaning that he accepts something close to Ptolemy's large obliquity of 23;51,20° in antiquity, nearly 20′ greater than his own 23;31,30°. He was cautious about doubting the observations and parameters of his predecessors, except for Copernicus close to his own time, and while admitting the possibility of errors by Hipparchus and Ptolemy, did not consider their errors as large as his own parameters would suggest, believing instead that their observations could not be seriously inaccurate and there had to be changes of some kind in parameters over so long a period.

Scaliger had pointed out (8.85), correctly, that some of Hipparchus's equinoxes were in error by a quarter of a day, as shown in *Almagest* 3.1, and accused Ptolemy of errors of an entire day, which we know also to be true. Tycho's answer is more cautious (8.101–02). He admits that because Hipparchus's instruments were not graduated to single minutes, but only to twelfths of a degree, and because of neglect of solar parallax and refraction, errors of six hours in times of equinoxes were possible, and further, that Ptolemy's observations have even less certainty. But he will not say that there was an error of an entire day in the entries into Ptolemy's equinoxes, for this would require admitting an error in the declination of the Sun of about five-twelfths of a degree, which the size and precision of the instruments, by which the interval between the tropics or the obliquity of the ecliptic was investigated within one-third of a minute (unless he also borrowed this from Hipparchus), does not allow. The reasoning here is that since Ptolemy states the obliquity as 23;51,20°, to a precision of $\frac{1}{3}'$, he could not possibly be in error by 25′, the daily change in declination of the Sun around equinox, so the equinoxes could not be in error by a full day. He also points out that the maximum latitude of the Moon of 5°, found by Ptolemy with parallactic rulers, does not show so great an error of the instrument, meaning close to 25′. Since so much depends upon the times of these equinoxes, as they are used to find the length of the tropical year, the eccentricity and direction of the apsidal line, the mean motion in longitude and epoch, and indirectly the longitudes of stars from which the rate of precession is found, that is, all the parameters which show long-period variation except the obliquity of the ecliptic, which depends upon altitudes of solstices, the absence of serious errors in the observations shows an absence of serious errors in the parameters. And a variation of 20′ in the obliquity is also accepted. Thus a variation of parameters over a long period must be taken seriously and accounted for by the universal hypothesis of the Sun in the complete restoration of astronomy, and that is what Tycho intended to do. Of course he did not do it, and it is not possible to know how or whether he would have changed his mind in attempting to do so. He, or an assistant to whom he assigned the work, would presumably have caught the error of applying the lunar latitude model to account for the nonuniform precession, at least as described in the letter to Scaliger, but more than this we cannot say. We may only conclude that he took the long-period variation of parameters in solar theory following from Ptolemy's observations, including the precession and obliquity as part of solar theory, as seriously as Copernicus did in his theory of the motions of the Earth. So although Tycho did not believe Copernicus had described these variations correctly or accurately, he was of the same mind as Copernicus with regard to the effects, although not the cause.

Christian Longomontanus

Christian Severinus Longomontanus (1562–1647) was Tycho's loyal and capable assistant for nearly ten years at Uraniborg, and was with him for part of his travels in Germany and then in Benatky and Prague. His last contribution while with Tycho was the final form of Tycho's lunar theory published in the *Progymnasmata*, most of which was Longomontanus's work, not always with Tycho's complete approval. He later became professor of mathematics at Copenhagen. His principal work, *Astronomia Danica*, published in 1622, was intended as a complete exposition of astronomy based upon Tycho's methods and observations, including the theory of the planets that Tycho did not live to complete, or even begin. Although no longer well known or much studied, since the contemporary work of Kepler made nearly everything in it obsolete, or about to be obsolete, it was regarded well enough in its day to be reprinted in 1640. The work is in two parts, the first on spherical astronomy, the second, of concern here, on the Sun, Moon, planets, and stars, and there is an appendix on temporary phenomena of the heavens, new stars and comets. The title of the second part is "Theories of the motions of the planets in accordance with the observations of Tycho Brahe, and in fact his very own, re-established in a three-fold form." The "three-fold form" means that everything is set out in Ptolemaic, Copernican, and Tychonic form, which Longomontanus prefers although giving the diurnal rotation, precession, and variation of obliquity to the Earth rather than the heavens, which are absolutely at rest. In making use of ancient observations, he does not take them as recorded by Ptolemy, as Copernicus did, but subjects them to examination and correction, as Tycho intended to do, and he considers his work to apply to all times, again as Tycho intended in his complete restoration of astronomy. This is specifically stated in the separate title page of Part Two, which is worth quoting: "The second part of Danish Astronomy, including the theories of the planets restored in two books, of which the former, after a description and comparison of the three-fold hypothesis of the world, namely, the ancient Ptolemaic, the astonishing Copernican, the modern of Tycho Brahe, treats the apparent motions of the fixed stars, likewise of the Sun and Moon in the same way, re-established and adapted to all ages of the world, together with the entire theory of eclipses and besides this a special treatment of the Moon; the latter treats the motions of the other five planets, on the basis of the three-fold hypothesis, similarly restored to the appearances of the heavens in the same way."

Although Tycho did not carry out his intended investigation of the motion of the Sun for all times, that is just what Longomontanus does in a lengthy history of solar observations and theory from antiquity to Tycho (28–49). Much of it does not meet with his approval, but he is also interested in explaining why things went wrong. He is, to say the least, direct in his evaluation (29).

> For although the proof of the perpetual constancy of the celestial phenomena of the single motion of the Sun is evident, yet if the observations and likewise theories of each of the astronomers are to be believed, in none other do I find more disgraceful inconstancy, and this not only concerning the measure of the annual revolution of the Sun, but also the change of its eccentricity (as it is called) and the location of its apogee. Thus, it was determined by Ptolemy in his demonstration of the hypothesis of the Sun, and proved by observations of some kind, that in the nearly 300 years between Hipparchus and Ptolemy they were without any change, but soon after in the course of the following centuries they appear to be subject to inordinate change. Considering the causes of this more carefully, I perceive that none belong to the absolutely simple motion of the divine star, but all fault is deservedly to be ascribed to the astronomers, whose records of the motion of the Sun in different ages, as they maintain derived from the heavens, have been transmitted to posterity, in which records the motion of the Sun is more or less erroneous in one way or another from rather obvious causes. This disgraceful situation continued until the beginning of the more accurate restoring of astronomy was divinely granted to our age and to our Atlas, Tycho Brahe, the celestial observations of whom alone, both because of the correct and careful preparation of instruments as well as skill in observing, exclude all sensible error, as I, who was a student of Brahe's astronomy for ten continuous years, can perhaps be the best witness. But since, as we know, equal care had by no means been shown by his predecessors, therefore it is no wonder that with the progress of time, very abundant error emerged, in other bodies, but especially in the Sun.

Longomontanus is suspicious of all early observations taken from declinations of the Sun, of equinoxes, where the daily change in declination is greatest, because of the insufficient size or skill in manufacture of instruments, incorrectly assumed latitude of the observer, and the effects of parallax and refraction. And the problems are worse for intermediate places where the daily change of declination is sensibly smaller, until the solstitial points where the location of the Sun cannot be obtained from observations because its declination remains invariable for many days. He believes that the length of the tropical year was obtained, not from such observations, but from cycles and syzygies of the luminaries, the Sun and Moon (30). All before Hipparchus believed the year to be $365\frac{1}{4}$ days, as appears in the institution of the Olympiad, beginning anew in the fourth year near the rising of Sirius, and likewise other times of the year were recognized by the rising and setting of fixed stars, the custom of the most ancient Hesiod and later the Greeks and Romans. While all the observations of the Sun, which without doubt existed in Babylon and Egypt during the rule of the Assyrians, have perished, first a certain Meton of Athens, who flourished 430 years before the birth of Christ, also by use of the common length of the year $365\frac{1}{4}$ days, a Julian year, estimated the mean periods (*simplices cursus*) of the luminaries, not so much with respect to the equinoctial and solstitial points, as to new Moons in his interval of 19 years, with a notable error which in the course of time to Hipparchus was found to be 5 days by the same Hipparchus—but to the correction of Callippus, instituted six years before the death of Alexander, within four of his (Meton's) periods, which contained 76 single years, an anticipation of one day was observed in the new Moon—that is, in an interval of 304 years, or somewhat shorter, 300 years, as Scaliger says, just as the following words ascribed by Ptolemy to Hipparchus make clear. He then quotes Scaliger's quotation of Ptolemy's paraphrase and quotation in *Almagest* 3.1 from Hipparchus's book "On intercalary months and days" that according to Meton and Euctemon the years is $365\frac{1}{4}$ days,[12] and that Hipparchus says he finds as many months in 19 years as they did, but the year less than the quarter day by 1/300 day, and thus in 300 years lacking five days from the years of Meton but only one day from the years of Callippus. He next paraphrases Copernicus's account in *De revolutionibus* 4.4, and explains everything at rather great length. Thus, Meton took the length of the year in the cycle of 19 years equal to 235 months to be $365\frac{1}{4}$ days, as did Callippus, who deducted one day in four cycles of 76 years equal to 940 months from observing an eclipse of the Moon six years before the death of Alexander. (There is obviously a contradiction if both took the year to be $365\frac{1}{4}$ days.) Hipparchus then corrected four cycles of Callippus, 304 years equal to 3760 months, by removing one day, and thus five days from Meton, so that, subtracting one day in 304 years, or shorter, in 300 years, he made the tropical year $365\frac{1}{4}$ days reduced by 1/300 day, that is $0;4,48^h$, so the time is judged to be $365^d\ 5;55,12^h$. His conclusion is striking (31).

> And thus Hipparchus, together with his predecessors, attempted to hunt two hares with one leap, that is, to restore the new Moons within a certain interval of years and determine the individual periods from the mean motions of the luminaries, and at the same time to measure the annual revolution. Since,

however, one does not at all depend upon the other in this way, he obtained a measure of the solar year, not, as it appears, from heaven or the Sun itself, but from certain syzygies of the luminaries, incorrect and in fact excessive in length. Unfortunately, Ptolemy chose to copy this error of Hipparchus rather than repudiate his opinion, so that this lunar cycle was also pleasing to him. What other evidence Ptolemy presents from Hipparchus for this assertion proves nothing since, as we proved earlier, the tropical or solstitial points were unobservable by the ancients, and moreover, the Hipparchan equinoxes notably oppose this opinion of Ptolemy (*ipsius*), as we shall soon demonstrate from Hipparchus's (*ipsius*) very observations. And we have treated these things at length so that men of our time will finally learn that the ancient astronomers, Hipparchus especially and Ptolemy, have been exposed in errors by reason of fairly obvious causes in assigning the period of the Sun.

This is, to say the least, strong language. Yet, although there is some confusion in Longomontanus's account, as the length of the year according to Meton, his principal point, that the tropical year of Hipparchus and Ptolemy was derived from a luni-solar cycle rather than from observations of the Sun alone is undoubtedly correct.[13] Very briefly, the length of the Callippic Cycle of 76 years = 940 months is $76 \cdot 365\frac{1}{4}^{d} = 27{,}759^{d}$. But Hipparchus had himself confirmed the Babylonian System B mean synodic month of $29;31,50,8,20^{d}$, from which 940 months are equal to $940 \cdot 29;31,50,8,20^{d} = 27{,}758;45,30,33,20^{d}$, less than the Callippic Cycle by about $0;15^{d}$, one-quarter day. Hence, in four Callippic Cycles, 304 years = 3760 months, called the Hipparchan Cycle, one day must be subtracted, and the length of the cycle is $4 \cdot 27{,}759^{d} - 1^{d} = 111{,}035^{d}$. The length of the tropical year is thus $111{,}035^{d}/304 = 365;14,48,9,28\ldots^{d}$, which was rounded to $365;14,48^{d} = 365\frac{1}{4}^{d} - \frac{1}{300}^{d}$. Hipparchus confirmed this year as well as he could from earlier observations of solstices, which is all he had, of which Ptolemy gives one example: the summer solstice observed by Aristarchus at the end of the fiftieth year of the first Callippic Period (−279) and by Hipparchus at the end of the forty-third year of the third Callippic Period (−134), an interval of 145 years in which the number of days was less than $145 \cdot 365\frac{1}{4}^{d}$ by one-half day, or one day in 290 years, close enough to 300 years to confirm a tropical year of $365\frac{1}{4}^{d} - \frac{1}{300}^{d}$. Ptolemy's confirmation uses pairs of equinoxes of Hipparchus and his own, autumnal (5) and (12), vernal (6) and (13), each pair separated by 285 years = $15 \cdot 19$ years, surely no coincidence, which Longomontanus may have noticed although he does not mention it. The tropical year of Hipparchus, and later of Ptolemy, thus rests upon the application of the Babylonian System B month to the cycle 19 years = 235 months, or 76 years = 940 months, multiplied to an integer number of days, 304 years = 3760 months ≈ 111,035 days, and an approximate confirmation from independent observation of the Sun.

What Longomontanus does next is to set out nine of Hipparchus's equinoxes, six autumnal and three vernal, dated to the Era of the Death of Alexander (−323 Nov 12, here EA, also called Era Phillip), and subject them to an "examination," or rather criticism (32). He believes the observations were made in Alexandria, not

Rhodes, and that the equinoxes were found, not by interpolating between meridian altitudes of the Sun, but by an equatorial ring, so the stated times were directly observed—this is curious since two of the times are midnight—and it is no wonder, he says, that Hipparchus reached a precision of only one-quarter day. Further, the reported times of most cannot be accepted because they were at sunrise or sunset where the effect of refraction in the horizon makes the autumnal later and the vernal earlier by over half a day at the least, although in part reduced by neglect of the parallax of the Sun. Thus, as we shall see, when using two equinoxes observed at dawn, 6 AM, he corrects for refraction: from the autumnal equinox (7) of −145 Sep 27 he subtracts five hours, and to the vernal equinox (6) of −145 Mar 24 he adds five hours, from dawn to one hour before noon, the time Ptolemy reports the equinox was observed by a ring in Alexandria. He then computes that the year derived directly from various intervals between the equinoxes does not exceed 365 days by $5;55,12^{h}$, but from the autumnal equinoxes by not more than $5;4^{h}$ and from the vernal equinoxes by not more than $5;43^{h}$, with a mean of only $5;24^{h}$, deficient from $5;55^{h}$ by $0;31^{h}$. He notes that the parallax of the Sun in the equator at Alexandria, at an altitude of 59°, is $1\frac{1}{2}'$, but does not use it to correct the times of the equinoxes near noon (by about $1\frac{1}{2}^{h}$). Instead, he uses a curious computation for finding the intervals between equinoxes equivalent to the following: The interval between the autumnal equinox (4) of EA 167 Epagomenal 1 (−157 Sep 27) at noon and the vernal equinox (6) of EA 178 Mechir 27 (−145 Mar 24), with the correction of $+5^{h}$ from dawn to one hour before noon, is $4195^{d}\ 23^{h}$. But eleven years of $365^{d}\ 5;24^{h}$ are $4017^{d}\ 11;40^{h}$. The difference of $178^{d}\ 11;20^{h}$ is the interval from the autumnal to the vernal equinox, which, subtracted from the year of $365^{d}\ 5;24^{h}$ gives $186^{d}\ 18;4^{h}$ from the vernal to the autumnal equinox. He makes one small mistake and finds the intervals $178^{d}\ 11;25^{h}$ and $186^{d}\ 17;59^{h}$, and notes that they differ, by several hours, from Ptolemy's intervals of $178^{d}\ 6^{h}$ and $187^{d}\ 0^{h}$. We have carried out everything precisely in accordance with the observations of Hipparchus, he says, not to show in them the truth itself, for neither the length of the tropical year nor the interval between the equinoxes which results is the truth, but so that it becomes clear how great are the errors in the observations of the ancients, lest we be so devoted and so bound to them that it will not be acceptable to change anything in them by applying the fair weighing of comparison.

Longomontanus is hard on Hipparchus, but he is harder still on Ptolemy (33).

> We explained earlier what the intention of Ptolemy was concerning the measure of the tropical year, and, unless I am very mistaken in this conjecture, he observed both autumnal equinoxes at the very limit of the horizon—provided that they differ (as without doubt they do) from the number of those which occurred twice in one day due to the instrument, although in fact the instrument rested on one side or the other with respect to the horizons—which remarkably led to what he intended. And it is certainly worthy of notice that in these observations Ptolemy has so far adapted himself to the Hipparchan demonstration and hypothesis (*constitutioni*), of the measure of the tropical year as well as of the immutable eccentricity of the Sun, that for this very

> reason he did not assign his observations to the exact cardinal points of the days (i.e. sunrise, noon, sunset), but a little later, at one hour etc., so that you would judge (he did this) to give satisfaction to the Ptolemaic computation rather than to heaven. But lest some astronomers to whom I write these things become indignant at our candor in investigating Ptolemy, prevailed upon by his ancient and exceedingly great authority, I ask that they consider what he relates elsewhere concerning the parallax of the Moon observed by him, and carefully compare (it) with our restoration which, to the best of my knowledge, in the lunar motion and distance corresponds exactly to the standard of heaven. And finally, let them notice in that passage (as I pass over others like it) Ptolemy reported from his observation the parallax of the Moon half a degree and more above the true parallax, for no other reason (as I believe) than that he pass off (*obtruderet*) upon posterity as genuine (*pro legitima*) that hypothesis of the Moon he previously established himself or, if you prefer, received from his predecessors, and only once confirmed by his computation. But now, I ask, what will be the prohibition (*religio*) from suspecting that here he was of the same intention, and relied upon those equinoctial observations of the Sun which served his purpose, but the others, of which it is very likely he made many more, he entirely concealed?

The remark about the instrument that showed two equinoxes in one day because it was out of alignment refers to Ptolemy's criticism of two bronze equatorial rings in Alexandria. So Ptolemy too, according to Longomontanus, followed Hipparchus in accepting the tropical year derived from the luni-solar cycle, as well as Hipparchus's eccentricity of the Sun, and adjusted his observations of equinoxes accordingly "to give satisfaction to the Ptolemaic computation rather than to heaven." This may be true, or one may say that like Hipparchus he took the year derived from the cycle to be correct in principle and confirmed it from observations of equinoxes, although we know not well since his own equinoxes are late by from 21 to 36 hours. In any case, it is evident that Longomontanus does not trust Ptolemy at all, as shown by the observation of lunar parallax he reported (*Almagest* 5.13), more than half a degree too large, and he suggests that Ptolemy's reason for this was pass off on posterity the defective hypothesis of the Moon that he invented or even received from his predecessors. Clearly, he does not approve of Ptolemy.

The examination of Ptolemy's equinoxes considers only one, the autumnal equinox (12) of EA 463 Athyr 9 at one hour after sunrise (139 Sep 26, 7 AM), which he believes was observed with an equatorial ring. Since the Sun was nearly in the horizon, the refraction in altitude was 32′, which in the horizon in Alexandria corresponds to about 32′ of longitude and 13 hours in time, all of which is about correct. And since refraction makes the autumnal equinox later, with correction for refraction the equinox occurred 13 hours earlier on Athyr 8 at 6 hours after noon (139 Sep 25, 6 PM). Then, the interval to the following vernal equinox (13), Pachon 7 at one hour after noon (140 Mar 22, 1 PM), taken here as exactly noon, is $178^{d}\ 18^{h}$—without the rounding, $178^{d}\ 19^{h}$—which Ptolemy and Hipparchus took as $178^{d}\ 6^{h}$ and the earlier correction of Hipparchus's interval $178^{d}\ 11;25^{h}$. From the corrected time of Ptolemy's equinox, he computes the length of the tropical year between Hipparchus and Ptolemy (35). He takes Hipparchus's autumnal equinox (4) of EA 167 Epagomenal 1 at noon (-157 Sep 27, 12 PM) and Ptolemy's equinox (12) of EA 463, corrected by -13^{h} to Athyr 8 at six hours after noon (139 Sep 25, 6 PM), and finds an interval in Egyptian years of $296^{\text{ey}}\ 72^{\text{d}}\ 6^{\text{h}}$. In 296 Julian years, the addition of one-quarter day is $296 \cdot \frac{1}{4}^{\text{d}} = 74^{\text{d}}$, exceeding $72^{\text{d}}\ 6^{\text{h}}$ by $1^{\text{d}}\ 18^{\text{h}} = 42^{\text{h}}$. The deficit in one year from $365\frac{1}{4}^{\text{d}}$ is thus $42^{\text{h}}/296 = 0;8,30\frac{4}{5}^{\text{h}}$ and the length of the year $365^{\text{d}}5;51,29\frac{1}{5}^{\text{h}}$, and from three pairs of equinoxes he finds it not greater and even a little smaller. Then the interval from the vernal to the autumnal equinox is $365^{\text{d}}\ 5;51,30^{\text{h}} - 178^{\text{d}}\ 18^{\text{h}} = 186^{\text{d}}\ 11;51,30^{\text{h}}$.

In addition to the equinoxes of Hipparchus and Ptolemy, Longomontanus also considers the equinoxes in the calendar of Julius Caesar, which he believes, following Pliny, was the work of Sosigenes, and which he finds in the agricultural calendar in Book 18 of Pliny's *Natural History*. These are definitely schematic, but so too are the intervals of Hipparchus and Ptolemy, from which they differ by one day, and the year of $365^{\text{d}}\ 6^{\text{h}}$ used for deriving the eccentricity, which is not the exact length of the year. In all, he now has four sets of intervals between the equinoxes, which we give with the length of the year in the following table.

Source	*Year*		*Vern. to Aut.*		*Aut. to Vern.*	
Hipparchus-Ptolemy	365^{d}	6^{h}	187^{d}	0^{h}	178^{d}	6^{h}
Sosigenes	365	6	186	0	179	6
Hipparchus corrected	365	5;24	186	17;59	178	11;25
Ptolemy corrected	365	5;51,30	186	11;51,30	178	18

For deriving a corrected eccentricity for the time of Hipparchus (36), since it is not possible to find the time of the solstice accurately, he uses the corrected interval between Ptolemy's vernal and autumnal equinoxes and an assumed longitude of the apogee near that found by Hipparchus, which makes for a very simple demonstration although it is set out a great length and computed to no less than seven places. We need not go through the steps, which have only the smallest inconsistencies. The interval from the vernal to the autumnal equinox of $186^{\text{d}}\ 11;51,30^{\text{h}}$ gives a mean motion of $183;49,12°$, and taking the longitude of the apogee $\lambda_{\text{A}} =$ Gemini $6°$, where the radius of the eccentric $R = 1$, the eccentricity $e = 0.0364837$, and the maximum equation $c_{\text{m}} = 2;5,26°$. Hipparchus and Ptolemy found $\lambda_{\text{A}} =$ Gemini $5;30°$, $e = 0.0417$, and $c_{\text{m}} = 2;23°$. It then follows that the mean motion from the vernal equinox to the summer solstice is $93;43,36°$ and the interval of time $94^{\text{d}}\ 2;40^{\text{h}}$, which he notes is about midway between Hipparchus and Ptolemy, $94^{\text{d}}\ 12^{\text{h}}$, and Sosigenes reported by Pliny, $93^{\text{d}}\ 12^{\text{h}}$, although somewhat closer to Hipparchus.

He then examines (37–47) the solar theories derived from the observations of al-Battānī, Walther, Copernicus, whose theory he corrects as he corrected Hipparchus and Ptolemy—Tycho had also corrected Copernicus's solar theory—and finally Tycho, finding that the maximum equations are all nearly the same, within

$\pm 3'$ of his own derivation of $c_{\mathrm{m}} = 2;4,48^{\circ}$, from $e = 0.035714 = 1/28$, based upon his own small correction of Tycho's observations, so the eccentricity has remained constant, which shows that the eccentricity of Hipparchus and Ptolemy is erroneous and Copernicus's model for the variation of the eccentricity is incorrect. And that the eccentricity is equal to 1/28, the divine *unalterable* proportion of the second number in the order of perfect numbers, equal to the sum of their factors, he takes as more evidence that Copernicus's variation of the eccentricity should be ignored. He also believes that the solar apogee has advanced with a uniform (sidereal) motion, from the beginning of Aries, with the Sun at the perigee at the beginning of Libra, at the Creation of the world—5554 years before 1588, thus 3967 BC at the autumnal equinox—to 95;30° in 1588 at the time of Tycho. We shall take up his chronology below. He sets out a table comparing locations of the apogee from Hipparchus to Tycho with his own corrected locations, which depend upon his theory of precession yet to be explained, omitting Ptolemy as just plain wrong and correcting Copernicus, in which the greatest differences are $+37'$ for Battānī and $-45'$ for Copernicus, and concludes that Copernicus's variation of the direction of the apogee, which reaches $\pm 7\frac{2}{5}^{\circ}$, is also incorrect. Indeed, he here cites the opinion of his friend Holger Rosenkrantz (48) that a variation of eccentricity of the Sun and the planets of the kind introduced by Copernicus is clearly contrary to the perpetual nature of the heavenly revolutions and is only derived from false principles, that is, from useless observations.[14] Finally (48–49), from Hipparchus's vernal equinox (6) of -145 Mar 24 corrected by $+5$ hours, and Tycho's equinox of 1587 Mar 10, with a preliminary correction for the inequality of precession, he finds the length of the tropical year $365^{\mathrm{d}}\ 5;49,20^{\mathrm{h}}$ but prefers $365^{\mathrm{d}}\ 5;49,30^{\mathrm{h}}$ based upon his correction of observations of ancient lunar eclipses. However, this is not the final length of the tropical year that underlies his tables of the mean motion of the Sun, which requires a more careful investigation, including of the precession.

The precession is taken up in the section on the fixed stars (53–56), and it is not uniform, so neither is the tropical year. Tycho had derived a rate of precession of $51''$ per year for his own time directly from the difference of the tropical and sidereal year, and then showed that it is mostly confirmed by observations of stars extending as far back as Timocharis, although he left open the question of whether it is in fact variable. Longomontanus instead begins with the observations, but he first corrects them: Timocharis's of occultations β Sco, η Tau, and Spica by the Moon corrected by Tycho's and Longomontanus's lunar theory; Hipparchus's of Spica in finding its longitude from its declination (and some unspecified coordinate); Ptolemy's longitude of Regulus measured from the Moon on an armillary by Longomontanus's solar theory; Battānī's longitude of Regulus by its distance from β Sco in Tycho's catalogue. These corrections are not consistent, and all are also corrected to longitude from the mean equinox using the equation of the nonuniform precession yet to be explained. The corrections for Timocharis, Hipparchus, and Battānī are less than 0;30°, but Ptolemy's longitude of Regulus is advanced by $+1;23^{\circ}$ from Leo 2;30° to 3;53°, of which $+1^{\circ}$ is from correcting Ptolemy's longitude of the Sun from the mean equinox and $+0;20^{\circ}$ from solar refraction reduced by parallax. Tycho's longitudes of Regulus and Spica are corrected only by the equation of precession of $-0;8°$. From all these, and a number of computational errors, he finds motions of from $44''$ to $57''$ per year, and settles on $0;0,49,45^{\circ/\mathrm{y}}$ as the mean rate of precession, close to $0;0,49,46^{\circ/\mathrm{y}}$ between Hipparchus and Tycho (which correctly computed is $0;0,49,30°$). For the obliquity of the ecliptic, he says he has corrected what his predecessors found for solar parallax and finds it to vary from 23;53° in about the year 3600 of the Creation of the world (-366) to about 23;31° in the year 5400 of the world (1434), with the mean of 23;42°, although the exact range is subject to a "perfect" criterion, as we shall see. Still, he is close to Copernicus, whose range is 23;52° to 23;28° with a mean of 23;40°.

Before considering the precession, we must say something of Longomontanus's chronology and epochs (47, 57–58). He says that earlier astronomers have used various epochs for mean motions, as the Olympiad, Nabonassar, Alexander, Caesar, and the Incarnation, but for we Christians, two beginnings ought to be especially distinguished before the others: first when this most beautiful theater of the world began to exist by the word of omnipotent God, second when the only-begotten Son of God himself took on our human flesh and deigned to be born to restore the fallen world and liberate us from the power of the devil and eternal death. He acknowledges that the years and the time of year of both epochs are disputed by chronologers, but this dissension does not involve the celestial motions in any difficulty since they can properly be derived from other intervals securely confirmed by celestial observation. The point is that astronomical chronology can correct historical chronology. From the Creation of the world to the passion of the Son of God on the cross, and through him the salvation of the world, there elapsed 4000 solar years less one-half year. Since the age of Christ was then about $33\frac{1}{2}$ years, the crucifixion was in AD 34 near the time of the vernal equinox and the Creation in $34 - (4000 - \frac{1}{2}) = -3966$ near the time of the autumnal equinox, that is, 3967 BC *at* the autumnal equinox. At this time, the apogee of the Sun was at the beginning of Aries and the Sun at perigee at the beginning of Libra. Further, the obliquity of the ecliptic was then greatest, the precession of the equinoxes zero, and the inequality of the precession zero. In the tables of mean motion, however, the Era of the World is set later to -3963 Jan 1 at noon at Copenhagen. The reason is that this is the first year of a Julian cycle of four years with the leap year as the fourth year, as is the Era of Christ, AD 1 Jan 1 at noon, the other epoch of the tables, so both can be used with the same tables of collected and single Julian years. Although the date of the autumnal equinox of -3966 is not given, it can be computed from the solar tables and is -3966 Oct 24 at about 11 AM in Copenhagen.[15]

There is a fine study of Longomontanus's model for the variable precession and obliquity (85–93) by Moesgaard (1975), and we have found it very helpful for our own exposition. In Fig. 2, the Earth is at O and the pole of the mean ecliptic is at $\bar{E}$, about which the pole of the equator N rotates, carrying with it the equator of the Earth and thus the celestial equator, which intersects the mean ecliptic with the mean obliquity $\bar{\varepsilon}$. This is a conical motion of the axis of the Earth, causing the mean vernal equinox $\bar{\Upsilon}$ to precess along the mean ecliptic opposite to the order of the signs, from east to west, through the mean precession of $0;0,49,45^{\circ/\mathrm{y}}$ in a

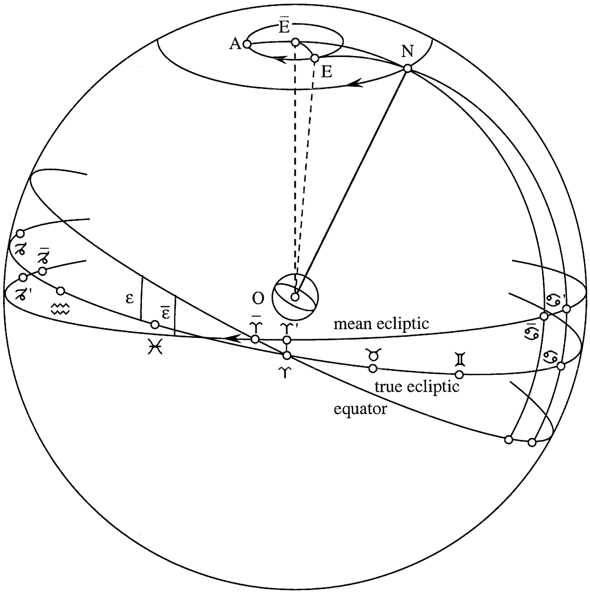

Fig. 2 Longomontanus. Hypothesis for precession of the equinoxes and variation of obliquity of the ecliptic

period of 26,050 years. Next, the pole of the true ecliptic E rotates in the same direction measured from A in a small circle about $\bar{E}$, causing a similar small circular motion of every point of the true ecliptic, the true path of the Sun around the Earth.[16] The result is that the intersection of the equator and the true ecliptic, the true vernal equinox Υ, oscillates along the equator on either side of the mean equinox $\bar{\Upsilon}$, and on the mean ecliptic there is a small inequality in the precession $c_{\mathrm{p}} = \bar{\Upsilon}\Upsilon'$, which is zero when the pole of the true ecliptic is at A. The motion of the true ecliptic also causes the true obliquity of the ecliptic ε to vary on either side of the mean obliquity $\bar{\varepsilon}$, with the maximum obliquity when the pole is at A, and the variation of the obliquity in turn causes the latitudes of stars to vary; the sphere of the fixed stars itself is absolutely at rest. The correction table gives the inequality of precession, variation of obliquity, and a proportional coefficient for the variation of latitude of stars, with the greatest variation at solstices, equal to the total range of the obliquity, decreasing to zero at equinoxes. The parameters are only slightly empirical. The range of the obliquity was given earlier as $23;42^{\circ} \pm 0;11^{\circ}$, but is now changed to $23;42^{\circ} \pm 0;10,53^{\circ}$. Why? Because $0;10,53^{\circ} \approx 90^{\circ}/496$, and 496 is the third in the order of perfect numbers. The maximum equation of precession, $c_{\mathrm{pm}} = \sin^{-1}(\sin 0;10,53^{\circ}/\sin 23;42^{\circ}) = 0;27,5^{\circ}$, is merely derived from the variation of obliquity in the model. The period of the anomaly of precession and obliquity, of the motion of E, is 3600 years = 1,0,0 years, a period considered significant since antiquity, so the anomaly is exactly $6'$ per year, the first in the order of perfect numbers, although Longomontanus does not mention it. (These are Julian, not tropical or sidereal, years, which means that the model "knows" the Julian calendar. Copernicus's period of the anomaly of the obliquity is 3434

Egyptian years and of precession half of that, 1717 Egyptian years.) At the Creation of the world, the mean precession is taken to be zero and the anomaly is zero, at *A*, so the inequality is zero and the obliquity is maximum. That all the parameters are determined by such criteria explains how Longomontanus can apply the equation of precession to longitudes of stars in order to find the mean rate of precession as he earlier did.

Longomontanus says that his model agrees well with the variation of obliquity, if Ptolemy is corrected to 23;49° for the effect of solar parallax at the solstices and Copernicus's 23;28° is in error as Tycho already showed. The only examples of precession, obliquity, and stellar longitude and latitude he computes are for the year 3000 of the world, 967 BC, 82 years after Hesiod flourished, and for the year 6000 of the world, sometimes taken as the year of the Second Coming, AD 2034, neither particularly helpful for empirical confirmation. But it is easy enough to compute the range of the variation of precession, which is minute, 0;0,49,45° ± 0;0,2,48° per year or 1;22,55° ± 0;4,40° per Julian century. Compare this with the *Prutenic Tables*, 0;0,50,12° ± 0;0,15,41° per year or 1;23,43° ± 0;25,33° per Julian century. Thus, the wide range of Copernicus's precession and the slow rate of Hipparchus and Ptolemy have been rejected entirely.

The determination of the refined length of the tropical year (94–96) is, to say the least, interesting. It is done by finding the intervals between pairs of vernal and autumnal equinoxes observed by Hipparchus and Tycho, taking the arithmetic mean of the deficits from integral Julian years, doing the same for equinoxes observed by Ptolemy and Tycho, again taking the mean of the deficits, and then taking the mean of both means. The result of the procedure is called *limitata*, which means bounded, placed within limits or accurately examined; the same term is used in Tycho's observational records for taking means and small adjustments, and it is possible that these too are the work of Longomontanus.[17] He first corrects for the solar inequality and the inequality of precession to find the time of the mean equinoxes unaffected by either.

The solar inequality is shown in Fig. 3, in which (a) is the configuration at the time of Hipparchus or Ptolemy and (b) at the time of Tycho; the difference is only in the longitude of the apogee λ_A as the eccentricity found by Longomontanus is invariable. The Earth is at O, from which the directions of the true equinoxes are ♈ and ♎, when the true longitudes are 0° and 180°, and the center of the eccentric at C, from which the directions of the mean equinoxes are $\overline{♈}$ and $\overline{♎}$, when the mean longitudes are 0° and 180°. This may be Longomontanus's own definition of mean equinox. The difference in direction is given by the solar equation c, which is the same at both equinoxes since the true distance of ♈ from apogee is λ_A and of ♎ is $180° - \lambda_A$, for which the equations are equal and of opposite sign. The equations are computed from the true distance of the vernal equinox from apogee by $c = \sin^{-1}(\sin e \sin \lambda_A)$ where $e = 1/28$ and λ_A is specific to the date of each observer; c is positive at the vernal equinox, mean equinox after true equinox, and negative at the autumnal equinox, mean equinox before true equinox. He next adds the inequality of precession c_p to the solar equation c and converts the sum $c + c_p$, which is not given, to the interval of time Δt between the mean and true equinox by dividing by the true hourly velocity of the Sun v_s, which is also not given, that is,

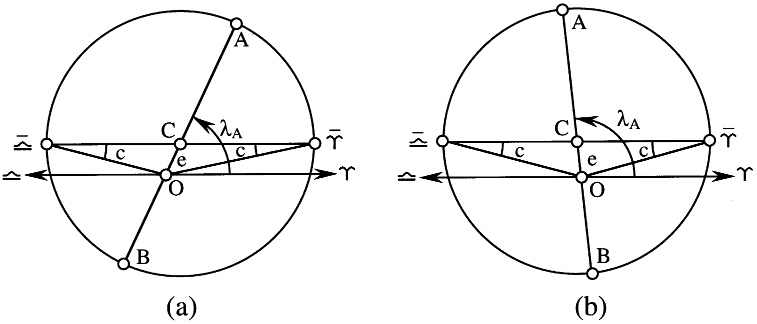

Fig. 3 Longomontanus. Determination of mean equinoxes at times of (**a**) Hipparchus and Ptolemy, (**b**) Tycho

$\Delta t = (c + c_p)/v_s$. The following table gives the observer, year of the equinoxes, λ_A, c, c_p, and for each equinox, vernal and autumnal, $c + c_p$ and Δt.

Observer	*Year*	λ_A	c	c_p	*Ver* $c + c_p$	*Ver* Δt	*Aut* $c + c_p$	*Aut*Δt
Hipparchus	−145	65;30°	±1;51,44°	−0;10,12°	+1;42,32°	$+1^d$ 17;50^h	−2; 1,56°	-2^d 0;50^h
Ptolemy	139/40	70; 0	±1;55, 2	−0;21, 0	+1;34, 2	+1 14;45	−2;16, 2	−2 6;45
Tycho	1587/88	95;30	±2; 2,14	+0; 7,12	+2; 9,26	+2 4;30	−1;55, 2	−1 22;50

The interval Δt is then added to the time of the true equinox to give time of the mean equinox. The equinoxes are paired such that each is the same year of a four-year Julian cycle, so the number of days in the interval of years is an integer. Hence, the difference ΔT of the calendar dates and times of the equinoxes is the deficit of the tropical years from an integral number of Julian years. The stated dates and times are from noon preceding by 12 hours the next Julian calendar date beginning at midnight. Hipparchus's vernal equinox (6) of −145 Mar 23 at dawn is corrected for refraction by +5 hours, from 18^h to 23^h, the time Ptolemy reports the ring in Alexandria showed this equinox, and his autumnal equinox (7) of −145 Sep 26 at dawn by −5 hours, from 18^h to 13^h. Tycho's equinoxes were found by interpolation between meridian altitudes already corrected for parallax and refraction. The meridian of Alexandria (A) is adjusted to Uraniborg (U) by $-1;35^h$, as did Tycho. Here is a tabulation of the steps for the equinoxes of Hipparchus and Tycho:

Observer	*True Equinox*	t(A)	t(U)	$\Delta t(c + c_p)$	*Mean Equinox*
Hipparchus	−145 23 Mar	23^h	21;25^h	$+1^d$ 17; 50^h	25 Mar 15;15^h
Tycho	1587 10 Mar	—	14;56	+2 4;30	12 Mar 19;26
					ΔT 1732^y − 12^d 19;49^h
Hipparchus	−145 26 Sep	13	11;25	-2^d 0;50^h	24 Sep 10;35^h
Tycho	1587 13 Sep	—	9;26	−1 22;50	11 Sep 10;36
					ΔT 1732^y − 12^d 23;59^h

The arithmetic mean of the two deficits of ΔT from 1732 years is 12^d 21;54^h, and thus the deficit of the tropical year from the Julian year is 12^d 21;54^h/1732 =

$0;10,44,8^h$. Now we do the same with equinoxes observed by Ptolemy and Tycho. Ptolemy's autumnal equinox (12) of 139 Sep 25 at one hour after Sunrise is corrected for refraction by -13 hours, as Longomontanus showed earlier, from 19^h to 6^h, but no correction is applied to the vernal equinox (13) of 140 Mar 22 since it is close to the meridian at an altitude where refraction is negligible.

Observer	*True Equinox*	$t(\mathrm{A})$	$t(\mathrm{U})$	$\Delta t(c + c_\mathrm{p})$	*Mean Equinox*
Ptolemy	139 25 Sep	6^h	$4;25^h$	$-2^d\ 6;45^h$	22 Sep $21;40^h$
Tycho	1587 13 Sep	—	9;26	−1 22;50	11 Sep 10;36
					ΔT 1448^y $-11^d\ 11;4^h$
Ptolemy	140 22 Mar	1	21 Mar $23;25^h$	$+1\ 14;45^h$	23 Mar $14;10^h$
Tycho	1588 9 Mar	—	20;45	+2 4;30	12 Mar 1;15
					ΔT 1448^y $-11^d\ 12;55^h$

Here the arithmetic mean of the two deficits of ΔT from 1448 years is $11^d\ 11;59,30^h \approx 11^d\ 12;0^h$, so the deficit of the tropical year from the Julian year is $11^d\ 12;0^h/1448 = 0;11,26,11^h$. Now we take the arithmetic mean of the two means just found:

Hipparchus–Tycho	$0;10,44,\ 8^h$
Ptolemy–Tycho	$0;11,26,11^h$
Arithmetic mean	$0;11,\ 5,\ 9,30^h \approx 0;11,5,10^h$

And since there is no sensible motion of the Sun in $0;0,0,10^h$, we round to $0;11,5^h$. The length of the tropical year is therefore $365^d\,6^h - 0;11,5^h = 365^d 5;48,55^h$. This exceeds Tycho's tropical year by ten seconds, and, for all of Longomontanus's trouble, is less accurate.

Since the mean precession is $0;0,49,45^{\circ/y}$, the difference between the sidereal and tropical year, the time for the mean Sun to move through this arc, is $0;0,49,45^\circ/0;2,28^{\circ/h} = 0;20,10^h$, although Longomontanus gives $0;20,18\frac{1}{3}^h$, following very nearly from $0;0,49,45^\circ/0;2,27^{\circ/h}$ and not consistent with his own mean motion of the Sun; but the length of the sidereal year, $365^d\ 5;48,55^h + 0;20,18\frac{1}{3}^h = 365^d\ 6;9,13\frac{1}{3}^h$, is, by luck, much better than Tycho's sidereal year. The variation in the length of the tropical year, determined by the annual change of the inequality of precession, is quite small. Since the greatest annual change is $\pm 0;0,2,48^\circ$, which the mean Sun covers in $0;0,2,48^\circ/0;2,28^{\circ/h} = 0;1,8^h$, the greatest variation of the tropical year is $365^d\ 5;48,55^h \pm 0;1,8^h$, that is, the excess over 365 days is from $5;47,47^h$ to $5;50,3^h$; this contains $5;49,16^h$ of the *Alfonsine* and *Prutenic Tables*, but is far short of Ptolemy's $5;55,12^h$. Finally, in the tables of the mean motion of the Sun, from the difference of the mean motions in longitude and anomaly, the apogee has a tropical motion of $1;42,59^\circ$ per Julian century or $0;1,1,47^{\circ/y}$. Subtracting the mean precession of $0;0,49,45^{\circ/y}$, the sidereal motion of the apogee is about $+12''/^{y}$ direct, differing notably from Tycho's $-6''/^{y}$ retrograde.

Longomontanus, as noted, attempted to do what Copernicus earlier attempted and Tycho intended, to derive a theory of the Sun, precession, and obliquity correct for all times. He believed himself to be highly critical of ancient observations and theory, and he was, which Copernicus was not, but he too had no choice but to use at least the observations, with corrections if necessary, in order to achieve his goal. His work is characterized by acute insights, as that Hipparchus's tropical year was derived from a luni-solar cycle rather than from observations of the Sun alone, which at best served for confirmation of the year derived from the cycle. But also wishful thinking, as the "perfect" parameters for solar theory, precession, and obliquity, even the model for precession itself, and carelessness, as his corrections and even selections of ancient observations; and his computations are all too often at least slightly inaccurate, as is also true in other parts of his work. Nevertheless, he does show that one can be aware of the problems of ancient observations, but attempt to correct and make use of them, and in this way he goes beyond what Copernicus did and what Tycho was willing to do. Had Tycho carried through his reform of astronomy for all ages, he too may have done much the same thing, but it is more likely that he would have done nothing and left the work to an assistant, Longomontanus if he returned to Tycho's service, or Kepler if he were willing to follow Tycho's orders. But when Kepler did address these problems, Tycho was long gone, and thus he pursued them in his own way, more ingeniously than Tycho but more cautiously than Longomontanus. It is to Kepler's investigations over a period of twenty years to which we now turn.

Johannes Kepler

Kepler was already concerned about the reliability of Ptolemy's observations when he wrote the *Astronomia nova*, the last two chapters of which (69–70) are devoted to attempting to correct them in order to establish accurate ancient positions of Mars for determining its mean motion and the motions of its aphelion and nodes. He assumes, reasonably, that Ptolemy observed Mars by measuring its distance from fixed stars with the armillary, although Ptolemy gives no details for the three oppositions, and only for his one observation outside of opposition does he give the distance from a star and the Moon. Since the armillary is aligned by setting it on the Sun or on a star, the longitude of which depends upon an earlier alignment on the Sun, it is necessary to investigate Ptolemy's solar theory. He is suspicious of Ptolemy's procedures for establishing solar theory, and is uncertain whether Ptolemy found the equinoxes using an equatorial ring or, as he would prefer, meridian altitudes. But he has determined that Ptolemy's equinoxes do not agree within a day and a half in comparison with earlier observations of Hipparchus and later observations of al-Battānī and Tycho, which all agree in the same uniformity from which Ptolemy's equinoxes alone depart. Thus, he has isolated the errors in Ptolemy's equinoxes by comparison with observations consistent with a uniform tropical year, and he specifically rejects models for a nonuniform precession, which would produce a nonuniform tropical year. But then he notes that if instrumental error made the vernal equinox late, meaning that the equator is placed too high, it would make the autumnal equinox early, and if two days were subtracted from the interval between the equinoxes, the eccentricity of the Sun would change greatly. And since Ptolemy left the eccentricity as great as Hipparchus found, we must believe that he correctly observed the time the Sun was at the beginning of Aries.

However, the constancy of the solar equations found in our age by Tycho, and about the same several centuries earlier by al-Battānī and az-Zarqāl, 20′ smaller than Hipparchus seems to have demonstrated for himself and Ptolemy retained, argues that the equations were the same in Ptolemy's age and his own equation in error. Since the equation is sensitive to small changes in the times of the observations, and the ancient observations, especially of the solstices, were not sufficiently accurate, we may use the modern equations to correct Ptolemy's equinoxes, not by over a day, but by correcting the time of day, making the vernal 8 (text: 3) hours later and the autumnal as many hours earlier, so that in both there was an error of 8′ in the declination of the Sun, for Ptolemy's instruments were surely graduated only to 10′.[18] And a change of a quarter of a day in the time of the solstice, which is easily possible because of its uncertainty, would produce a large change of 8° in the direction of the apsidal line. Thus, we see that while Kepler recognizes the possibility of large errors in Ptolemy's equinoxes, like Tycho, he is not willing to believe that he could go so wrong, and instead makes smaller corrections by applying the modern eccentricity, which does show that he considers the eccentricity, as well as the tropical year, to be constant. He then attempts to correct Ptolemy's longitudes of Mars by making a variety of assumptions about the eccentricity and apsidal longitude of the Sun and the longitudes of the fixed stars, by which he means the observed longitude of Mars since its longitude was measured by setting the armillary on some star. The investigation of seven different cases is, to say the least, bewildering, and he finds that changes in the longitude of stars, that is, of Mars, make a greater difference than changes in the solar theory. He also examines, critically, Ptolemy's report of an occultation, or contact, of β Scorpii by Mars on −271 18 Jan at dawn, which he decides applies better to ν Scorpii, and the report by Aristotle in *De caelo* 2.12 of an occultation of Mars by the dark part of the half-Moon, which he dates to −356 4 May (the text reads 4 April).[19]

Although the investigation of Ptolemy's solar observations and theory is inconclusive, Kepler does take seriously the decrease of the obliquity of the ecliptic and the variation of the latitude of fixed stars. Tycho had found that, compared to the time of Ptolemy, for stars located near the solstices, near summer solstice latitudes of northern stars increased and of southern stars decreased, near winter solstice latitudes of northern stars decreased and of southern stars increased, and these variations diminished approaching the equinoxes, where there were no changes. In the correspondence with Scaliger, he accounted for both the decrease of the obliquity and the variation of the latitude of stars by a variation of the obliquity of the ecliptic over a range of about 20′ with respect to a fixed celestial equator and sphere of fixed stars, like the variation of the inclination of the Moon's orbit to the ecliptic in his lunar theory of about the same range and by essentially the same hypothesis. This would indeed produce both effects although, as we have noted, the shift of the equinoxes as the Sun crosses the equator at points moving successively westward, producing a rotation of the ecliptic along the equator corresponding to the regression of the nodes in the lunar theory, would also cause a second, unwanted and incorrect, variation in the latitude of stars.

Kepler's hypothesis to account for both the decrease of obliquity and the variation of latitude of stars is entirely different, and avoids the problems of Tycho's. It is part of his theory of planetary latitude, and also accounts for the change of extreme latitudes and regression of the nodes of Mars and, in principle, the other planets since Ptolemy (68).[20] It consists of a rotation of the "true ecliptic," defined by the motion of the Earth about the Sun, not along the celestial equator, which causes the problems of Tycho's hypothesis, but along the "mean ecliptic," also called the "royal road" and "royal circle," defined by the plane of the equator of the rotating Sun. Although either direction is possible, he believes it more likely that the rotation takes place to the west, that is, the nodes and limits of the true ecliptic regress in longitude as do the lunar nodes and limits, but very slowly, with a period of many thousands of years. The effect on the latitude of stars is shown in Fig. 4, in which the mean ecliptic is in the equatorial plane of the Sun *S* and the true ecliptic shifts from the position "ecl 1" to "ecl 2", shown by the westward shift of the nodal line ☋ ☊ from 1 to 2. The nodal line is directed to the vicinity of the summer solstice ♋ and winter solstice ♑, the limits are near the vernal equinox ♈ and autumnal equinox ♎,

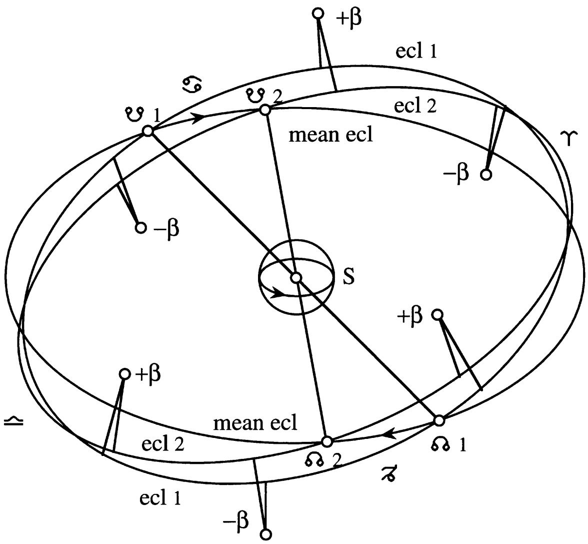

Fig. 4 Kepler. Rotation of true ecliptic to account for variation in latitude of stars

although these locations will change slowly with time. As shown in the figure, the latitudes of northern stars $+\beta$ and southern stars $-\beta$ change as Tycho found near the solstices, the nodes, where the true ecliptic is most inclined to the mean ecliptic, and the changes are small or zero near the equinoxes, the limits, where the true ecliptic is parallel to the mean ecliptic. The very same motion of the true ecliptic accounts for the variation of the obliquity. According to Kepler, the reason for the decrease of the obliquity from $23;51\frac{1}{2}^{\circ}$ in antiquity to $23;31\frac{1}{2}^{\circ}$ at present, the range of $20'$ recognized by Tycho, is that the Earth's equator holds a fixed inclination, not to the true, but to the mean ecliptic; consequently the inclination varies with respect to the moving true ecliptic, and this motion of the true ecliptic is also the cause of an inequality in the precession of the equinoxes. In Fig. 5, the initial intersections of the equator and true ecliptic, the equinoxes, are ♈$_1$ and ♎$_1$, and the solstices are ♋$_1$ and ♑$_1$. As the nodal line ☋ ☊ of the true ecliptic shifts westward from 1 to 2, so do the true equinoxes to ♈$_2$ and ♎$_2$ and the true solstices to ♋$_2$ and ♑$_2$, although nonuniformly because of the obliquity of the true ecliptic, which the same motion causes to decrease from ε_1 to ε_2.

The theory is not worked out quantitatively or in detail, but it is clear that only an inequality of the precession could result, not the mean precession itself, which must be due to a motion of the Earth's axis, because, compared to the precession, the motion of the true ecliptic is very slow, according to Kepler's speculation, none other than the sidereal motion of the Earth's apsidal line, although he later decided that it is independent but still of very long period. Nevertheless, Kepler is on to something important and entirely original. Attributing the change in latitude of fixed stars and the decrease of the obliquity of the ecliptic to a rotation of the ecliptic, of the Earth's orbit around the Sun, is essentially correct, although the rotation, produced by planetary perturbations, does not take place in a fixed plane of the solar equator and is more irregular. So while the (Newtonian) celestial mechanics of these motions is more complicated, Kepler here devised the first even remotely

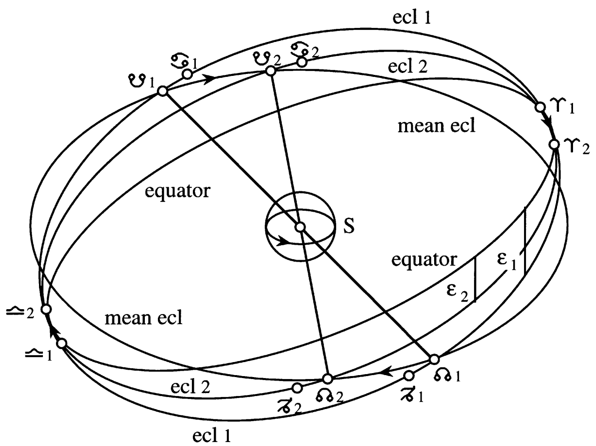

Fig. 5 Kepler. Rotation of true ecliptic to account for variation of obliquity of the ecliptic

correct model for secular changes in the orbit of the Earth, and likewise of the other planets since such motions are not unique to the Earth.

In the *Epitome of Copernican Astronomy* 7, Kepler quantifies this model. In Fig. 6a, $\bar{E}$ is the pole of the mean ecliptic, and thus of the solar equator, E the pole of the true ecliptic, which moves about $\bar{E}$ in a small circle through ϑ, and N the pole of the celestial equator, which moves about $\bar{E}$ through the mean precession, both motions in the direction of decreasing longitude, from east to west. The motion of E about $\bar{E}$ corresponds to the rotation of the true ecliptic in Figs. 4 and 5, which show great circles a quadrant from the poles, and is a geometrical result of the physical causes moving the Earth about the Sun just as the motion of N is a geometrical result of the precessional motion of the Earth's axis. Note that this differs from Longomontanus's model in Fig. 2, in which the motion of the pole of the true ecliptic E in the small circle about the pole of the mean ecliptic $\bar{E}$ produces, not a rotation of the true ecliptic with respect to the mean ecliptic, but a motion of each point of the true ecliptic in a circle equal to the radius of the small circle. The mean obliquity $\bar{\varepsilon} = \bar{E}N = 24;17,40°$ and the radius $r = \bar{E}E = 1;47,40°$; hence the true obliquity $\varepsilon = EN$ varies from 22;30° to 26;5,20°, a very wide range. This implies an inclination of the solar equator to the ecliptic of 1;47,40°; correctly, as later found from the motion of Sunspots, it is 7;15°. And the maximum equation of precession, where $\bar{E}N$ and EN extended meet the true ecliptic (not shown), is $\sin^{-1}(\cot\bar{\varepsilon}\sin r) = 3;58,45°$ (corr. 3;58,40°). But, Kepler says, half the period is more than 36,000 years, and E was at E_{o}, with the obliquity at its mean value, "at the beginning of the world." When would this be? Although no date is given in the *Epitome*, in the 1621 edition of the *Mysterium Cosmographicum* (23) he takes the evening of 24 July 3993 BC in Chaldea as the beginning of the second day, when God created the firmament. Thus, only about 5600 years have elapsed, and the pole of the true ecliptic has not moved all that far; in the *Rudolphine Tables*, from the Creation to 1600 the motion is less than 26° and the entire period just over 77,758 years, far longer than the precession with a period of about 25,412 years. He notes that the ratio of the motion of the pole of the ecliptic ϑ to the motion of the pole of the world, the mean precession $\bar{\pi}$, is fairly precisely as 4/3, a perfect fourth, although that is not mentioned. In fact it is the sum $(\bar{\pi} + \vartheta))/\bar{\pi} \approx 4/3.01 \approx 4/3$.

The model described here is the second, "entirely archetypal," of no less than five for the variation of the obliquity and the inequality of precession in the *Rudolphine Tables*. Three are "mixed," partly archetypal and partly observational, for $\bar{\varepsilon}$ and r and the periods and epochs of ϑ. The first, based "entirely upon trust of the ancient observations," has a smaller range of the obliquity of 23;28,28° to 23;53,16°, close to Copernicus's 23;28° to 23;52°—from $\bar{\varepsilon} = 23;40,55°$ and $r = 0;12,24°$—a maximum equation of precession of 0;30,31°, and a period of just 2665 years. These numbers are not consistent, and there are other inconsistencies among the different methods, as computing the obliquity for motion of E on the small circle, which is strictly correct, or for a libration on the diameter, as in Copernicus's model, in which it is the pole of the equator that librates. The libration, however, is intended only as a simpler, approximate computation as it would not produce the rotation of the true ecliptic that is essential to the model. Nevertheless, all the models show that

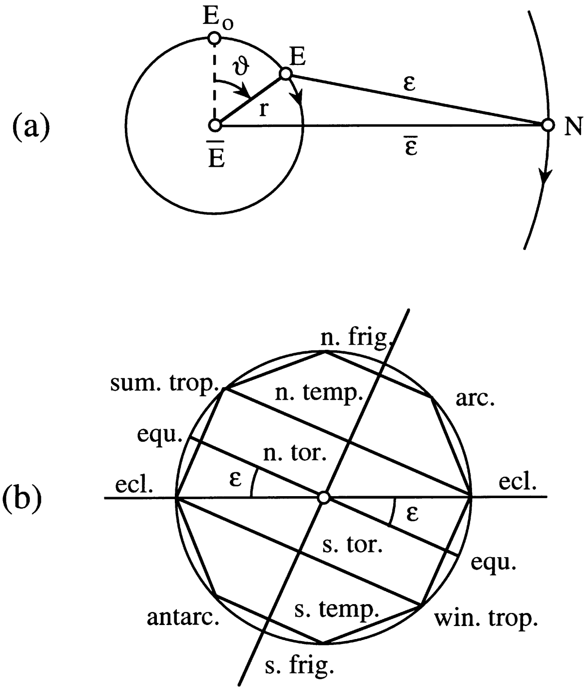

Fig. 6 Kepler. (**a**) Motions of the poles of the true ecliptic and equator about the pole of the mean ecliptic. (**b**) Climate zones of the earth at the minimum obliquity of the ecliptic

a variation of the obliquity and an inequality of precession are still a part of Kepler's astronomy.

In fact, Kepler established the "archetypal" variation of the obliquity from its consequences for climate zones of the Earth (*Epitome* 3.4), shown in Fig. 6b. With the minimum obliquity $\varepsilon = 22;30°$, the Earth is divided by sides of an octagon subtending $2\varepsilon = 45°$, that is, the torrid zone between the tropics subtends two sides, the two frigid zones beyond the arctic circles subtend two sides, and the two temperate zones in between subtend four sides, making eight sides of 45°. And at Creation, with the mean obliquity $\bar{\varepsilon} = 24;17,40°$, the sum of the surface areas of the torrid and frigid zones equals the surface areas of the temperate zones. Considering a hemisphere on one side of the equator, the area of the torrid zone is as $\sin \bar{\varepsilon} = 0.4114$, of the frigid zone as $1 - \cos \bar{\varepsilon} = 0.0886$; their sum is 0.5 and the remaining 0.5 from 1 is as the equal area of the temperate zone. Since the same relation holds for the other hemisphere, just as Kepler says, the sum of the areas of the torrid zone and two frigid zones equals the sum of the areas of the two temperate zones. Interestingly, this clever idea follows from Pappus's theorem (*Collection* 5.36), which Kepler also uses for summing the increments of libration in his physical planetary theory.

We have digressed from our principal subject of examinations of Ptolemy's solar observations and theory. In the *Astronomia nova*, Kepler recognized the possibility of large errors in the observations, but made only small corrections and substituted the modern eccentricity and equation in the solar theory. Some years later, he decided that Ptolemy's equinoxes could not be corrected so easily, but remained uncertain about the cause of the errors. In a series of manuscript notes concerning the obliquity of the ecliptic, dates of equinoxes and solstices, and length of the tropical year, he considered evidence and reports from antiquity, in fact from Hercules, the founder of the Olympiads, who observed the solstices and equinoxes at eight degrees of their signs 1260 years before Christ, through Meton and Euctemon, Hipparchus and Ptolemy, al-Battānī and az-Zarqāl, to Regiomontanus, Walther, Copernicus, and Tycho.[21] Like Longomontanus, he believes that Hipparchus and also Ptolemy observed the equinoxes with an armillary, so the stated times are those observed, not interpolated, and could be affected by refraction and misalignment of the instrument. There are two remarks we shall quote here. The first is (21.1.316): "Since Hipparchus varied so much (by quarter days) in the autumnal equinoxes, is it believable that Ptolemy found nothing clearly which differed from the Hipparchan computation? Or did Hipparchus reach his goal unknowing (*caecus*, blind), with fortune as his guide? Or should we rather believe Ptolemy favorable to Hipparchus through trust in the observations, namely, (because) something of Pythagorean philosophy lay hidden in the mystic numbers 94;30, 92;30, 178;15 (in margin: 378, 370, 713)? I note also that the year does not so precisely fill this number of hours." That the intervals in days between the equinoxes and solstices, multiplied to integers (of quarter-days) in the margin, are based upon Pythagorean philosophy can hardly be taken seriously, and Kepler poses it only as a question (to which the answer is surely no). The second remark is one that has defined the problem of Ptolemy's equinoxes to this day (21.1.324): "It therefore remains that either Ptolemy committed fraud with fabricated observations, or from a kind of awe and reverence for the ancients preferred to confirm rather than refute them, neither of which is likely in the philosopher Ptolemy, a defender of candor and truth, as is witnessed by many judgments (*gnomis*), especially since he could expect no advantage or fame from this, but rather greater advantage and fame from correcting the ancients. But that he was not obsequious to the ancients, he left witnessed in many ways, refuting Hipparchus where it was required. Therefore in fact the year was longer." This last appears to hold that the observations were correct and the year in fact longer, but is probably just speculation and not an opinion Kepler held.

In the *Epitome of Copernican Astronomy* 7, he is more certain of the error in the equinoxes. He notes (7.523) that in the eleven or twelve centuries since Proclus, the equinoctial points have precessed at a uniform rate, in which the observations of Hipparchus and Timocharis also agree for eighteen centuries "if you disregard Ptolemy alone." "Therefore, if something happened to the axis of the Earth by which it moved irregularly away from its proper position, it occurred between Hipparchus and Ptolemy, in an interval shorter than 300 years, and it was restored to its former state between Ptolemy and Proclus, again in an interval of three centuries. Therefore, not unjustly can there be doubt concerning Ptolemy's observations of the equinoxes." He also notes (7.527) that Hipparchus determined the length of 300 years by omitting one day in four Callippic Cycles of 304 years, the same explanation given by Longomontanus and, as it appears here, perhaps originally by

Tycho. "Ptolemy retained this opinion of Hipparchus, much too carelessly, as was evident to Tycho Brahe, even though Ptolemy himself appeared to prove it with his observations. For immediately after Ptolemy, it (the length of the year) was found to lose one day far more rapidly (than in 300 years). And thus if we disregard Ptolemy alone, a uniform reckoning (of the year) will be consistent from Hipparchus, through Proclus, al-Battānī, Persians, Arabs, Jews, Germans, up to our own time, which makes the equinoxes earlier by one day in 134 years, 3 days in 400 years, as the regulation of the Gregorian civil year represents very nearly."

He now has a new speculation for the cause of Ptolemy's errors, which we give in the question and answer form of the *Epitome* (7.523–24).

> Is it possible that Ptolemy was in error concerning the observation of the correct day of the equinox, and in what way?
>
> He was not in error in the altitude of the pole, as this is confirmed by many proofs, nor in the altitude of the Sun as this depends upon the altitude of the pole. Perhaps, therefore, what follows happened to him, that since under Augustus the observation of the Egyptian year was abolished, Ptolemy sought the day of the Egyptian year through the Moon if he was concerned with the Moon, or through the Sun and its calculation handed down by Hipparchus if he was concerned with observation of the Sun; then neglecting agreement with observations of the Moon and trusting too much in the calculation of Hipparchus, he thought to himself that it was only necessary to be concerned about the hour of the entry into Aries. For Ptolemy could not trust the Roman calendar, which was necessarily observed in Egypt, because even after the correction of Augustus, at some time on the authority of the priests one day was omitted from the year and restored in the following year.

The point here is that because of arbitrary omissions and restorations by the priests in the Roman calendar, in use in Egypt since introduced by Augustus, and even after Augustus's correction of the initial errors of intercalation following the Julian reform (three incorrect additional leap years, compensated by making three following leap years common years), Ptolemy would use the computation of the Moon or Hipparchus's computation of the Sun to determine the date of an observation in the Egyptian year. Thus, he would determine only the hour of an equinox by observation and trust Hipparchus's solar theory to determine the day, since he could not believe it to be in error by a full day, and then fail to check the position of the Moon, which would immediately show the error of one day since the Moon moves about 13° per day. This ingenious explanation would then account for the errors in the dates of Ptolemy's equinoxes without accusing him of fabricating observations, although it is evident that he should have been more careful.

But is there evidence for the omission or addition of days in the Roman calendar, and could the Roman calendar in fact be the cause of Ptolemy's errors without considering computation from Hipparchus's theory? This is what Kepler believed he found by 1622, the year after the publication of *Epitome* 7, as he explains in a memoir called "Against the nonuniform precession of the equinoxes" addressed to

Emperor Ferdinand II.[22] He enlarges upon, and indeed contradicts, his speculation in the *Epitome*, and is now more specific (20.1.134–35).

> I reasoned that Ptolemy, an inhabitant of Egypt, was deceived by the Roman calendar and by the license of the priests and impetuosity of the rulers of Egypt, who intercalated at Rome, not as heaven required, but just as they were incited (to do), this way and that, in accordance with some national superstition; indeed those (priests and rulers) entirely annulled the perfectly equal Egyptian months and introduced the Roman calendar into public use. And thus the annual calendars were not computed locally, but were sent from Rome, not the least instance of servitude. Of this not unsuitable conjecture, there was lacking only historical testimony, by which it would be confirmed, that in the year 139 after Christ or the previous year, one day had been removed out of order. But behold the very thing, unless all sound reason deserts me. For in the year 139 after Christ, Antoninus Pius II and Bruttius Praesens Consuls, Censorinus, the most scrupulous and careful reckoner of chronology, attributes the first of the Egyptian month Thoth to the twelfth day before the Kalends of August or 21 July, which observed in regular order, as elsewhere Censorinus preserves, ought to be attributed to the thirteenth day before the Kalends of August or 20 July, unless a day was removed out of order and the days of the Roman year occurred earlier.
>
> One may not doubt that Ptolemy, since he had not given attention to what Censorinus gave attention to, that an omission (of one day) had been made out of order, believed that with the twelfth day before the Kalends of August (21 July), which day was then observed in Rome, there still coincided, as before, the second day of Thoth, which nevertheless was (because of the omission of one day) the first day of Thoth, and that it (the first day of Thoth) ought from the perpetual reckoning of years be called the thirteenth day before the Kalends or 20 July. In this way a superfluous day insinuated itself into his calculation between Hipparchus and his own age and produced a longer year and a slower motion of the Sun than are correct.

What Kepler is referring to is one of the most famous passages in ancient chronology, the pertinence of which he appears to have discovered only recently, Censorinus, *De die natali* 21.10: "But of these (Egyptian years), the beginnings are always taken from the first day of its month the name of which among the Egyptians is Thoth, and which in this year (238) was the seventh day before the Kalends of July (25 Jun) although one hundred years ago (139), Emperor Antoninus Pius II and Bruttius Praesens consuls at Rome, the same day was the twelfth (corr. thirteenth) day before the Kalends of August (21 July, corr. 20 July), at which time Canicula is accustomed to make its rising in Egypt." Censorinus here gives the Roman calendar date of the beginning of a Sothic Cycle, when the (nominal) heliacal rising of Sirius in Lower Egypt occurs on 1 Thoth in the Egyptian calendar, in the year of the consulship of Emperor Antoninus Pius for the second time and of Caius Bruttius Praesens, 139, which date appears as *ante diem XII Kal. Aug.* (21 July). This 1 Thoth was the beginning of Antoninus 3, the year in which Ptolemy observed both equinoxes and the summer solstice about one day late, as Kepler knows. Now, correctly 1 Thoth was on *ante diem XIII Kal. Aug.* (20 July), a simple enough emendation of XII to XIII in the text of Censorinus, which was made by Scaliger, as Kepler soon learned or perhaps already knew. But here he takes XII as the correct reading and explains that in the year 139 the priests in Rome omitted one day "out of order" (*extra ordinem*) so 1 Thoth occurred on XII Kal. Aug. instead of XIII Kal. Aug., which Ptolemy in Egypt had not noticed, not "given attention to" (*attenderet*), meaning that he did not know it. How this affects the conversion between the Roman and Egyptian calendars will be taken up after reviewing Kepler's later consideration of this subject.

For it comes up again in a letter of 8/18 February 1624 to Paul Crüger as part of a description of the configuration of the heavens at Creation, which we shall also consider below. Kepler notes that the observations cited in the *Almagest* in the calendar of Dionysius appear three or four days early, and suggests that it was difficult for Hipparchus to convert those dates to the Egyptian calendar without error. He then continues (18.157):

> But also in the case of Ptolemy, I think that the three cardinal points, two observations of Venus, and one of Mercury all correspond to the preceding days. Unless there were observations of the Moon, which do not allow a day to pass unnoticed, I believe that many of the preceding (observations) are to be placed back to earlier days on account of what Censorinus observes, that in the first (corr. third) year of Antoninus, the first day of Thoth was not on 20 July but on 21 July, from which you will gather that a displacement of the Roman year was made for the sake of superstition or flattery, as was sometime done earlier by the testimony of Dio, with a restitution made in the following year. Therefore, if this displacement was announced in Egypt and received in use there after the last observation of the Moon, since already the use of the Egyptian year was abolished, Ptolemy could be deceived.

Now Kepler does not specify whether a day had been removed from or added to the Roman calendar, only that there had been a displacement (*luxatio*), and the single example he cites, from Cassius Dio (48.33), of one day added and later subtracted, was around 41 BC, when intercalation was irregular, nowhere near the age of the Antonines. Crüger must have pointed out to Kepler that the text of Censorinus is in error and need only be emended from XII to XIII for the correct correspondence of the Roman and Egyptian calendars, a conclusion Crüger seems to have reached on his own. Kepler's reply, in a letter of 1 May 1626 (18.264), shows that he already knew this, but would prefer not to accept it as the alternatives are either accusing Ptolemy of fraud or proposing a long-period inequality of the Sun not supported by observation.

> Joseph Scaliger warned me of the passage from Censorinus, and he, as you, identifies it as an error.[23] If I could excuse Ptolemy so that I were not compelled to accuse him of fraud, I would congratulate myself. But if

> the probability of error in Censorinus will melt this buttery support for me (*colloquabit mihi hoc fulcrum butyraceum*), I shall have recourse to secular equations, to similar proofs (*experimenta*) in all the planets; in the Sun itself, which, through eclipses of the Moon, is found to progress nonuniformly with respect to the fixed stars (only) in minutes of arc, which I prove by four or five eclipses of the Moon.

The "buttery support" is the text of Censorinus, but only if XII be considered correct. And this Kepler does not give up, although he does change his interpretation of just what happened to the Roman calendar that led Ptolemy astray. His latest explanation, which may have been written before the preceding letter, is in the *Rudolphine Tables*, in the rule (188) for finding the day of any year on which the equinox occurs. After explaining the rule and giving a computed example for what he calls Hipparchus's vernal equinox of 147 BC, that is, −146 Mar 24—Ptolemy does not record that equinox although he does record Hipparchus's autumnal equinox (5) of −146 Sep 26/27 and vernal equinox (6) of −145 Mar 24—he continues (10.238):

> Caution: The days of the equinoxes are not in every case shown by this rule, as for example those Ptolemy asserts were observed by himself. Consequently, in this case, however much the equinoxes differ, either in time among themselves or from the uniform precession, we should in no way be influenced by the authority of Ptolemy, who appears to have been altogether mistaken in reckoning the days of the Egyptian year, perhaps mislead either by Hipparchus's calculation of the motion of the Sun or by the calendar and the Roman intercalation. This conjecture is confirmed by one passage of Censorinus, who—in that very year (139) in which Ptolemy last observed the Moon, and after that, when an extraordinary Roman intercalation had just been announced in Egypt, observed both equinoxes—refers the first day of the first Egyptian month Thoth to the twelfth day before the Kalends of August (21 July) although it should be assigned to the thirteenth day (20 July) if the same uniformity of Julian intercalation was observed then as now and no extraordinary intercalation was revealed that year by the Priests.
>
> But if the opinion of Ptolemy's care is higher than (allows) that he could be deluded by either calculation or the Roman year, one will have to have recourse to the desperate measure of saying that around the time of Ptolemy the equinoxes made a leap (forward in time), which they compensated in the next centuries up to the time of Proclus. And in fact, I prove from the most secure examples of observations of eclipses that the progress of the Sun with respect to the fixed stars themselves is near the least degree nonuniform. If God wills, I will publish one book on this subject.

An additional inequality in the motion of the Sun, also referred to in the letter to Crüger, must be very small, for if it were large enough to change the time of the equinox by one day, about 1° in longitude, it would affect the times of lunar eclipses by about 2 hours, which is ruled out by the records of ancient eclipses, including those observed by Ptolemy. Hence the motion of the Sun is "near the least degree nonuniform" (*circa minima inaequalem*), that is, very nearly uniform. Since any additional inequality in the motion of the Sun, if present at all, is too small to affect the times of equinoxes by one day, Kepler here offers two explanations for what can now only be Ptolemy's errors. The first is that he was misled (*seductus*) by Hipparchus's solar theory, as he had suggested in the *Epitome* and as Longomontanus also said although more strongly. In fact, Kepler notes in the margin: "Longomontanus (*Theor. Ast.* 1.33) said that he (Ptolemy) was not only mistaken in observing, but also clearly fabricated (*finxisse*) the observed (equinox), which he computed from Hipparchus." The other explanation is Censorinus's correspondence of 1 Thoth of Antoninus 3 in the Egyptian calendar to XII Kal. Aug. (21 July) instead of the correct XIII Kal. Aug. (20 July), but he now attributes the correspondence, not to the omission of one day, but to an "extraordinary Roman intercalation" (*intercalatione Romana extraordinaria*), an intercalation out of order, which would appear to be an *addition* of one day, made by the Priests (*Pontificibus*) in Rome, which was then announced in Egypt. The intercalation occurred after Ptolemy's latest dated lunar observation, on 25 Phamenoth (139 9 February) of the Moon near quadrature (*Almagest* 5.3), since the position of the Moon in that observation corresponds to the correct date in the Egyptian calendar. The point in either case, omission or addition, is that Ptolemy did not know that there had been a displacement in the Roman calendar, which was announced in Egypt in an annual fasti, a calendar, sent from Rome, and thus made errors in converting Roman to Egyptian dates in the year Antoninus 3 in which he observed the equinoxes and solstice.

So which is it, an omission or an addition of one day? Kepler assumes, as noted, that Ptolemy dated his observations in the Roman calendar and then converted to the Egyptian calendar without knowing that a displacement had occurred in the Roman calendar. The Egyptian calendar runs continuously with no displacement. Kepler is not concerned with the different beginning of the day in each calendar, the Julian day at midnight, the Egyptian day at the following sunrise, just with a difference of one day. Figure 7 shows the effect of the conversion in three ways. The first line is the Egyptian calendar in 139 beginning with the date of Ptolemy's latest lunar observation, 25 Phamenoth of Antoninus 2, and the next date shown

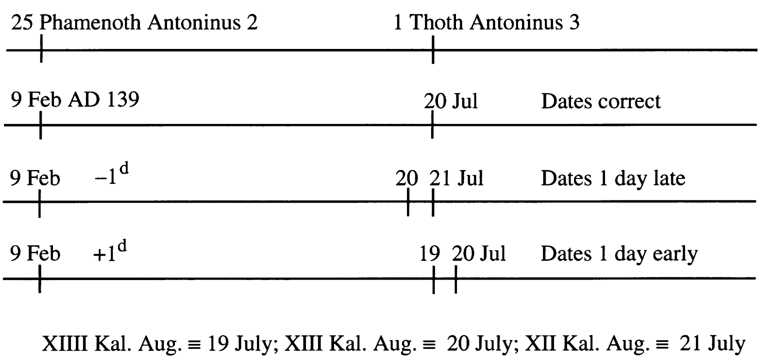

Fig. 7 Kepler and Censorinus. Conversions of Roman to Egyptian calendar in 139 for 1 Thoth

is 1 Thoth of Antoninus 3. The second line shows the correct corresponding dates in the Roman calendar, 9 February and 20 July (XIII Kal. Aug.); the conversion of 20 July to 1 Thoth is correct for the undisplaced Roman calendar. The third line with -1^{d}, as in the memoir to Ferdinand II, shows the effect of the omission of one day after 9 February, by which 20 July (XIII Kal. Aug.) occurs one day *before* 1 Thoth, which thus occurs on 21 July (XII Kal. Aug.), as in the text of Censorinus, and all following dates in the Egyptian calendar correspond to one day later in the Roman calendar. Hence, if Ptolemy did not know this, he would convert Roman dates to Egyptian dates one day later than the undisplaced conversion, and this would have the result of making the equinoxes and solstice observed in Antoninus 3 one day late in the Egyptian calendar. For example, the autumnal equinox (12) in 139 actually observed on VII Kal. Oct. (25 Sep) would, because of the omission of one day in the Roman calendar, be dated VI Kal. Oct. (26 Sep) and converted to 9 Athyr instead of 8 Athyr corresponding to VII Kal. Oct., thus one day late in the Egyptian calendar.[24] Finally, the fourth line with $+1^{d}$ shows the effect of the "extraordinary Roman intercalation" in the *Rudolphine Tables*, which appears to be an addition of one day after 9 February. Now 20 July (XIII Kal. Aug.) occurs *after* Thoth 1, which occurs on 19 July (XIIII Kal. Aug.), so this cannot account for Censorinus's conversion of 1 Thoth to XII Kal. Aug., and the conversion of all subsequent dates from the Roman to the Egyptian calendar would be one day early, not one day late. Thus, if Ptolemy were not aware of the intercalation of one day, his conversion of the dates of the equinoxes and solstice in Antoninus 3 from the Roman to the Egyptian calendar would be one day early, which is clearly not so as they are all late. The equinox observed on VII Kal. Oct. (25 Sep) would, because of the addition of one day, be dated VIII Kal. Oct. (24 Sep) and converted to 7 Athyr instead of 8 Athyr, which did not happen as the equinox is dated 9 Athyr. Hence, Kepler's explanation in the earlier memoir of an omission of one day can, in principle, explain the late dates of the equinoxes and solstice in the *Almagest*, but the addition of one day in the *Rudolphine Tables* cannot. Why he should have changed his mind about this I do not know, and it does not seem likely that by "extraordinary Roman intercalation" he still means an omission; but I can say from the effort of working it out and explaining it that, as simple as it appears, it can be confusing, and it is easy to think that adding one day to the Roman calendar will advance the date in the Egyptian calendar by one day.

Without invoking an extraordinary omission or addition of a day, it might be suggested that there was a systematic error of one day in the conversion between the Roman and Egyptian calendars for Antoninus 3, and perhaps other years, common to Censorinus and Ptolemy, as unlikely and inexplicable as that might appear, especially since they lived a hundred years apart. But even then there would be the problem that the autumnal equinox (11) of 25 September 132, used to establish the Sun's epoch (*Almagest* 3.7), which correctly occurred on 24 September and is thus also one day late, is several years before, not only the presumed extraordinary omission or addition of a day, but also five correctly dated observations of the Moon, including three eclipses. Thus, any error would have to occur intermittently. And the very idea that Ptolemy would date observations in the Roman calendar in Alexandria, which had its own Alexandrian calendar, is hardly possible.[25] So as clever as Kepler's explanation may be, and it is clever, it cannot be correct. Still, he has no doubt that Ptolemy's equinoxes are late by one day, and he later says (10.242) that all of Ptolemy's longitudes of planets are reduced by about −1;3°, which is very nearly correct since the error in the mean longitude of the Sun at the autumnal equinox (11) of 25 September 132 is −1;5°.

Kepler had a yet more ambitious reason for correcting Ptolemy's equinoxes than finding a correct and consistent solar theory. From the time of the *Mysterium Cosmographicum* (1596), and doubtless before that, he reasoned that God would not create the world with the various bodies in arbitrary positions, but must have chosen some rational initial configuration. He set out such a configuration in the *Mysterium* (23) for 27 April 3978 BC, but later changed his mind, and in a note in the 1621 edition gives the date 24 July 3993 BC, with the Sun and Moon at the beginning of Cancer near Regulus and the planets in the direction of solstices or equinoxes. After finding that Longomontanus had done something similar in *Astronomia Danica*, the Sun at apogee at the autumnal equinox in 3967 BC, he gives more details in the letter to Paul Crüger of 8/18 February 1624, without the date, but with the locations near or at the solstices and equinoxes, together with a diagram in which the locations are heliocentric (18.155–57). As we saw earlier, he notes possible errors in the conversion of dates from the Dionysian to the Egyptian calendar in ancient observations cited by Ptolemy and errors in Ptolemy's observations of equinoxes and the solstice, two observations of Mercury and one of Venus, and to explain them refers to the displacement in the Roman calendar shown by Censorinus's conversion of 1 Thoth. This shows clearly that Kepler's investigation of errors in Ptolemy's observations is related to the configuration of the heavens at Creation and thus to the date of Creation. The locations at Creation, that is, the evening of the second day (*feria secunda*, Monday) in Chaldea, when God created the firmament, 24 July 3993 BC at 0;33,26 hours after noon at Uraniborg, are finally set out in the *Rudolphine Tables*. As examples of summing mean motions, he computes the mean heliocentric longitude of each planet, the longitude of its aphelion and ascending node, and the equivalent geocentric longitudes of the Sun and Moon (10.121–23), which he then places in the tables of epochs. And for each, he asks "What if?" (*Quid si*), and gives the locations exactly at the equinoxes and solstices according to God's plan. The computed and intended locations are as follows:

	Computed			*What if? (Quid si)*		
Planet	$\bar{\lambda}$	λ_A	☊	$\bar{\lambda}$	λ_A	☊
Saturn	♎ 5;29,57°	♌ 28;24, 6°	♈ 0°	♎ 0°	♎ 0°	♈ 0°
Jupiter	♑ 7; 3,21	♋ 23;34,18	♋ 0	♑ 0	♋ 0	♋ 0
Mars	♋ 10;43,52	♉ 15	♓ 15	♋ 0	♈/♋ 0	♈ 0
Sun (Earth)	♋ 0	♈ 0; 0, 1	—	♋ 0	♈ 0	—
Venus	♋ 0	♎ 0	♈ 0	♋ 0	♎ 0	♈ 0
Mercury	♈ 0; 0, 1	♋ 0	♑ 0	♈ 0	♋ 0	♑ 0
Moon	♊ 22;57, 2	♎ 0	♋ 0;0,1	♋ 0	♎ 0	♋ 0

What Kepler wishes to find is the mean longitude of every body and the longitude of aphelia (apogees for Sun and Moon) and nodes at an equinox or solstice at some date close to Creation according to scriptural chronology. Since there are four possible locations, all mean longitudes near, if not exactly at, equinoxes and solstices must occur periodically, and over a period of thousands of years very small changes in mean motion in longitude can place each body exactly where required. But the aphelia and nodes move so slowly that these locations occur very infrequently, although a surprisingly large number of the computed positions are already at them. Presumably the exact date and time are determined by the Sun at the summer solstice, Cancer 0°; by remarkable luck, Venus at Cancer 0° and Mercury within 1″ of Aries 0° would seem to determine the year (in fact these are in error by several degrees). The time required for the Moon to move to Cancer 0° is half a day, Mars was at Cancer 0° 20 days, Jupiter at Capricorn 0° 85 days, and Saturn at Libra 0° 164 days earlier, so these could easily be adjusted over so long a period. But Jupiter's aphelion was at Cancer 0°, if ever, 1800 years earlier, and Saturn's aphelion will not be at Libra 0° for 1500 years. Since the aphelion of Mars is at Taurus 15°, it is uncertain whether it should be at Aries 0° or Cancer 0°; one way or the other, the motion would take 2400 years. (That the nodes are, except for Mars, at the required locations is the result of errors in their motions in the tables. For Saturn and Mercury the differences are over 60°, although Mars happens to be close to Aries 0°.)

It is here that an investigation of Ptolemy's observations is essential because errors of, in fact, many degrees in the longitudes of apsides and nodes found by Ptolemy when corrected could bring these where they belong at the date of Creation, or so Kepler could hope. And this investigation he intends to take up, for after the examples of computing the positions at Creation, he remarks (10.123): "Concerning this situation and disposition of the initial positions from which all the motions come forth, there is a large subject for philosophizing, if the proposed material is accessible. But this speculation is to be deferred until another treatise where the reasons and foundations will be set out from which the positions at the time of Ptolemy have been brought to light." Kepler is here referring to two works. The first may be one, not completed but surviving among Kepler's manuscripts, known as the "Examination of the Observations of Regiomontanus and Walther," which also considers ancient observations reported by Ptolemy, with the object of finding secular equations such that, perhaps, the bodies all could be at their required positions at Creation.[26] The second is a separate treatise on the positions at the time of Ptolemy, perhaps also on Ptolemy's observations in general with an analysis of the errors and their causes and any applicable secular equations. This would have been of interest, and with more detail than he had offered thus far. But it does not appear that the treatise was ever written, although notes in Kepler's manuscripts may have been intended or useful for it. So it is evident that, just as for Tycho and Longomontanus, Ptolemy's observations were of serious concern, presenting problems that had not been solved. And the literature of the last two hundred years shows that they are still subject to discussion and speculation, much of it merely repeating what was already written long ago, although not nearly so interesting or ingenious.

Appendix: Equinoxes and Solstices in the *Almagest*

Ptolemy cites twelve equinoxes and two solstices with specific dates and times and several others with implied dates. For those with dates and times, here numbered in chronological order, we give the observer, year, date, and time in the *Almagest* along with the date and apparent time by modern computation, the difference in time Δt, cited time minus computed time, and for the cited time the differences in declination $\Delta\delta$ and longitude $\Delta\lambda$ from the declination and longitude at the computed time. Thus, Δt, $\Delta\delta$, and $\Delta\lambda$ are the errors in time, declination, and longitude at the cited times. Ptolemy's times of earlier observations are approximate, midnight, dawn, noon, sunset, evening, but he interprets them as occurring at quarter days, even for the solstice of Meton at dawn, which we give here as 0, 6, 12, and 18 hours. Since the cited times are approximate, the errors are also approximate. The computations, of apparent (not mean) local time for the meridians of Athens (1), Rhodes (2–10), and Alexandria (11–14), are geocentric, may have an uncertainty of a few minutes, and small inequalities cause the intervals between equinoxes and the tropical year to vary slightly from year to year. Reduced to the meridian of Alexandria, (1) is $+0;25^{h}$ and (2–10) $+0;7^{h}$ later. We have used the Alcyone Ephemeris for these calculations.

No.	λ	*Obs.*	*Year*	*Date*	*Time*	*Mod. Date*	*Time*	Δt	$\Delta\delta$	$\Delta\lambda$
1	Can 0°	Met.	−431	27 Jun	6^{h}	28 Jun	$10;29^{h}$	$-28;29^{h}$	−0; 0,18°	−1; 8°
2	Lib 0	Hip.	−161	27 Sep	18	27 Sep	2;29	+15;31	−0;15,36	+0;38,49
3	Lib 0	Hip.	−158	27 Sep	6	26 Sep	19;57	+10; 3	−0;10, 7	+0;25, 8
4	Lib 0	Hip.	−157	27 Sep	6	27 Sep	1;43	+4;17	−0; 4,19	+0;10,41
5	Lib 0	Hip.	−146	27 Sep	0	26 Sep	17;49	+6; 4	−0; 6,13	+0;15,26
6	Ari 0	Hip.	−145	24 Mar	6	24 Mar	15; 1	−9; 1	−0; 8,47	−0;21,52
7	Lib 0	Hip.	−145	27 Sep	6	26 Sep	23;41	+6;19	−0; 6,21	+0;15,46
8	Lib 0	Hip.	−142	26 Sep	18	26 Sep	17; 9	+0; 51	−0; 0,51	+0; 2, 7
9	Ari 0	Hip.	−134	24 Mar	0	24 Mar	6;59	−7; 6	−0; 6,50	−0;16,58
10	Ari 0	Hip.	−127	23 Mar	18	23 Mar	23;23	−5;23	−0; 5,15	−0;13, 5
11	Lib 0	Ptol.	132	25 Sep	14	24 Sep	4;58	+33; 2	−0;33, 6	+1;22,25
12	Lib 0	Ptol.	139	26 Sep	7	24 Sep	21;44	+33;16	−0;33,18	+1;22,55
13	Ari 0	Ptol.	140	22 Mar	13	21 Mar	16;15	+20;45	+0;20,18	+0;50,31
14	Can 0	Ptol.	140	25 Jun	2	23 Jun	14; 9	+35;51	−0; 0,28	+1;25,31

Observations (2–4) were perhaps only reported by Hipparchus and not made by him at Rhodes. The time of (8), with its very small Δt, is given as "evening," which could be later than 18^{h}, but before 0^{h} at which $\Delta t = +6;51^{h}$. The negative $\Delta\delta$ in (2–10) implies that the equator was set too low, in most by about $6' \pm 2'$, and, aside from (2–3), are consistent enough to show that they must be from interpolation between measurements of meridian altitude, and not observed with an equatorial ring close to the horizon where refraction would produce a larger range or even positive values of $\Delta\delta$. Using meridian altitudes, refraction would change the times of the equinoxes by less than $\pm 0;45^{h}$, a small fraction of Δt. Ptolemy's observations were probably of meridian altitudes even though Δt is so large, since he criticizes the use of rings. Longomontanus assumes that both Hipparchus and Ptolemy observed with a ring, and attempts to correct for refraction the times of equinoxes believed to be observed close to the horizon. In fact, (6) was also observed in Alexandria with an equatorial ring, which showed the time about 5 hours later, at 11^{h}, reducing Δt to $-4;1^{\mathrm{h}}$, $\Delta\delta$ to $-0;3,55^{\circ}$, and $\Delta\lambda$ to $-0;9,44^{\circ}$.

Notes

1. This is the way Battānī was understood in Europe, by the writers we are considering, but in Chapter 52 he suggests, and appears to favor, a variable precession and tropical year although he proposes no model or parameters. There is a detailed study of this subject by Ragep (1996) and further reference in his paper in this volume.
2. In the section on each, references in parentheses are to volume and page numbers in: Brahe (1913–1929), Longomontanus (1622), and Kepler (1937– and 1858–1871). Full descriptions of these are in the bibliography.
3. The text reads Capricorn 5°. There is another error here as by Copernicus's tables in 1580 the eccentricity is about 0.03214 or 1;55,42, less than 2;5 by 0;9,18, and is never less than 0.0321 or 1;55,34; but the longitude of the apogee is 98;42°, about $3\frac{2}{3}^{\circ}$ to the east of Cancer 5°, and the *Prutenic Tables* are nearly the same.
4. How did Tycho find $\pi = 28;5\frac{1}{2}^{\circ}$? From his star catalogue, for the end of 1600 the longitude of Spica is 198;16°, and from the table of precession, for 12 Sep 1588 $\Delta\pi = -0;10,28^{\circ}$, so the longitude of Spica is 198;5,32°. Then, taking *Ptolemy's* interval from Spica to γ Arietis, $-170;0^{\circ}$, the longitude of γ Arietis and the precession $\pi = 198;5,32^{\circ} - 170;0^{\circ} = 28;5,32^{\circ} \approx 28;5,30^{\circ}$. Tycho explains that from his own interval from Spica to γ Arietis, $-170;39^{\circ}$, $\pi = 27;26^{\circ}$ (strictly 27;26,32°), but since the fixed stars do not move in relation to each other, it does not matter which interval he applies to find the *difference* of precession as long as he applies it at *both* equinoxes, and Ptolemy's interval from Spica is consistent with the interval from Regulus. This is correct since what Tycho finds is $\Delta\bar{\lambda}_{\mathrm{s}}^{*} = \Delta\bar{\lambda}_{\mathrm{s}} - \Delta\pi$.
5. Copernicus's longitude of Spica in 1515 is 197;14°—in fact, computed from his precession theory and altered from his original computation of 197;10°—and Tycho's 197;3,30° is his recomputation based upon the corrected latitude of Frauenburg (2.223). In 1586 he corrected Copernicus's 1525 longitude of 197;21° in the same way to 197;13,55° (10.125, text by error 53 for 13; 2.223 has 197;14°), and paired it with his own longitude of 198;4,24° for 1586, from which $\pi = \Delta\lambda/\Delta t = 0;50,29^{\circ}/61^{\mathrm{y}} = 0;0,49,39^{\circ/\mathrm{y}}$ or 1° in about $72\frac{1}{2}$ years. By modern computation, the longitude of Spica in 1515 is 197; 5° and in 1525 197;14°, close enough to Tycho's corrections.
6. This is correct, for in the late sixteenth century by Copernicus's theory the rate of precession is about 36″ per year and the tropical year $365;14,48^{\mathrm{d}}$, the same as Ptolemy found, very nearly the slowest precession and longest year, while Tycho found 51″ per year, faster by 15″, and $365;14,31,52,30^{\mathrm{d}}$, shorter by $0;6,27^{\mathrm{h}}$, which refutes Copernicus's theory. However, Tycho does note that Copernicus's mean precession of $0;0,50,12,5^{\circ/\mathrm{y}}$ differs from his by only $-0;0,0,48^{\circ}$.
7. This is the year of Jālāl ad-Dīn Malik Shāh of 1079, which Scaliger earlier favored and had learned of from Ignatius Na'matallah, Jacobite Patriarch of Antioch, then a refugee in Rome, his source for much information on eastern calendars. The year is stated in various forms, but Scaliger gives it as $365^{\mathrm{d}}\ 5^{\mathrm{h}}\ 880^{\mathrm{ch}}$, where 1 hour = 1080 chalakim, a division of the hour in the Hebrew calendar; the tropical year is thus $365^{\mathrm{d}}\ 5;48,53,20^{\mathrm{h}}$, in fact superior to the Alfonsine and differing from Tycho's by $+0;0,8,20^{\mathrm{h}}$.
8. Cited by Grafton (1993, 201), from *De emendatione temporum* (1583, 128).

9. Cynosura, literally "dog's tail," is a name for Ursa Minor, used notably by Aratus. Copernicus also uses it in his star catalogue. That Hipparchus's found Polaris 12;24° from the pole is cited by Ptolemy from Marinus in *Geography* 1.7.4. Since it was then the most distant of the stars in Ursa Minor in declination from the pole, it is called the "southernmost," which Scaliger later criticizes as an error because he believes the pole of the world is always at or near Polaris. Grafton (1993, 487), notes that "southern" was sometimes emended to "northern" in translations of the *Geography* since by the sixteenth century Polaris had become the northernmost star. He also notes (464) that in the second edition of *De emendatione temporum*, Scaliger gives the least distance as 3;24° and concludes that the pole of the equator has approached the pole star by about 9°. The present distance of less than 3° he perhaps received from Tycho, who also mentions it in his letter below.
10. It appears from examples for Regulus and η Gem that he was computing true risings, when the Sun and star cross the horizon at the same time, rather than apparent risings, which are more difficult and uncertain to compute. Nevertheless, his conclusion is correct, for by modern computation at 100-year intervals, the apparent heliacal rising of Sirius in Alexandria for -1800 to 900 is on 20 July and 1000 to 1600 on 21 July (Julian), although different methods of computation can differ slightly. Scaliger refers to acronychal risings, although perhaps not explicitly for Sirius, but these are not as constant, for -1800 to 1600 advancing from 25 December to 1 January. Tycho's statement that after about 500 years Polaris will be $27\frac{1}{2}'$ from the pole is very accurate, for its minimum distance (without nutation), will be 0;27,15° in 2102–03.
11. Scaliger's objections to the precession, including in the *Diatribe*, are treated by Grafton (1993, 459–488). The *Diatribe* was already much criticized in its day; a detailed analysis and very sharp criticism by Kepler is published by Frisch (Kepler 1858–1871, 8.273–93).
12. The Greek text has $365\frac{1}{4} + \frac{1}{76}$ days, but the fraction $\frac{1}{76}$ seems to be omitted in earlier paraphrases of this passage and accounts of the Metonic cycle. It was commonly thought that the cycle was 19 Julian years of $365\frac{1}{4}$ days, $6939\frac{3}{4}$ days, as in the ecclesiastical calendar, rather than Meton's 6940 days. It should be noted that a luni-solar calendrical cycle as applied to months must be an integral number of days since new Moons appear only in the evening separated by (nearly) integral days.
13. Longomontanus's explanation was later proposed, surely independently, by Tobias Meyer in a letter to Euler. Of course, neither knew the Babylonian origin of Hipparchus's mean synodic month. There is a rather detailed discussion of Hipparchus's tropical year and precession by Swerdlow (1980).
14. Holger Rosenkrantz (1574–1642), a friend and correspondent of Tycho's, was married to Tycho's niece, supported Tycho's claims in Denmark after he had left Hven, and doubtless knew Longomontanus well. The correspondence is published in Dreyer's edition and there is a biography by Christianson (2000, 344–346). He assembled a great library and was particularly concerned with theology, although sufficiently unorthodox and fanatical to be charged with heresy in his later years.
15. For -3963 Jan 1 noon, $\bar{\lambda}_s = 248;33,54°$, for -3966 Jan 1 noon, $\bar{\lambda}_s = 248;17,45°$. Since the equinoxes were then in the apsidal line where the equations are zero, the difference in longitude to the following vernal equinox is 111;42,15° and to the autumnal equinox 291;42,15°, for which the difference in time is $291;42,15°/0;59,8,19,45^{°/d} = 295^d\, 22;50,50^h$, that is, 23 Oct at $22;50,50^h$ from noon or 24 Oct at about 11 AM Curiously, if one takes the true longitude for -3966 Jan 1 noon, $\lambda_s = 248;17,45° + 1;55,32° = 250;13,17°$, the difference in longitude to the autumnal equinox is 289;46,43°. If one then uses the mean motion of the Sun, the difference in time is $293^d\, 23;57,39^h$, that is, 21 Oct at $23;57,39^h$ from noon or 22 Oct at about noon. This is not strictly correct, but the result would be that God created the world at about noon in Copenhagen.
16. Pole E is described as "in the surface of the globe of the Earth," but that must be only a geometrical direction as it makes no sense to give the motion of E to the Earth, and Moesgaard is surely correct in describing it as the pole of the true orbit of the Sun around the Earth, that is, of the true ecliptic itself. And the motion in the small circle produces only a motion of each point of the true ecliptic, as the true vernal equinox, in a small circle centered on the mean ecliptic,

unlike Kepler's model, described later, which produces a rotation of the entire true ecliptic with respect to the mean ecliptic. Moesgaard also notes inconsistencies in Longomontanus's table of the inequality of precession and suggests that the table was computed for an earlier model in which the inequality of precession and variation of obliquity were produced by motions of the pole of the Earth, thus of the equator with no motion of the ecliptic, as in Copernicus's model. The model does seem inconsistent, or hard to follow, and my description is only of what is supposed to result from it.

17. There is a recent study by Buchwald (2006, 635–643) of this procedure applied to Tycho's observations along with other early methods of refining measurements or computations.
18. The text, three hours, is incorrect since at the equinoxes 1^h of time corresponds to $1'$ of declination. If the maximum equation is reduced by $20'$, then with Ptolemy's apogee, the reduction at equinox is $20' \sin 65;30^\circ \approx 18'$ of longitude, corresponding to $0;24 \cdot 18' \approx 7'$ of declination and seven hours of time. Kepler must have computed $0;24 \cdot 20' = 8'$ of declination and thus eight hours of time, which would apply at 90° from apogee, not at the equinoxes.
19. By modern computation, the closest approach of Mars to β Sco was under $37'$ on 19 Jan 2^h and to ν Sco over $2'$ on 16 Jan 15^h, which is much closer. There could be an error in converting the date from the Dionysian to the Egyptian calendar, but there was no occultation of either star and nothing even close at dawn in Alexandria on any nearby date. An occultation of Mars by the Moon approaching first quarter was visible in Athens the evening of -356 4 May—from about 20^h to $21;15^h$ apparent time although the exact time varies by a few minutes depending upon the value of the secular acceleration—as Kepler surely determined correctly, and "April" must be only a transcription error since on 4 April the Moon was about 20° from Mars, which he could not have missed. In manuscript notes on the occultation (Kepler 1937–, 20.2.497–505), with 25 computations of the Sun, Moon, and Mars dated to the Foundation of Rome, AUC 380–431, the one for -356 is a fragment for AUC 395 (504–505), April completed, hence May, plus day 5, but the occultation is not noted there.
20. Here we consider the theory only geometrically; there is an explanation of the underlying physical theory of latitude by Stephenson (1987, 130–137), which we have found very helpful.
21. First published by Frisch (Kepler 1858–1871, 6.101–09) and more completely by Bialas (Kepler 1937–, 21.1.314–29), who dates the notes to ca. 1616 and December 1621. The parameters for the variation of obliquity in the *Epitome* and *Rudolphine Tables* are found in the latter part of the notes. There are related notes published by Frisch (Kepler 1858–1871, 6.78–87, 593–596) and Bialas (Kepler 1937–, 20.1.115–33), from both before and after the publication of *Epitome* 7. Placing the equinoxes and solstices at eight degrees of their signs is Babylonian and found in a number of Greek sources. That Hercules did it first is more surprising.
22. First published by Frisch (Kepler 1858–1871, 6.87–89) and then by Bialas (Kepler 1937–, 20.1.134–36), who provides the date 1622.
23. *Loci ex Censorino admonuit me Jos. Scaliger, agnoscitque pro sphalmate, ut tu*; 'admonuit me' also means 'reminded me' or 'advised me'. A. Grafton, in considering the passage, believes it refers only to Kepler's seeing the correction in *De emendatione temporum*, not to a personal communication from Scaliger since there is no evidence that Scaliger ever wrote to Kepler following Kepler's letter of May/June 1605, and Scaliger died in 1609, seventeen years before this letter to Crüger.
24. In fact, equinox (12) occurred on 24 Sep at about 22^h, earlier than the recorded 26 Sep 13^h by $1^d\,9^h$, as in the Appendix.
25. The Alexandrian calendar, introduced under Augustus in -24, uses Egyptian month names and equal months of thirty days numbered consecutively forward, with five epagomenal days in common years and a sixth in a leap year. Ptolemy uses it in the *Phases of the Fixed Stars*, and nowhere does he give a date in the Roman calendar, even when citing observations made in Rome by Menelaus. In 139, 1 Thoth of Antoninus 3 in the Egyptian calendar corresponds to 26 Epiphi of Antoninus 2, or Augustus 168, in the Alexandrian calendar.
26. First published by Frisch (Kepler 1858–1871, 6.725–74) and more recently by Bialas (Kepler 1937–, 20.1.395–455). This work is also referred to in the preface to the *Rudolphine Tables* (10.44).

Dunthorne, Mayer, and Lalande on the Secular Acceleration of the Moon

J.M. Steele

Introduction

On 19th October 1692 Edmond Halley read a paper to the Royal Society in which he claimed for the first time that the motion of the Moon was subject to a long-term acceleration. The *Journal Book of the Royal Society* notes the occasion as follows:

> October 19, 1692. Halley read a paper, wherein he endeavoured to prove that the opposition of the Medium of the Æther to the Planets passing through it, did in time become sensible. That to reconcile this retardation of the Motions the Ancients and Moderns had been forced to alter the differences of the Meridians preposterously. That Babylon was made more westerly than it ought by near half an hour, both by Ptolomaeus, and those since him. And to reconcile the Observations made by Albategnius at Antioch, and Aracla on the Euphrates, they have been forced to make these places ten degrees more Easterly, than they ought, particularly Mr. Street has made Antioch of Syria in his Table of Longitudes, and Latitudes of places half an hour more Easterly than Babylon, whereas in truth it is about 40 minutes more Westerly. That this difference is found by 4 Eclipses observ'd about the year 900 and that by an Artist not capable of mistaking, that they all 4 agree in the same result and are noe other ways to be reconciled. Hence he argued, that the Motions being retarded must necessarily conclude a finall period and that the eternity of the World was hence to be demonstrated impossible. He was ordered to prosecute this Notion, and to publish a discourse about it. (MacPike 1932, p. 229)

Halley's reasoning here is that the movement of the Sun, Moon and planets through the æther causes their motions to be retarded and that as a consequence geographical longitudes that have been derived from earlier astronomical observations are incorrect. During the following year Halley read further papers on the long-term change in length of the year and an analysis and correction of the Latin translation of al-Battānī by Plato of Tivoli.

Halley's paper on his corrections to al-Battānī was published in volume 17 of the *Philosophical Transactions of the Royal Society* (Halley 1693). It includes a detailed analysis of four eclipses, the solar eclipses of 8th August 891 and 23rd January 901 and the lunar eclipses of 23rd July 883 and 2nd August 901, but Halley remarked that Plato's translation contains too many errors to allow him to determine the parameters of the Moon's motion (Mercier 1994, pp. 194–195).

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Although Halley does not mention to issue of whether the Moon's motion is subject to a long-term acceleration in this paper, at the end of a report on the ruins of Palmyra published in volume 19 of the *Philosophical Transactions* (Halley 1695), he briefly returns to al-Battānī's observations. Halley remarks that the city of Aracta, where al-Battānī made some of his observations, is the present-day city of Racca on the Euphrates river. Halley writes:

> The Latitude thereof was observed by that *Albatâni* with great accuranteness, about eight hundred years since; and therefore I recommend it to all that are curious of such Matters, to endeavour to get some good Observation made at this Place, to determin the Height of the *Pole* there, thereby to decide the Controversie, whether there hath really been any Change in the *Axis* of the Earth, in so long an Interval; which some great Authors, of late, have been willing to suppose. And if any curious Traveller, or Merchant residing there, would please to observe, with due care, the *Phases* of the *Moons Eclipses* at *Bagdat, Aleppo* and *Alexandria*, thereby to determine their Longitudes, they could not do the Science of *Astronomy* a greater Service: For in and near these Places were made all the Observations whereby the Middle Motions of the Sun and Moon are limited: And I could then pronounce in what Proportion the *Moon's* Motion does Accelerate; which that it does, I think I can demonstrate, and shall (God willing) one day, make it appear to the Publick.

Halley's remarks are the first direct claim for the existence of a secular acceleration of the Moon. Despite his plea for better determinations of the sites of ancient observations in order to analyse ancient eclipse observations in order to derive a quantitative assessment of the Moon's secular acceleration, and his desire to make such a study public, Halley apparently did not return to the subject, at least in print.

Over the next fifty years, Halley's discovery of a secular acceleration of the Moon went largely unremarked by other astronomers. A brief comment by Isaac Newton inserted following his final remarks on comets in Proposition 42 of Book III of the *Principia* in the second edition (but removed in the third edition), credits Halley with the discovery of the secular acceleration of the Moon:

> As the body of Sun also decreases, the mean motions of the planets around the Sun are slightly delayed, and as the Earth increases, the mean motion of the Moon around the Earth is slightly advanced. And by bringing together the eclipse observations of the Babylonians and of Albategensis with those of today, our Halley showed that the mean motion of the Moon accelerates slightly by comparison with the diurnal motion of the Earth, the first of all, so far as I know, to have discovered it. (trans. Cook 1998, p. 479)

but no further details are given. Between 1749 and 1757, however, three attempts were made to confirm the existence of the secular acceleration of the Moon and to determine its magnitude. All three investigations of the secular acceleration, by Richard Dunthorne, Tobias Mayer and Jerome Lalande, used ancient and medieval records of eclipses, principally those reported by Ptolemy in his *Almagest* and the eclipses known from al-Battānī and Ibn Yūnus, to derive the secular acceleration. Following Lalande's results of 1757 an acceleration of about 10″ per century was accepted and after 1757 attention switched to trying to understand and model the physical causes of this acceleration.

In this paper I will discuss the pioneering work in using historical eclipse records to determine the secular acceleration by Dunthorne, Mayer and Lalande. Each scholar approached the task in a different manner, placing greater or less trust on the observations reported in each historical sources and using different techniques to derive the magnitude of the secular acceleration from the ancient observations.

Richard Dunthorne

Richard Dunthorne was born in 1711 at Ramsey in Huntingdonshire where his father worked as a gardener (Lynn 1905). Dunthorne attended the free grammar-school in Ramsay, where he was taught enough basic arithmetic to be able to pursue his own study of mathematics. After the completion of his school education in Ramsay, Dunthorne ran a private school in the nearby town of Alconbury, before being persuaded by Dr Roger Long, the Master of Pembroke Hall, to come to Cambridge in Long's service in order to continue his learning. Long later recommended Dunthorne for the post of Master of the free school at Coggeshall in Essex, and then brought Dunthorne back to Cambridge to act as his personal assistant and butler of Pembroke Hall (Cooper 1864). Dunthorne held this post for the remainder of his life, combining it with a role as superintendent of the Bedford Level, directing the construction of the locks on the Cam, and later in life undertook comparisons for the *Nautical Almanac*.

In 1739 Dunthorne published *The Practical Astronomy of the Moon: or, new Tables of the Moon's motions, Exactly constructed from Sir Isaac Newton's Theory, as published by Dr Gregory in his Astronomy, With Precepts for computing the Place of the Moon, and Eclipses of the luminaries*. The book contained tables for calculating the position of the Moon based upon Newton's lunar theory. Although Newton's theory had been published and described, Dunthorne claimed that no tables had previously been constructed from this theory which allowed lunar positions to be simply calculated. Dunthorne explained his motivations in a note to the reader:

> The following Tables of the lunar Motions were at first calculated purely for my own private use; but being informed by several friends who are more conversant in Books than myself, that there has not been a complete Set of such Tables framed from the Theory of Gravity, as yet published: I was willing to present you with these: They are constructed from Sir Isaac Newton's Theory, as published by Dr. Gregory in his Astronomy

Dunthorne's tables were a remarkably accurate presentation of Newton's theory (Kollerstrom 2000), but Dunthorne was well aware that there were slight discrepancies between calculated lunar positions and those determined from observation. He continued his note to the reader:

> and though I find, by comparing them with observations, that the Newtonian Numbers are a little deficient, they will (at least) have this use, that such Persons as desire further to

> rectify the Lunar Astronomy, may be being assisted with Tables already framed from nearly true Numbers, be better enabled to compare those Numbers with Observations, and by that means obtain Numbers still more exact.

Dunthorne continued this theme in a letter sent to the Rev. Charles Mason published in the *Philosophical Transactions* for 1747. Dunthorne compared several sets of solar and lunar observations by Flamstead and others reported in Flamstead's *Historia Cœlestis*, the *Philosophical Transactions* and the *Memoires de l'Académie Royal des Sciences*, finding good agreement between observation and theory after applying small corrections to some of the parameters underlying the tables. The observations Dunthorne analysed in his 1747 paper range in date from A.D. 1652 to 1732.

Dunthorne now turned his attention to earlier observations. The *Philosophical Transactions* for 1749 included an eleven page "Letter from the Rev. Mr. *Richard Dunthorne* to the Reverend Mr. *Richard Mason* F. R. S. and Keeper of the *Woodwardian Museum* at *Cambridge*, concerning the *Acceleration* of the *Moon*". Dunthorne began his paper:

> After I had compared a good Number of modern Observations made in different Situations of the Moon and her Orbit in respect of the Sun, with the *Newtonian* Thoery, as in my Letter of Nov. 4, 1746; I proceeded to examine the mean Motion of the Moon, of her Apogee, and Nodes, to see whether they were well represented by the Tables for any considerable Number of Years, and whether I should be able to make out that Acceleration of the Moon's Motion which Dr. *Halley* suspected.

Dunthorne's groundbreaking analysis of historical eclipse records in this paper provided the first proof of the existence of the secular acceleration of the Moon, and the first (and remarkably accurate) estimate of its magnitude.

Before discussing the content of Dunthorne's paper, some words concerning the title are in order. Within the twenty-nine words of the title are two printing errors. The first error is in naming Dunthorne "The Rev. Mr. Richard Dunthorne." This has given rise to many descriptions of Dunthorne as a Reverend in modern literature, but Dunthorne was never a clergyman. The second error is in naming Charles Mason as "Richard Mason." As we shall see, there is also a typographical error in the date of one of the eclipses analysed by Dunthorne in the paper.

Dunthorne proceeded by comparing observed accounts of historical eclipses with the circumstances of those eclipses as computed using his 1739 tables as corrected in his 1747 paper. He discussed six sets of eclipse observations, in roughly reverse chronological order:

(i) Several lunar eclipses observed by Tycho Brahe and reported in Tycho's *Progymnasmata*. Dunthorne finds as good agreement as could be expected given the inaccuracy of Tycho's clocks. In any case, Dunthorne remarks, Tycho's observations are too near in time to those of Flamstead to be of use in his present enquiry.

(ii) Several observations of lunar eclipses made by Bernard Walther and Regiomontanus. Dunthorne found that the computed longitudes of the Moon were generally about 5′ too great, which he believed was too large to be attributed to observational error. Accordingly, Dunthorne concluded that these observations indicated the presence of a secular acceleration of the Moon, "though the Disagreement of the Observations between themselves is too great to infer any thing from them with Certainty in so nice an Affair" (Dunthorne 1749, p. 163).

(iii) The four eclipses observed by al-Battānī. Dunthorne found that the computed longitude of the Moon was considerably too great in three of the four cases, but due to uncertainty in the geographical longitude of Aracta, he cannot claim this as proof of the presence of a lunar secular acceleration.

(iv) Two eclipses of the Sun and one of the Moon observed in Cairo by Ibn Yūnus. Dunthorne's source for these eclipses is Albertus Curtius' *Prolegomena* to his *Historia Cœlestis* containing a compilation of Tycho Brahe's observations put together by Curtius (Mercier 1994, pp. 197–198). Dunthorne finds good agreement with his tables.

(v) The solar eclipse observed by Theon of Alexandria in AD 364. Dunthorne summarizes the account of the eclipse. Interestingly, Dunthorne, without comment, says that Theon observed the eclipse to begin at 2;50 seasonal hours after noon, and ended at 4;30 seasonal hours afternoon, and converts these to 3;18 and nearly 5;15 equinoctial hours after noon respectively. Dunthorne cites the Basel edition of 1538 as the source of this record. However, the Basel edition clearly gives the observed times as in equinoctial, not seasonal hours. Fotheringham (1920), followed by most recent authors including Stephenson (1997, pp. 364–364) and Steele (2000, pp. 103–104), accepted the Basel edition without question, and noted that Theon's eclipse times seem to be in error by about half an hour. Rome (1950), however, has convincingly demonstrated that the Basel edition is in error and that the times must be in seasonal hours, for only then is there the agreement between Ptolemy and observation that Theon claims. It appears that Dunthorne must have come to the same conclusion.

(vi) The lunar eclipses recorded by Ptolemy in the *Almagest*. Dunthorne uses three of these eclipses to clearly demonstrate the presence of the secular acceleration of the Moon, and to estimate it to be equal to about 10″ per century.

Dunthorne's analysis of the lunar eclipses is fairly straightforward. He simply calculated the time of mid-eclipse using his corrected tables and compares this with the time of mid-eclipse derived from the report of the observation. The difference in time can then be converted easily into a difference in lunar longitude, and an estimate of the secular acceleration made. For the solar eclipses, however, Dunthorne knew that this simple method would not work due to the small size of the solar eclipse shadow. Therefore he devised a geometrical analysis that allowed him to derive the difference in longitude between the Sun and Moon at the moment of the eclipse maximum, which he can then compare against the same quantity calculated from his tables.

In attempting to quantify the magnitude of the secular acceleration, Dunthorne restricts himself to three solar eclipses (two observed by Ibn Yūnus and the one seen by Theon of Alexandria) and three of the *Almagest* lunar eclipses. He discusses the solar eclipses first. These eclipses, Dunthorne notes, are very valuable for his study because they were observed in locations, Cairo and Alexandria, whose geographical latitudes and longitudes have been precisely determined by M. Chazelles of the French Académie Royale des Sciences, whereas the geographical coordinates of the site of al-Battānī's observations, for example, are not accurately known. Dunthorne finds that the longitude correction needed for the two eclipses observed by Ibn Yūnus in Cairo is +8′ 45″ for the eclipse of 8 June

978 and +7′ 36″ for the eclipse of 12 December 977. For Theon's eclipse, Dunthorne finds a correction of –4′ 16″ is needed.

Dunthorne now turns to the eclipses reported in Ptolemy's *Almagest*. Being much earlier in date than the other eclipses available to Dunthorne, and hence affected to a greater extent by any secular acceleration of the Moon, these eclipses are potentially of great importance for Dunthorne's study. However, as Dunthorne notes:

> The Eclipses recorded by *Ptolemy* in his *Almagest*, are most of them so loosely described, that, if they shew us the Moon's mean Motion has been accelerated in the long Interval of Time since they happened, they are wholly incapable of shewing us, how much that Acceleration has been.

Dunthorne's conclusion that most of the *Almagest* eclipses are too imprecise to be of use in studying the long-term behaviour of the Moon has been borne out by recent studies (e.g. Steele 2000, pp. 91–103). However, Dunthorne rightly notes that three of the eclipses in the *Almagest* clearly prove the existence of the secular acceleration and provide critical constraints upon its magnitude.

The first of the *Almagest* eclipses discussed by Dunthorne is the eclipse observed in Babylon in the 366th year of Nabonassar. The Julian date of this eclipse is given in the paper as 22 December 313 B.C., instead of 22 December 383 B.C. This is not a mistake by Dunthorne (as assumed by Mercier 1994, p. 198), but simply a misprint (Britton 1992, p. 62). Full Moon in December 313 B.C. took place on the 28th, not the 22nd, thus Dunthorne could not have calculated the circumstances of an eclipse on 22 December 313 B.C. Furthermore, Dunthorne's calculated circumstances for the eclipse are what we would expect for the correct date.

Dunthorne realised that the eclipse of 22 December 383 B.C. was critical because the eclipse began just half an hour before sunrise and the Moon set eclipsed. Using his tables, however, Dunthorne computed that the Moon would have set more than an hour before the beginning of the eclipse. The visibility of the eclipse alone indicated the existence of the secular acceleration of the Moon. Dunthorne estimated that the Moon's longitude at the time of the eclipse was about 40′ or 50′ in advance of that given by his tables.

Dunthorne next discusses the eclipse observed in Alexandria on 22 September 201 B.C. From his tables, Dunthorne computed that the beginning of the eclipse was at 6 h 12 min after midnight, or about 10 min after the rising of the Moon. However, according to the account in the *Almagest* the eclipse began half an hour before the Moon rose. A correction to the calculated time of at least 10 min is needed in order that the Moon rose eclipsed, and 40 min for the beginning of the eclipse to agree with the estimated time given in the *Almagest* report. Dunthorne converts this difference in time into a difference in longitude of near 20′.

Finally, Dunthorne discusses the most ancient eclipse in the *Almagest*, observed in Babylon on 19 March 721 B.C. Comparing the computed time from his tables with the observational report, and ensuring that the eclipse began after the Moon rose, Dunthorne concludes that the correction in longitude at this epoch could be no more than about 50′.

Dunthorne's treatment of the *Almagest* eclipses demonstrates his ingenuity in this new field. He was aware that the eclipse reports were often vague, and the timings could be inaccurate, but realised that for certain eclipses the fact that the eclipse was visible at all demonstrated the presence of a secular acceleration of the Moon, and even allowed estimates of its magnitude. This approach is still one of the tools applied in using historical eclipse records in investigating the long-term changes in the Earth's rate of rotation (e.g. Stephenson 1997, pp. 76–79 and 86–89).

The importance Dunthorne places on the eclipse seen in Babylon on the night of the 22/23 December 383 B.C. is illustrative of Dunthorne's approach. This eclipse required a large secular acceleration in order to make the eclipse visible at all. Later investigators such as E. Hartwig in 1860 remarked that the secular acceleration of the Moon implied by this eclipse was incompatible to that derived from the timings of other eclipses reported in the *Almagest*. Simon Newcomb, who undertook an extremely detailed investigation of the secular acceleration in 1878, argued that the eclipse had been falsely recorded. Subsequently, Theodore von Oppolzer (1881) and E. Nevill (1906) suggested that the eclipse was seen in Athens, not Babylon, but there is no evidence in support of this interpretation (van der Waerden 1958b). More recently, this eclipse was claimed by R. R. Newton (1977) to have been faked by Ptolemy. However, recently a Babylonian account of the eclipse has come to light (Hunger 2001, No. 10). Although badly damaged this fragment does indicate that the eclipse was indeed seen in Babylon (Steele 2005). This would seem to vindicate Dunthorne's choice to rely on a few critical eclipses reports in the *Almagest* where the Moon rose or set eclipsed, rather than a larger number of imprecise and inaccurate timed reports.

One problem Dunthorne faced in his analysis of the eclipses that had been seen in Babylon was uncertainty of the geographical location of the city. Although Dunthorne could utilize Chazelles' recent measurements of the latitude and longitude of Alexandria, he had to rely on Ptolemy's statement that Babylon was 50′ in time to the east of Alexandria, or only 12.5° of longitude; the true longitude difference is about 14.5° or 58′ of time. Since Dunthorne only uses the timings of the Babylon eclipses to make a rough estimate of the magnitude of the secular acceleration, however, the error in his assumed geographical longitude of Babylon was not too significant.

At the end of his 1749 paper Dunthorne presents a table of longitude corrections to be applied to his lunar tables at one hundred year intervals, and an estimate of the value of the secular acceleration as 10″ per century. As Dunthorne notes, the aggregate longitude correction increases as the square of the time. Since the solar eclipses observed by Ibn Yūnus in A.D. 997 and 998 required a positive correction to the longitude, and that observed by Theon in A.D. 364 required a negative correction, Dunthorne estimates that the correction must be zero in about A.D. 700 (note this is not the time of Ibn Yūnus as remarked by Mercier 1994, p. 198).

Dunthorne's analysis of historical eclipse records was one of the most significant contributions to the development of lunar theory in the eighteenth century. He provided the first proof of the existence of the secular acceleration of the Moon and an estimate of its magnitude that turned out to be more or less correct. Dunthorne never returned to the question of the secular acceleration. However, his subsequent astronomical papers published in the *Philosophical Transactions*, an analysis of a set of medieval cometary observations in the hope of identifying a short period comets whose return could be predicted and the derivation of new elements for planetary tables, continued his interest in the development of accurate astronomical tables and the study of early astronomical records. Following the death of his mentor Roger Long in 1770, Dunthorne wrote the final parts of Long's *Astronomy in Five Books*. Dunthorne died on 3 March 1775 and was buried at St Benedict's Church in Cambridge.

Tobias Mayer

Tobias Mayer was born in the small town of Marbach near Stuttgart on 17 February 1723 (for detailed biographical studies, see Forbes 1967 and 1980). More or less self-taught in mathematics, Mayer published his first work, a study of analytic geometry, shortly after his eighteenth birthday. His productive career, cut short by his untimely death in 1762 at the age of thirty-nine, encompassed studies on geography and map-making, the science of artillery, the theory of the magnet, the science of colour, and astronomy. His astronomical works included the development of new techniques of stellar observation, making a map of the Moon, and the construction of lunar tables. Mayer's lunar tables were significantly more accurate than other tables available at the time and were submitted by Mayer to the British Board of Longitude as being suitable for allowing the determination of longitude at sea. Mayer's widow would eventually be awarded the sum of £3000 in recognition of his work (Forbes 1966).

Mayer's first lunar tables, published with the title "Novae Tabulae Motuum Solis et Lunae" in the *Commentarii Societatis Regiae Scientiarum Gottingensis* in 1753, are the first set of tables to include a table for the correction of the mean longitude of the Moon to take into account the secular acceleration of the Moon. Whereas Dunthorne's table of corrections to his lunar tables was constructed several years after he had published his tables, for Mayer, the secular acceleration was integral to the formation of the tables. Mayer remarked in the introduction to the tables that he has examined the ancient eclipses from Babylon and those observed by Hipparchus and Ptolemy, as well as the eclipse observations by Ibn Yūnus and al-Battānī's in order to determine the secular acceleration, and also tested his tables against the more recent observations by Tycho, Walther and Regiomontanus.

Mayer's table to correct the mean longitude of the Moon to take into account the secular acceleration of the Moon gives values of this correction in degrees, minutes and seconds at hundred year intervals between 800 B.C. and A.D. 2000.

Mayer takes the year A.D. 1700 as his epoch and the corrections are a straightforward parabolic function of time. The table as published contains a typographical error in the entry for A.D. 100, giving the correction as 0º 38′ 35″ instead of 0º 28′ 35″. The error is easily seen by inspecting the table and does not appear in the draft of the table found in his manuscript notes held at Göttingen University Library (Mayer 15_{41}, fol. 167 recto).

Mayer's table is constructed on the basis of a secular acceleration of 6.7″ per century, a value considerably lower than Dunthorne had estimated. Mayer gives no details of how he has obtained this value for the secular acceleration in his paper, but his reference to examining the eclipses reported by Ptolemy, Ibn Yūnus and al-Battānī indicates that Mayer derived his value from the same body of observations a Dunthorne had used. However, Mayer's manuscript notes (Mayer 15_{41}, fol. 156 verso) indicate that he was mistrustful of Ptolemy's account of the eclipse of 22/23 December 383 B.C., which as we have seen was crucial for Dunthorne's analysis. Mayer remarks that the account of this eclipse should be examined in detail, especially its duration, which is crucial for whether the eclipse could have been seen before the Moon set. However, Mayer does not appear to have returned to this eclipse, and does not include it in the list of Ptolemy's eclipses that he has analysed on Mayer 15_{41}, fol. 163 verso where he concluded that the secular acceleration is 6″ 50‴ per century.

Mayer was clearly suspicious of Ptolemy's accounts of ancient observations. His manuscript notes frequently refer to Bullialdus's *Astronomia philolaica*, which had pointed to problems in Ptolemy's reporting. In his introduction to his published tables Mayer remarks there are good reasons to suppose that Ptolemy altered the time of some of the eclipses he reported in order to achieve agreement with his own numbers, and refers to Bullialdus's book for examples of these. Mayer was equally suspicious of Ptolemy's accounts of equinox observations. In a letter sent to Leonhard Euler on 22 August 1753, Mayer remarked

> It can be that Ptolemy perceived this error of his solar tables in his observations of the equinoxes, which are the very last of all his remaining observations; only, because he had already built his whole system upon it, perhaps he rather wanted to discard his observations than to attempt to revise his system from the outset. Since, however, no one could object to it, he pretended that the erroneous equinoxes of his tables were true and observed. There are more and newer examples of an astronomer, from too great a love for his constructions, falsifying observations. This is certain at least in Lansberg and Riccioli. How much more simply could not Ptolemy, who perhaps did not imagine that one would ever be able to disclose his deception through more accurate observations, have fallen into error. (Forbes 1971, p. 75)

Mayer made similar remarks about Ptolemy's reporting of these observations in paragraphs 26–33 of his unpublished *Vorlesungen über Sternkunde* (Forbes 1972, pp. 83–86), albeit without the reference to Lansberg and Riccioli. With this in mind, Mayer expressed the hope that users of his tables will not object that calculations of the times of one or two of the eclipses reported by Ptolemy are off by more than half and hour.

In analysing the eclipses reported by Ptolemy to have been observed in Babylon, Mayer's notes reveal that he took Babylon to be at a latitude of 32° 30′ north and a longitude 2 h 46′ east of the Paris meridian. In his analysis of the eclipses seen at Alexandria, Mayer took the longitude difference between Paris and Alexandria to be 1 h 52′. This implies a difference in longitude between Alexandria and Babylon of 54 min, somewhat above Ptolemy's estimate of 50 min. Mayer gives no indication as to how he has obtained these co-ordinates, which are considerably more accurate than those adopted by either Dunthorne or Lalande (modern determinations place Babylon at a latitude of about 32° 33′ north and a longitude about 2 h 48′ to the east of Paris).

Although Mayer tested his table for the secular acceleration using the eclipses from Ptolemy's *Almagest*, he appears to have relied predominantly on the reports of the observations by Ibn Yūnus of two solar eclipses found in Curtius' *Prolegomena* in deducing the magnitude of the secular acceleration.

In early 1754 Mayer received from Euler copies of letters sent by the Jesuit Father Antoine Gaubil in Beijing concerning supposed references to solar eclipses in two ancient Chinese books: the *Shujing* 書經 and the *Zhoushu* 周書. Gaubil believed that the eclipse described in the *Zhoushu* must have taken place in 2128 B.C., but Mayer wrote back to Euler on 6 March 1754:

> I return my most sincere thanks for the communication of the letter of P. Gaubil; as soon as I have studied it with proper attention, I shall sent it back to you. Meanwhile, I can state so much, that according to my tables the solar eclipse of the year 2128 B.C. was invisible in the whole of China. The Chinese reports seem to me very suspect, or at least to require severe criticism. (Forbes 1971, pp. 84–85)

Mayer discussed these eclipses further in paragraphs 54–55 of his unpublished *Vorlesungen über Sternkunde* (Forbes 1972, pp. 96–97), showing remarkable caution. The great antiquity of these eclipses, if they could be confidently identified, would have provided important constraints on the magnitude of the secular acceleration, and it is to Mayer's credit that he did attempt to use them for this purpose.

Following the publication of his first lunar tables, Mayer frequently returned to the problem of the lunar theory (Wepster 2007). By the time of his final tables, published following his death by Nevil Maskelyne in both Latin and English, and which were used in computing the solar and lunar ephemerides for the Nautical Almanac, Mayer had revised the magnitude of the secular acceleration of the Moon to 9″ per century.

Joseph-Jérôme Lalande

Joseph-Jérôme Lalande was born on 11 July 1732 at Bourg-en-Bresse in France. The son of the director of the town's post office and tobacco warehouse, Lalande was first educated at the Jesuit Collège de Lyon before moving to Paris to study law. Whilst in Paris, Lalande also attended lectures by Joseph-Nicholas Delisle, and in time helped Delisle at his observatory. In 1751, Lalande travelled to Berlin to undertake measurements of the lunar parallax, and while there was welcomed into the Prussian Academy where he met Euler, Maupertuis and other mathematicians. Returning to Paris, Lalande was elected to the Paris Academy. In an eventful life, Lalande later became editor of the *Connaissance des temps* and in 1760 succeeded Delisle as astronomy professor at the Collège Royal. He died in 1807.

Lalande's 1757 paper on the secular acceleration of the Sun, Moon and outer planets brought to an end the search for an observationally derived value for the Moon's secular acceleration for the next hundred years or so. Lalande's paper, entitled "Mémoire sur les Équations Séculaires" and published in the *Mémoires de l'Académie Royale des Sciences*, reviewed the earlier studies of Dunthorne and Mayer into the Moon's secular acceleration and then proposed a value of 10″ per century for this acceleration. Lalande's value was widely accepted and became the standard used in almost all theoretical investigations of the secular acceleration until at least the mid-nineteenth century.

Lalande begins his discussion of the Moon's secular acceleration by reviewing Halley's discovery of the phenomenon and its investigation by Dunthorne and Mayer. In Lalande's opinion the two most important eclipse observations for the investigation of the Moon's secular acceleration are the two solar eclipses observed by Ibn Yūnus. Dunthorne and Mayer had known these eclipses from translations by Wilhelm Schickhard quoted in Curtius' introduction to his *Historia Coelestis.* Schickhard had translated these observations from a copy of a manuscript of Ibn Yūnus's *zīj* in the possession of Golius in Leiden (Mercier 1994, p. 197). According to Lalande, Delisle had obtained a copy of this manuscript from a M. Luloss, correspondent of the Leiden Academy, and they were hoping to have the work translated as it was known to contain further observations, though this does not seem to have happened.

The two solar eclipses observed by Ibn Yūnus were considered particularly important by Lalande because the altitude of the Sun at the beginning and end of the eclipses had been carefully observed. Lalande converts these altitudes into local times obtaining almost identical results to Dunthorne. Comparing the longitude of the Moon deduced from these eclipses with Clairaut's tables, Lalande finds clear evidence that there exists a secular acceleration and estimates its magnitude.

Lalande was wary of Ptolemy's accounts of these eclipses, remarking that "the observations which have passed through the hands of Ptolemy are suspect". However, the discrepancies between the times of the eclipses given by Ptolemy and Clairaut's tables are too great to be caused by Ptolemy's adjustments and so provide further evidence of a secular acceleration of the Moon, even though the size of this acceleration deduced from these eclipses often disagree with one another. Lalande discusses the most ancient eclipse reported by Ptolemy, since the affect of the secular acceleration will be greatest for this eclipse, and so adjustment to the details of the observation by Ptolemy will be less significant.

Ptolemy's report of the observation of the eclipse of 19 March 721 B.C. in Babylon gives the time of the eclipse in seasonal hours. In order to concert this time into equinoctial hours, it is necessary to know the latitude of Babylon. Using the map of Delisle, Lalande estimates that Babylon must be at a latitude of about 32 2/3° North. However, Lalande suspects that Babylon must have been further north than this. The eclipse of 22 December 383 B.C. took place very close to the time of the solstice, and Ptolemy's report indicates that the Sun rose at 4 h 48′. Correcting for refraction, Lalande deduces that the latitude of Babylon is 36° 10′. In order to bring the latitude down to even 33½°, Lalande notes that it would be necessary to suppose that Ptolemy gave the length of day wrong by one-quarter of an hour, or that the obliquity of the ecliptic was 24°, neither of which seem likely. In truth, the latitude of the site of Babylon is close to 32½°, very close to what Lalande had estimated from Delisle's map. It is worth remarking here that Lalande gives the incorrect date for the eclipse of 22 December 383 B.C., wrongly taking the year as 313 B.C. Although this had negligible affect on his use of the record to deduce the latitude of Babylon, it is interesting that Lalande make this mistake, especially as in Dunthorne's paper this appears as a typographical error. Does this suggest that Lalande relied predominantly on Dunthorne for his information on the eclipses in Ptolemy's *Almagest*? It is perhaps significant that the eclipse of 721 B.C. analysed by Lalande was one of the three eclipses from Ptolemy that Dunthorne reported in his paper.

Returning to the analysis of the eclipse of 721 B.C., Lalande derives the time of the eclipse at Paris by converting to equinoctial hours and correcting for the equation of time. He takes the difference in longitude between Babylon and Paris to be 2 h 32′, probably on the basis of Delisle's map; the true longitude difference is about 2 h 48′. Using Clairaut's lunar tables, Lalande calculates that the longitude of the Moon implied by the observation is too great by 1° 27′, strong evidence for the existence of a secular acceleration. The magnitude of the acceleration implied by this eclipse, not given by Lalande, would be just a fraction under 9″ per century

Lalande concludes his study of the secular acceleration of the Moon by proposing a value of 10″ per century, the same as Dunthorne. Lalande deduces his value from the two eclipses observation by Ibn Yūnus, and then tests that value against the eclipse of Theon of Alexandria. Again, it appears that Lalande simply takes the times of this eclipse from Dunthorne as he gives the times in equinoctial hours, without reporting the times as recorded by Theon.

Aftermath

Dunthorne's proof of the existence of a secular acceleration of the Moon was quickly accepted by a wide variety of scholars. Within three years of the publication of Dunthorne's paper, the secular acceleration was mentioned in a letter by the Rev. Mr George Costard, Fellow of Wadham College, Oxford concerning the date of the eclipse of Thales, published in the *Philosophical Transactions* of 1753. Costard was using Halley's tables in order to calculate the circumstances of solar eclipses visible in Asia Minor and claimed that the eclipse of 603 B.C. was the one predicted by Thales. Costard writes:

> You will see, Sir, how this agrees with what is said in the Petersburg Acts, pag. 332, which, therefore, I shall not transcribe. I shall only add, that, if any allowance is to be made for the moon's acceleration, or any other cause, the track here given, as you know, will be a little different.

In a later paper (Costard 1753b) concerning an eclipse described by Xenophon, Costard again refers to the Moon's acceleration.

Following Lalande's confirmation of Dunthorne's value for the magnitude of the Moon's secular acceleration attention shifted away from attempts to investigate the secular acceleration using ancient observations towards constructing a theoretical model that accounted for the phenomenon. After unsuccessful attempts by Lagrange, Bernoulli and others, in which the reliability of Ptolemy's reports was frequently questioned, in 1786 Laplace eventually showed that a slow variation in the eccentricity of the Earth's orbit would produce such an acceleration and his predicted magnitude (11.135″ per century, later reduced to 10.18″ per century) agreed well enough with the 10″ per century found by Dunthorne and Lalande (Britton 1992, p. 157). Thus the matter rested until the middle of the nineteenth century when Adams and Delauney both pointed out an error in Laplace's determination which took the theoretical value down to about 6″ per century. There followed an intense and at times bitter dispute between Delauney and Leverrier, Pontécoulant and others (Kushner 1989), eventually resolved with the realisation that tidal retardation was responsible for the remainder of the observed acceleration (Stephenson 1997, pp. 10–14).

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